

Akiva M. Liberman

*Editor*

# The Long View of Crime

*A Synthesis of Longitudinal  
Research*

 Springer

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Foreword by Daniel S. Nagin



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## Foreword

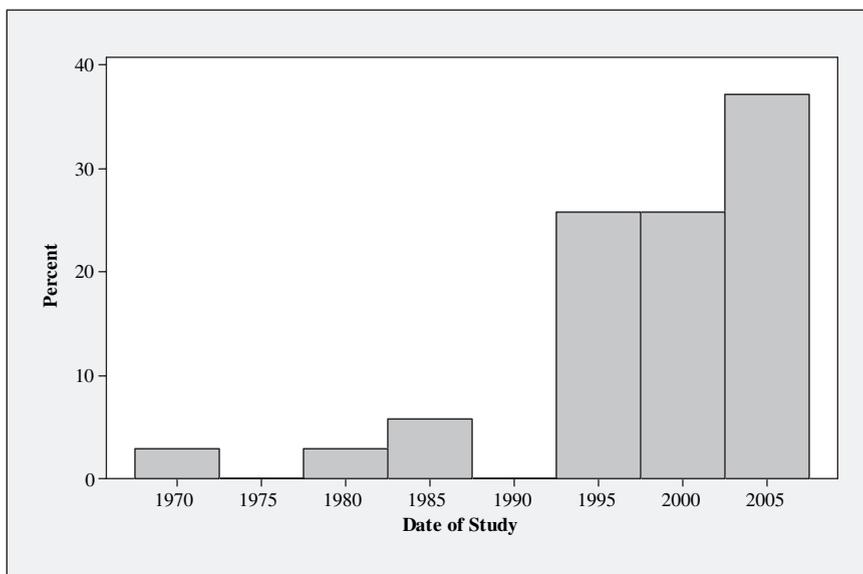
*The Long View: A Synthesis of Recent Longitudinal Studies of Crime and Delinquency* is an impressive volume. Akiva Liberman is to be commended for enticing an impressive group of scholars to contribute to the volume and the authors themselves deserve praise for the quality of their contributions.

The contributions cover diverse topics—the effect of gangs on violence, the relationship between work and crime, the effect of early childhood on the development of delinquency and so. Across chapters, however, I was struck by a number of shared characteristics. One was the recentness of the findings that were reported on. An example is the informative chapter on work and crime by Chris Uggen and Sara Wakefield. Table 1 of this chapter lists thirty-five papers using longitudinal data that address the work-crime nexus. The median date of publication is 2000. Figure 1 is a histogram of their date of publication. The histogram suggests an exponential growth in publication rate. I did not attempt a similar analysis of publication dates of papers reviewed in the other chapters but the date distribution seemed to mirror the pattern in Uggen and Wakefield.

What then explains the exponential growth in longitudinally-based analyses of crime and delinquency? I attribute it to a concomitant growth in data availability, access to and appreciation of statistical methodologies for analyzing longitudinal data, and awareness of the value of longitudinal data compared to cross-sectional data in making causal inferences.

As a rule, appendices are not very interesting which usually explains why they are appendices. The appendix to this volume, however, is an exception, at least for people such as myself who like data. The appendix lists more than sixty longitudinal data sets that were used by one or more of the studies reviewed in any one of the chapters. I was surprised that the number was so high; I hadn't fully appreciated that there were so many longitudinal data sets that tracked criminal and related behaviors. More telling, however, for the growth in longitudinal analyses was the vintage of the cohorts tracked in these data sets. About half of the data sets involved longitudinal data on individuals born in 1975 or later.

Of the data sets tracking post-1974 cohorts, twenty-two are prospective longitudinal studies. I make special note of this category of data set because the prospective studies are crucial to the advance of research on the developmental course of crime and problems behaviors. To take a “long view” of the causes and consequences of crime and delinquency requires a data set with many years and waves of data



**Figure 1** Data of publication of studies reviewed in Uggen and Wakefield

collection. The earlier of the post-1974 data collection efforts are just now reaching their prime value for such long-view analyses. We are now beginning to see and receive the full return of the more than twenty year investment in data collection that was required to assemble many of these data sets. They are a lasting testimony to the dedication of the research teams that have assembled them. I salute them. These data sets are also enormously expensive to assemble and require sustained financial support, generally from the Federal government. More on this point below.

I also single out the prospective longitudinal studies for another reason. One of the hallmarks of these studies is the richness of the measurements that are made on the psychosocial characteristics and circumstances of the subjects of the studies. The variety and subtlety of measurements opens the door to extraordinarily sophisticated and informative analyses of the relationship between early life experiences and later outcomes, as reflected, for example in the review of the relationship of early childhood to delinquent development by Daniel Shaw and Heather Gross. It also allows for comparably sophisticated and informative analyses of the contemporaneous relationship of life experiences and antisocial behavior as reflected in the reviews of Sonja Siennick and Wayne Osgood on crime and life transitions, of Chris Uggen and Sara Wakefield on work and crime, of Marvin Krohn and Terry Thornberry on gangs and violence, and David Huizinga and Kimberly Henry on arrest and crime.

Another distinctive feature of many of the studies reported upon in this volume is their sophisticated use of modern methods for longitudinal analysis. In the introduction to this volume, Akiva Liberman cites an observation of Farrington, Ohlin, and Wilson made in 1986: “Most longitudinal researchers are still essentially analyzing

their data as though it was obtained cross-sectionally.” (p. 55) In the twenty years since this pointed and just criticism matters have improved greatly. There has been an outpouring of analyses that take advantage of the longitudinal dimension of the data.

I was both pleased and honored that Akiva Liberman commissioned Alex Piquero to prepare a review of what has been learned from group-based trajectory modeling. As Piquero nicely explains and as I have elaborated in my own writings (Nagin, 2005), group-based trajectory modeling is designed to identify clusters of individuals who *longitudinally* follow approximately the same developmental course for one or more outcomes of interest. Back in 1993 when Kenneth Land and I first introduced the method (Nagin & Land, 1993), we were motivated in part by the above cited criticism of Farrington, Ohlin, and Wilson.

Piquero’s balanced and thorough review makes clear that the introduction of group-based trajectory modeling along with the ready availability of software for estimating models has spawned an outpouring of studies that take advantage of the method’s capacity for exploiting the longitudinal character of the data. Group-based trajectory modeling, however, is hardly the only method available for conducting analyses with a true longitudinal character. For example, most of the studies reviewed in Siennick and Osgood and in Uggen and Wakefield employ either hierarchical linear models or fixed effect regression models, both of which represent still other approaches for conducting sophisticated analyses that take advantage of the longitudinal dimension of the data.

Another shared characteristic of many of the studies reviewed in this volume is that they are attempting to make causal inferences about the effect of some life event such as gang membership, arrest, or work on individual propensity for crime. As the contribution of Rolf Loeber and David Farrington emphasizes, the gold standard for making causal inferences is the randomized experiment. A properly conducted experiment provides the most compelling and elegant evidence on causality available to science. However, more often than not experiments are not possible for practical or ethical reasons. We can’t, for example, randomly assign gang membership. For science and evidenced-based public policy to proceed we must also make inferences about causality from non-experimental, observational data. Thus, I underscore the argument of Loeber and Farrington on the importance of applying quasi-experimental methods to observational data from longitudinal studies.

In the past decade there have been major advances in methods for making more confident causal inferences from observational data. These methods are only just beginning to be used in criminology. An example is a 2006 paper in *Criminology* by Sampson, Laub, and Wimer that uses the method of “inverse probability of treatment weighting” developed by Jamie Robins and colleagues (Robins, Greenland, & Hu, 1999). I have been working with Paul Rosenbaum (Haviland, Nagin, & Rosenbaum, forthcoming) to combined group-based trajectory modeling with Rosenbaum’s pioneering work, done in collaboration with Rubin on propensity score matching (Rosenbaum & Rubin, 1983, 1984; Rosenbaum, 2002) for the purpose of identifying causal effects in longitudinal data. Applications include Apel et al. (2007) on the effect of work on delinquency during adolescence and Nieuwbeerta, Nagin, and Blokland (2007) on the effect of imprisonment on criminal career development.

The work of Robins, Rosenbaum, and Rubin, however, are just a sampling of newer methods for causal inference that are now available. I commend to readers, for example, the work of Imbens and Angrist (1994) on estimation of local average treatment effects, Heckman on control functions (Heckman & Navarro-Lozano, 2004), and Manski (1995) on bounding treatment effects. In the decades to come I hope to see these methods become mainstays in the arsenal of statistical methods used in criminological research.

Most of the longitudinal studies reviewed in this volume were made possible by very large investments from public sources, mostly commonly from the US Federal government. The richness and variety of findings summarized in each chapter are a testimony to the high return on that investment. For the next generation of longitudinal studies to be conducted and for analyses thereof to be reviewed in volumes such as this, adequate financial support must continue to be made available. The collection and analysis of longitudinal data on the developmental course of crime and delinquency does not come cheaply. The Federal government's alarming cut backs in research support for the behavioral and social sciences in general and for crime and delinquency in particular jeopardizes this next generation of research.

We need more not fewer longitudinal studies. Future studies should extend over longer periods of the life course by starting very early in life, preferably prior to birth, and extending for decades after birth. There are a handful of studies that have already done this but they are too few. It is also imperative that the already rich set of measurements that are currently routinely collected be expanded to include biologic and genetic data. Longitudinal studies of this type are required to keep pushing forward our knowledge of the developmental origins and consequences of crime and related problems behaviors.

Daniel S. Nagin

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**Part I**  
**Introduction and Executive Summary**

# Synthesizing Recent Longitudinal Findings

Akiva M. Liberman

## Overview

- Does arresting juvenile delinquents deter later offending or only worsen the problem?
- Does adolescent employment reduce or increase offending?
- Why does offending generally decline after adolescence?
- Does gang involvement make already bad kids even worse?
- Can we detect likely delinquents in early childhood—with the chance of early prevention and intervention?

These critical policy-relevant questions regarding adolescent offending and how to reduce it are the substantive topics of this volume. Each of these questions has been addressed by numerous studies. Why another book?—To capitalize on recent investments in longitudinal research.

These questions have been examined repeatedly through cross-sectional research, comparing people to see if, say, employed adolescents offend more or less often than unemployed adolescents. However, longitudinal research following people over time produces much stronger conclusions, but it takes much longer to conduct. Fortunately, investments in longitudinal research over the last twenty years are now being harvested, and are ripe for synthesis. Each chapter in this volume focuses on longitudinal findings regarding a different substantive issue.

Longitudinal research has the unique potential to address several long-standing questions in criminology. Questions concerning the passage of considerable time over the life course—such as whether late adolescence is the peak offending period for almost everyone, followed by a decline in early adulthood—are most directly addressed with longitudinal data, and only poorly addressed with cross-sectional data. Other questions also turn out to be conceptually longitudinal on close inspection. For example, can we identify risk factors for crime that can be changed through intervention? This may not seem inherently longitudinal. On reflection, this question concerns whether changing particular risk factors will lead to changes in criminal behavior. Questions about behavior change are most directly addressed by data on the same individuals collected over time.

Until recently, not enough longitudinal research was available to address many such questions. While cross-sectional studies have often been used to illuminate such issues, their results are generally provisional. For example, if school drop-outs are found to be more delinquent than their school-attending peers, does this mean that school attendance reduces delinquency, and/or that drop-out increases it? Perhaps. But such results may instead merely reflect “selection,” with non-delinquent youth choosing to attend school, while delinquent youth choose not to. If so, school attendance may be a symptom rather than a cause of delinquency; reducing attendance and drop-out may do nothing to affect delinquency. Longitudinal research can address such questions much more powerfully. If data collection begins before students drop out of school, a longitudinal study can address the chicken-and-egg problem of whether delinquency precedes or follows school drop-out, whether delinquency increases after dropping out, and how future drop-outs differ from their peers. These methodological issues are discussed in many chapters in this volume. Recent analytical developments have also improved the potential for longitudinal data to address causal questions (e.g., Haviland & Nagin, 2005).

### *The Present Volume*

In view of the recent growth in longitudinal studies of crime and delinquency, and the many recent reports from these studies, the time is ripe to take stock of what has been learned. Because of the complexity of each of these studies, synthesizing across studies is a formidable task. The present volume approaches this problem by narrowing the topics of each review, to allow a level of detail all but impossible when broad topics are addressed.

### **Recent Growth in Longitudinal Research**

In 1986, Farrington, Ohlin, and Wilson surveyed the state of knowledge and concluded that too little longitudinal research had been conducted on crime, and that even when longitudinal data were collected, they were often analyzed cross-sectionally.

With the exception of a few experiments on the effects of penal treatments . . . few criminological researchers have taken advantage of the potentialities of longitudinal surveys . . . Most longitudinal researchers are still essentially analyzing their data as though it was obtained cross-sectionally . . . (p. 55)

At the time, they found eleven prospective longitudinal surveys with information about crime and delinquency, and which included at least two interviews with subjects, spanned at least five years, and had samples of hundreds or more. Farrington et al. (1986) thus called for more investment in prospective longitudinal studies of crime, as did the National Research Council’s Panel on Research on Criminal Careers (Blumstein, Cohen, Roth, & Visher, 1986).

These calls for additional investment in longitudinal research were not without controversy. Gottfredson and Hirschi (1986, 1987; Hirschi & Gottfredson, 1986) asserted that advantages to longitudinal research were insufficient to justify their expense. Methodologically, they argued that longitudinal research was insufficient on its own to allow causal inference. Substantively, they also argued that longitudinal research was unnecessary, based on their theoretical assumptions that the age-crime curve was universal and invariant, that the propensity for criminal behavior was simply a function of self-control, and that self-control was set early in childhood and largely stable thereafter (Gottfredson & Hirschi, 1990). Perhaps because it required accepting these assumptions, which themselves can only be tested with longitudinal data, their argument for the adequacy of cross-sectional research was not heeded.

In the ensuing years, considerable investments have been made in longitudinal criminological research. Investments by the Department of Justice include the Office of Juvenile Justice and Delinquency Prevention's (OJJDP) support of the *Seattle Social Development Project*, and the three *Causes and Correlates* studies in Rochester, Pittsburgh, and Denver. The National Institute of Justice (NIJ) has supported the *Project on Human Development in Chicago Neighborhoods*, as well as reanalyses of older data sets. OJJDP and NIJ have jointly funded the ongoing *Pathways to Desistance* study of serious adolescent offenders. Other federal agencies, including the National Institute on Drug Abuse and the National Institute of Mental Health, have also supported these and many other longitudinal studies. Private foundations have also supported longitudinal research. In addition to many U.S. projects, studies have been conducted in other countries including Canada, New Zealand, England, Germany, and the Netherlands. And these are only the studies specifically focused on the development of crime and delinquency. Some studies with broader goals have also included crime and delinquency measures, such as the *National Longitudinal Survey of Youth*, and the *National Longitudinal Survey of Adolescent Health*. In short, tens of millions of dollars have been invested in longitudinal research on crime and delinquency.

This volume reports on findings from over 60 longitudinal data sets, including about 20 international samples. Most of these studies examine people born since the 1970s. The reviews in this volume draw on about 200 empirical reports of these studies, and most been published since 2000. Some basic features of these studies and, are summarized in the Appendix. Table 1 crosslists the longitudinal datasets by the reviews; Table 2 summarizes basic features of the studies including characteristics of the samples, the timing of data collection, and the birth years of the study subjects.

These longitudinal studies have been enormously productive. Hundreds, if not thousands, of papers have been published. The recent yield has been tremendous, so that the vast majority of papers reviewed in this volume were published since 1990, and the majority since 2000. Yet, as can be seen in the Appendix, most of these longitudinal samples have only been used to address one or two of the questions examined in this volume. Although already enormously productive, the longitudinal data already collected clearly contain much untapped potential.

Many longitudinal studies have also produced one or two volumes. Each volume often has a distinctive focus, as illustrated by some of the most recent single-study volumes:

- Werner and Smith’s (2001) *Journeys from Childhood to Midlife: Risk, Resilience, and Recovery* from their study of the 1958 birth cohort of Kauai;
- Laub and Sampson’s (2003) *Shared Beginning, Divergent Lives*, following the Gleucks’ delinquent sample from Boston into their 70s;
- Thornberry, Krohn, Lizotte, Smith and Tobin (2003) *Gangs and Delinquency in Developmental Perspective*, from the Rochester Youth Development Study;
- Moffit, Caspi, Rutter and Silva’s (2003) *Sex Differences in Anti-Social Behavior*, from their study of the 1958 birth cohort in Dunedin, New Zealand;
- Piquero, Farrington and Blumstein’s (2007) *Key Issues in Criminal Career Research*, from the Cambridge study of South London men;
- Loeber, Farrington, Stouthamer-Loeber and White’s (in press) *Violence and Serious Theft: Development and Prediction from Childhood to Adulthood* from the Pittsburgh Youth Study.

What is the combined yield of these studies? The sheer volume of recent output can be daunting, and the challenges to reading across studies are many. Although many U.S. projects were initiated in the 1980s with participants born in the 1970s, such as the *Causes and Correlates* studies in Pittsburgh, Rochester, and Denver, earlier projects include studies of subjects born in Boston in the 1930s, in Wisconsin in the 1940s and 1950s, and in Philadelphia and Kauai in the 1950s, not to mention non-U.S. studies. Some studies include males only, while others include females; some study whites only, others diverse ethnic groups; some study general population samples, others high-risk samples, and yet others study offender samples. Studies enroll subjects at different ages, follow them for different lengths of time, and collect data at differing intervals. Different measures and instruments are used, and data include different mixes of self-report data, official data, and reports from informants besides the study subjects themselves (e.g., parents). Finally, different studies focus on different questions, approach them from different theoretical orientations, and use different analytic methods.

Taken as a whole, the literature can be quite bewildering, and the sum may seem less informative than the parts. Even for a given substantive topic, synthesizing findings across studies is not a trivial task. As a result, summaries across studies are often at a high level of abstraction. With such abstraction, summaries sometimes seem to merely affirm conventional wisdom (cf. Huizinga, Weiher, Menard, Espiritu, & Esbensen, 1998, *Some Not So Boring Findings from the Denver Youth Survey*), and the complexities of findings are lost.

But in fact, the findings are often complex and fascinating. The answers to apparently simple questions may involve contingencies among several interacting variables. How important are biological risk factors in early childhood on adolescent misbehavior? The result may depend on a child’s social environment (Shaw, this volume). Does youth employment during high school prevent crime? Several studies suggest that intensive employment (more than 20 hours per week) has different

effects from less intensive employment, and that intensive employment actually increases crime. Yet that conclusion may itself be too simple. The effect of intensive employment may depend on the developmental stage of an adolescent, the nature of the work experience, and the youth's attachment to school (Uggen & Wakefield, this volume).

Such complex and contingent findings often frustrate attempts at quick summaries, and require discussing details about particular studies and particular analyses. The reviews in this volume were given room for such complexity and detail.

## **The Unique Potential of Longitudinal Research**

In a recent review of 100 years of delinquency research, Laub (2002) concludes that empirical findings from studies taking a multi-factor approach have been quite consistent since Healy (1915). Multi-factor studies have consistently identified a wide array of factors associated with delinquency, including individual biological factors, family factors, community factors, and delinquent peers. Different factors, such as poverty or intelligence, have been emphasized at different periods, but Laub concludes that these cycles of emphasis are largely independent of the empirical findings, as are the cycles of theoretical concerns.

Why then should we expect anything new from recent longitudinal studies? Advances in quantitative analytic methods, especially for longitudinal data, are one reason. Modern analytic methods allow much greater headway to be made in teasing apart the relative contributions of multiple factors (i.e., correlates or risk factors) associated with crime over the life course, in moving toward stronger causal inferences, and in examining patterns of offending over time. The effects of these advances are exemplified in Sampson and Laub's (1993) modern reanalysis of the longitudinal data collected by Sheldon and Eleanor Glueck in the 1940s and 1950s (Glueck & Gleuck, 1950, 1968). While some of Sampson and Laub's conclusions echo the Gluecks's own conclusions, they also diverge in important ways. A key conclusion for Sampson and Laub, which depends heavily on modern analytic methods, is that the effects of adult social bonds in promoting desistance from crime cannot simply be reduced to preexisting differences in self-control or to earlier effects of family.

When analytic advances are combined with the many longitudinal studies launched over the last twenty years, many long-standing questions can be addressed. Criminology has long been concerned with questions that are obviously longitudinal. Is there one general pattern of offending over the life-course, and if so, why? Which early risk factors, if any, strongly predict adolescent delinquency? How well can we predict future criminal behavior from current measurement of risk factors?

There are also other questions common in criminology that may not appear longitudinal at first glance, but turn out to be so on closer inspection. For example, research on risk factors for crime seems to involve two related but separable questions. One question concerns identifying at-risk individuals, perhaps for early intervention. This question concerns differences between people and may draw

directly upon cross-sectional analyses. The second question involves identifying dynamic risk factors for violence and crime, as potential points of intervention. This question too has often been approached through cross-sectional data. On reflection, however, the question of interest is whether individuals' criminal behavior would be changed by changes to those risk factors. The most direct examination of what leads individuals to change comes from following people over time, in longitudinal studies. Recent analytic innovations increasingly allow multiple-factor studies to progress from identifying correlates of delinquency to assessing the causal role of these correlates.

Gang involvement provides a concrete example. Knowing that teens involved with gangs commit more crime than other teens is a finding of differences between people. But for policy purposes, the question of interest is whether the teens involved with gangs would commit less crime if they were not gang-involved. If these same youth commit more crime during periods when they are gang involved than at other times, this would provide much stronger evidence suggesting that preventing gang involvement should reduce crime. (Such a within-person finding is only possible if gang involvement changes over time for individuals. Longitudinal findings suggest that gang involvement is surprisingly intermittent; see Krohn & Thornberry, this volume.)

For questions such as these, cross-sectional data can be thought of as providing provisional answers. The extent to which the cross-sectional correlates of crime, which primarily identify between-person differences, reasonably approximate the longitudinal within-person correlates of criminal and violent behavior is an empirical question yet to receive much attention. Farrington, Loeber, Yin and Anderson (2002; see Loeber & Farrington, this volume) provide a simple illustration. Using data from the Pittsburgh Youth Study, they examine the relationship between ten reasonably continuous risk factors for delinquency. All ten show statistically significant between-person correlations (for over 400 subjects, averaged over seven waves of data). These results were then compared to within-person correlations (over seven waves of data, averaged over the respondents). When contemporaneous correlations were computed, only four factors remained significant. When delinquency outcomes were lagged by one data wave (either 6 or 12 mos.), to better approximate causal ordering, only two risk factors remained significant, and one other reemerged as significant. (One other backward lagged correlation also emerged as significant.) Such results cast some doubt on the ability of cross-sectional analyses to approximate longitudinal results.

Of course, we would be wise to remember that even lagged within-person correlations can be spuriously produced by third variables, and even by reverse causal processes. Longitudinal data are no guarantee for correct causal analysis. Nor is the temporal ordering of measurement equivalent to the causal ordering of factors (e.g., Gottfredson & Hirschi, 1987), given stability in the predictor or outcome variables over time. A provocative illustration is provided by Kaplan, Tolle and Yoshida (2001). Substance use and violence are generally assumed to be positively related, and usually show positive correlations. Perhaps, however, substance use sometimes decreases violence, if illegal drugs are used to alleviate distress that would otherwise lead to violence (i.e., "self-medication"). Kaplan et al. explored this question

using two waves of longitudinal data, separated by 3 years, on over 2,000 Houston adolescents. Confirming prior findings, substance use and violence showed positive cross-sectional correlations. Similarly, substance use and violence were correlated across waves, meaning that they show some stability. However, once contemporaneous associations and between-wave stabilities were controlled in a structural equation model, Kaplan et al. found a *negative* lagged effect, so that substance use at wave 1 was associated with less violence at wave 2.

As this example illustrates, merely collecting longitudinal data may only be the first step in addressing many questions. Analyzing the rich longitudinal data collected in prospective longitudinal studies can be daunting. To maximize the potential of longitudinal data will require the continuing development and utilization of sophisticated longitudinal analytic strategies (see McCord, 2000; Nagin, 2005; Sampson & Laub, 2005). Although such developments are not the main subject of this volume, they are discussed in many chapters, and particularly by Piquero.

## Recent Efforts to Take Stock of Longitudinal Findings

What have we learned? Are findings consistent across studies? Occasionally, equivalent analyses of several data sets can be conducted to examine their consistency, although this can be quite difficult when data sets were not planned for joint analysis. For example, Broidy et al. (2003) examined the developmental link between childhood physical aggression and adolescent violent and non-violent offending, and how this varies by sex. Fairly consistent results were found from analyses conducted on data from six recent longitudinal studies, two each from Canada, New Zealand, and the U.S. Across studies, males' problem behavior generally showed continuity from childhood to adolescence, especially when childhood problem behavior was violent. In contrast, females showed little continuity in problem behavior from childhood to adolescence. This sex difference in continuity of problem behavior, with little continuity for girls, is somewhat surprising. That it emerged from analyzing six longitudinal samples makes it considerably more compelling. We would surely benefit from more studies conducting such parallel analyses, but such projects are daunting; Broidy et al. (2003) involves fourteen authors at twelve institutions. Instead, we must usually assess consistency across longitudinal studies by synthesizing the published literature.

Two general approaches have been used to synthesize the empirical literature.<sup>1</sup> The first is to summarize key findings for each longitudinal study, across topics, and then compare these key findings across studies. For example, Thornberry and Krohn's (2003) edited volume *Taking Stock of Delinquency: An Overview of Findings from Contemporary Longitudinal Studies* contains summary papers of key

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<sup>1</sup> An alternative approach to synthesis is through theories, on the assumption that contemporary theories integrate recent findings. Farrington (2005) approaches the problem in this way in his edited volume *Integrated Developmental and Life-Course Theories of Offending*.

findings from seven separate studies, followed by Thornberry and Krohn's review of these summaries. Along with the commonalities that result from a developmental focus, the diverse interests, theoretical perspectives, and foci of the different studies are well-illustrated.

The other approach to synthesis is to choose particular topics, and then attempt to summarize across studies. Such syntheses are rare. Notable recent examples include Rutter, Giller, and Hagel's (1998) *Anti-Social Behavior by Young People*, and Loeber and Farrington's (1998, 2001) edited volumes on *Serious Violent Offenders (SVO)* and *Child Delinquents*, respectively, which were produced by OJJDP study groups. The SVO volume also included two original empirical syntheses of the findings on risk factors (Hawkins, Herrenkohl, Farrington, Brewer, Catalano, & Harachi, 1998; Lipsey & Derzon, 1998), and a meta-analysis concerning effective intervention for serious violent offenders (Lipsey & Wilson, 1998).

## The Present Volume

The present volume, too, chooses specific topics and then examines findings across studies. In view of the difficulties in comparing findings across studies, narrow topics were chosen for review, so that findings could be discussed at a level of detail that would be difficult for broader reviews. This also allows conclusions that are nuanced or complex, and works to counter the tendency to reach conclusions at only a high level of abstraction. But this also necessarily means that one volume cannot comprehensively cover recent longitudinal findings. This set of reviews summarizes some of that yield, while also illustrating the enormous untapped potential that remains.

In choosing topics for review, policy relevance was a key consideration. In addition, because the goal was explicitly to read *across* longitudinal studies, we attempted to choose topics with a critical mass of studies. When one or two studies seem to dominate the current literature, it was not seen as appropriate here. For example, the Project on Human Development in Chicago Neighborhoods was designed to combine a longitudinal study of youth with a study of neighborhoods. Longitudinal findings are now emerging concerning the role of neighborhoods in the development of crime and delinquency (e.g., Raudenbush, Johnson, & Sampson, 2003; Sampson, Morenoff, & Raudenbush, 2005; see Liberman, 2007). While of considerable current interest, the few other studies addressing this topic to date (e.g., Elliott, Menard, Rankin, Elliott, Wilson, & Huizinga, 2006) suggest that a cross-study review is not yet warranted.

The process of choosing topics for review was begun at a workshop convened in October, 2004 by the National Institute of Justice. Several preliminary reviews were presented, and about twenty prominent researchers involved in longitudinal research were invited to comment on this approach to synthesis, and to suggest topics worthy of reviews. Participants generally showed great enthusiasm for the work of synthesizing recent longitudinal findings, and generated a list of about thirty candidate topics. Several of these topics did not yet seem to

have a critical mass of primary studies worthy of review. Especially for such topics, considerable enthusiasm was expressed for promoting collaboration among researchers to conduct parallel analyses of several different longitudinal data sets. Although beyond the scope of this volume, NIJ and the National Consortium On Violence Research have since conducted workshops aimed at encouraging such collaboration.

## Overview

A major focus of this volume is adolescence and delinquency, as in much criminological research from a developmental or life-course perspective. This volume examines several adolescent experiences directly, including employment, gang involvement, and first arrests. In addition, adolescence is viewed from several other vantage points: from early childhood; from a focus on the end of adolescence; and as situated in the longer context of criminal careers.

The reviews are organized into two sections. In *Crime and the Life Course*, basic research is reviewed on the developmental course of delinquency and some primary life-course experiences that shape it. This section begins with a long view, asking how research on criminal careers has been informed by group-based trajectory modeling (Piquero). The three following chapters then examine pivotal developmental experiences: early childhood experiences (Shaw and Gross), gangs (Krohn and Thornberry), and transitions to adult roles (Siennick and Osgood). This basic research has clear policy implications in identifying important points of intervention for reducing crime and violence, but the literature reviewed does not itself tell us *how* to intervene at those points.

In *Intervening in the Life-Course of Crime*, more applied questions are examined. The literature on the effects of employment on crime is reviewed, with a particular emphasis on adolescent employment (Uggen and Wakefield). While employment is clearly normative for adults, whether and under what conditions employment for adolescents should be promoted remains a critical policy question. The effects of arrest and criminal-justice sanctioning on recidivism are then reviewed (Huizinga and Henry). Does formal criminal justice intervention nip delinquency in the bud, or label and stigmatize a small subset of adolescent offenders? Each chapter reports on a complex landscape of findings, where the effects of these naturalistic interventions vary, and may depend on details of the youth involved as well as details of the intervention.

In the final chapter, Loeber and Farrington consider future directions in longitudinal research, and how the yield of longitudinal studies might be optimized.

Taken together, this volume illustrates how recent longitudinal studies are strengthening the evidence regarding long-standing questions. While data for longitudinal research can take painfully long to collect, we are now at the fortunate moment when we are reaping the fruit of data collected over the last 20 years. These studies are producing findings at an increasing rate, so that most of the findings reported in this volume were published since 2000, and more analysts are utilizing

advanced methods. We can thus reasonably anticipate that compelling longitudinal evidence addressing many additional long-standing questions will reach critical mass in the near future.

## Summary of Chapters

### *Crime and the Life Course*

#### Trajectories

In Chapter 2, Piquero examines what has been learned about the nature of criminal careers through the use of a relatively new method, group-based trajectory modeling. Piquero identifies over 80 papers using the method; most have been published since 2000. Piquero first briefly reviews the theoretical literature that motivated development of the method, which concerns the controversy about the relationship between the aggregate age-crime curve and the developmental course of individual offending. Piquero then provides an overview of the method itself, and the studies that have applied the method.

Piquero finds that while many details of trajectory findings depend on details of samples and data, several commonalities emerge. Studies typically find a small number of trajectories. While many offenders follow the traditional age-crime curve, with offending peaking in late adolescence and then declining, most studies also find groups of offenders with other developmental trajectories. Many studies find groups of offenders who begin offending early and persist beyond adolescence, and many also find an offending trajectory that starts late but that persists beyond adolescence.

Finding groups of offenders with different offending trajectories may suggest trying to classify offenders for intervention purposes. But a critical feature of the method is that it classifies people *retrospectively*, after we know about their offending, and not *prospectively*. Studies combining trajectory analyses with risk factors analyses may generate more direct implications for policy and practice. The trajectory method has also recently been combined with methods for improving causal inferences from non-experimental data. While Piquero reviews some of these findings along the way, systematically reviewing them is beyond the scope of the review. Piquero ends with questions for future research, including several methodological questions as well as substantive questions the trajectory method can fruitfully address.

#### Early Childhood

In Chapter 3, Shaw and Gross examine early childhood and the development of delinquency. Many have hypothesized that early childhood factors will be related to adolescent antisocial behavior, and some have argued that early childhood is a

unique developmental window for both risk and opportunities for intervention. Yet, the empirical literature has been fairly sparse until recently, because few studies followed subjects from early childhood to adolescence. The relevant literature is now growing considerably, as studies initiated in the 1980s and 1990s mature. Most of the papers reviewed were published within the last decade.

Shaw and Gross reach a mixed conclusion about the first year of life. Later anti-social behavior only begins to be predicted by misbehavior at age 2, and more so at 3—but not by misbehavior in the first two years. Nonetheless, later anti-social behavior is better predicted by risky social contexts during the first year than during preschool or early school age. They find several early childhood risk factors for anti-social behavior in adolescence, including characteristics of the prenatal environment (e.g., maternal age and maternal smoking and drinking), and hostile, rejecting and abusive parenting. Other early risk factors have also been suggested, and most are similar to demonstrated risk factors in later childhood, but to date methodological limitations preclude firm conclusions.

A pattern of particular note concerns interactions in which child biological risk factors are particularly risky for children who live in a risky family context. Perinatal and delivery complications, prenatal exposure to substances, and a genetic marker (MAOA) each seem to be most strongly associated with anti-social behavior for children who also develop in risky social contexts. This suggests that interventions that improve the social context may mitigate even biological risk factors. Yet, the literature on adolescent effects of early childhood interventions is particularly sparse; to date, Shaw and Gross find only one early childhood intervention with demonstrated effects at adolescence.

## Gangs

In Chapter 4, Krohn and Thornberry review longitudinal research on gangs. A set of cross-sectional findings about gangs has been subject to considerable debate. For example, it is well documented that gang members offend more actively than other youth. Does this imply that gangs increase their members' criminal behavior, by facilitating delinquency or enhancing preexisting delinquent tendencies, or does it result merely from already-delinquent adolescents congregating together in gangs? Cross-sectional data are hard-pressed to convincingly answer such questions, but Krohn and Thornberry report that longitudinal studies have been more conclusive. Longitudinal studies in Rochester, Denver, Pittsburgh, Buffalo, Seattle, Montreal, and Norway have found that gangs do increase their members' offending during periods of active gang membership. This conclusion is also bolstered by the recent application of newer and more complex analytic techniques.

The extent to which gangs increase delinquent behavior sometimes varies for different youth, although results have varied across studies. An ancillary finding from the longitudinal data is that individuals' active periods of gang membership are surprisingly short or intermittent. For example, most street-gang members in the Rochester and Denver studies, and about half of gang members in the Pittsburgh study, report being members during only one year.

Longitudinal studies have also advanced the study of risk factors for gang membership. Although too few datasets have yet been utilized, and they vary in their particular measures, some important findings have emerged. First, risk factors are consistently found across several domains, such as family, school, peers, and individual characteristics. Several risk factors emerge consistently including prior delinquency, involvement with deviant peer networks, and low parental supervision. At the same time, some other hypothesized factors have *not* emerged as risk factors, including parental warmth, family poverty, and self-esteem. Second, there is no key risk factor or two that massively increase gang involvement. Rather, there are many risk factors for gang involvement that accumulate, so that gang members tend to have multiple risks. This implies that there will be no silver bullet to preventing gang involvement.

Longitudinal studies should also be able to inform us about the short- and long-term consequences of gang membership. Early work on such questions suggests that gang membership interferes with transitions to adult roles and has long-term negative consequences. The methodological challenge is to isolate the causal effect of gang membership from the effects of background risk factors. Early work suggests some of these effects may be attributed to gang membership *per se*.

Krohn and Thornberry conclude that effective gang intervention and gang prevention programs would yield a large societal benefit. However, no gang prevention or intervention program has yet been rigorously shown to be effective. Krohn and Thornberry thus call for investment to develop and rigorously test gang prevention and intervention programs, building on what has been learned about gang membership and its risk factors.

## **The Transition to Adulthood**

In Chapter 5, Siennick and Osgood examine the effects on criminal behavior of adolescents' transitions to adult roles. They motivate their review by a discussion of the typical age-crime curve, in which delinquent behavior peaks in late adolescence and declines with early adulthood. Could transitions to adult roles be responsible for the decline in early adulthood? Siennick and Osgood also contrast several theoretical perspectives with differing assumptions about early adulthood and desistance from crime. For example, for some theories, adult roles reduce offending primarily by reducing time spent with peers, whereas for others the key mediator is the quality of bonds to conventional society fostered by adult roles.

In reviewing the empirical evidence, Siennick and Osgood emphasize studies' methodological features. They review different approaches to isolating the causal impact of particular role transitions in the context of many changes occurring. In addition, some adolescents choose (or are chosen) to marry, become parents, or find jobs, and those adolescents probably differ in important ways from those who don't. These individual differences can themselves be associated with offending. Longitudinal studies, and particularly studies of within-individual change, can also help distinguish such "selection" effects from causal effects.

The literature is reviewed concerning several different role transitions associated with adulthood, specifically marriage, parenthood, student status, employment, and living arrangements. Among these transitions, marriage has shown the largest and most consistent effect in inhibiting offending. These findings emerge from both cross-sectional and longitudinal studies, with both general and offender samples, and with several self-report outcomes and arrest. The beneficial effect of marriage, however, may depend on several contingencies. Most importantly, perhaps, marriage to a criminal spouse may increase rather than inhibit offending. Surprisingly, several studies find that cohabiting with romantic partners is associated with increased offending.

Parenthood is also expected to reduce crime, for a variety of reasons. Nonetheless, parenthood has *not* been found to inhibit offending. However, most of the available studies are cross-sectional, and too few studies have examined parenthood to yet warrant firm conclusions. Living away from parents seems to be conducive to college students' alcohol and drug use, but the relationship to non-drug offending has not yet been examined. School enrollment/attendance itself seems to inhibit offending, especially in samples of released offenders; general population samples have been studied infrequently.

Beneficial effects of employment in reducing crime have been found in studies using varying methods. This effect is sometimes limited to some crimes, to offenders of particular ages, or other contingencies, but these contingencies themselves vary across studies. Additional research is, therefore, warranted to further specify the relationship between employment and crime, and its contingencies.

Although findings are emerging on transitions to adult roles, Siennick and Osgood report that the research base is still relatively small, and that the methods used vary considerably. As a result, we do not yet know whether varying results are due to methodological differences. Siennick and Osgood also report that studies have not yet been related back to important theoretical issues, either for distinguishing between different theoretical perspectives, or illuminating our general understanding of the age-crime curve.

## *Intervening in the Life-Course of Crime*

### **Work and Crime**

In Chapter 6, Uggen and Wakefield review the research on work and crime, with a focus on two policy-relevant contexts, adolescent work and work for (ex-)offenders. They first review theories concerning the relation between work and crime. Most criminological theories predict that work will reduce criminal activity, but several theories make counter-predictions. Control theory predicts that both unemployment and crime will result from low self-control, but that their association will not be causal; routine activity theory adds the prediction that unemployment may increase guardianship over property, thus sometimes reducing crime; life-course approaches predict that precocious entry into work may interfere with age-appropriate roles such as school involvement, and ultimately have detrimental effects. Moreover, the

work-crime relationship is sometimes hypothesized to vary depending on particulars of the jobs and of those employed. Finally, there is ample reason to expect that criminal involvement may affect employment as much as the reverse.

In the face of such complex possibilities, longitudinal data can help disentangle the temporal order of events underlying any observed relationships. When used with strong analytic methods designed to isolate the causal role of work, longitudinal studies have considerably advanced our knowledge concerning the relation of work and crime.

Studies find work to be most beneficial to adults. For young adults, aggregate-level research also suggests that unemployment is associated with crime. For adolescents, the picture is more complex. A variety of studies, including cross-sectional studies, have suggested that the effect of work varies, and that intensive work (over 20 hours/wk) may be detrimental and conducive to delinquency. More recent research, however, questions that relationship. Adolescents who work intensively differ from their peers in ways that partly explain the cross-sectional relationship. Moreover, longitudinal studies suggest that the relationship of intensive adolescent employment to delinquency may be contingent on job quality, the relationship of jobs to school, the ways that particular jobs affect unsupervised time, and the nature of work peers. While longitudinal studies have advanced the state of knowledge, Uggen and Wakefield report that, to date, longitudinal data have been underutilized and often analyzed cross-sectionally. Because results have also varied with the methods used to control selection bias in observational studies, Uggen and Wakefield also call for more experimental research.

Research with ex-offenders or at-risk populations, too, has not uniformly found employment or job training to reduce offending. Research does show, however, that a history of incarceration interferes with work. Some experimental studies of job programs have found that work helps reduce offending especially for high-school dropouts or older offenders. Ironically, however, Uggen and Wakefield report that many job programs for returning offenders are restricted to relatively young offenders (e.g., below age 25). In addition, most of the jobs available in these programs are not of high quality. In view of the complexities associated with the relationship between work and crime, and because of the salience of employment for ex-offenders, Uggen and Wakefield call for further investment in methodologically strong research.

## **Arrest and Deterrence**

In Chapter 7, Huizinga and Henry examine the effect of arrests on subsequent offending. They situate their review in the context of the competing theoretical perspectives of deterrence theory, which predicts that criminal justice sanctions will reduce offending, versus labeling theory, which predicts that criminal justice sanctions may actually be criminogenic and increase subsequent offending.

The vast majority of studies fail to find specific deterrent effects of arrests, finding either that arrests have no effect or are associated with more subsequent offending. This pattern of findings refutes a simple prediction that arrest has an appreciable specific-deterrent effect.

Do arrests, then, lead to more reoffending? The pattern of findings is suggestive, but not yet conclusive. The key problem is that, in general, the same individuals who are likely to get arrested are also likely to reoffend—even absent any effect of the arrest itself. Our confidence that arrests actually *cause* more reoffending depends on how well the non-arrested comparison group can be shown to have been as likely to reoffend as those arrested. This is hard to do convincingly. Excepting true experiments with random assignment, the greatest potential may lie in analyses of prospective longitudinal data that include individuals' pre-arrest self-reported offending histories, relevant psychological variables, and others. Too few prospective longitudinal studies have yet examined this issue, using strong comparisons, for us to conclusively accept the apparent labeling findings. The one true experiment that has been conducted did find more re-arrests among those formally processed than those counseled and released, but found no effect on self-reported offending.

Huizinga and Henry also briefly review the effects of criminal justice sanctions beyond arrest; that literature, too, fails to support deterrence predictions. They then review other effects of arrest and sanctions. Consistent with a labeling perspective, arrests seem to increase high-school dropout, and arrests are consistently associated with reduced subsequent employment.

Finally, they consider how being arrested affects one's *perceptions* of the risk of being arrested. The few available studies suggest that being arrested does increase one's perceived likelihood of being arrested. This perceptual effect is a key mediator of specific deterrence predictions, and so this finding contrasts sharply with the failure to actually find deterrent effects. Thus, Huizinga and Henry end by discussing possible reasons why the specific deterrent hypothesis may not be borne out.

## ***Future Directions***

### **Advancing Knowledge about Causes**

In Chapter 8, Loeber and Farrington consider how longitudinal studies can best be used to advance knowledge about causes of delinquency, which is a primary goal of longitudinal criminological research study. Longitudinal data is important but not itself sufficient to establish causality.

Loeber and Farrington first consider evidence for causes that can be obtained from true experiments with random assignment. After reviewing the general advantages of experiments, they argue that considerable advances could be made by embedding experiments within longitudinal studies, with prospective data collected over several years before, and several years after, an intervention. They argue the prospective longitudinal studies and experimental studies have complementary strengths and weaknesses, and discuss advantages and possible problems of combining experiments with longitudinal studies.

They review several notable experiments with lengthy longitudinal follow-up. Such data allow us to see trends on post-experimental outcomes, to distinguish short- from long-term effects, and to distinguish effects that diminish with time

from those that persist or even increase with time. For example, the Perry Preschool Project's pre-school intervention generated academic effects at age 4–5, which washed out by ages 8–9. Yet, at ages 15–19 and later at 27, the treatment group had been arrested only half as often as the controls. No longitudinal-experimental study yet has collected longitudinal pre-experimental data for several years. Such data would situate an intervention in the context of the *development* of participants' delinquency, and allow us to verify that comparison groups were equivalent, not only immediately preceding the intervention, but also in trends preceding the intervention.

Turning to the quasi-experimental context, Loeber and Farrington distinguish between evidence drawn from comparing different individuals' delinquent behavior, and those from comparing delinquent behavior of the same people over time and circumstance. While the latter are generally the goal of research into causes of delinquency, most analyses have examined the former. Little research has compared between- and within-individual results. Their own research finds that the two sources of evidence are not at all equivalent.

Loeber and Farrington also consider several other issues largely neglected in criminology's examination of causes, such as whether desistance and cessation of offending have the same underlying causal factors as onset and increases in offending. They end by enumerating several strategies to invigorate the study of the causes of delinquency.

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**Part II**  
**Crime and the Life Course**

# Taking Stock of Developmental Trajectories of Criminal Activity over the Life Course

Alex R. Piquero

**Abstract** Worldwide, criminologists have long been interested in the longitudinal patterning of criminal activity. Recently, methodological and statistical advances have “caught up” with longitudinal data and have provided criminologists with a unique window within which to study, document, and understand developmental trajectories of criminal activity. One such technique, the trajectory procedure, allows researchers to study how criminal activity changes over time in a group-based framework. This methodology is well suited for studying crime over the life course because there may be different groups of offenders, their offending trajectories may exhibit different shapes at different ages, and they may be differentially affected by distinct factors. This paper presents an overview of the trajectory methodology, outlines its strengths and weaknesses, and summarizes key conclusions of the well over 80 studies that have used this technique. It concludes by pointing to several future research directions.

**Key words:** trajectories, crime, life-course, developmental

## Introduction

Charting the course of development over long periods of time occupies a central place in the social and behavioral sciences, and criminology is no exception. In particular, the onset, continuation, and cessation of criminal activity has occupied the imagination of criminologists since the beginning of the discipline (Kobner, 1893; Shaw, 1930; von Mayr, 1917; von Scheel, 1890), through its ascendance in the 20th century (Wolfgang, Figlio, & Sellin, 1972), and continues in force today (Blumstein, Cohen, Roth, & Visher, 1986; Piquero, Farrington, & Blumstein, 2003, 2007). With this interest in the natural history of offending, researchers have relied on longitudinal studies which follow individuals for lengthy periods of time. A key part of all longitudinal studies is the repeated measure of a behavior of interest. With the promise of increased knowledge about criminals and their crimes (McCord, 2000:113), longitudinal studies have allowed criminologists to pay close attention to the longitudinal patterning of criminal activity over the life course. For the study

of criminal careers, longitudinal data are necessary as it provides the mechanism by which to study change in offending across the life-course. In fact, longitudinal studies are necessary for making proper inferences about individual trajectories of stability and change (Sampson & Laub, 1993:251) as well as how life events alter trajectories of criminal activity over the life-course (Laub, Nagin, & Sampson, 1998).

There have been many longitudinal studies focusing on criminal activity, in different eras and parts of the world. For example, longitudinal studies exist in the United States, Canada, Puerto Rico, England, Scotland, Denmark, Sweden, Finland, China, Japan, Switzerland, Australia, New Zealand—as well as cities within those countries, and employ a varied array of sampling frames (general population, high risk population, and offenders) (see Weitekamp & Kerner, 1994). This material has generated an impressive and important amount of information about the natural history of criminal offending that would not have been possible through the use of cross-sectional designs that only provide a snap-shot of individuals at one particular point in time.<sup>1</sup> Although there exists several efforts that take stock of the key findings of longitudinal studies (see Thornberry & Krohn, 2003; Weitekamp & Kerner, 1994), it is useful to briefly review several of the findings that are common to the many longitudinal studies.

First, misbehavior starts early in the life-course (Tremblay et al., 1999), and some of the factors that influence misbehavior can be identified early in life while others emerge later in life (Stouthamer-Loeber, Wei, Loeber, & Masten, 2004). Second, these studies have charted the course of persistence (the fraction of the population who continues criminal activity over time), but less so on desistance (the fraction of the population who ceases criminal activity over time). This is so because many studies do not follow sample members past their 30s. Moreover, there is a long recognized difficulty in operationalizing desistance (Bushway, Piquero, Broidy, Cauffman, & Mazerolle, 2001; Laub & Sampson, 2001) and a recognition that there is much intermittency, or stops and starts, throughout an individual's criminal career that may be misconstrued as desistance (Laub & Sampson, 2003; Nagin & Land, 1993; Piquero, 2004). The issue of intermittency is one that is completely lost with cross-sectional studies. Third, longitudinal studies appear to suggest that the correlates of onset, persistence, and desistance may not necessarily be the same and that some factors influencing onset may have little to do with the factors that influence persistence or desistance (Piquero et al., 2003). Fourth, the advent of new methodological/statistical tools has aided researchers' ability to more directly examine individual and group-based patterns of criminal activity. One in particular is the trajectory methodology, which allows researchers to study how criminal activity changes over time in a group-based framework (Nagin, 1999). This methodology seems particularly well-suited to studying criminal activity over the life course because according to some developmental, group-based theories of crime (e.g., Moffitt, Patterson, Loeber) there may be different groups of offenders (low

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<sup>1</sup> Debates regarding the merits of longitudinal vs. cross-sectional designs exist elsewhere (see Blumstein, Cohen, & Farrington, 1988a, 1988b; Gottfredson & Hirschi, 1986, 1988).

rate, medium rate, and high rate), their offending trajectories may exhibit different shapes at different ages, and they may be differentially affected by distinct crime-exacerbating factors. Findings from this line of research appear to suggest that there is meaningful variation within offenders and the factors that predict the offending of one group are not necessarily the same as the factors predicting the offending of another group. Moreover, applications of this methodology to various data sets lead to the conclusion that developmental patterns of criminal activity differ with regard to crime type and whether offender-based samples are used. For example, offender-based samples yield higher and more stable rates of offending over longer periods of the life course.

Given the amount of research undertaken in recent years with this methodology and the unique window within which it provides criminologists the ability to peer into offenders' criminal careers, the rest of this essay will attempt to document what is known about the natural history of offending with the use of the trajectory technique. Consider this, then, a "taking stock" of what we have learned about developmental trajectories of criminal activity. The essay begins with a description of the trajectory methodology, followed by a review of the studies that have used this approach to document the longitudinal patterning of criminal activity, and closes with a summary statement of key findings and an identification of several future research directions.<sup>2</sup> Because the studies reviewed in this essay employ a varied array of sampling frames, and cover various periods of the life-course, the essay will describe the results of these studies by the type of sample used (offender-based/general population), as well as the time period covered (childhood/adolescence/adulthood). Before these studies are reviewed, a brief description of the theoretical debates that the trajectory methodology can help empirically assess is presented.

### *Theoretical Backdrop*

A core issue in criminology is the shape of the age/crime curve, not the aggregate age/crime curve per se, but the age/crime curve at the individual level (Blumstein et al., 1986; Britt, 1992; Gottfredson & Hirschi, 1990; Greenberg, 1991; Le Blanc & Loeber, 1998; Loeber & Le Blanc, 1990; Nagin & Land, 1993; Piquero et al., 2003). Is this relationship the same for everyone such that external life events do not matter after ages 8/10 once self-control is developed as static/general theorists like Gottfredson and Hirschi claim? Or is it that variation within individuals over time in sources of informal social control (e.g., marriage, military, etc.) leads to changes in criminal trajectories after ages 8/10 as static/dynamic theorists like Sampson and Laub claim? Or even still, is the aggregate age/crime curve characterized by a

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<sup>2</sup> Of course, integrating findings across these studies is particular challenging because of differences in sample characteristics, age ranges, length of follow-up measures, and analytic strategies and coding decisions (i.e., should crime information be dichotomized or left continuous?) (Tucker et al., 2005:309).

mixture of static/dynamic/developmental processes such that individuals may differ not only in their offending rates at any given age but also exhibit distinctive trajectories of offending over the life-course as hypothesized by Moffitt, Patterson, Loeber, and others? In short, the theoretical models described above make strong—yet competing—predictions about the development of criminal activity over the life-course.

These theoretical debates can be considered within the context of developmental criminology, which refers to the study of temporal within-individual changes in offending over the life-course (Le Blanc & Loeber, 1998:117). Importantly, the theoretical processes reviewed above make fundamentally different predictions about the existence of groups. Both Gottfredson and Hirschi (1990:132, Figure 9) and Laub and Sampson (2003:248 and 249, 278 and 279) would likely offer that a group-based framework is both problematic and uncertain not only because of a tendency to reify groups that may “not, in fact, exist” (Sampson & Laub, 2003:587), but also because both sets of theorists suggest and find that all offenders, even the most high-rate of all offenders, desist.<sup>3</sup>

On the other hand, the developmental theoretical models advanced by Moffitt, Patterson, Loeber, and others are exclusively group-based. For example, Moffitt’s (1993) developmental taxonomy predicts that two types of offenders characterize the age/crime curve, each of whom have a unique set of predictors and each of whom evince distinct offending patterns over the life-course. One of these groups, life-course persistent, comprises a very small subset of offenders, whose antisocial activity begins in early childhood, persists throughout life, and is unlikely to respond to points of intervention. Members of the life-course persistent group share deficits in neuropsychological functioning which, when met with family adversity and ineffective parenting, create very difficult children and adolescents who fail in multiple life domains and engage in all sorts of criminal activity, including violence. On the other hand, adolescence-limited offenders engage in adult-like antisocial behaviors (except violence) during adolescence largely due to the interaction between *recognition* of the maturity gap<sup>4</sup> and the peer social context. Most adolescence-limited offenders, because they do not suffer injurious childhoods, desist by the time adulthood approaches because of their ready stock of prosocial skills and recognition that they can now afford all of the previously coveted adult activities. For Moffitt then, the two groups of offenders evince unique causes as well as unique shapes, peaks, and changes in offending rates over the life-course.

Clearly, in order to adjudicate between Moffitt’s predictions of distinct offender typologies versus the more single-group frameworks advanced by Gottfredson and Hirschi and Sampson and Laub, researchers need methodological/statistical

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<sup>3</sup> Although Sampson and Laub (2003) are critical of group-based theoretical models as well as statistical tools developed to find them (Sampson et al., 2004), they do suggest that there may be “multiple pathways to desistance” (Laub & Sampson, 2003:278); yet desistance is facilitated by a range of turning points in combination with individual actions for all offenders (Laub & Sampson, 2003:278).

<sup>4</sup> Here, the maturity gap refers to the adolescent’s recognition that they physically resemble adults, but society places age restrictions on their partaking of adult activities.

techniques that have the ability to parcel out distinct offending trajectories that change in shape and level over time. One such methodology, which is the focus of the current chapter, is the trajectory method. Next, we turn to an overview of this methodology, which has become a staple in researchers' ability to study the theoretical models articulated above.<sup>5</sup>

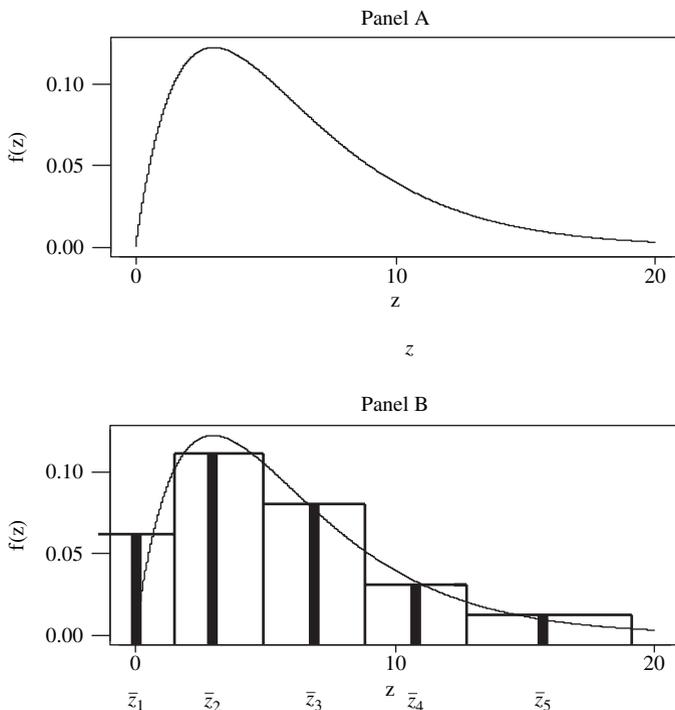
### *The Trajectory Methodology*

The use of finite mixture models has a long history in criminology, and the trajectory methodology owes a great debt to the rich history in criminal careers research that attempts to sort out within-offender heterogeneity (see Blumstein, Farrington, & Moitra, 1985; Blumstein & Moitra, 1980). Recognizing that there may be meaningful sub-groups within a population that follow distinctive developmental trajectories (Loeber & Hay, 1994; Moffitt, 1993; Patterson, 1993), Nagin and Land (1993) developed a modeling strategy that makes no parametric assumptions about the distribution of persistent unobserved heterogeneity in the population. Unlike other techniques, the semi-parametric mixed Poisson model assumes that the distribution of unobserved persistent heterogeneity is discrete rather than continuous, and thus the mixing distribution is viewed as multinomial (i.e., a categorical variable). Each category within the multinomial mixture can be viewed as a point of support, or grouping, for the distribution of individual heterogeneity. The model, then, estimates a separate point of support (or grouping) for as many distinct groups as can be identified in the data.

It is important to remember that the trajectory groups *approximate* population differences in developmental trajectories. A higher number of points of support (groups) yields a discrete distribution that more closely approximates what may be a true continuous distribution (Nagin & Tremblay, 2005a, b). This is easily illustrated with an example. Figure 1 displays two panels (e.g., Nagin, 2005:47, Figure 3. 1). The first panel, (A), depicts the population distribution of some behavior, *z*, while the second panel, (B), replicates the same distribution but is overlaid with a histogram that approximates its shape. Panel (B) illustrates that any continuous distribution with finite end-points can be approximated by a discrete distribution (a histogram) or alternatively by a finite number of "points of support" (Nagin & Tremblay, 2005a, b). A higher number of points of support yields a discrete distribution that more closely approximates what may be a true continuous distribution (Nagin & Tremblay, 2005a, b).

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<sup>5</sup> The trajectory method, according to Nagin (2005:2), is "based upon a formal statistical model for conducting group-based analysis with time- and age-based data [and it] provides the capacity for testing whether the hypothesized trajectories emerge from the data itself. . . . As such, it can be thought of as a methodology for identifying meaningful subgroups in time-based data. It also provides the capacity for statistically identifying the factors that both predict and alter these distinctive time-based progressions."



**Figure 1** Using groups to approximate an unknown distribution

Further, because each individual has a non-zero probability of belonging to each of the various groups identified, s/he is assigned to the group to which s/he has the highest probability of belonging to. This is a particularly important feature of this methodology because it allows researchers to assess the claims of extant developmental models that make predictions about different groups of offenders, including their size. This cannot be accomplished with approaches that treat unobserved heterogeneity in a continuous fashion.

The semi-parametric model (SPM) developed by Nagin and Land (1993) has three additional features that make it appealing for studying developmental trajectories of criminal activity. First, it makes use of a number of different estimators, including the Poisson, the zero-inflated Poisson, the Bernoulli, and the censored normal. The censored normal model is useful for psychometric scale data, the Poisson and the zero-inflated Poisson models are useful for count data, and the Bernoulli model for dichotomous data. By allowing for the use of different types of estimators, the outcome data under investigation can be more appropriately modeled. Second, the Poisson and zero-inflated Poisson versions of the model take into account periods of non-offending, or intermittency. Third, the group-based approach is more systematic in the way it categorizes offenders because it identifies distinctive groups by applying a formal, objective statistical criterion. Therefore, it avoids subjective

classification of individuals into groups that reflect only random variation.<sup>6</sup> In short, the trajectory methodology is well-suited for research problems with a taxonomic dimension whose aim is to chart out the distinctive developmental trajectories, to understand what factors account for their distinctiveness, and to test whether individuals following different trajectories also respond differently to an intervention (Nagin & Tremblay, 2005a, b).<sup>7</sup>

The results from employing the trajectory methodology are used in a number of ways, and they are documented in detail by Nagin (2005). Here, a few of the more common approaches are highlighted. First, the most common approach is to, after sorting individuals into the various trajectory classifications, treat the groups as nominal categories and then examine how an array of risk/protective factors vary across the groups. This is commonly referred to as the classify/analyze approach, and provides basic descriptive information regarding how the various trajectory groups differ along key variables of interest. Relatedly, researchers can use the trajectory groups as outcome variables in a multinomial logistic regression framework, where key independent variables are used to predict membership in the various groups. A second substantive analysis taken with the trajectory results is to use the group classifications as predictors, along with other key theoretical variables, in a regression-based framework to predict the outcome of interest (i.e., crime counts). This approach allows for an examination of how key theoretical variables, e.g., local life circumstances, relate to criminal offending after taking into consideration unobserved individual differences (measured through group membership) (for examples, see Laub et al., 1998; Piquero, Brame, Mazerolle, & Haapanen, 2002).<sup>8</sup>

### Trajectories are not the only Approach

It must be recognized, of course, that the trajectory methodology is not the only approach one could take to study criminal activity over the life course. Alternative methods exist, principally hierarchical modeling and latent growth curve modeling.

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<sup>6</sup> Subjective classifications are quite often found in the literature. For example, Wolfgang et al. (1972) classified those individuals with five or more police contacts as “chronic” offenders. Other scholars categorize early- and late-starters based on an arbitrary age cut-off (i.e., 14). Blumstein et al. (1985) have shown that many of these arbitrary designations may not be supported by the data. At the same time, there may be some element of subjectivity involved when choosing between models identifying high numbers of trajectory systems. Nagin (2005:75) suggests that in certain instances when BIC is not a useful criterion for choosing a model, for reasons of parsimony and comprehensibility, “the fewer the groups the better”. In short, Nagin (2005:77) recommends selecting a model with “no more groups than is necessary to communicate the distinct features of the data”.

<sup>7</sup> Software and documentation to employ the trajectory methodology through the SAS platform is available at [www.ncovr.org](http://www.ncovr.org). Additionally, other statistical applications, such as LATENT GOLD and M-PLUS, also perform trajectory estimations, and other researchers have developed methods for trajectory methods including Bengt Muthén (see Muthén, 2004; Muthén & Muthén, 2000).

<sup>8</sup> In short, a major pragmatic advantage of the trajectory method is that it performs a major data reduction, such as the case in cluster analysis. The method allows one to reduce  $n$  individuals  $X$   $t$  repeated measures of an outcome variable into  $k$  nominal groups for subsequent analysis.

One of the key differences between the trajectory approach and these other methods is that the latter treat the population distribution of criminal activity as continuous whereas the trajectory model approximates this continuous distribution with points of support, or groups. The trajectory method, then, is designed to identify distinctive, developmental trajectories within the population, to calibrate the probability of population members following each such trajectory, and to relate those probabilities to covariates of interest (Nagin, 1999:153).

Raudenbush (2001:59) provides a further clarification of the issues surrounding the various methodologies: "In many studies of growth it is reasonable to assume that all participants are growing according to some common function but that the growth parameters vary in magnitude." He offered children's vocabulary growth curves as an example of such a growth process. Two distinctive features of such developmental processes are (a) they are generally monotonic—thus, the term *growth*—and (b) they vary regularly within the population. For such processes it is natural to ask, "What is the typical pattern of growth within the population and how does this typical growth pattern vary across population members?" Hierarchical and latent curve modeling are specifically designed to answer such a question.

Raudenbush also offered an example of a developmental process—namely, depression—that does not generally change monotonically over time and does not vary regularly through the population. He observed (p. 59), "It makes no sense to assume that everyone is increasing (or decreasing) in depression. . . . many persons will never be high in depression, others will always be high; some are recovering from serious depression, while others will become increasingly depressed." For problems such as this, he recommended the use of a multinomial-type method because development, or modeled trajectories, varies regularly across population members. Indeed, some trajectories vary greatly across population subgroups both in terms of the level of behavior at the outset of the measurement period and in the rate of growth and decline over time. According to Raudenbush (2001:60), the trajectory methodology is "especially useful when trajectories of change involve sets of parameters that mark qualitatively different kinds of development." For such problems, a modeling strategy designed to identify averages and explain variability about that average is far less useful than a group-based strategy designed to identify distinctive clusters of trajectories and to calibrate how characteristics of individuals and their circumstances affect membership in these clusters (Raudenbush, 2001:60).

### **Limitations of the Trajectory Methodology**

As is the case with all methods that seek to understand the longitudinal patterning of criminal activity, the trajectory methodology is limited in some respects.<sup>9</sup> First, because the SPM assumes that unobserved heterogeneity is drawn from a discrete (multinomial) probability distribution, there will likely be model misspecification

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<sup>9</sup> For an excellent review of the various types and implications of latent trajectory models for the study of criminal and antisocial activity over the life course, see Curran and Willoughby (2003).

bias if unobserved individual differences are actually drawn from a continuous distribution.<sup>10</sup> Second, the identification of parameter estimates is difficult with small periods of observations and where the prevalence of observations is small. Third, classification of individuals to distinctive groups will never be perfect (Roeder, Lynch, & Nagin, 1999). Fourth, the number of groups extracted is variable (as shown in D'Unger, Land, McCall, & Nagin, 1998; Sampson, Laub, & Eggleston, 2004) and partly a function of sample size—the more individuals the more groups one is likely to find (see also Nagin & Tremblay, 2005a:30).<sup>11</sup> According to Sampson et al. (2004), this result is not surprising if indeed the underlying distribution is more or less continuous in nature. Still, the number of groups appears to plateau at about sample size 200, and the conclusions reached about the number of groups above this sample size do not vary much (D'Unger et al., 1998). Fifth, recent research seeking to understand how the model behaves under various conditions indicates that three more general concerns in longitudinal research, (a) length of follow-up, (b) the inclusion of incarceration time, and (c) data on involuntary desistance through death, influence developmental trajectories (Eggleston, Laub, & Sampson, 2004). Regarding length of follow-up, these authors found that length can influence group shape, peak ages, and group membership, and that the length of follow-up issue seems most relevant for high-rate offenders who continue offending into adulthood. Regarding incarceration time, they found that excluding such information results in underestimating the rate of offending and can affect group shape, peak age, and group membership, and that the incarceration information seems most relevant for high-rate offenders (see also Piquero et al., 2001). Regarding mortality, the analyses indicated that the population of the high-rate chronic group is greatly affected by the exclusion of mortality data (Eggleston et al., 2004:21). The issue here is that those who are dead are assumed to have desisted. And once again, mortality data seems most relevant for high-rate offenders. In short, longer-term data on offending and the inclusion of incarceration and mortality information alter the group number, shape, and group assignment in trajectory research.<sup>12</sup> Sixth, there have been some statistical concerns raised regarding the trajectory-based approach, specifically in an effort to address model fit to the data (Kreuter & Muthén, 2006a,b). According to Muthén (2007), two specific things are needed. First, analysts need to show that

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<sup>10</sup> Bauer and Curran (2003) recently argued that latent trajectory classes can be estimated even in the absence of population heterogeneity. In their simulation study, these authors asked if the components from the trajectory mixture model represent true latent subgroups in the population, or whether they were serving only to approximate what is in fact a homogenous but nonnormal distribution. On this last point they noted that nonnormality was in fact a necessary condition for the extraction of multiple latent components or classes (p. 345). Their results indicated that multiple trajectory classes were estimated and appeared optimal for nonnormal data even when only one group existed in the population. For a slightly different view/interpretation of this, see Nagin and Tremblay (2005a).

<sup>11</sup> However, if the underlying distribution is indeed discrete and not continuous, then an increase in sample size will not artificially lead to an increase in the number of groups identified.

<sup>12</sup> To be sure, these specific concerns also influence the other methodological techniques for studying criminal careers using longitudinal data (Nagin, 2004a). That is, hierarchical and growth-curve modeling techniques must also deal with these exact same problems.

the group-based model fits the data better than the standard HLM random effect model (with a single class) – otherwise, there are no meaningful groups to be found. Second, there needs to be an effort to show that a more flexible model – such as the growth mixture model that Muthén has proposed – does not fit significantly better than the group-based model. If it does, the group-based approach is insufficient and does not fit the data. This is oftentimes ignored in the group-based research tradition and may be regarded as statistically unacceptable especially because it could make a difference in substantive conclusions (see Muthén & Asparouhov, 2006: 17–20 to see what a difference the choice can make; see also Muthén, 2004, section 19.5.2). In short, Muthén (2006) argues that the group-based analysis field is poorly developed statistically and there exists some alternative modeling viewpoints.

Finally, Sampson et al. (2004) are concerned not with the model itself, but how the results emerging from it are interpreted. The issue here is that the method is vulnerable to misappropriation by those pre-disposed to believe in the idea of a high-rate group of offenders (pp. 38–39).<sup>13</sup> As these authors suggest, “The SPM [semi-parametric model] begins with the assumption that groups exist, often leading to the notion that a wide array of group configurations is possible. Is it then easy for the naïve user to conclude (tautologically?) that groups exist because they are discovered, even though a model cannot be said to discover what it assumes. SPM will estimate groups from an underlying continuous distribution, a fact that can bedevil even the most sophisticated user (p. 41).”

Recently, Nagin and Tremblay (2005a,b) have noted that there has been some confusion about the interpretation of the model, stemming primarily from the interpretation of what it means to say “person *x* belongs to trajectory group *j*.” Three misconceptions in particular have been identified: (1) individuals actually belong to a trajectory group, (2) the number of trajectory groups in a sample is immutable, and (3) the trajectories of group members follow the group-level trajectory in lock-step (Nagin & Tremblay, 2005a).

With regard to the first caution, the methodology and its developers caution against reification of groups. Regarding the second caution, it must be remembered that the groups are intended as an approximation of a more complex underlying reality. Thus, what the model does is simply to display the distinctive features of the population distribution of trajectories. As such, the number of groups and the shape of each group’s trajectory are not fixed. This is so because longitudinal data are limited not only by the number of individuals but more importantly by the number of periods or sets of observations for which individuals are observed. As more periods of data are added, trajectories may vary. As Nagin and Tremblay (2005a) surmise, “more data allows for more refined statistical inferences.” Finally, because the trajectory methodology creates a summary that describes the behavior and characteristics of individuals following similar developmental courses (i.e., it summarizes the average behavioral trend of a collection of individuals), individuals assigned to

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<sup>13</sup> This is an interesting point because while most (if not all) criminologists agree that there is in fact a small, high-rate group of offenders, the debate seems to be more about what that observation means (i.e., is it a difference in degree or a difference in kind?). This issue has not been resolved and remains a legitimate subject of debate.

specific trajectory groups may not follow the overall trajectory pattern perfectly. In other words, it is not necessarily the case that all individuals in a trajectory will follow that trajectory, only that individuals assigned to a particular trajectory resemble one another and the overall trajectory more so than they do another trajectory. In this regard, a group within the trajectory context is a cluster of approximately homogeneous individuals in the sense that they are following about the same developmental course, and have distinctive characteristics from other clusters of individuals following different developmental courses (Nagin & Tremblay, 2005a).<sup>14</sup> In short, it must be remembered that: (1) individuals do not actually belong to a trajectory group; (2) the number of trajectory groups in a sample is not immutable; and (3) individuals do not follow the group-level trajectory in lock step.<sup>15</sup>

## Review of Studies

We now turn to a review of the trajectory studies that have emerged in the literature, with a specific focus on criminal activity. Noted here is the fact that the group-based methodology has begun to permeate other disciplines and applications have been made to obesity (Mustillo et al., 2003), cocaine (Hamil-Luker, Land, & Blau, 2004) and marijuana/other drug use (Guo et al., 2002), binge-drinking (Chassin, Pitts, & Prost, 2002; Hill, White, Chung, Hawkins, & Catalano, 2000), cigarette smoking (Chassin, Presson, Pitts, & Sherman, 2000), women's employment patterns (Hynes & Clarkberg, 2005), women's public assistance receipt (Hamil-Luker, 2005), perceptions of depression (Stoolmiller, Kim, & Capaldi, 2005) and legal socialization (legitimacy and legal cynicism) (Piquero, Fagan, et al., 2005), software piracy (Piquero & Piquero, 2006), individual world-wide web usage (Christ, Krishnan, Nagin, Kraut, & Gunther, 2001), and so forth. However, for purposes of the current chapter, attention is paid in particular to criminal activity.

Appendix presents an overview of the papers that have used the trajectory methodology between 1993 and 2005. This appendix indicates the authors of the paper, the year of its publication, the sample used, the age, race, and gender of the subjects, the measurement of the outcome variable (official and/or self-reports), the number of groups identified, and some interesting findings emanating from the research. Because a listing and summary of each paper is beyond the scope of this chapter, the review of studies that follows is based on substantive efforts aimed at identifying trajectories. For ease of presentation, the studies are cataloged by whether they were based on an offender or non-offender sample, as well as whether the data presented a portrait of crime in childhood, adolescence, adulthood, or

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<sup>14</sup> It is important to bear in mind that the variation within the trajectory is random variation conditional on trajectory (group) membership, while the variation between the trajectories is structural.

<sup>15</sup> According to Muthén (2007) however, the group-based model does indeed assume that individuals belong to a trajectory group. His view is that to the extent that the trajectory methodology views this as merely an approximation, then researchers should use a growth mixture model where within-class variation is allowed.

some combination thereof. It is important to note the impressive array of studies employing this methodology. Over 80 studies have used the trajectory analysis in urban, suburban, and rural settings, in the US and abroad, and spanning birth to age 70, and integrating findings across these studies is particularly challenging because of differences in sample characteristics, age ranges, length of follow-up measures, and analytic strategies and decisions (e.g., Tucker, Ellickson, Orlando, Martino, & Klein, 2005:309). Nevertheless, such a review can begin the process of synthesizing key conclusions across empirical studies.

### ***Methods Papers***

There have been several methodologically-oriented papers written about the trajectory methodology, and in general these papers employ longitudinal data, identify distinct trajectories, and then discuss the strengths and weaknesses of this approach. These papers include: (Brame, Nagin, & Wasserman, 2006; Bushway, Brame, & Paternoster, 1999; Eggleston et al., 2004; Haviland & Nagin, 2005; Jones, Nagin, & Roeder, 2001; Kreuter & Muthén 2006a,b; Land, McCall, & Nagin, 1996; Land & Nagin, 1996; Land, Nagin, & McCall, 2001, Loughran & Nagin, 2006; Nagin, 1999, 2005; Nagin & Land, 1993; Nagin & Tremblay, 2005a,b; Roeder et al., 1999; Sampson & Laub, 2005; Wang, Brown, & Bandeen-Roche, 2005). Because they still employ data to identify trajectories, they are reviewed below but the specifics of the studies are not discussed in this section. Readers interested in a more detailed exposition of the methodology should consult these papers as well as Nagin (2005). Finally, two papers (Griffiths & Chavez, 2004; Weisburd, Bushway, Lum, & Yang, 2004) apply the trajectory methodology to the analyses of crime at street segments and census tracts. Since these are not concerned with an application of crime at the individual level, they are not reviewed here.

### ***Substantive Papers***

#### **Offender Samples**

There have been nine trajectory studies undertaken with offender samples. Four different data sets have been used, and all subjects were followed through portions of adulthood. Thus, no studies considering only childhood and/or childhood/adolescence are reviewed here. Three studies employ the Boston area delinquents (i.e., the Glueck sample) (Eggleston et al., 2004; Laub et al., 1998; Sampson & Laub, 2003), three involve cohorts of California Youth Authority parolees (Ezell & Cohen, 2005; Piquero et al., 2001, 2002), two involve a Dutch conviction cohort (Blokland, Nagin, & Nieuwebeerta, 2005; Blokland & Nieuwebeerta, 2005), and one involves an offenders index from the British Home Office

(Francis, Soothill, & Fligelstone, 2004).<sup>16</sup> In general, with the exception of the British data, the pattern and similarity of results from the Boston delinquents, the Dutch offenders, and the CYA parolees is striking.

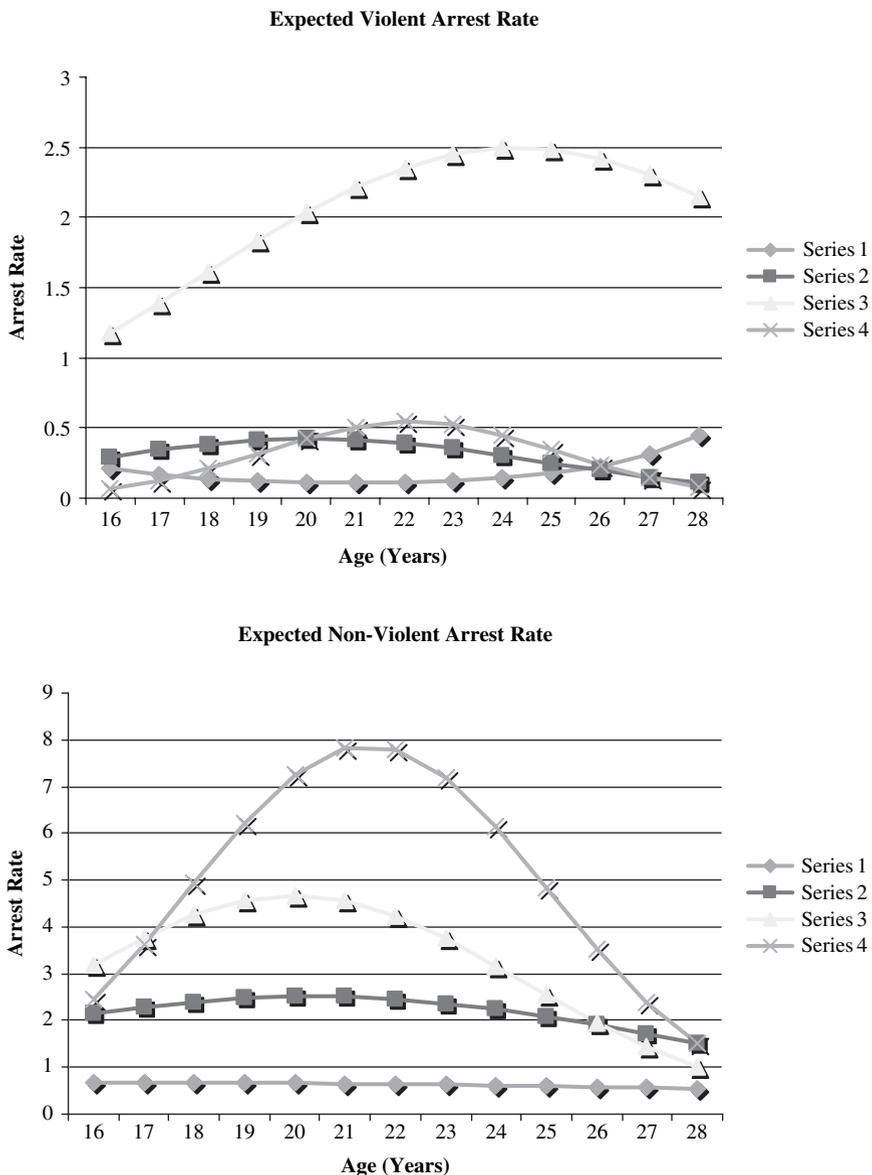
Using data on 500 offenders paroled from California Youth Authority institutions and followed for seven consecutive years post-parole, Piquero et al. (2002) modeled joint trajectories<sup>17</sup> of violent and non-violent offending throughout the early 20s, as well as their covariates. A number of key findings emerged from their paper. First, four trajectories each were identified for violent and non-violent offending, but non-violent offending rates were always much higher than the violent offending rates. (These joint trajectory graphs are reproduced here in Figure 2). Second, when the trajectory systems were modeled jointly, one of the four systems (shown as Series 1) had a decreasing non-violent arrest rate, but an increasing and stable violent offending rate. Third, when these authors estimated the effects of various covariates on arrest activity after conditioning on group membership, they found that some of the covariates exerted different effects across the various groups on both violent and non-violent criminal activity. For example, their measure of stakes in conformity (which included employment and marriage) was inhibitive of non-violent arrests for their second trajectory system but had no effect whatsoever on the offending of their third trajectory system. Similarly, heroin and alcohol dependence was predictive of non-violent arrests for the second trajectory but no effect for violent arrests for the same trajectory. Finally, in an analysis that examined the effects of covariates on arrest activity and including controls for group membership, they found that non-whites were more likely to accumulate violent arrests, that heroin dependence was positively related to non-violent arrests, and a measure of stakes in conformity (including marriage and employment) inhibited non-violent arrests.

Analyses of the offending samples have consistently identified four to six trajectories. The Piquero et al. (2002) paper just discussed, which contains offending information through age 28 for a sample of CYA parolees, identified four trajectories (for both violent and non-violent arrests, respectively). When total arrests were analyzed, Piquero et al.'s (2001) analysis of a different sample of CYA parolees through age 33 converged on six trajectories. Ezell and Cohen's (2005) trajectory analysis for three different samples of CYA parolees (paroled in 1981–2/1986–7/1991–2) also each favored a six-group model. These trajectories differed both in overall level of offending and in the trajectory shape. For example, for the parolee cohort with the longest follow-up period (1981–2 sample, age 7–37), trajectories differed in average

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<sup>16</sup> A recent study in South Australia applied the trajectory methodology to the juvenile criminal careers of a 1984 birth cohort and identified six trajectories (Marshall, 2005), but this study only exists, at present, in presentation format.

<sup>17</sup> Unlike the modeling of a single outcome, joint/dual trajectory analyses model the developmental course of two distinct but related outcomes. According to Nagin (2005:141), the “dual trajectory model provides a rich, yet easily comprehended, statistical summary of the developmental linkages between the two outcomes of interest. It can be used to analyze the connections between the developmental trajectories of two outcomes that are evolving contemporaneously (such as depression and alcohol use) or that evolve over different time periods that may or not overlap (such as prosocial behavior in childhood and social achievement in adolescence)”.



**Figure 2** Summary of violent and non-violent arrest trajectories under assumption of 12 months street time each year

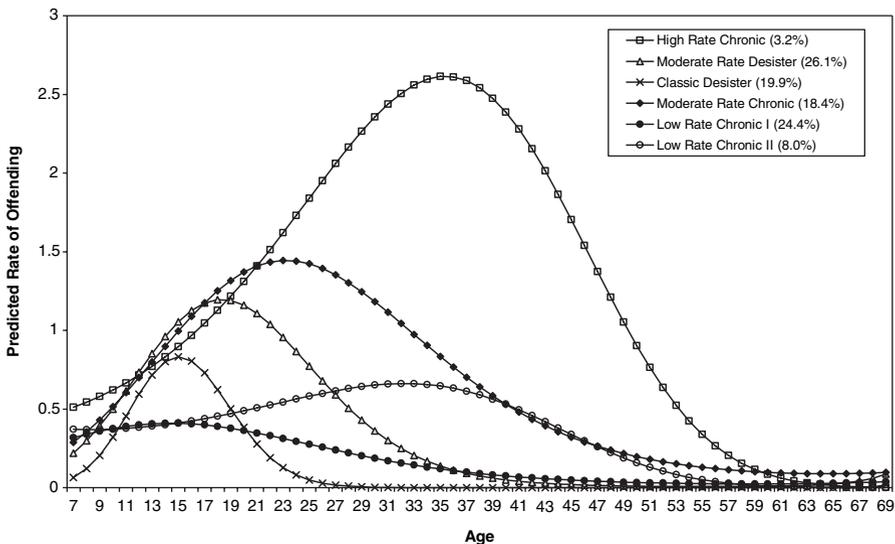
(Source: Piquero et al., 2002, Fig. 2, p. 154)

Note: Trajectory group classification distribution was as follows: Series 1 = .145; Series 2 = .511; Series 3 = .124; and Series 4 = .220 (Piquero et al., 2002:153; Table 4)

arrest rates and also in terms of the growth and decline of arrests over time (Ezell & Cohen, 2005:185).

Similarly, when total arrests among the Boston males, first identified as delinquent at age seven, were analyzed to age 32, Laub et al. (1998) identified four trajectories. While all of these trajectories appear to be declining by age 32, one trajectory had a much higher offending peak than the other three groups. These “high rate chronics” consisted of only eleven individuals. Their offending peaked in the early 20s and then began a slow, gradual decline by the late 20s and early 30s. Laub et al. then examined the factors that were related to such trajectories, and found that after controlling for trajectory group membership, involvement in “good marriages” was inhibitive of continued criminal activity in early adulthood.

When Sampson and Laub (2003; Laub & Sampson, 2003) extended the data to include arrests through age 70, two other trajectories emerged for the total arrests analysis, thus totaling six distinct trajectory systems (p. 582, Figure 11, reproduced here as Figure 3). Three points are in order regarding these trajectories: (1) a small group of men, about 3.2%, were labeled as “high-rate chronics”, whose offending activity peaked in the late 30s, and then dropped close to zero by age 60; (2) three other trajectory groups peaked in middle adolescence, late adolescence, and early adulthood respectively, and then began a slow decline toward zero in adulthood; and (3) a small group of offenders, “low-rate chronics”, representing about 8% of the sample, had a steady offending trajectory between the ages of 19 and 39, at which point their offending began to decline. Controlling for exposure time (with data through age 32), had the interesting effect of only identifying five groups, but the predicted number of offenses was much higher with exposure time taken into



**Figure 3** Official arrest trajectories—Glueck Males—ages 7–70 (Source: Sampson & Laub, 2003, p. 582, Figure 11.)

consideration. Still, by age 32, the offending patterns in the exposure time analysis appeared to diminish.<sup>18</sup>

The second main finding from the Sampson and Laub (2003) follow-up study concerned the crime-specific trajectories. With regard to violent crime, although there were five distinct trajectory groups, the mean rate of offending was always low for these groups (never above 0.4 arrests per year). Still, the offending trajectories for violent offending were quite erratic; that is, all five groups, while having somewhat similar shapes, differed in their peaks and their declines. For alcohol/drug offending, once again five trajectories were observed, with the majority peaking in the mid to late 30s, and then evincing a decline through middle to late adulthood.

The third key finding concerned the comparison of selected childhood/adolescent risk factors by trajectory group membership. These results showed inconsistent patterns and no statistically significant differences in means across the six groups of offending (for total crime only), though the trend was to show that the high-rate chronic group did evince the worst risk factors. It is likely that the differences did not emerge as significant because there were only fifteen men in the high-rate chronic group for which to make comparisons. Sampson and Laub conclude that “life-course-persistent” offenders seem to stop offending by middle adulthood and that they are difficult to identify prospectively using a wide variety of childhood/adolescent risk factors. It is also worth noting here that the age 7–70 Boston sample may not only be the longest longitudinal dataset in the world, but also is the first to track the offending of serious delinquents throughout the full life course. Analyses from this dataset find no evidence of a flat-trajectory group with age, which has been previously undetected because of middle-adulthood censoring (see Laub & Sampson, 2003:105).

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<sup>18</sup> It is interesting to note that in Sampson and Laub (2003), the authors, when considering the mean number of days incarcerated per year between ages 7–32, are able to reproduce the classic aggregate age/crime curve and conclude that the trajectories are very similar with street time taken into consideration. In their companion book, Laub and Sampson (2003:100–103) conclude that the age-crime curve and the similarity of offending trajectories is stable (p. 103). Yet in Eggleston et al. (2004), the authors reach a somewhat more guarded conclusion. Aside from the fact that the predicted level of offending is much higher when incarceration is taken into consideration, offending is predicted to peak at an older age (23 as opposed to 20), and there is a fair amount of instability with the moderate-rate chronic group of offenders: “Thirty-nine percent of the moderate-rate chronics in the model without the incarceration parameter are also in this group when incarceration time is included. In other words, over 60% of the men are no longer identified as moderate-rate chronics once incarceration time is taken into account” (pp. 17–18). Moreover, the differences with and without exposure time are most dramatic in the high-rate chronic group: “With incarceration time in the model, offending peaks in the early twenties at about six offenses per year and slowly declines thereafter to 1.5 offenses per year by age 32. Without incarceration in the estimation, offending consistently increases into the early twenties before leveling off at about 2 offenses per year” (p. 19). Importantly, “56% of the high-rate chronics in the model without incarceration are no longer classified as high-rate chronic offenders when incarceration is taken into account” (p. 19). They conclude that “the exclusion of incarceration time results in underestimating the rate of offending and can affect group shape, peak age, and group membership” (p. 21).

The Dutch offender data and trajectory results provide a counter-point to the Boston-area studies. Blokland et al. (2005) used conviction data for a sample of 5,164 Dutch offenders over an age span from ages 12–72 and identified four trajectory groups: sporadic offenders, low-rate desisters, moderate-rate desisters, and high-rate persisters. Importantly, the high-rate persisters engaged in crime, especially property crime, at a very substantial rate even after age 50, and their estimated trajectory resembled a fairly flat and stable average conviction rate through age 72. When comparing their findings to those obtained by Sampson and Laub (2003:588), Blokland et al. (2005:944) indicate that while about 98 percent of the individuals in their sample follow offending patterns similar to those observed in the Boston-area studies, whom would likely be characterized as “life-course-desisters”, a small group of persistent offenders, making up less than two percent of the sampled population, exhibits a relatively flat trajectory of about 2 to 2.5 convictions per year from age 30 onward, and “does not conform with the Sampson and Laub conception of life-course desisters. Their course of offending is in fact best described by the Moffitt label of life-course persisters.” In a companion piece, Blokland and Nieuwebeerta (2005) found that life circumstances (work, becoming a parent, and marriage) substantially influenced the chances of criminal behavior, and that the effects of these circumstances on offending differed across offender groups. For example, in general, high-rate offenders were less affected by life circumstances when compared to other trajectory groups, while marriage was associated with a drop in conviction rates among those in the low-rate trajectory group (p. 1224).

The British data and analysis are somewhat unique not only because they are of a birth cohort born in 1953 and followed-up with criminal conviction records in 1993 at age 40, but because it approaches the trajectory issue in a slightly different way. Francis et al. (2004) first search for latent types of criminal activity and then aggregate criminal careers into fixed five-year age periods of each offender’s criminal history. Focusing more on types of crimes within clusters over time (i.e., they did not assign individuals to clusters), these authors identified nine different male clusters and three different female clusters.<sup>19</sup> For example, male offending showed greater diversity than female offending. Additionally, for males, each type of offending had a distinct age profile, but this was not evident with the females (Francis et al., 2004).

In sum, the pattern of findings emerging from the offender-based samples, especially the two very different Boston delinquent and CYA parolee samples, which are not only based on different “types” of delinquents but also in different contexts and time periods, points to two findings. First, offending appears to decline as early adulthood approaches for all groups. Second, there appear to be about 4–6 distinct trajectories, on the higher end with more data (i.e., length of observation window), as is expected (Nagin & Tremblay, 2005a). Unfortunately, given the small number of studies that have used offender-based samples, and the limited capacity that such

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<sup>19</sup> In the Francis et al. study, each individual can contribute up to six strips (offending age bands), and each strip could theoretically be assigned to a different cluster. The interest underlying their work was an examination of how offenders change their offending behavior as they age (i.e., quality or nature of offending as opposed to quantity of offending).

studies have for prediction-based analysis, little information is known about the factors that relate to trajectory differences within serious offenders followed into adulthood. Laub et al. (1998) and Laub and Sampson (2003) have shown that good marriages lead to low offending trajectories, while Piquero et al. (2002) have found that a mixture of stakes-in-conformity and alcohol/heroin dependence relate to criminal activity (sometimes violent, sometimes non-violent) differently across different trajectories: whereas stakes-in-conformity decreased crime, heroin dependence increased it. Similarly, Blokland and Nieuwbeerta (2005) found that marriage had an inhibitory effect on crime for some trajectory groups but not others (i.e., high-rate offenders). Importantly, the substantive results emerging from these studies were observed even after controlling for trajectory group membership, which as noted earlier is one vehicle for controlling for unobserved individual differences. That these effects emerged after controlling for such unobserved individual differences indicates that the effects are quite real.

### **General Population Samples**

There are over a dozen unique US-based longitudinal studies that have employed the trajectory methodology, most of which have used data from the 1958 Philadelphia Birth Cohort Study. Some of these studies have been based on strictly general population and/or birth cohort samples, while others have made use of more high-risk, urban samples (such as the OJJDP-funded Pittsburgh Youth Study and the Rochester Youth Development Study). Other national datasets have contributed to this line of research including the 1945 Philadelphia Birth Cohort Study, the three Racine, WI cohorts, the National Longitudinal Survey of Youth, the Seattle Social Development Project, the Oregon Youth Study, and so forth. A number of international studies have also been undertaken including several using data from the South London males participating in the Cambridge Study in Delinquent Development (CSDD, used nine different times) as well as the Montreal Longitudinal Studies (used ten different times). Other internationally based longitudinal studies that have employed the trajectory methodology include the Christchurch and Dunedin Health and Human Development Studies, and the Quebec Longitudinal Study. In short, it is likely that the data which have been used the most (the 1958 Philadelphia Birth Cohort Study and the Cambridge Study in Delinquent Development) have done so likely because they are both publicly available and because they contain yearly observations of criminal activity for relatively large samples of individuals into the mid 20's (Philadelphia) and into the 30's and beyond (South London). As other longitudinal studies age and data become publicly available, there is no doubt that they too will be used in great detail.

Here, these studies and their overall findings are grouped by the time period examined: (1) childhood only, (2) adolescence only, and (3) childhood/adolescence/adulthood. (Note: Adulthood is defined here as post age 20). No studies were located that used the trajectory methodology on an adult-only general population sample.

## Childhood

Only five studies used data in the first ten years of life. Broidy et al. (2003) used data from six different longitudinal studies, and one of these in particular was the Pittsburgh Youth Study, which covered a portion of the first decade of life. Based on teacher- and self-reports between the ages of 7.5 and 10.5, Broidy and her colleagues found that there were four distinct trajectories of physical aggression among the Pittsburgh boys. Interestingly, though not surprising, all four trajectories of physical aggression were increasing over the age range studied. This may be due to the fact that the males were entering late childhood/early adolescence and/or because a subset of the Pittsburgh males was recruited from high-risk areas.

In the second paper, Tremblay et al. (2004) used maternal assessments of physical aggression at 17, 30, and 42 months from 572 Quebec families who had a five-month old infant. Using the trajectory methodology, three distinct clusters of physical aggression were identified (little aggression, modest aggression, and high aggression). The best predictors before or at birth of the high physical aggression trajectory group, comprising about 14% of the sample, were having young siblings, mothers with high levels of antisocial behavior before the end of high school, mothers who started having children early, families with low income, and mothers who smoked during pregnancy. At five months of age, the best predictors were mothers' coercive parenting behavior and family dysfunction.

In a ten-city, large scale study (Study of Early Child Care and Youth Development) conducted by the National Institute of Child and Human Development (NICHD, 2004), maternal ratings of offspring aggression were collected between ages two and nine on 1,195 subjects. Trajectory analyses indicated that a five-group model provided the best fit to the data, comprised of groups corresponding to very low, low, moderate/declining, moderate, and high trajectories. Specifically, while all five trajectories evinced different starting points, they all tended to decrease in mother-rated aggression from ages two to nine. Additionally, several variables were able to distinguish membership across the five trajectory systems.

Finally, in two separate papers, Shaw, Gilliom, Ingoldsby, and Nagin (2003) and Shaw, Lacourse, and Nagin (2005) examined trajectories leading to school-age conduct problems among 284 low-income boys in the Pittsburgh metropolitan area. Using the trajectory methodology, four distinct groups of overt conduct problems were identified between ages two and eight. Further analyses indicated that while all four groups were evincing declines in parental-reports of conduct problems, the groups differed on various risk factors such as maternal depression, maternal rejecting parenting, and fearlessness. In a subsequent paper, the authors assessed developmental trajectories of conduct problems and hyperactivity from ages two to ten and identified four trajectories for each outcome, and one in particular—the chronic trajectory on hyperactivity/attention problems, remained high and stable throughout the observation period. The authors also reported some overlap between conduct problems and hyperactivity, but the overlap was far from complete.

## Childhood and Adolescence

Over twenty studies have applied the trajectory methodology to longitudinal data through adolescence, with the Montreal Longitudinal Study and Seattle Social Development Project accounting for several of these efforts.

Several common themes emerge from these studies. First, the majority of these efforts use self-reports of a varied nature (teacher, parent, and self) to document developmental trajectories. Second, many of these studies examine what psychologists call externalizing behaviors which are not always delinquent behaviors, such as conduct problems, physical aggression, oppositional behavior, hyperactivity, non-aggression, delinquent peer affiliations, fearfulness, helplessness, and so forth. Third, a few of these studies examine multiple or joint/dual trajectories such as violent/non-violent offending (Brame et al., 2001b), or childhood/adolescent aggression (Brame et al., 2001a; Nagin & Tremblay, 2001a, b).

Two studies in particular are worth highlighting. First, Nagin and his colleagues (2003) examined whether an important life event, grade retention, affected the life-course of physical aggression, and whether its impact varied according to the age at which the turning point occurred. Using data from 1,037 males aged 10–15 participating in the Montreal Longitudinal Study, they found that the influence of grade retention depended on the developmental course of physical aggression, but that the evidence regarding timing was less clear.

In particular, three main conclusions can be drawn from this paper. First, of four distinct trajectory groups identified, grade retention had the largest impact for the two largest trajectory groups (“moderate declining” and “high declining”), but had no impact upon the physical aggression of those in the “low” and “chronic” groups. Second, regarding the timing hypothesis, Nagin et al. found that the effect of grade retention on physical aggression was unrelated to timing for the “high declining” group, but that it mattered for the “moderate declining” group such that for these individuals grade retention aggravated classroom physical aggression when it occurred prior to age 13 but not after age 13. In short, the effects of grade retention appear to depend upon an individual’s developmental history. Third, given trajectory group membership, the results showed that the impact of grade retention on physical aggression appeared unaffected by the child’s early life characteristics and circumstances implying that any impact of grade retention on physical aggression was independent of pre-existing individual characteristics.

Second, Broidy and her colleagues (2003) used data from six sites (Montreal Longitudinal Study, Quebec Provincial Study, Christchurch Health and Development Study, Dunedin Multidisciplinary Health and Human Development Study, Pittsburgh Youth Study, and the Child Development Project (a three-site study in Knoxville and Nashville, TN and Bloomington, IN), and three countries (United States, Canada, New Zealand) to examine the developmental course of physical aggression in childhood (earliest age was six) through early adolescence (latest age was 15) and to analyze its linkage to violent/non-violent offending in adolescence. This analysis included whites and non-whites, males and females, and teacher- and self-report ratings in order to understand the development of disruptive behaviors. A number of key findings emerged from their effort.

First, in general, three or four trajectories were routinely observed across the six studies (e.g., four among boys in the Montreal Longitudinal Study, four for boys and three for girls in the Quebec Provincial Study, three for boys and girls, respectively, in the Christchurch Health and Development Study, three for boys and two for girls in the Dunedin Multidisciplinary Health and Human Development Study, four for boys in the Pittsburgh Youth Study, and three for boys and girls, respectively, in the Child Development Project). Second, while most trajectories of physical aggression were decreasing over the follow-up periods, one of the Child Development Project trajectories (for both boys and girls) was observed to be increasing and several of the Pittsburgh trajectories were observed to increase. The Pittsburgh result should not be too surprising since the last age of follow-up for the Pittsburgh males was 10.5 years; still, since half of the Pittsburgh sample was recruited as “high-risk”, it would be interesting to see how long into adolescence the physical aggression trajectories continue on their upward path. Third, among boys across the samples, there was continuity in problem behavior from childhood to adolescence. In particular, chronic physical aggression increased the risk for continued physical violence as well as non-violent delinquency during adolescence. This finding however, was not replicated for females as there appeared to be no clear linkage between childhood physical aggression and adolescent offending among females. This is not meant to indicate that there were no females in the chronic physical aggression group, because there were females in that group; however, while males displayed similar patterns of physical aggression, there were mean-level differences (boys scored higher than females) in these trends and there was very little delinquency in adolescence among females. In sum, the trajectory groups, which were obtained via teacher reports, predicted involvement in delinquency in adolescence, obtained via self-reports. This suggests that there is continuity in problem behavior across informants from childhood into adolescence.

Aside from these two case studies, two summary statements can be made from the trajectory studies that cover the childhood/adolescence period. First, regardless of the use of self-, teacher-, parent-, or objective antisocial behaviors (including criminal and non-criminal outcomes), similar substantive conclusions have been reached regarding the shape/trend of the various trajectories. That is, by the end of adolescence, *most* trajectories, regardless of the outcome being assessed, are on a decline. Second and most importantly, it is impressive that across all the various studies in different parts of the world using different methodologies to measure criminal activity over a similar age range, there have been a consistent number of trajectories identified in these studies. Typically, three to four trajectories are identified, namely low, medium, and high groups.<sup>20</sup> Whether these groups continue in a similar fashion into adulthood is an important question because it deals with

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<sup>20</sup> This, of course, does not imply that more or fewer trajectories have not been identified in the trajectory studies covering the childhood/adolescence time period. For example, Nagin and Tremblay (2001b) identified six distinct trajectories based on self-reports of property offending. Interestingly, one of the groups, “rising chronic”, comprised of 5.9% of the sample, started committing property offending at a high level at age 11 and continued high through age 17. Bongers et al. (2004) also identified six trajectories (of oppositional behavior) using longitudinal data from Holland. As

fundamental theoretical (are there two groups of offenders as Moffitt predicts?) and policy-related issues. And this is particularly important because some trajectory studies have identified a group of relatively late-onset offenders who appear to be increasing in antisocial activity. Data that follow these subjects into adulthood will be better able to continue charting the offending course of these late-onset chronic offenders. The next section reviews those studies that identify trajectories using data from childhood, adolescence, and adulthood.

### Childhood/Adolescence/Adulthood

Close to thirty studies have applied the trajectory methodology to criminal and anti-social activity in a period covering childhood/adolescence/adulthood. These efforts have used longitudinal data from various locations in the United States, England, Canada, New Zealand, and so forth. Moreover, these efforts have employed random, non-random, and birth cohort samples, males and females, whites and non-whites (including blacks), and in some cases cover offending careers through age 70.

Several common themes cut across these studies. First, many of the studies have utilized both self-reports and official records (police contacts, arrests, and convictions) to study the shape and patterning of criminal activity over the life course. Second, although most efforts have focused on criminal activity, including violent and non-violent crime, others have examined non-criminal trajectories for binge drinking, heavy drinking, and cigarette smoking. Third, several efforts have paid attention to gender differences in criminal activity trajectories.

Because a review of each of these studies is beyond the scope of this chapter, five studies are highlighted. This section begins with a paper that set out to answer a very basic, yet fundamental question regarding offending trajectories: how many latent classes of delinquent/criminal groups are there?

In a 1998 paper, Amy D'Unger and her colleagues used data from three different longitudinal studies, the Cambridge Study in Delinquent Development (CSDD), the 1958 Philadelphia Birth Cohort, and the 1942, 1949, and 1955 Racine, WI birth cohorts. Using the trajectory methodology, these authors identified four latent classes in the CSDD, and five in the Philadelphia cohort. In the Racine data, five classes were detected for the 1942 and 1955 cohorts, but only four for the 1949 cohort.

Specifically, the four groups indicated in the CSDD data showed a non-offender class, a low-rate chronic group whose offending was small but sustained between ages 10 and 30, an adolescent-limited group whose offending peaked at age 16 and then declined to about zero at age 22, and a high-rate chronic group whose offending was always much higher than the other groups, peaked at about ages 18/20, and was approaching zero by age 30.

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indicated earlier, a key advantage of the trajectory methodology is its ability to identify distinct trajectories and then to examine how these groups differ on an array of covariates.

The Philadelphia results indicated five groups, two of which stand out: a high-rate adolescence-peaked group, peaking at age 16 and dropping to almost zero by age 21, and a high-rate chronic group who peaked at ages 17/18 and continued at a steady rate through the early and mid 20s, only to decrease toward the late 20s.

Finally, the three Racine cohorts evidenced pretty different patterns, though this may be due to the nature of offending in the Racine data (which included traffic contacts). The 1942 cohort, which yielded five classes, had three groups of interest: a late-onset chronic group, a group not theoretically anticipated in extant developmental criminology models, who evidence late-onset offending and then an increasing and sustained rate of offending through the 20s; a high-rate chronic group whose offending peaks earlier and then declines through the 20s, and a low rate chronic group who evinces a slow but steady rise in offending through the period between ages 15 and 25 only to decline slowly by the late 20s. The 1949 Racine cohort, where four classes were identified, had two groups of interest: the first, a high-rate chronic group whose offending peaked in the late teens, remained relatively stable through the early 20s, and then began to decline in the mid 20s; and a high-rate adolescence-peaked group whose offending also peaked at age 18 but then steadily dropped through the early to mid 20s. Finally, the 1955 Racine cohort, which also yielded evidence of five distinct classes of offenders, indicated three groups of interest: an early-onset adolescence-peaked group exhibiting an early onset, adolescent peak, and a precipitous decline through the early 20s; a high-rate chronic group who began a slow and steady increase at age eight, peaking at age 17, and slowly decreasing offending activity in the early 20s, and a late-onset adolescence-peaked group whose offending did not begin in earnest until age 15, peaking at age 18, and then dropping throughout the 20s.

In short, across all three data sets, the authors observed a consistent set of two classes of offenders: an adolescent-peaked group and a chronic group. Although identification of these groups is consistent with the extant developmental theories of Moffitt and Patterson, the proportion of sample members in these two groups was not as expected (i.e., the identified chronic group contained many more persons than extant theory would predict). Moreover, other findings from these data sets indicated that another group, a late-onset chronic group, routinely emerged, again calling into question the simple two offender-group typology.

Using longitudinal data from 808 youths who participated in the Seattle Social Development Project (SSDP), Chung, Hill, et al. (2002) sought to identify childhood predictors of different offense trajectories through age 21. Using a self-report measure of offense seriousness, these authors found five distinct classes of offenders: non-offenders, chronic offenders, late onsetters, desisters, and escalators. The last group in particular was one not anticipated by extant developmental theory. Regression analyses were employed to examine which childhood predictors (at ages 10–12) distinguished the offending of these five groups. Their analysis indicated that among initial nonoffenders at age 13, late onsetters were distinguished from nonoffenders by individual factors, while among youths already delinquent at age 13, escalators were distinguished from desisters by peer, school, and neighborhood factors. In short, it is important to point out that the escalator and desistor groups, which are not identified in Moffitt's or Patterson's theories, represent more

than half of the SSDP sample. That other studies have also identified such groups indicates that modifications are needed to these and other taxonomic theories that do not expect such groups, especially the escalating/late-onset chronic group of offenders.

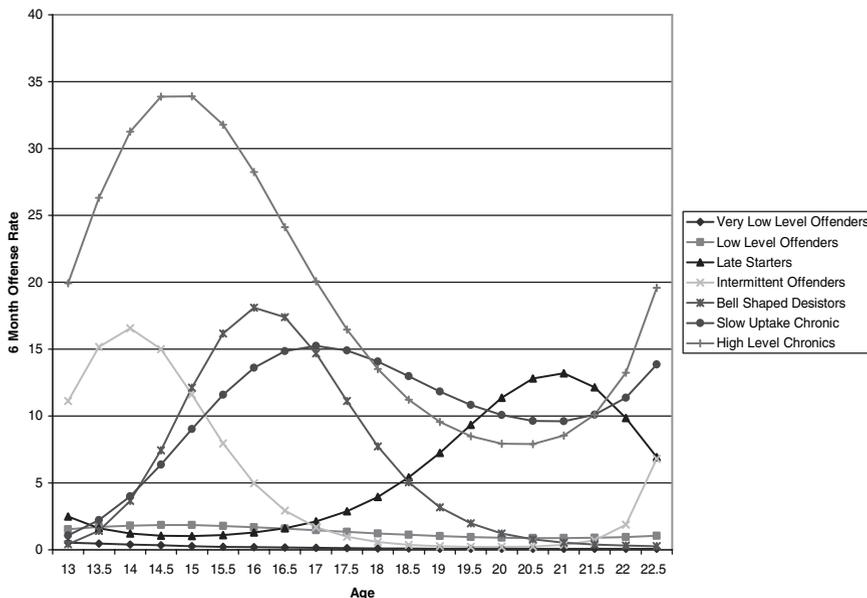
D'Unger, Land, and McCall (2002) address the issue of sex differences in offense trajectories by examining the offending patterns of males and females in the 1958 Philadelphia Birth Cohort Study. Utilizing a random sample of 3,000 females and 1,000 males from the cohort, their trajectory analysis identified five trajectories for males and three for females.

Regarding the male trajectories, two groups in particular stood out: the high-rate adolescence-peaked group whose offending began in early adolescence, peaked at age 16, and then began a drop toward zero throughout late adolescence and early adulthood; and a high-rate chronic group whose offending, though not higher than the afore-mentioned group through age 19, remained stable between mid-adolescence and the mid 20s, only to begin to decline at that point.

Regarding the female trajectories, two of the three groups (the final group being the non-offender group) are worth describing. The first of these groups, the low-rate adolescence-peaked group, offended for about a seven-year span, peaking at age 15, and dropping soon thereafter. The second group, the high-rate adolescence-peaked group, evinced the highest rate of offending at every age, peaking at age 17 and then dropping throughout the early 20s. Aside from these differences, perhaps the main overall difference is that while there were similar shapes and trends across the groups, the male offending rates were always significantly higher than the corresponding female rates.

Bushway, Thornberry, and Krohn (2003) used self-report data from the Rochester Youth Development Study to examine issues related to desistance using the trajectory framework. Following their subjects from age 13.5 to 22, these authors examined whether key conclusions regarding desistance would vary according to two distinct definitions of desistance. The first was a "static" definition of desistance which classifies as desistors those individuals who offended at least once before age 18, but not afterwards (through age 22). Using this definition, 27.6% of the sample met the desistor definition. The second definition of desistance used a "developmental" definition which is based on the trajectory methodology. This definition not only indicates which individuals approach a zero rate of offending, but it also provides information regarding how long they have been there.

Their trajectory analysis identified seven distinct clusters of offenders, two of which approximated individuals who looked like they had desisted (p. 144, Figure 1, reproduced here as Figure 4). The authors however, settled on only one of these groups as fitting their definition of desistance (i.e., "experienced real change. . . and [provided] no evidence [of] an upswing of offending at the end of the period" (Bushway et al., 2003:143 and 144). Using this definition, 8.4% of the sample was classified as desistors. Interestingly, of the 291 individuals identified by the two methods as desistors, there was only agreement by the two methods in 4.8% of the cases. Thus, different proportions of the sample were classified as desistors and different people were classified as desistors. As Bushway et al. (2001) suggest, the trajectory methodology represents a unique approach to study the process of desistance.



**Figure 4** Self-report offending trajectories in the Rochester Youth Development Study (RYDS) (Source: Bushway et al., 2003: p. 144, Figure 1.)

Moffitt and colleagues applied the trajectory methodology to counts of conduct disorder symptoms assessed (via self-, mother-, and teacher-reports) for 525 males at ages 7, 9, 11, 13, 15, 18, 21 and 26 years in the Dunedin Multidisciplinary Health and Human Development Study (Moffitt, 2006). Their analysis detected five groups: (1) a life-course persistent group, comprised of 7% of the cohort, having a fairly stable high trajectory, and evincing more symptoms than any of the other groups at every age; (2) a group whose trajectory resembled an adolescence-limited pattern (14%), who began with two symptoms at age seven but increased to a peak of 4.5 symptoms at age 18, and then decreased on a slight downward trajectory to 3.5 symptoms at age 26; (3) a recovery group comprised of 21% of the cohort, who started at the same high point as the life-course persistent group but decreased steadily with age, having only one symptom by ages 21 and 26; (4) an abstainer group, comprised of 11% of the cohort, and having less than one symptom on average at every age; and (5) a low-level group (47%) who average about one symptom a year between ages 7 and 26. The pattern of conduct disorder regarding Moffitt’s two offender typologies (adolescence-limited and life-course persistent groups) over the follow-up period were as expected. At age 26, males on the adolescence-limited trajectory were still engaging in property offending and substance abuse, but not serious offending, while males on the life-course persistent trajectory were elevated on mental-health problems and substance dependence, numbers of children sired, financial and work problems, domestic abuse of women and children, and drug-related and violent crimes.

Lastly, we close this section by reviewing two studies that employ the trajectory methodology in a novel way. Paternoster, Brame, and Farrington (2001) sought to examine the relationship between adolescent and adult conviction frequencies using the CSDD data. To examine this relationship, these authors first identified distinct trajectories using data through adolescence. This analysis yielded three trajectories, a group of low-rate offenders (including about 72% of the sample), a medium-rate offender group (comprised of about 23% of the sample), and a high-rate group comprised of about 5% of the sample. Then, after conditioning on adolescent offending behavior (using the trajectories), they asked whether variation in adult offending was consistent with a random process, and found that indeed it was. This implies that, after conditioning on adolescent variation in convictions, random variation in criminality during adulthood was sufficient to account for the adult conviction frequency distribution (Paternoster et al., 2001:213).

Because this finding challenges life-course views of criminal activity across age, and specifically the hypothesis that post-adolescent events matter in significantly altering the pattern of adult criminal activity, Piquero, Brame, and Moffitt (2005) sought not only to replicate the Paternoster et al. finding with a different data source, but also examine whether the finding could be replicated across gender. Using conviction data from ages 13 to 26 for males and females participating in the Dunedin Multidisciplinary Health and Human Development Study, Piquero and his colleagues not only replicated the main finding of the Paternoster et al. paper, but also replicated it across gender. Their full sample analysis, which yielded three trajectories, indicated that variations in adult offending through age 26 in the Dunedin data were consistent with a random process after conditioning on adolescent differences in the propensity to offend. Both the male analysis, which identified three trajectories, and the female analysis which identified only two trajectories, also led to the same conclusion.

## Discussion

The purpose of this chapter is to take stock of what criminologists have learned regarding the longitudinal patterning of criminal activity using the trajectory methodology. Although not without its share of criticism, the group-based methodology is well-suited for the study of a behavior that does not vary regularly throughout the population (Raudenbush, 2001), but instead tends to reveal itself in markedly different intensities in sub-population clusters of individuals (Nagin, 2005), like crime. Here, a number of key findings that have emerged from this line of research are provided. The chapter closes by identifying a number of important research directions.

Use of group-based methods to estimate trajectories of criminal activity over the life-course suggests that there is a fair degree of consistency among and across a wide range of samples with respect to group number and shape, but that length of follow-up and age range may affect substantive conclusions regarding the shape of the trajectory (though this no fault of the methodology per se). It is imperative then,

that users bear in mind that the group-based approach is just an approximation. That being said, the current review of the various longitudinal studies centers on four main conclusions.

First, consistent with taxonomic theories of crime over the life-course (e.g., Moffitt, 1993; Patterson, 1993), trajectory-based empirical research does show an adolescent-peaked pattern and a chronic offender pattern, the latter which evidences declines in most studies. These findings emerge across a range of studies around the world, with different follow-up lengths, sample depictions, and use of self-, parent-, and teacher-ratings as well as different sources of official records including police contacts, arrests, and convictions. At the same time, the trajectory methodology has also identified another group of offenders not anticipated by most developmental, group-based theories. This late-onset chronic group, which begins offending in the middle to late portion of adolescence and continues offending at a steady rate into adulthood shows up in a number of different studies, regardless if offending is measured according to self-report or official records. Identification of this group is a good illustration of the value of the trajectory methodology because “continuous” methods would not have identified this group.<sup>21</sup> Further, some trajectory-based analysis indicates that some groups exhibit different age peaks than other groups (i.e., some peak in adolescence while others peak in early or middle adulthood, and sometimes these peaks vary across crime types—see Sampson & Laub, 2003). In short, the trajectory method has identified interesting peaks, shapes, and patterns of offending that bear relevance for criminological theory and that warrant continued exploration and explanation.

Second, on average, between three and five groups tend to be identified by the trajectory methodology, slightly more with self-reports than official records (likely due to more frequencies in the self-reports to partial individuals out better). That the methodology consistently identifies this number of groups in over 80 empirical studies certainly suggests some sort of generality in the findings.

Third, there has been some discussion regarding the identification of groups, and specifically the number of groups identified, dependent on sample size (Eggleston et al., 2004; Nagin & Tremblay, 2005a; Roeder et al., 1999). In one paper, D’Unger et al. (1998) drew three samples from the 1958 Philadelphia Birth Cohort of sizes 500, 1,000, and 2,000 and found that a model with five categories of offenders was robust to sample size. This is no small matter as it implies that, “rather than merely representing a discrete approximation to an underlying continuous distribution of unobserved delinquent/criminal propensity, the small number of latent offending categories estimated in [the] models may represent distinct classifications of offenders with respect to age trajectories of offending that are meaningful in and of themselves” (D’Unger et al., 1998:1622). In short, group characterization seems to be robust to sample sizes over 500.

Fourth, despite the varying numbers of latent offending classes, there clearly emerge a small number of typical age patterns. Generally, there tends to be a low rate

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<sup>21</sup> The use of the term “chronic” is solely illustrative. Criminologists need a new definition of the term chronic that goes beyond the flat  $\lambda$  definition and deals with both time and frequency.

group, a high rate group, a moderate but declining group, and a late onset group. At the same time, there is no simple answer to the number of groups because this varies by a wide range of conditions (i.e., the number of time points (more assessments, more trajectory groups), spacing of time points (annual spacing identifies more trajectories than bi-annual), informant method (self-reports identify more trajectories), conceptualization of outcome variable (delinquency scales generate more trajectories), age spans (longer age spans generate more trajectories), and time spans (longer studies yield more trajectories). It is also likely that the shape of the trajectories is due, in part, to the amount of information contained in the dependent variable(s). For example, self-reports of delinquency—especially in adolescence—contain very healthy offending frequencies, whereas police contacts, arrests, and in particular convictions, are likely to be thinner.

### *Where do we go from here?*

It is clear that much has been learned about the longitudinal patterning of criminal activity via the trajectory methodology. But as is commonly case, answers to some questions open up a whole host of new questions. A number of these questions, organized into five broader areas of inquiry, are outlined below with the hope that researchers will find them sufficiently appealing to address.

The first set of questions deal with methodology and the robustness of findings, especially regarding the number of groups, across some methodological variation. First, earlier in this chapter, the observation was made that there is a slight tendency to uncover more trajectories—and with different shapes—in self-report records as opposed to official records. It may be then, that the greater amount of information available in self-report records allows for a better partialing of the offenders into more latent classes. There is a pressing need to document and understand these differences, and ideally such an investigation would be undertaken with a longitudinal sample followed from birth to adulthood that contain yearly observations of both self-report and official records. Second, there has been some work on the sensitivity of the trajectory methodology to non-random sample attrition, specifically incarceration and mortality (Eggleston et al., 2004; Piquero et al., 2001). Since both of these studies utilized official records, it remains unknown whether trajectories identified via self-report records also reveal similar sensitivities.<sup>22</sup> Further, there may be other forms of non-random sample attrition, such as refusal to continue participating in a longitudinal study, that are in need of studying within the context of the trajectory methodology. Third, there is a need to focus on individual-level variations within a trajectory group. Because individuals are assigned to the trajectory group to which they have the highest probability of belonging to, it is possible (and actually realistic) that some members may deviate from the overall pattern. It will be interesting to examine these deviations in further detail generally, and the factors

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<sup>22</sup> As stated earlier, the trajectory methodology is not the only approach that is sensitive to these issues. All other statistical techniques for studying crime over the life-course face the same concerns (Nagin, 2004a).

that are associated with such deviations specifically. Fourth, it is useful that the trajectory methodology allows and can control for periods of intermittency, or the stops and starts that characterize many offending careers. While theoretical explanations of the zig-zagging between offending and non-offending periods throughout individual criminal careers are lacking (Nagin & Land, 1993; Piquero, 2004) and are needed in order to develop more complete accounts of criminal activity, further exploration of intermittency within the trajectory method is needed especially across reporting method (official versus self-report records). Finally, there is a need for the kind of cross-site, cross-sample replication that was undertaken by Broidy et al. and D'Unger et al. where the same question was applied across a number of different data sources. While such research is difficult to undertake because of differences in measurement, operationalization, age/time spans, and so forth, it provides an important means of replication of substantive results.

A second set of questions deal with the exploration of more substantive issues. First, one of the key findings emerging from studies using the trajectory methodology to identify longitudinal patterns of offending is the almost routine identification of a "late onset chronic" group. Because extant developmental theory does not anticipate this group (see Patterson, 1997 for an exception), theoretical modification is in order. Moreover, extant criminological theory must also come to grips with this group and further examine if the traditional processes assumed for all offenders (in terms of persistence and desistance) similarly hold for this group. Second, the study of desistance is central to matters of theory and policy and longitudinal studies are ideal for helping researchers better understand the process of desistance and its time-varying correlates. The trajectory methodology stands in a good position to study this question (Bushway et al., 2001; Laub et al., 1998), and the findings reviewed earlier by Bushway and colleagues (2003) using the trajectory methodology to study desistance in the Rochester Youth Development Study are illustrative. Future efforts should shy away from arbitrary desistance definitions and consider employing the trajectory methodology to study the desistance question. Third, Sampson and colleagues (2004) have called into question the existence of a stable group of high-rate offenders, or what Moffitt has termed the "life-course persistent" offender. One wonders whether Moffitt meant that life-course persistent offenders offend at similarly high, stable rates throughout their entire lives right up until their death (see Moffitt, 2006 for a counterpoint). Thus, while Sampson et al.'s findings are important in documenting that very few 70 year olds offend at stable rates, we do not think that Moffitt (and others) had this in mind when constructing their taxonomic theories (Piquero & Moffitt, 2005). Still, it would be interesting to examine whether Sampson and Laub's findings hold in other datasets, especially since prior research shows continued self-report criminal activity even in the absence of officially recorded criminal activity well into adulthood (see Nagin, Farrington, & Moffitt, 1995). Fourth, while researchers have begun to examine the role of gender with regard to offending trajectories (Broidy et al., 2003; D'Unger et al., 2002; Piquero, Brame, & Moffitt, 2005), there has yet to be an analysis that has examined race differences in offense trajectories over the life-course. Important questions emerge here: do Blacks have different trajectories than whites and/or Hispanics? Do these patterns differ across violent and non-violent crimes? Given that Blacks persist in crime longer than Whites (Elliott, 1994), and that this is due largely

to prevalence differences in violence over time, it seems important to document not only race differences in longitudinal patterns of crime and the factors associated with such trajectories, but also to do so across reporting method. As Moffitt (1994) anticipates more African-Americans in the life-course persistent group, this is an important theoretical hypothesis deserving of attention.

The third set of questions deals with changing contexts and changing life events and how they relate to trajectories. First, the Moving to Opportunity studies have demonstrated the importance of assessing the role of changing contexts on individual outcomes. However, to date, such an examination has not been considered with the trajectory methodology. For example, what happens to offending trajectories when individuals move from one context (say high poverty/high crime) to another context (say low poverty/low crime)? Or what happens when an individual changes school districts? What happens to their offending trajectory? What happens to their grades trajectory? These questions, as well as several not listed above, should prove to be very interesting applications of joint trajectory models. Second, and more generally, the use of the trajectory methodology stands in a unique position to examine how life events alter either upward or downward trajectories of offending holding individual differences constant (Nagin, Pagani, Tremblay, & Vitaro, 2003). Unfortunately, very few studies have been completed in this regard. One in particular that is worthy of exploration stands out: the joining of a gang and how that influences a criminal trajectory thereafter. Relatedly, the effect of life events on crime trajectory may vary according to the phase in the life-course in which the life event occurs; for example having a child at age 17 may exert a different kind of effect on crime when compared to having a child at age 27. In short, extensions of the trajectory methodology can be used to make causal inferences about the impact of turning points and interventions on developmental trajectories (Haviland & Nagin, 2005; Wang et al., 2005).

A fourth question concerns the relevance of the results emerging from trajectory applications for addressing policy-related questions. Here, a specific example is presented along with a specific word of caution. In the first example, consider the case where two trajectory systems start at the same point, but then go in two different directions (one stays high on crime while the other evinces a sharp decrease). The correlates associated with these two trajectories may differ and to the extent that they do, this would potentially imply different points of intervention (while at the same time recognizing that the same intervention may not be applicable to all offenders or even all offenders in a particular trajectory). At the same time, a specific word of caution must be noted. Researchers need to be careful that policymakers do not take high-rate chronic offenders (or whatever label is applied to the highest-rate offending group) and “do” something with them. The fear here, of course, is that the high level of offending portrayed by these individuals will make them candidates for specific and harsh punishment experiences.<sup>23</sup>

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<sup>23</sup> Stated differently, the risk is more about “doing something” to people *predicted to be* high-rate offenders. This is the classic problem of prospective identification of high-rate offenders (Gottfredson & Hirschi, 1986). The issue may be more magnified within the trajectory context, when group identification combined with the ever-present problem of reification seems to heighten the risk.

Finally, for the most part, the trajectory methodology has been used to identify distinct trajectory systems and determine how they vary along relevant covariates. Another interesting though under-developed use of the trajectory methodology is that by identifying distinct groups, it can afford researchers new opportunities. For example, if a researcher using the trajectory methodology identified four distinct offender groups by age 18 using police arrest data, s/he could then take a random sample of five or ten individuals from each of those groups and then conduct a series of qualitative interviews from members of each trajectory system. This approach allows the trajectory methodology to use the empirical information to form a qualitative component to the study without selecting individuals based on some relatively arbitrary criterion.

This chapter reviewed what has been learned about crime over the life-course with the adoption of the trajectory methodology. This new method is being used increasingly often and offers some glimpses to criminal activity that are unavailable with other methods. Of course, this does not mean that this approach is superior to other approaches; it is but one in the criminologist's toolkit. Researchers studying the natural history of offending have at their disposal a number of important methodological tools that can be used to document the patterning of criminal activity. All of these tools make assumptions, and all of these tools have both strengths and weaknesses. Research should capitalize on these strengths, develop approaches that recognize and modify weaknesses, and continue to investigate the issues addressed in this chapter. Replication and convergence of substantive conclusions across different methodologies, is important because it speaks directly to fundamental debates in the field of criminology in general, and developmental criminology in particular.

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**Appendix** Group-based studies, 1993–2005, in order of publication

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Nagin and Land	1993	South London (CSDD) (n = 411)	10–32	White	Males	Convictions	4	First paper to use group-based methodology
Nagin et al.	1995	South London (CSDD) (n = 411)	10–32	White	Males	Convictions Self-reports (ages 10–14, 15–18, 28–32)	4	AL-group had no convictions, but did self-report drinking, drugs, and fighting
Land, McCall, and Nagin	1996	1958 Philadelphia Birth Cohort (n = 1,000 random subsample)	8–26	White Non-White	Males	Police contacts	4	
Land and Nagin	1996	South London (CSDD) (n = 411)	10–32	White	Males	Convictions	3	
D'Unger et al.	1998	(1) South London (CSDD) (n = 411); (2) 1958 Philadelphia Birth Cohort (n = 27,160); (3) 3 Racine, WI cohorts, n = 353–1942 cohort, n = 721–1949 cohort, n = 1,067–1955 cohort.	(1) 10–32 (2) 8–26 (3) 1942 cohort, 8–30, 1949 cohort, 8–25, 1955 cohort 8–22	(1) white (2) white/black (3) white/black	(1) males (2) males (3) males	(1) convictions (2) police contacts (3) police contacts	(1) four (2) five (3) 1942-five 1949-four 1955-four	
Laub et al.	1998	Boston area delinquents (n = 500)	7–32	White	Males	Arrests	4	Marriage effect

(continued)

## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Nagin	1999	(1) South London (CSDD) (n = 411) (2) Montreal Longitudinal Study (n = 1,037)	(1) 10–32 (2) 6–15	(1) White (2) White	(1) Males (2) Males	(1) Convictions (2) Teacher ratings	(1) 3 (2) 4 physical aggression, 3 symptoms of physical aggression	Illustrative article detailing methodology
Roeder et al.	1999	South London (CSDD) (n = 411)	10–32	White	Males	Convictions	4	Allow for uncertainty of latent class membership; develop a model for multivariate analysis of risk factors
Nagin and Tremblay	1999	Montreal Longitudinal Study (n = 1,037)	6–17	White	Males	Teacher ratings of externalizing behavior problems	4 physical aggression; 4 opposition; 4 hyperactivity	
Muthén and Shedden	1999	NLSY1979 (n = 935)	18–25	White Non-White	Males Females	Self-reports	3	Heavy drinking
Bushway et al.	1999	1958 Philadelphia Birth Cohort (n = 13,160)	6–26	White Non-White	Males	Police contacts	3	
Fergusson, Horwood, and Nagin	2000	Christchurch Health and Development Study (n = 1,265)	1–18	White	Males Females	Self-reports	4	

Muthén and Muthén	2000	NLSY1979	18–30	White Non-White	Males Females	Self-reports	4	Heavy drinking
Maughan, Pickles, Rowe, Costello, and Angold	2000	Great Smokey Mountains (n = 1,419)	9–16	White Non-White	Males Females	Self-reports Parent reports	3 aggression; 3 non-aggression	Boys had more aggression than girls.
Hill et al.	2000	Seattle Social Development Project (n = 808)	10–21	White Non-White	Males Females	Self-reports	4	Focused on binge-drinking and adult outcomes
Jackson, Sher, and Wood	2000	Freshman Midwestern University (n = 449)	18.5–24	White Non-White	Males Females	Self-reports	5	Examined alcohol-tobacco use disorder comorbidity
Chassin et al.	2000	Midwest Cohort Sequential Study of Natural History of Smoking (n = 8,556)	12–18 through 21–31	White	Males Females	Self-reports	6 initially, but settled on 4	Cigarette smoking
White, Johnson, and Buyske	2000	New Jersey Telephone Survey	15–28	White Non-White	Males Females	Self-reports	4 alcohol; 3 cigarette use	Alcohol and Cigarette Use

(continued)

## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Colder et al.	2001	Project Star (n = 323)	11–13 through 15–17	White Non-White	Males Females	Self-reports	5	Cigarette smoking
Land et al.	2001	South London (CSDD) (n = 411)	10–32	White	Males	Convictions	2	Concerned with hidden heterogeneity issue
Brame et al.	2001	Montreal Longitudinal Study (n = 1,037)	13–17	White	Males	Self-Reports	3	childhood aggression; 6 adolescent aggression (but focus on 4); 7 joint childhood & adolescent aggression
Jones et al.	2001	South London (CSDD) (n = 411) Montreal Longitudinal Study (n = 1,037)	(1) 10–32 (2) 10–15	(1) white (2) white	(1) males (2) males	(1) convictions (2) teacher ratings of oppositional behavior	(1) 4 using counts, 3 using prevalence (2) 5	
Piquero et al.	2001	California Youth Authority parolees (n = 272)	18–33	(1) white (2) non-white	males	arrests	6 with exposure time; 6 without exposure time	Size and shape of groups vary with and without controls for exposure time

Paternoster et al.	2001	South London (CSDD) (n = 411)	10–17 18–40	White	Males	Convictions	3 for adolescent convictions	Use adolescent groups to predict adult conviction frequencies
Nagin and Tremblay	2001a	Montreal Longitudinal Study (n = 1,037)	6–15	White	Males	Teacher ratings of physical aggression, oppositional behavior, hyperactivity, inattention, anxiety, and prosocial behavior	4	
Nagin and Tremblay	2001b (Psych Methods)	Montreal Longitudinal Study (n = 1,037)	6–17	White	Males	Teacher ratings (10–15) Self-reports (11–17)	4 in teacher reports; 6 in self-reports of property crime	
Brame et al.	2001	1945 Philadelphia Birth Cohort (n = 9,945) 1958 Philadelphia Birth Cohort (n = 13,160)	(1) 10–17 (2) 10–17	White Non-White	Males	Police contacts	(1) 3 (2) 3	Examined association of violent and non-violent crime over time
Li, Duncan, and Hops	2001	Longitudinal Study in Two Northwestern Urban Areas (n = 179)	11–18	White Non-White	Males Females	Self-reports	2	Alcohol use

(continued)

## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Cote, Zoccolillo, Tremblay, Nagin, and Vitaro	2001	Quebec Longitudinal Study (n = 820)	6–12	White	Females	Teacher ratings of disruptive behaviors	4	Female specific analysis examining predictors of conduct disorder
Christ et al.	2001	HomeNet Project (n = 339)	Unknown	Whites Non-Whites	Males Females	Computer-generated use records of internet use	4	Examined internet use trajectories
White, Bates, and Buyske	2001	Rutgers Health and Human Development Project (n = 698)	T1 (12–18)/T4 (25–31)	White Non-White	Males	Self-Reports	4	Persistent delinquents only scored higher than adolescence-limited delinquents on one risk factor, disinhibition
White, Pandina, and Chen	2001	New Jersey Telephone Survey	T1 (12)/T5 (30/31)	White Non-White	Males Females	Self-reports	3	Cigarette use
Cote, Tremblay, Nagin, Zoccolillo, and Vitaro	2002a	Quebec Longitudinal Study (n = 1,569)	6–12	White	Males Females	Teacher ratings of hyperactivity, fearfulness, helpfulness	Hyperactivity: 4 for boys/girls; Fearfulness: 3 for boys/girls; Helpfulness: 3 for boys/girls	Used combinations of groups to predict conduct disorder

Cote, Tremblay, Nagin, Zoccolillo, and Vitaro	2002b (JCPP)	Quebec Longitudinal Study (n = 1,865)	6-12	White	Males Females	Teacher ratings of impulsiveness, fearfulness, helpfulness, 5	Hyperactivity: 4 for boys/girls; Fearfulness: 3 for boys/girls; Helpfulness: 3 for boys/girls	Descriptive paper
Fergusson and Horwood	2002	Christchurch Health and Development Study (n = 1,265 - 896)	12-21	White	Males Females	Self-reports of conduct problems	Parent/Teacher reports of conduct problems	Binge-drinking
Chassin, Pitts, and Probst	2002	Longitudinal Study of Binge Drinking (n = 454)	12-23	White Non-White	Males Females	Self-reports	4	Binge-drinking
Li, Barrera, Hops, and Fisher	2002	National Youth Survey (n = 188)	14-18	White Non-White	Males Females	Self-reports	2	Alcohol use
Colder, Campbell, Ruel, Richardson, and Flay	2002	No-Treatment Control Condition of the Television, School, and Family Smoking Prevention and Cessation Project (n = 1,918)	Grades 7-12	White Non-White	Males Females	Self-reports	3 (frequency) 4 (quantity) 5 (quantity & frequency)	Alcohol use
D'Unger et al.	2002	1958 Philadelphia Birth Cohort (n = 3,000 females, 1,000 males)	10-26	White Non-White	Males Females	Police contacts	3 females 5 males	Similar patterns across gender, but higher lambda for males

(continued)

## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Chung, Hill, Hawkins, and Gilchrist	2002	Seattle public school students (SSDP) (n = 1,053)	13–21	White, asian, black, other	Males Females	Self-reported offense seriousness	5	Escalator group still offending into adulthood
Casswell, Pledger, and Pratap	2002	Dunedin Multidisciplinary Health and Human Development Study (714 drinkers)	18–26	White	Males Females	Self-reports	4 (typical quantity per occasion) <sup>3</sup> (frequency of drinking)	Alcohol
Piquero et al.	2002	California Youth Authority parolees (n = 524)	16–28	White Non-White	Males	Arrests	4	Estimated violent and non-violent arrest trajectories simultaneously emerged for violent and non-violent, respectively
Lacourse et al.	2002	Montreal Longitudinal Study (n = 1,037)	11–17	White	Males	Self-reports	6 theft; 6 vandalism; 6 phys aggress	
Chung, Hawkins, Gilchrist, Hill, and Nagin	2002	Seattle public school students (SSDP)	13–18	White, asian, black, other	Males Females	Self-reported offense seriousness	5	

Guo et al.	2002	Seattle public school students (SSDP)	13-18	White, asian, black, other	Males Females	Self-reports	4 binge-drink; 5 cigarette use; 4 marijuana use; 3 other drug use	Used alcohol and drug trajectories to predict sexual behavior at age 21
Lacourse, Nagin, Tremblay, Vitaro, and Claes	2003	Montreal Longitudinal Study (n = 1,037)	11-17	White	Males	Self-reports	3 groups of deviant peer affiliations	Looked at gang membership and crime
Tucker, Orlando, and Ellickson	2003	Rand Adolescent/Young Adult Panel Study (n = 5,694)	13-23	White Non-White	Males Females	Self-reports	4 groups (non-bingers defined a prior, 5 total)	Binge-drinking
Broidy et al.	2003	(1) Montreal Longitudinal Study (n = 1,037) (2) Quebec Provincial Study (n = 2,000) (3) Christchurch Health and Development Study (n = 1,265) (4) Dunedin Multidisciplinary Health and Human Development Study (n = 1,037) (5) Pittsburgh Youth Survey (n = 1,517) (6) Child Development Project (n = 585 families)	(1) 6-15 (2) 6-12 (3) 7-13 (4) 7-13 (5) 7.5-10.5 (6) 6-12	(1) white (2) white (3) white (4) white (5) white/black (6) white/non-white	(1) males (2) males and females (3) males and females (4) males and females (5) males (6) males and females	Teacher ratings Self-reports	(1) 4 boys (2) 4 boys, 4 girls (3) 3 boys, 3 girls (4) 3 boys, 2 girls (5) 4 boys (6) 3 boys, 3 girls	Trajectories increasing in PYS.

(continued)

## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Wiesner and Capaldi	2003	Oregon Youth Study (n = 204)	T1 (9 and 10) T2 (23 and 24)	White (mostly) Non-White	Males	Self-Reports	6 groups	There was more specificity than commonality in correlates of distinctive offending trajectories
Brame, Bushway, and Paternoster	2003	1958 Philadelphia Birth Cohort; sub-sample of 2,657 who were juvenile offenders	18–27	White Non-White	Males	Police contacts	3 using absolute specification, 4 using split-population specification	
Bushway et al.	2003	Rochester Youth Development Study (n = 846)	13.5–22	White Non-White	Males Females	Self-reports	7 (but only four are sizeable)	
Mustillo et al.	2003	Great Smokey Mountains Study (n = 991)	9–16	White	Males Females	Objective measures of weight/height	4	
Schaeffer, Petras, Ialongo, Poduska, and Kellam	2003	Evaluation Study of Universal Preventive Interventions in Baltimore City (n = 297)	6–19/20	White Non-White	Males	Teacher ratings	4	Aggression; Also, focused on impact of trajectories on later adult outcomes

Sampson and Laub	2003	Boston-area delinquents (n = 500)	7-70	White	Male	Arrests	6 for total arrests; 5 for violent and alcohol/drug arrests	Longest longitudinal dataset in the world
Shaw et al.	2003	Allegheny County Women's, Infants, and Children Program in Pittsburgh (n = 284)	2-8	White, black, other	Males	Parent reports of child adjustment problems	4	
Nagin et al.	2003	Montreal Longitudinal Study (n = 1,037)	10-15	Whites	Males	Teacher ratings of physical aggression, inattention, prosocial behaviors	4	Examined effect of grade retention on life-course dynamics
Wiesner and Silbereisen	2003	German adolescents (n = 318)	11-17	White	Males Females	Self-reports of delinquency	4	They found that time-averaged covariates distinguished between trajectory groups better than initial covariates. For example, academic achievement was significant in the time-averaged manner but not in the initial rating approach.

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## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Oesterle et al.	2004	Seattle public school students (SSDP)	13–18	White, asian, black, other	Males Females	Self-reports of heavy drinking	4	Used heavy drinking trajectories to predict health status and behavior at age 24
Eggleston et al.	2004	Boston-area delinquents (n = 500)	7–70	White	Male	Arrests	6	for total arrests, different numbers for other analyses
NICHD	2004	NICHD Study of Early Child Care (10 US Cities, n = 1,195)	2–9	White Non-White	Males Females	Mother ratings	5	Aggression
Orlando, Tucker, Ellickson, and Klein	2004	Rand Adolescent & Young Adult Panel Study (n = 5,914)	13–23	White Non-White	Male Female	Self-reports	6	Smoking (quantity and frequency)
Tremblay et al.	2004	Quebec families with a five-month old newborn (n = 572 families)	17–42 months	White	Males Females	Mother ratings of physical aggression at 17, 30, and 42 months after birth	3	

Hix-Small, Duncan, Duncan, and Okut	2004	National Youth Survey (n = 698)	11-17 (T1)3 waves	White Non-White	Males Females	Self-reports	7 alcohol use7 marijuana use
Ellickson et al.	2004	Rand Adolescent/Young Adult Panel Study (n = 5,833)	13-23	White Non-White	Males Females	Self-reports	4 (abstainers defined a priori, 5 total)
Francis et al.	2004	1953 Home Office Offenders Index Birth Cohort (n = 11,402)	Birth-40	White	Males Females	Convictions	9 males; 3 females Examined in different age bands
Chassin, Flora, and King	2004	Longitudinal Study of Binge Drinking (n = 454)	11-30	White Non-White	Males Females	Self-reports	3 consumption (abstainers defined a priori, 4 total); 3 dependence (persistent and no-diagnosis groups defined a priori, 5 total) Alcohol and illegal drug use and dependence
Weisburd et al.	2004	Seattle, WA street segments (n = 1,544,604)	1989-2002	N/A	N/A	Incident reports	18 (8 stable, 7 increasing, 7 decreasing) One of the few studies to examine trajectories at the macro level

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## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Griffiths and Chavez	2004	Chicago, IL homicide file (n = 831 census tracts)	1980–1995	N/A	N/A	Homicide reports	3 (total homicide rate); 2 (street gun homicide rate); 3 (other weapon homicide rate)	One of the few studies to examine trajectories at the macro level
Hamil-Luker et al.	2004	National Longitudinal Survey of Youth (NLSY) 1979 (n = 2,509)	14–16 through 33–35	White Non-White	Males Females	Self-reports	4 adolescence. Then, among delinquents, partiers, and troublemakers, 3 groups emerged in adulthood; among conformists, 2 groups emerged in adulthood	Diversity in early adulthood cocaine use within groups
Wiesner and Windle	2004	Middle Adolescent Vulnerability Study (n = 1,218)	15.5–17	Whites Non-Whites	Males Females	Self-reports	6	
Bongers, Koot, van der Ende, and Verhulst	2004	Zuid-Holland Longitudinal Study (n = 2,600 – 2,076)	4–16	Whites	Males Females	Parent-reports of CBCL	3 aggression; 4 property viol; 4 status viol; 6 opposition	Males and females had same shape, but male rates were higher than females

Séguin, Assaad, Nagin, and Tremblay	2004	Montreal Longitudinal Study (n = 303)	6-22	Whites	Males	Self-reports	4 aggression; 4 hyperactivity	Relate cognitive-neuro-psychological function to trajectories of aggression and hyperactivity
Piquero et al.	2005	1972 Dunedin Multidisciplinary Health and Human Development Study (n = 1,000)	13-17 18-26	Whites	Males Females	Convictions	3 for males, 2 for females	Use adolescent groups to predict adult conviction frequencies. Results replicate across gender.
Brame, Bushway, Paternoster, and Thornberry	2005	RYDS (n = 727)	11-15 (T1) 15.5-19.5 (T9)	White Non-White	Males	Self-reports Official Arrest Records	2	Violent & Non-Violent Offending
Piquero et al.	2005	Research on the Pathways to Desistance (n = 1,355)	14-18 (T1) 15.5-19.5 (T4) (every 6 months)	White Non-White	Male Female	Self-reports	4 legal cynicism; 5 legitimacy	First to investigate trajectories of perceptions of legal socialization

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## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Shaw et al.	2005	Women, Infants, and Children (WIC) Nutritional Supplement Program in Pittsburgh (n = 284)	1.5–10	White Non-White	Males	Parent reports-conduct problems and Teacher reports-hyperactivity	4	Investigated separate and co-occurring trajectories
Hynes and Clarkberg	2005	National Longitudinal Survey of Youth 1979 (n = 2,093)	14–21 1979 33–40 1998	White Non-White	Females	Self-reports of employment	6	First paper to examine employment trajectories
Abroms, Simons-Morton, Haynie, and Chen	2005	Going Places Problem Behavior Prevention Program (Maryland, n = 1,320)	6th grade – 9th grade	White Non-White	Males Females	Self-reports of cigarette smoking	5	
Stoolmiller et al.	2005	Oregon Youth Study (n = 206)	15–24	White Non-White	Males	Self-reports of depression	4	First paper to examine depression trajectories
Blokland et al.	2005	Criminal Career and Life-Course (CCLS) Study (n = 5,164)	12–72	Whites	Males Females	Official Conviction Records Self-reports	4	Small group of high-rate persisters exhibits relatively high and flat trajectory through age 72

Jackson, Sher, and Schulenberg	2005	Monitoring the Future (n = 21,177 / 15,162)	18–20 / 24–26	White / Non-White	Male / Female	Self-reports	4 alcohol use; 5 cigarette smoking; 7 dual trajectory	4 alcohol use
Jacob, Bucholz, Sartor, Howell, and Wood	2005	Vietnam Era Twin (VET) Registry (n = 330)	<21–40	White / Non-White	Males	Self-reports	4 alcohol use	
Windle, Mun, and Windle	2005	Lives Across Time (LTA, n = 760)	16–25	White / Non-White	Male / Female	Self-reports heavy drinking	4 males; 5 females	
Hamil-Luker	2005	1979 National Longitudinal Survey of Youth (n = 754)	19–27 / 33–41	White / Non-White	Female	Self-reports	4	First paper to examine trajectories of public assistance receipt
Blokland and Nieuwbeerta	2005	Criminal Career and Life-Course (CCLS) Study (n = 5,164)	12–72	Whites	Males / Females	Official Conviction RecordsSelf-reports	4	Links life circumstances to offense trajectories
Tucker et al.	2005	Rand Adolescent/Young Adult Panel Study (n = 5,914 smoking; n = 5,694 binge-drinking, n = 5,833 marijuana use)	13–23	White / Non-White	Males / Females	Self-reports	5 (smoking); 4 (binge drinking); 4 (marijuana use)	Smoking, binge drinking, and marijuana use trajectories

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## Appendix (continued)

Authors	Year	Sample	Age	Race	Gender	Outcome Variable	Number of Groups	Interesting finding
Marshall	2005	1984 South Australia Birth Cohort (n = 3,343 persons with at least one police apprehension between ages 10–17)	10–20	White Non-White	Males Females	Official Police Apprehension	6	Very-high chronic group (n = 25, 1% of the sample), 88% male, 48% indigenous, average of 37 events and average age of 11 at first event
Ezell and Cohen	2005	California Youth Authority Parolees 1981 and 1982 (n = 1,989) 1986 and 1987 (n = 1,443) 1991 and 1992 (n = 1,434)	1981 and 1982 (ages 7–37) 1986 and 1987 (ages 7–33) 1991 and 1992 (ages 7–27)	Whites Non-Whites	Males Females (but analysis limited to males)	Official Arrest Records	6	6 trajectories emerged for each of the three CYA parolee release cohorts
Moffitt	2006	1972 Dunedin Multidisciplinary Health and Human Development Study (n = 525 males)	7–26	Whites	Males	Self-, Mother-, and Teacher- reports	5	Examined conduct disorder symptoms from ages 7 to 26
Piquero and Piquero	2006	Business Software Alliance Global Software Piracy Study (n = 87)	1995–2000	Countries	N/A	Software-shipments Software-pirated	6	Examines software piracy trajectories

# What we have Learned about Early Childhood and the Development of Delinquency

Daniel. S. Shaw and Heather E. Gross

There has been growing interest in identifying very young children at risk for early and persistent trajectories of antisocial behavior. This interest has been motivated by several studies on early- versus late-starting antisocial youth (Moffitt, 1993; Patterson, Capaldi, & Bank, 1991). Several researchers have documented that compared to late starters, who begin delinquent activity in mid- to late-adolescence, early starters show a more persistent and chronic trajectory of antisocial behavior extending from *middle* childhood to adulthood. (Moffitt, 1993; Moffitt & Caspi, 2001). Early starters represent approximately 6–7% of the population, yet are responsible for almost half of adolescent crime and three-fourths of violent crimes (Offord, Boyle, & Racine, 1991). During the past two decades, researchers have become increasingly interested in the possibility that early-starting children can be identified at younger and younger ages (Campbell, Pierce, March, Ewing, & Szumowski, 1994; Richman, Stevenson, & Graham, 1982). The goal of the present chapter is to review what we have learned about factors in early childhood that are associated with the development of serious antisocial behavior in later childhood, adolescence, and adulthood.

## Impetus for Focusing on very Young Children

The impetus for identifying young children and pregnant women (whose children are) at risk for early-starting antisocial pathways (Olds, 2002; Tremblay & Nagin, 2005) is based on findings from two interrelated areas, onset patterns for externalizing behavior and preventive intervention research. First, children who have been found to *not* demonstrate high levels of disruptive behavior during the toddler period are unlikely to begin showing clinically-elevated levels of aggression or other types of externalizing behaviors in later childhood or adulthood, with very few children initially demonstrating high rates of disruptive behavior after age five (Shaw, Gilliom, & Giovannelli, 2000). An example comes from the Pitt Mother & Child Project (PMCP), a study of 310 ethnically-diverse, low-income boys followed from infancy to adolescence. Among boys in the PMCP identified at or above the 90th percentile (i.e., cutoff for clinical meaningfulness) on broad factors of externalizing

symptoms at age 2, 63% remained above the 90th percentile at age 5, and 97% remained above the median (Shaw, Gilliom, et al., 2000). At age 6, 62% remained at or above the 90th percentile and 100% (all 18) remained above the median. In terms of the percentages of children who began showing high rates of externalizing symptoms at school entry, rates were low. Only 13% and 16% of boys below the 50th percentile on externalizing behavior at age two moved into the clinical range at ages five and six, respectively. Thus, the results suggest that it is relatively uncommon for children to begin showing clinically-meaningful conduct problems as late as ages 5 or 6 without a history of demonstrating such behavior in early childhood.

Second, child externalizing problems and parenting practices associated with its persistence appear to be more malleable during early versus later childhood (Reid, 1993). Specifically, prevention and intervention studies initiated prior to school entry have shown greater efficacy for treating children with clinically-elevated rates of externalizing problems than for older children (Baydar, Reid, & Webster-Stratton, 2003; Olds, 2002; Shaw, Dishion, Supplee, Gardner, & Arnds, 2006). The more positive outlook for intervening earlier is likely attributable to several factors, including the shorter duration of the child's problem behavior (i.e., increased malleability), the decreased likelihood of incurring serious damage to parents' optimism for change, and the greater probability of children "growing" out of problem behavior in early versus later childhood.

Despite the potential benefits for early identification and successful prevention of early-starting pathways, caution is warranted. First, aggressive behavior and other types of disruptive behavior are normative during early childhood, particularly in the second and third years when aggressive-like behavior peaks during the life course (Goodenough, 1931; Jersild & Markey, 1935; Shaw, Gilliom, et al., 2000; Tremblay, 1998). Most children learn alternative coping strategies with the advent of increasing verbal skills during the toddler and preschool periods and are not at risk for demonstrating serious forms of antisocial behavior. Related to the issue of stability of disruptive behavior during early childhood is its generalizability to contexts outside of the home at school-age and beyond. In general, only modest links have been documented between parental reports of child disruptive behavior in early childhood and later forms of delinquent activity at school or in the community (Bates et al., 1985; Guerin, Gottfried, & Thomas, 1997; Sanson, Oberklaid, Pedlow, & Prior, 1991; Shaw et al., 2000). It is therefore recommended that the reader maintain a healthy dose of skepticism in digesting the review that follows in which we explore our ability to identify infants and toddlers at risk for becoming delinquent adolescents and adults.

## Outline of Review

After describing criteria for the inclusion of specific studies, we review existing research by timing and domains of risk, beginning with risk factors examined during the prenatal and perinatal periods, then moving to child factors (e.g., early signs of

externalizing symptoms, negative emotionality), parental attributes (e.g., childbearing age, psychiatric history), and parenting factors assessed in the first three years of life. For each domain of risk, we then provide a summary of the overall pattern of findings, acknowledge limitations of the review, and point to future directions that would inform our current state of knowledge.

## Criteria for Inclusion

Before reviewing the literature, it is important to specify our criteria for including and highlighting studies. First, our focus is on early childhood, meaning the period spanning from ages 0 to 3, including studies that were originated during the prenatal period. As the primary purpose of the chapter is to investigate correlates of antisocial behavior stemming from early childhood, we chose to emphasize studies that were prospectively initiated during the infancy and toddler period. Several researchers have reliably established pathways of antisocial behavior for studies that were initiated in the late-preschool and school-age periods (Bates, Pettit, Dodge, & Ridge, 1998; Conduct Problems Prevention Research Group, 2002; Nagin & Tremblay, 2001). Studies that were begun when children were preschool-age (4–5 years old), but assessed earlier predictors retrospectively were excluded to optimize methodological rigor. We did include studies formally initiated after early childhood but that used official records to document parent, child, or socioeconomic risk during the prenatal period or infancy (Arsenault, Tremblay, Boulerice, & Saucier, 2002; Raine, Brennan, & Mednick, 1994). As our focus was on predictors of serious antisocial behavior, we also chose to emphasize studies for which follow-ups of antisocial outcomes were extended to at least middle childhood and preferably adolescence or young adulthood. A listing of these studies is provided in Table 1 at the end of the chapter. In addition, studies that assessed more serious and generally more covert types of antisocial behavior (e.g., delinquent activity) were accorded more weight than those assessing less serious and more normative externalizing symptoms, the latter being generally confined to the home setting (e.g., oppositional behavior). We also chose to highlight studies that used multiple informants and/or methods for measuring early childhood factors and later antisocial outcomes to minimize risk of reporter and method bias. Thus, ideal studies were initiated during infancy or the toddler period and followed children through adolescence or beyond, and included assessments of delinquent activity from an independent reporter or method at later follow-ups. As studies of this type are relatively uncommon (Arsenault et al., 2002; Caspi, Moffitt, Newman, & Silva, 1996; Kandel & Mednick, 1991; Raine et al., 1994; Stattin & Klackenberglarsson, 1993), investigations with less optimal assessments of serious antisocial behavior (e.g., Aguilar, Sroufe, Egeland, & Carlson, 2000; Olson, Bates, Sandy, & Lanthier, 2000) and/or shorter follow-ups (Campbell, Pierce, Moore, Marakovitz, & Newby, 1996; NICHD Early Child Care Research Network, 2004; Shaw, Gilliom, Ingoldsby, & Nagin, 2003) also are

included, with their limitations noted (e.g., follow-up limited to the school-age period, sole reliance on parental report for reports of youth antisocial behavior). Based on the paucity of studies that meet our ideal standard, it is not surprising that even fewer studies include an experimental or genetically-informed design. However, notable exceptions do exist (Cadoret, Yates, Troughton, Woodworth, & Stewart, 1995; Caspi, et al., 2002; Olds, Hill, & Rumsey, 1998).

## **Child Attributes**

A wide array of child attributes have been linked to later antisocial behavior, including perinatal factors such as perinatal and delivery complications (Arsenault et al., 2002; Beck & Shaw, 2005; Breslau, Klein, & Allen, 1988; Kandel & Mednick, 1991; Raine, Brennan, Mednick, & Mednick, 1996; Rasanen et al., 1999) and prenatal exposure to substances (Brennan, Grekin, & Mednick, 1999; Gibson, Piquero, & Tibbetts, 2000; Olson et al., 1997; Wakschlag et al., 1997) in addition to early child behaviors, such as early disruptive behavior, temperament characteristics (Aguilar et al., 2000; Bates et al., 1985; Caspi et al., 1996; Guerin et al., 1997; Sanson et al., 1991; Shaw et al., 2003), attachment security (Aguilar et al., 2000), and language and intellectual skills (Stattin & Klackenber-Larsson, 1993; Stevenson & Goodman, 2001; Werner & Smith, 1992).

While theoretically one might presume that early symptoms of antisocial behavior (e.g., aggression, oppositionality) might merit special consideration in this review because such behaviors represent measures of stability of the target outcome variable, the evidence is rather modest for such continuity, particularly in the first two years of life. In conceptualizing continuity of child effects, both direct and interactive pathways have been hypothesized to predispose children with specific risk attributes to become antisocial youth (Moffitt, 1993; Raine, 2002). As such, the range of child behaviors thought to increase the probability of later antisocial outcomes has been broader than aggressive and oppositional behavior (e.g., lack of inhibition, negative emotionality). In addition, early-identifiable individual differences on specific child attributes are postulated to be moderated by the quality of the child's environment, including family, peer, and community factors (Shaw, Bell, & Gilliom, 2000).

Many of the studies that have measured child attributes and followed youth through adolescence have taken advantage of medical records from the prenatal period to examine associations with later criminal activity. Unfortunately, as most of these studies were initiated when children reached school-age or older (Arsenault et al., 2002), child and family characteristics were not measured during the early childhood period, or were evaluated in a superficial and/or retrospective manner. These are important limitations in advancing our understanding of the progression of externalizing symptoms in early childhood and shedding light on how specific environmental factors may moderate the course of antisocial trajectories (Tremblay & Cote, 2005).

## **Perinatal Factors**

### ***Perinatal and Delivery Complications***

The largest group of studies on prenatal factors has focused on perinatal and delivery complications, with many of these studies examining interactions with proximal and distal indicators of environmental adversity. Studies examining direct linkages with criminal activity, including indicators of violence, have been mixed. For example, one study found 80% of adult male violent offenders to have scores in the high range on delivery complications (versus 30% for property offenders and 47% for nonoffenders; Kandel & Mednick, 1991). However, closer analysis of the data set suggested that rates of later violence were high only among children with prenatal issues *and* unstable family environments (Mednick & Kandel, 1988). Several other studies have found no direct associations between prenatal and delivery complications and later antisocial behavior (Arsenault et al., 2002; Beck & Shaw, 2005; Denno, 1990; Farrington, 1997; Raine et al., 1994), including assessments of serious antisocial activity in middle childhood, adolescence, and adulthood. However, echoing the findings of Mednick and Kandel (1988), those individuals experiencing both perinatal issues *and* some form of psychosocial risk consistently show higher rates of antisocial behavior across informant, method, and developmental period studied (Arsenault et al., 2002; Beck & Shaw, 2005; Hodgins, Kratzer, & McNeil, 2001; Laucht et al., 2000; Tibbetts & Piquero, 1999; Raine, Brennan, & Mednick, 1997; Raine et al., 1994). Domains of psychosocial adversity have ranged from more distal indicators of socioeconomic status (e.g., family income, parental education), generally measured during the school-age period (Arsenault et al., 2002; Laucht et al., 2000), to more proximal measures of parenting quality assessed during the toddler period (Beck & Shaw, 2005), with consistent support for a biosocial framework across studies. The findings by Raine and colleagues (1994) typify this group of studies. Among Danish males, risk of violent crime was found to be predicted by the interaction of perinatal complications and maternal rejection, where “rejection” was defined as “public institution care of infant,” “attempt to abort fetus,” or an “unwanted pregnancy.” Beck and Shaw (2005) provided more intensive measurement of parenting factors (e.g., nurturant parenting measured observationally at age 2) and more distal environmental risk during early childhood (e.g., neighborhood quality, stressful life events, family size and income). Again, no direct effects were found between perinatal complications and youth report of antisocial behavior at age 10; however, a significant interaction emerged between family adversity and perinatal complications, such that low-income boys with high perinatal complications and family adversity were at elevated risk for antisocial behavior.

### ***Prenatal Exposure to Substances***

A substantial number of studies have also been conducted on the effects of prenatal smoking and later antisocial behavior. Similar to research on perinatal

complications, interactions have been found between prenatal smoking and social adversity (Gibson & Tibbetts, 2000; Rasanen et al., 1999). In addition, direct effects of prenatal smoking on serious antisocial behavior have been consistently documented after accounting for distal environmental adversity experienced during the school-age period (Brennan et al., 1999; Fergusson, Horwood & Lynskey, 1993; Piquero, Gibson, Tibbetts, Turner, & Katz, 2002; Rasanen et al., 1999). In addition, heavy and early prenatal smoking has distinguished early- versus late-starting antisocial youth (Brennan et al., 1999; Gibson et al., 2000) such that smoking predicted early-starting, life-course persistent antisocial behavior but not late-starting antisocial patterns.

A similar result has been found for prenatal exposure to alcohol, particularly when children have been subsequently diagnosed with Fetal Alcohol Syndrome (FAS) or Fetal Alcohol Effects (FAE) in early childhood (La Due, Streissguth, & Randels, 1992; Steinhausen, Willm, & Spohr, 1993; Streissguth et al., 1991), with greater consumption or binge drinking early in the pregnancy associated with higher rates of self- and parent-reported antisocial behavior in adolescence and adulthood (Olson, O'Connor, & Fitzgerald, 2001; Jacobson & Jacobson, 1994; Olson et al., 1997). In one clinical sample of 415 adolescents who had been diagnosed with FAS or FAE, 60% had already been arrested (Streissguth et al., 2004). Children of parents who drink moderately to heavily but show no symptoms of FAS or FAE also appear to be at increased risk for antisocial outcomes. However, as parents often continue drinking during the offspring's childhood and their family environments tend to be suboptimal, it is difficult to tease apart the effects of prenatal exposure from being reared by an alcoholic parent (Olson, O'Connor, & Fitzgerald, 2001). Again, the effects of prenatal alcohol use on antisocial outcomes appear to be moderated by the quality of the environment, including high levels of family conflict, child maltreatment, and parental psychiatric diagnosis (Ellis, Zucker, & Fitzgerald, 1997; Loukas, Zucker, Fitzgerald, & Krull, 2003; Zucker, Ellis, Bingham, & Fitzgerald, 1996).

Prenatal exposure to cocaine has showed less consistent results, although few of these studies have followed offspring into adolescence. Nonetheless, among 36 studies reviewed, after accounting for the effects of alcohol, cocaine exposure generally was not associated with school-age externalizing problems (Frank, Augustyn, Knight, Pell, & Zuckerman, 2001). Studies of the long-term effects of other substances on child antisocial behavior are limited. In one study, prenatal marijuana exposure was associated with increased "delinquency," but follow-up of child antisocial behavior was extended only to age 10 and relied exclusively on parent reports of antisocial behavior (Goldschmidt, Day, & Richardson, 2000).

## **Early Child Behavior**

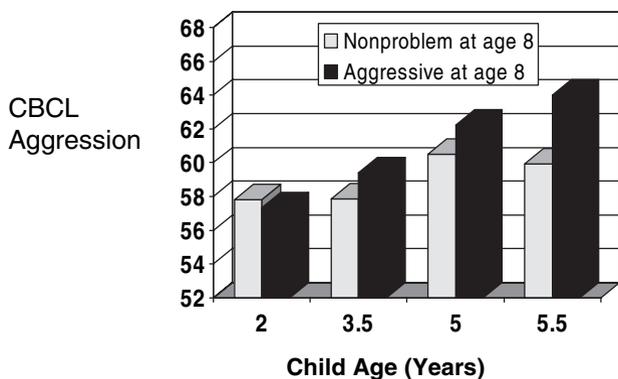
### ***Continuity of Early Disruptive Behaviors***

Several types of behavior in early childhood have been hypothesized to be associated with later serious antisocial behavior. Moffitt (1993) and others (e.g., Rothbart,

Ellis, Rueda, & Posner, 2003) have emphasized behaviors that have been linked to neuropsychological impairment and deficits in executive functioning in older children (e.g., impulsivity, inhibitory control, attention). Others have drawn attention to early signs of specific types of disruptive behavior, particularly aggression (Tremblay & Nagin, 2005). In practice, researchers have investigated broadly defined indices of negative emotionality (Aguilar et al., 2000; Bates et al., 1985; Sanson et al., 1991) and disruptive behavior (Caspi, Henry, McGee, Moffitt, & Silva, 1995; Caspi et al., 1996; Henry, Caspi, Moffitt, & Silva, 1996; Shaw, Bell, et al., 2000; Stevenson & Goodman, 2001; White, Moffitt, Earls, Robins, & Silva, 1990), and recently, more narrowly-defined symptoms, such as fearlessness (Shaw et al., 2003) and physical aggression (Tremblay et al., 2004). Unfortunately, as studies investigating these more narrowly-defined constructs have been initiated more recently, longitudinal data on adolescent outcomes are wanting. A challenge in establishing direct linkages between any of these behaviors and serious antisocial behavior in adolescence is continuity, both homotypic and heterotypic. Homotypic continuity refers to the stability of early and later attributes of the same overt manifestations of behavior, while heterotypic continuity focuses on the sequencing of overtly different behaviors that serve similar underlying functions (Burke, Loeber, Lahey, & Rathouz, 2005). In research on developmental psychopathology in general and antisocial behavior in particular, much work has been focused on heterotypic continuity, by examining how early forms of disruptive behavior in early childhood (e.g., aggression, oppositional behavior) might lead to age-specific forms of antisocial behavior in adolescence and young adulthood (e.g., substance use, unsafe sexual behavior).

In general, the stability of most types of child behavior assessed during infancy in relation to the same or comparable behavior in adolescence is modest, including behaviors known to have high levels of stability and heritability (e.g., intelligence). It is therefore unclear why one might postulate that the stability of disruptive behavior, including aggression, would be high between infancy and adolescence. In fact, when different informants have been used to measure continuity between initial disruptive behavior in children less than 2 years of age and later assessments of disruptive behavior or temperamental attributes associated with disruptive behavior (e.g., negative emotionality, attention), continuity has been modest to nonexistent (Aguilar et al., 2000; Bates et al., 1985; Rende, 1993; Shaw, Owens, Giovannelli, & Winslow, 2001). However, consistent with studies of other types of behavior shown to be stable during childhood (e.g., intelligence, sociability), when measured between the ages of 2 and 3, disruptive behavior and temperamental attributes linked to undercontrolled behavior begin to show modest to moderate correlations with antisocial behavior assessed in late middle childhood and adolescence (Campbell et al., 1996; Caspi et al., 1995; Henry et al., 1996; Olweus, 1979). In most cases the magnitude of association decreases markedly when a second informant or method is used at the follow-up. Interestingly, studies that have found the strongest support for stability, or at least heterotypic continuity in behavior, have assessed behavior in early childhood using observational methods (Caspi et al., 1995).

A study that typifies the level of association between early maternal reports and later outcomes was conducted by Shaw, Bell et al. (2000), in which maternal reports of aggression at ages 2, 3.5, and 5 (i.e., CBCL Aggression factor) were correlated



**Figure 1** Teacher-reported aggression at age 8, as predicted by mother-reported aggression at ages 2 to 5.5

Note: At age 8, “aggressive” is factor scores at or above 90th percentile; mother-reported aggression based on CBCL Aggression factor; effect sizes range from  $-.1$  to  $.48$  *sd*.

Note to publisher: figures are original, based on data reported in Shaw, Bell and Gilliom (2000)

with clinically-elevated levels of teacher reports of aggression at age 8 (i.e., at or above the 90th percentile on the Aggression factor of Achenbach Teacher Report Form). As displayed in Figure 1, those boys viewed by teachers as aggressive at age 8 could not be discriminated from maternal reports of aggression at age 2 ( $d = -.1$ ), only modestly so at age 3.5 ( $d = .3$ ) and moderately so at age 5 ( $d = .5$ ). The results, albeit limited to follow-up at age 8, suggest that relying solely on maternal reports of child disruptive behavior for identifying toddlers who will show early starting pathways may be misguided. Other studies aimed at investigating heterotypic or homotypic continuity between initial maternal reports of disruptive behavior or negative emotionality between 6 months and 2 years in relation to later externalizing problems during the school-age period or adolescence using a second informant have yielded similarly disappointing results (Bates et al., 1985; Fagot & Kavanagh, 1990; Olson et al., 2000; Sanson et al., 1991; Shaw et al., 1999), generally indicating nonsignificant associations between maternal reports of infant and toddler behavior and later teacher or youth report of antisocial activity.

Moving to slightly older children has yielded more promising findings. Two pioneering studies that followed selected youth from age three to the school-age period have demonstrated moderate continuity of child disruptive behavior, including ADHD symptoms. Richman et al. (1982) identified the top 14% of 3-year-olds from a parental questionnaire of behavior problems, and followed them in comparison to a control group of children from similar backgrounds. Problems persisted in 62% at age 8 compared to 22% of the controls. In a follow-up of the original Richman sample, including 828 of the original 955 children (i.e., one-in-four random sample of London cohort), Stevenson and Goodman (2001) examined associations between age-3 maternal reports of 24 individual problem behaviors and adult criminal convictions when participants were ages 23 and 24. Of the 24 behaviors,

four were associated with later total number of criminal convictions in univariate analyses: soiling, activity level, daytime enuresis, and management difficulty, with daytime enuresis and temper tantrums associated with adult *violent* convictions. It is important to note that no statistical controls were introduced to control for the number of analyses computed (6 of 48 significant or 12.5%). However, when other significant factors were controlled for in multivariate analyses (i.e., child gender, child social development), activity level and management difficulties continued to be associated with all adult offenses and temper tantrums continued to be associated with violent offenses.

Campbell and colleagues have followed two cohorts of hard-to-manage children from preschool through school-age (Campbell et al., 1996). In the first cohort, for children identified at age 3, moderate continuity in behavior problems was found at ages 6, 9, and 13. Fifty and 48% of those with problems at age 3 showed clinically-significant problems at ages 6 and 9, respectively. Campbell (1994) followed a second cohort of overactive and inattentive boys and found comparable rates of continuity from preschool to school-age. Rates of continuity need to be tempered by the use of parent report at both time points for both studies above. In Campbell's second study, teacher reports were also used at follow-ups and showed significant, but more modest levels of continuity in child disruptive behavior.

Perhaps the most impressive example of continuity in early disruptive behavior and later criminality comes from the work of Moffitt, Caspi, and colleagues (Caspi et al., 1995; Henry et al., 1996; Moffitt, 1990; Moffitt, Caspi, Dickson, Silva, & Stanton, 1996; White et al., 1990). Following a birth cohort of 1,037 New Zealand children from Dunedin, the authors used rater impressions of children's behaviors during 90 minutes of psychological tests (e.g., intelligence tests) at age 3 to examine associations with later reports and official criminal records of antisocial behavior during middle childhood, adolescence, and adulthood. Children who demonstrated a pattern of "lack of control" during the tested period were found to show higher rates of externalizing symptoms at ages 9, 11, 13, and 15 according to parent and teacher reports. In addition, children who showed undercontrolled behavior at 3 were more likely to show a record of convictions for violent (but not nonviolent) offenses at age 18 (Henry et al., 1996), to meet diagnostic criteria for antisocial personality disorder (i.e., odds ratio (*OR*) of 2.9), and be recidivist offenders (*OR* of 2.2) at age 21 (Caspi et al., 1996).

Consistent with the data on perinatal complications, children who showed this early undercontrolled behavior and followed a persistent and severe course of antisocial behavior also were more likely to live in families marked by psychosocial adversity, including low parental educational and occupational status, low income, and higher rates of teen pregnancy, single-parent status, large family size, and poor maternal mental health (Campbell, Shaw, & Gilliom, 2000; Moffitt, 1990). Thus, individual differences in child behavior appear to be moderated by the context of the child's proximal and distal caregiving environment. In addition, as child behavior was not initially assessed until age three, it is likely that ratings of child "temperament" were already moderated by the quality of the caregiving environment, which we can surmise was generally poor in these families, including the quality of prenatal care.

### ***Infant Attachment Security***

According to attachment theory, children who develop insecure attachments with caregivers during early childhood would be expected to develop distrust towards others and an increased probability for demonstrating later noncompliant and hostile disruptive behavior (Greenberg & Speltz, 1988; Shaw, Bell, et al., 2000, Sroufe, 1983). Specifically, those with insecure avoidant (1983) and disorganized (Lyons-Ruth, Alpern, & Repacholi, 1993) attachment classifications towards mothers have been postulated to be at risk for showing an early-starting antisocial pathway. Similar to the study of other child factors in the prenatal period and infancy, risk for showing a persistent pattern of externalizing symptoms is thought to be moderated by the quality of the child's environment beyond the infant and toddler years, and by the presence of other types of child risk factors (e.g., low intellectual abilities) and contextual adversity (Greenberg, 1999). Several studies have documented associations between the avoidant or disorganized infant classification during infancy and preschool-age conduct problems, particularly within high-risk samples (Erickson, Sroufe, & Egeland, 1985; Keller, Spieker, & Gilchrist, 2005; Lyons-Ruth, et al., 1993; Shaw, Owens, Vondra, Keenan, & Winslow, 1996; Shaw & Vondra, 1995), with less consistent results for lower-risk, middle-class samples (e.g., Bates et al., 1985; Fagot & Kavanagh, 1990; Lewis, Feiring, McGuffog, & Jaskin, 1984). However, only one high-risk sample has established an association between insecure infant attachment and antisocial behavior in adolescence (Aguilar et al., 2000; Appleyard, Egeland, Manfred, & Sroufe, 2005; Renken et al., 1989). Children in this Minnesota-based, low-income sample with avoidant attachments at 12 and/or 18 months of age were more likely to show an early-starting and persistent pattern of externalizing symptoms (i.e., not delinquent activity or criminal offenses) from early childhood through age 16. As attachment insecurity has been linked to parenting practices, more discussion on this topic is included in the section on early parenting and antisocial outcomes.

### ***Intellectual Skills and Language Development***

Among school-age children, adolescents, and adults, a consistent albeit at times modest, association has been found between lower intellectual skills and antisocial behavior (Binder, 1988; Fergusson, Horwood, & Ridder, 2005; Moffitt, 1993; Frost, Moffitt & McGee, 1989; Raine, 1993). Prospective studies of intellectual abilities are limited in early childhood, in part because of the lower predictive validity for IQ when assessed during early versus later childhood. However, a few longitudinal studies initiated in early childhood have collected data on formal verbal or spatial skills, or language development (Moffitt, 1990; Raine, Yaralian, Reynolds, Venables, & Mednick, 2002; Stattin & Klackenber-Larsson, 1993; Werner & Smith, 1992). In the Moffitt study using the Dunedin, NZ cohort, visuomotor deficits at age 3 were associated with an early-starting and persistent pattern of antisocial behavior

through early adolescence. A similar pattern was identified by Raine and colleagues (2002), in which a subsample of 330 of a larger cohort of 3-year-old children from Mauritius (i.e., total  $N = 1,795$ ) were tested at age 3 and followed until age 17. Those children with spatial and not verbal deficits were more likely to show a persistent pattern of antisocial activity. The authors hypothesize that spatial deficits interfere with children's ability to form social relationships with parents (i.e., attachment) because of disruptions in the right hemisphere of the brain where affect regulation and expression are modulated. Werner and Smith (1992), following a birth cohort of 614 youth from Kauai, found court records of delinquency during adolescence were predicted by self-help skills for boys and below-average intellectual skills for girls, both of which were assessed at age 2. Below-average intellectual skills for girls was the strongest predictor of adolescent antisocial behavior during the early childhood period ( $r = .38$ ).

Finally, Stattin and Klackenberg-Larsson (1993) followed a sample of 122 unselected Swedish boys from birth to adulthood, assessing parent reports and examiner ratings of language development from 6 to 24 months and child intellectual skills at age 3 in relation to official records of criminal behavior up to the age of 30. Overall intelligence at age 3 was related to frequency of offending during adulthood. In addition, examiner ratings of language ability at 6, 18, and 24 months were also negatively associated with adult registered offending (i.e., correlations between  $-.16$  and  $-.19$ ,  $p < .05$  at all time points).

Overall, there is some evidence to suggest that deficits in spatial and verbal abilities in early childhood are associated with later serious antisocial behavior. Effect sizes tend to be modest, as they are for other early childhood factors described above, and appear to be moderated by the quality of the environment. Specifically, those in the Moffitt (1990) study showing low visuomotor skills and persistent antisocial pathways also were likely to live in adverse psychosocial contexts. In the Stattin and Klackenger-Larsson (1993) study, deficits in language ability were associated with family SES and the time parents spent playing with and reading to the child.

## Parental Attributes

Several parental attributes and "social address" factors (e.g., socioeconomic status) that have been associated with antisocial behavior in school-age children and adolescents (Bovet, 1951; Loeber & Dishion, 1983) also have been linked to externalizing problems in early childhood (Shaw, Vondra, Dowdell Hommerding, Keenan, & Dunn, 1994; Shaw, Winslow, Owens, & Hood, 1998; Tremblay et al., 2004). In addition, factors such as maternal depression, parental history of antisocial behavior, parental educational attainment, occupational status, and income, all measured in early childhood, have been associated with early *and* later antisocial behavior in school-age children and adolescents (Cadoret et al., 1995; Fergusson & Woodward, 1999; Moffitt & Caspi, 2001; Nagin, Pogarsky, & Farrington, 1997; Nagin & Tremblay, 2001; Shaw et al., 2001). As most of the aforementioned

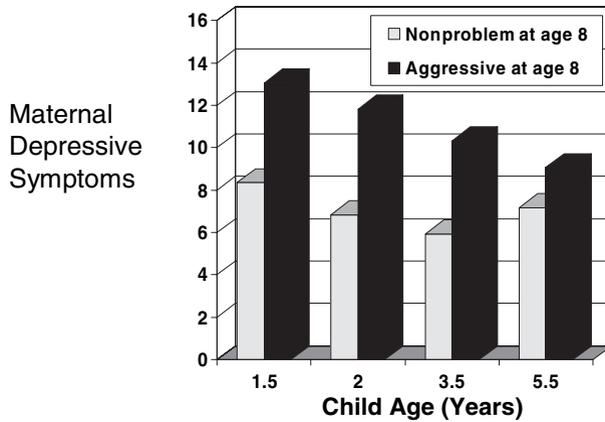
variables are presumed to be fairly stable (SES, parental *history* of psychiatric illness and antisocial behavior), many can be comparably, or even more accurately measured, in early versus later childhood, including maternal childbearing age (Nagin et al., 1997, Nagin & Tremblay, 2001), and the parent's own adolescent and early adult history of criminality and psychiatric disorders (Cadoret et al., 1995; Kandel & Mednick, 1991).

### ***Maternal Child-bearing Age***

Evidence for the effects of early childbearing age have been consistently found across cultures (Fergusson & Woodward, 1999; Moffitt & Caspi, 2001; Morash & Rucker, 1999; Nagin et al., 1997, Nagin & Tremblay, 2001), with most studies demonstrating associations with adolescent or adult antisocial activity among offspring of mothers who began having children as a teenager. It is important to note that in many of these studies, early childbearing was defined by the age mothers had their first child, but not necessarily the target child being studied (Nagin et al., 1997, Nagin & Tremblay, 2001). Theoretically, teen mothers would be less prepared to handle the psychological challenges of child rearing, have fewer economical and educational resources, and a low likelihood of changing their socioeconomic standing for the better because of childrearing responsibilities. Thus, teen parent status is likely a marker variable for multiple indicators of risk. Consistent associations have been documented between early childbearing and offspring's trajectories of persistent antisocial behavior through adolescence (Moffitt & Caspi, 2001; Nagin & Tremblay, 2001) and juvenile and adult offending (Fergusson & Woodward, 1999; Nagin et al., 1997). Given the hypothesized relationships between childbearing age and SES, it is important to note that in most studies, early childbearing status appears to continue to account for significant, albeit modest, variance in offspring antisocial outcomes after accounting for socio economic factors. In addition, early childbearing age also has been found to moderate the effects of prenatal smoking, as offspring of teen parents who smoke prenatally have an increased risk of violent crime and being a repeat offender above and beyond the direct effects of teen parenthood or prenatal smoking (Rasanen et al., 1999).

### ***Parental Psychiatric History***

Similar direct and interactive effects have been found for parental history of antisocial activity or psychiatric illness (Cadoret et al., 1995; Kandel & Mednick, 1991; Shaw, Gilliom et al., 2000). For example, Kandel and Mednick (1991) found that the association between pregnancy complications and adult violent offending was amplified when there was a history of parental psychiatric disorder. As displayed in Figure 2, Shaw, Bell et al. (2000) found a direct link between maternal depressive symptoms when children were 1.5 and 2 years of age and clinically-elevated reports of school-based conduct problems (CP) using the Achenbach Teacher Report Form



**Figure 2** Teacher-reported aggression at age 8, as predicted by maternal depression at child ages 1.5 to 5.5

Note: At age 8, “aggressive” is factor scores at or above 90th percentile; maternal depression based on Beck Depression Inventory; effect sizes range from .27 to .73 *sd*.

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when children were age 8 ( $d = .73$  at age 1.5), associations that were appreciably stronger than for child aggressive behavior measured via parent report during the same age period. Interestingly, the magnitude of effects of depression on age–8 CP decreased with the child’s increasing age ( $d = .27$  when maternal depression was measured at age 5.5). In one of the few studies to incorporate a genetically-informed design, Cadoret and colleagues (1995) did not find a direct link between biological parent’s history of antisocial behavior and offspring reared by adopted parents, but biological parent’s history of antisocial behavior was associated with offspring’s adult antisocial behavior if the quality of the adoptive environment was adverse (i.e., cumulative index of family stress including marital problems, parental divorce/separation, parental anxiety/depression or substance abuse, and legal problems). As environmental adversity was measured retrospectively, these findings need to be interpreted with caution. Nonetheless, the results converge with findings from other studies suggesting that impairments in the biological parents’ functioning increase risk for offspring’s antisocial behavior, particularly within environments characterized by other risk factors (Keenan & Shaw, 1994; Tremblay & Nagin, 2005). From a genetic perspective, the parent’s antisocial activities could represent a propensity for demonstrating impulsive or aggressive behavior. In a similar vein, parental depression could represent a nonspecific marker for psychopathology that is transmitted to the child. It is also likely that antisocial and depressed parents would not be the most responsive caregivers, in the case of the antisocial parent, modeling impulsive and aggressive coping strategies for children, and using verbally and physically aggressive discipline methods. Research on depressed parents also documents higher rates of hostile, passive, and inconsistent patterns of childrearing than among nondepressed parents (Shaw & Bell, 1993; Zahn-Waxler, Iannotti, Cummings, & Denham, 1990).

## Parenting and Family Process Factors

Aspects of caregiving have been highlighted as central causal factors in the development of early-starting antisocial pathways, as postulated by social learning and attachment theorists (Greenberg & Speltz, 1988; Patterson, 1982; Patterson, Reid, & Dishion, 1992; Shaw & Bell, 1993, Shaw, Bell, et al., 2000). From a social learning perspective, parenting management practices that model and reinforce disruptive behavior are hypothesized to be associated with increasingly frequent and severe externalizing symptoms that begin during the “terrible twos” and escalate during the preschool and school-age years. As noted above, with respect to attachment theory, parenting characterized by insensitivity and low responsiveness has been linked with avoidant and disorganized infant attachments and subsequent externalizing problems (Aguilar et al., 2000; Erickson et al., 1985; Lyons-Ruth et al., 1993). In addition, direct measures of maternal unresponsivity and low positivity during infancy and the toddler period have been linked to later externalizing problems (Gardner, 1987; Martin, 1981; Shaw et al., 1994, 1998; Shaw, Bell, et al., 2000; Wakschlag & Hans, 1999). However, the follow-up for most of these studies has been generally limited to the late preschool or early school-age periods. In addition, parenting practices have been hypothesized to mediate associations between more distal risk factors (e.g., maternal age, single-parent status, low SES) and child antisocial outcomes (Conger, Ge, Elder, Lorenz, & Simons, 1994; Patterson, 1982), but again few studies addressing the issue of mediation have been initiated when children were three years of age or younger (Fergusson, Lynskey, & Horwood, 1993).

Fortunately, there are a few studies on parenting and antisocial outcomes that have spanned from early childhood to adolescence (Aguilar et al., 2000; Caspi et al., 2002; Fergusson & Woodward, 1999; Moffitt & Caspi, 2001; Olds, Henderson et al., 1998). As in studies of other risk factors (e.g., perinatal complications), official records have been utilized to extend the measurement period, in particular for child maltreatment (Thornberry, Ireland, & Smith, 2001). However, as it is difficult to gauge when child maltreatment began and over what period of time it occurred, studies are included only when data on childhood maltreatment was initiated during early childhood (Aguilar et al., 2000; Caspi et al., 2002).

In reviewing the literature on early parenting practices and adolescent antisocial behavior, similar themes emerge with risk factors from other domains reviewed earlier. In terms of direct effects, two studies have shown that aspects of parenting differentiate early-starting children from other groups through adolescence. Using the Dunedin cohort, Moffitt and Caspi (2001) found a nonsignificant trend for greater deviant mother-child interaction at age 3 for early-starters versus adolescent-limited youth. Aguilar and colleagues (2000), using the Minnesota, low-income cohort, found that early-starting youth were significantly more likely to be physically maltreated between the ages of birth and two than nonoffending youth, and tended to have parents who were more psychologically unavailable, less involved, and more hostile at age 3. One other study has traced direct associations between early parenting and adolescent outcomes. Using the same factor of observed harsh parenting at age 2 that discriminated maternal-reported trajectories of conduct problems

from ages 2 to 8 (Shaw et al., 2003), significant associations with youth (National Youth Survey's Self Report of Delinquency; Elliott, Huizinga, & Menard, 1989) and teacher report (Delinquency factor from Achenbach Teacher Report Form) of antisocial behavior have emerged for boys at ages 11 and 12 ( $r_s = .20$ , and  $.20$ ,  $p < .01$ , for youth and teacher reports, respectively). As both the Aguilar and Shaw studies were conducted on samples of low-income children, it is likely that the effects of parenting were amplified by the context of social adversity that characterized the vast majority of participants in both samples. Similar interactive effects have been documented in the Dunedin cohort and other studies, where children with early-starting trajectories have been characterized by multiple contextual risk factors (Moffitt, 1990) in addition to compromised parental functioning (Shaw et al., 1994, 1998).

Although not a direct measure of parenting, but related to quality of caregiving (Emery, 1988), are data from family the Kauai longitudinal study on family instability. Werner and Smith (1992) found a composite of serious marital conflict, father absence, parental separation, divorce, and desertion to be the strongest early childhood predictor of adolescent delinquency for boys.

Two other series of studies involving parenting are worth reviewing because of their unique methodological features. The first incorporated a genetically-informed design to examine potential gene-environment interactions with respect to child maltreatment or harsh parenting (Caspi et al., 2002). Using a subsample of boys in the Dunedin cohort, the authors obtained DNA to examine interactions between extreme parenting and monamine oxidase A (MAOA), a neurotransmitter-metabolizing enzyme linked with aggression in mice and humans. With respect to rates of adolescent conduct disorder, and antisocial personality disorder and convictions for violent offenses by age 26, although direct effects of maltreatment or harsh physical discipline were found for each outcome, these associations were significantly amplified when expression of MAOA activity was low. Although the precise age and duration of maltreatment could not be specified, this study remains critical because it offers a method by which risk for antisocial trajectories can be identified based on gene *and* environmental context, echoing other interactive effects that have been found between biologic and social context variables (Arsenault et al., 2002; Beck & Shaw, 2005; Raine et al., 1994).

The second study is a longstanding program of prevention research initiated by Olds and colleagues (Eckenrode et al., 2001; Olds, 2002; Olds, Henderson et al., 1998; Olds, Hill et al., 1998; Olds et al., 2004) studying three cohorts of parents with very young children at heightened risk for maladaptive outcomes, including antisocial behavior. The preventive intervention was designed to begin during the prenatal period and extend through the child's first two years, focusing on reducing adverse maternal behaviors during pregnancy (e.g., smoking, alcohol and drug use) and promoting positive mother-child relationships during infancy via a nurse visitation program. In the first study, a group of 400 European American, rural expectant mothers were randomly assigned to intervention or control groups. Group differences were found in several domains among the 315 offspring followed to age 15, with youth in the intervention group demonstrating significantly fewer arrests and convictions than adolescent offspring in the control group. Interestingly, the strongest results of the intervention were found among low-SES, single-parent fam-

ilies. In understanding the potential mechanisms associated with treatment effects, the authors attribute the changes to reductions in maternal health-related behaviors during pregnancy (i.e., smoking, drinking, alcohol), improvements in maternal health and lifestyle choices during the child's early years (e.g., 43% lower rates of subsequent pregnancy, 84% higher participation in work force, and 82% fewer arrests than control mothers), and significant reductions in rates of child abuse and neglect from birth to age 15 (79% lower rate than control group). Child maltreatment within the sample was associated with early-starting delinquency (as defined by contact with the criminal justice system by age 15), but even among maltreated children in the intervention group, risk of arrest was significantly less than it was for maltreated children in the control group (Eckenrode et al., 2001).

Results from the initial Olds study in Elmira, NY have been followed up in two samples of urban (i.e., Memphis, TN, and Denver, CO), more ethnically-diverse families. Children have not reached adolescence in either of these cohorts, but results from the Memphis sample suggest similar, but more muted effects on child problem behavior (i.e., maternal but not teacher reports show intervention effects, Olds et al., 2004) and maternal functioning (e.g., fewer subsequent pregnancies and pregnancy-induced hypertension) up to age six. The Olds' program is critical because it suggests that pathways leading to serious offending are potentially malleable when interventions are initiated prenatally and during infancy. Importantly, the intervention targets multiple issues, including the mother's health behaviors, the quality of the environment parents are generating for the child (e.g., maternal work skills, number of subsequent children born in the next couple of years), and parenting skills. Methodologically, Olds' program is groundbreaking because it includes an experimental design and a long-term follow-up of child antisocial outcomes, both of which are extremely rare for interventions initiated during early childhood.

Although several interventions have been designed to address the prevention of conduct problems among preschool- and school-age children (Conduct Problems Prevention Research Group, 2004; Webster-Stratton & Hammond, 1997), very few have targeted the "terrible twos" when rates of disruptive behavior increase and parents might be quite motivated to find alternative coping strategies. A new intervention that combines elements of family-based behavioral training with motivational interviewing has recently showed some success in reducing rates of child externalizing symptoms from ages two to four. Motivational interviewing is a clinical method developed by Miller and Rollnick (2002) to treat adults with alcohol problems that provides clients with direct feedback on how their problem behavior has adversely affected their lives (e.g., relationships, employment), generating motivation for clients to provide an internal impetus for change. Dishion and Kavanagh (2003) adapted the method for working with adolescents and families, and it has since been adapted to young children and parents in the midst of the terrible twos. Using an experimental design, in which 120 toddler-age boys with socioeconomic, family, and child risk factors were randomly assigned to an intervention or control condition, reductions in destructive and aggressive behavior and improvements in maternal involvement and positive, proactive parenting were found at ages 3 and/or 4 using Dishion's Family Check Up (Gardner, Shaw, Dishion, Supplee, & Burton, in press; Shaw et al., 2006). Interestingly, families characterized by a profile associ-

ated with early-starting conduct problems (Shaw et al., 2003), namely high levels of maternal depressive symptoms and child fearlessness, showed the greatest between-group improvement in conduct problems at age 4. These results mirror those of Olds and colleagues (1998), demonstrating more improved outcomes for families with higher levels of initial risk. Although follow-up of this sample is needed and we are currently validating the model with a larger number of boys and girls from urban, rural, and suburban contexts ( $N = 731$ ), the initial results support the notion that efficacious interventions tailored to prevent early starting pathways can be initiated in early childhood.

### ***So what have we Learned about Early Childhood and Delinquent Behavior***

Using conservative standards, it is difficult to draw firm conclusions about what we know regarding early childhood indicators of serious antisocial behavior because of the relative dearth of prospective studies that have been conducted spanning from early childhood to adolescence (e.g., Aguilar et al., 2000; Moffitt & Caspi, 2001; Olds, Henderson et al., 1998). Even among studies meeting our most stringent criteria, all have important limitations in terms of the quality of the measurement of the environment during early childhood (Moffitt, Olds), or the measurement of serious antisocial behavior during adolescence (Aguilar). Despite these critical caveats, based on convergent evidence from multiple studies, we can make some tentative statements about risk factors in early childhood and their level of association with later serious antisocial behavior.

1. Consistent, albeit modest, associations have been documented between characteristics of the prenatal environment (e.g., tobacco and alcohol use, maternal age, perinatal complications) and later serious antisocial activity, and these associations tend to be magnified in the context of social adversity during early childhood (e.g., SES, quality of parenting, cumulative family adversity).
2. Associations between child disruptive behavior in early childhood and adolescent antisocial outcomes begin to emerge around age 2 and have been more reliably found when child disruptive behavior is assessed at age 3 and using observational methods. Among children with high levels of early disruptive behavior at ages 2 and 3, only a subgroup go on to demonstrate early-starting, chronic pathways of antisocial behavior, and these youth tend to come from families marked by multiple types of family adversity (low SES, compromised parental functioning and caregiving quality).
3. A few studies have documented associations between history of parental antisocial behavior and/or psychiatric illness during early childhood or the prenatal period and later antisocial outcomes, but as methodologically-refined studies in this area are scarce (e.g., measurement of parental functioning in early childhood and consideration of effects of parental functioning in middle

childhood; follow-up of antisocial behavior through adolescence), corroboration of these associations using more rigorous research designs is needed.

4. Associations between hostile, rejecting, and abusive parenting in early childhood and serious antisocial outcomes in adolescence have been documented in a few studies. Associations tend to be consistent, albeit modest, and similar to the risk factors listed above, have been found to be moderated by the presence of other risk factors (e.g., perinatal risk, expression of low MAOA activity in child, environmental adversity).
5. Only Olds' program of research has demonstrated that interventions initiated in early childhood and the prenatal period can be associated with significant decreases in *adolescent* delinquent behavior, changes that appear to be mediated by modifications in maternal health and well being during the prenatal period and caregiving practices and maternal health/lifestyle choices during early childhood.

So what do we know about assessing risk for serious antisocial behavior in early childhood? We know that it is possible to identify at least a "trace" of what is to follow for many children by focusing on individual risk factors, and that our probability of identification is significantly enhanced by accounting for factors in the child's social context (Greenberg, 1999; Rutter, Cox, Tupling, Berger, & Yule, 1975; Shaw et al., 1994, 1998). Specifically, both direct and interactive effects have been found for the quality of the mother's prenatal care and substance use (e.g., tobacco, alcohol, Olds, Hill et al., 1998), the parents' history of antisocial activity and psychological well-being (Cadoret et al., 1995; Kandel & Mednick, 1991; Shaw, Gilliom, et al., 2000), the quality of early caregiving (Aguilar et al., 2000; Shaw et al., 2003; Shaw, Gilliom, et al., 2000), and beginning around age 2, the child's level of disruptive behavior (Caspi et al., 1995; Henry et al., 1996; Shaw et al., 2003).

Three points are worth highlighting from this conclusion. First, overall this list of risk factors is remarkably similar to those that have been identified for preschool- and school-age children, and with the exception of peer influence (Dishion, Andrews, & Crosby, 1995), similar to risk factors associated with antisocial behavior during adolescence (Tremblay & Cote, 2005). Second, in terms of emphasis, the one exception to the comparability of risk factors for older children is the child's level of disruptive behavior. Whereas for older children, disruptive behavior would be the most reliable predictor of future antisocial behavior, this is not the case for children under the age of 2 (Aguilar et al., 2000) and only approaches this level at age 3 (Campbell et al., 1996; Moffitt & Caspi, 2001; Shaw, Gilliom, et al., 2000). Why would continuity in disruptive behavior increase beginning around age 2? Clearly multiple factors are involved, including children's cognitive capacities for carrying out planful behavior and appreciating the consequences of their aggressive behavior (Maccoby, 1980), as well as their lack of physical mobility and coordination during the first 18 months of life (Shaw & Bell, 1993). There are also the issues of informant bias and contextual continuity. Parents typically see their own children for more hours of the day than other caregivers (with the exception of children in full-time day care) and should therefore be knowledgeable informants of their child's behavior. However, because parental exposure to other same-age children and knowledge of normative behavior is often limited (particularly at this

age period before play dates with other children become more frequent and longer in duration), and many parents assume that their child's behavior is fairly stable and not heavily influenced by their own responses, their perceptions of child externalizing symptoms may show modest continuity across context or over time. Relatedly, when caregivers in other contexts respond differently than parents to children's emerging disruptive behavior, it would not be surprising to see variation in continuity in contexts, variation that would be magnified over time.

The issue of modest continuity in disruptive behavior in the first two years has critical implications for designing prevention programs in early childhood, which would suggest that our emphasis be on modifying *multiple* risk factors in the child's proximal environment rather than focusing primarily on child behavior. Appropriate targets would include maternal health practices during pregnancy (e.g., tobacco and alcohol use), and parents' well being and caregiving practices during early childhood (Olds, Hill et al., 1998; Shaw et al., 2006).

In addition, although direct and interactive effects of specific risk factors have been highlighted throughout the review, it is also important to stress the cumulative impact of some risk factors. Rather than representing interactions, a select number of risk factors appear to contribute independent variance to the prediction of antisocial behavior after accounting for variances associated with other correlates of antisocial behavior. These factors include maternal age (Nagin et al., 1997), child maltreatment and harsh parenting (Aguilar et al., 2000; Shaw et al., 2003), and prenatal tobacco (Brennan et al., 1999) and alcohol (Olson et al., 2001) use. Thus, the impetus for highlighting the impact of multiple risk factors rests not solely on interactive effects, but also on the cumulative impact of individual risk factors on delinquent behavior.

### ***Purposeful yet Promising Omissions from the Review***

Several factors that have shown short-term associations with antisocial behavior were not discussed or only mentioned briefly in the review, primarily because there is a dearth of data on their predictive validity between early childhood and adolescence. Thus, such factors as parental conflict (Jouriles et al., 1991; Yates, Dodds, Sroufe, & Egeland, 2003), marital quality (Shaw et al., 1994), maternal social support (Shaw et al., 1998; Shaw, Bell et al., 2000), sibling conflict (Garcia, Shaw, Winslow, & Yaggi, 2000), and proactive parenting (Gardner, 1987) have all been related to conduct problems in early childhood or the preschool period. In some cases, longitudinal associations have been established extending to the early school-age period or beyond, but this is clearly the exception rather than the rule (see Werner & Smith, 1992 for an exception regarding associations between early marital instability and adolescent delinquency). These variables could also be potential targets for intervention if prospective studies can confirm their long-term associations with delinquent behavior, which are likely to be amplified in the context of social adversity. Other factors in early childhood with less empirical but strong theoretical relevance include the involvement and parenting quality of fathers and nontraditional alternative caregivers (García Coll & Magnuson, 2000), culture and social-

ization practices (Deater-Deckard, Dodge, Bates, & Pettit, 1996; McLoyd, Cauce, Takeuchi, & Wilson, 2000), and the quality of day care settings (NICHD Early Child Care Research Network, 2004) and neighborhoods (Caspi, Taylor, Moffitt, & Plomin, 2000). Regarding the role of fathers, as single-parent families continue to become more normative for European American, African American, and other minority families, it becomes imperative to develop a better understanding of how paternal involvement in single-parent families is associated with more serious antisocial outcomes for offspring.

While there has been a greater emphasis on characteristics and caregiving practices of mothers versus fathers, there has been a dearth of attention devoted to studying predictors of early childhood predictors of girls' delinquent behavior relative to boys. Initially, this was partially understandable because of higher rates of serious and chronic delinquent behavior for boys. However, as a significant number of girls go on to show serious antisocial behavior, and many of these same girls often bear children at an early age while continuing to engage in risky health behavior while pregnant (e.g., smoking, alcohol use, Nagin et al., 1997) and become responsible for rearing these children, from a prevention perspective it is imperative that resources be dedicated to furthering our understanding of risk factors in early childhood associated with girls' delinquent behavior (Serbin et al., 1998).

Two other issues also merit attention based on their potential implications for prevention and intervention research. The first involves distinguishing between different types of disruptive behavior, specifically physical aggression and less violent types of externalizing symptoms (e.g., oppositionality). In particular, Tremblay and colleagues (1999) and Tremblay (2000) have argued that physical aggression *may* follow a dissimilar developmental course and have different consequences for the individual and society than other forms of disruptive behavior (e.g., noncompliance). As no studies have actually examined this issue prospectively from early childhood through adolescence, it remains a point of speculation. In the author's own intervention work, we have found physical aggression to be malleable in early childhood among a high-risk sample of boys using a brief, family-based intervention (Shaw et al., 2006). In fact, while significant reductions in child aggression were found between ages two and four, decreases in other types of disruptive behavior were only found to persist until age three. These preliminary results suggest that physical aggression might be easier to modify than other forms of disruptive behavior in early childhood, and is clearly more malleable to change in early childhood than during later periods of childhood.

The second issue that has been the source of much debate is the primacy of early childhood relative to other developmental periods. Over the past two decades, there has been much speculation about the need to ensure that children's first year is not marked by environmental adversity because of the fear of irreparable harm and a lifelong destiny of psychopathology. This perspective is in sharp contrast to data from prospective studies showing nonexistent associations between measures of disruptive behavior in the first year of life and later antisocial behavior. However, moving to the toddler period, there are now a couple of examples where assessments of the child's *social context* in early childhood have been found to show greater predictive validity of later antisocial outcomes than assessments of social adversity in

the preschool or early school-age period (Appleyard et al., 2005; Shaw, Bell, et al., 2000). In the Appleyard study of Minnesota, low-income children, a cumulative index of contextual adversity in early childhood continued to be associated with adolescent antisocial behavior after accounting for the effects of contextual risk in middle childhood. In the Shaw, Bell, et al. (2000) study of urban, low-income boys, effects of risk factors such as maternal depression, social support, parenting hassles and parenting all showed greater effect sizes on teacher-reported clinically-elevated aggression at age 8 when measured between and 1.5 to 2 years than when assessed between 3.5 to 5.5 years of age. Results from these studies suggest that the primacy issue merits further attention, particularly in reference to the social context of low-income children during the second and third year.

Why would one expect to find greater associations with later antisocial outcomes during the toddler versus the preschool or school-age periods? One possible explanation is the challenges associated with the “terrible twos,” which involve critical increases in children’s mobility without concomitant advances in cognitive or emotion regulation skills (Shaw & Bell, 1993). This disconnect in children’s physical and cognitive/emotional skills creates a period of transition for parent-child dyads, generating higher levels of child aggressive and noncompliant behavior and parental distress than the relatively calmer periods of infancy or the preschool and school-age periods (Fagot & Kavanagh, 1993). While one could argue that a period of upheaval might over-identify children living in families at risk for early-starting pathways, it might also provide a sampling of how children and their parents might respond to similar transitions during development (e.g., transition to formal schooling, transition to adolescence) or life events in the family (e.g., marital transition, family move, death of a parent). Thus, the terrible twos might be a prime time for identifying parent-child dyads at risk for early-starting antisocial pathways, particularly among children living in least favorable environments (Moffitt, 1990). However, as only a couple of studies have validated this finding, further research is needed to uncover whether the toddler period will prove to be an opportune time to prevent serious delinquent behavior in adolescence.

### ***Future Directions: Where do we go from here?***

In an ideal world where time and funding resources were not an issue, new genetically-informed, prospective studies of boys *and* girls from high-risk contexts would be mandated, focusing on factors that have yet to be adequately assessed in the few existing prospective studies (e.g., early child maltreatment x MAOA interactions, paternal involvement, culture, role of early aggression versus other disruptive behaviors). Although piecing together findings from adjacent developmental periods is tempting, and accelerated longitudinal designs provide a time- and cost-effective alternative to traditional longitudinal studies (Bell, 1954), to fully answer the question of whether early childhood factors contribute to delinquency, neither of these methods is adequate substitutes for prospective longitudinal research in understanding *how* factors in early childhood contribute to the development of

delinquency. Additionally, it is important to gauge how much new knowledge would be uncovered by initiating new studies in early childhood, studies that would take approximately 20 years to undertake. Certainly such investigations could provide novel and critical information about understudied factors (e.g., paternal involvement, culture, primacy of early childhood versus other age periods), and from a qualitative perspective, we clearly have only begun to scratch the surface in identifying gene-environment interactions associated with antisocial outcomes (Cadoret et al., 1995; Ge et al., 1996; Moffitt, 2005), interactions that might be identifiable in early childhood. Nonetheless, despite the relatively few studies from which conclusions for the current review were drawn, we suspect that they will, in all probability, continue to be valid two decades from now. Specifically, it is likely that significant, albeit modest, direct, cumulative, and interactive associations with delinquent behavior will continue to be evident among a small number of risk factors in early childhood.

Perhaps the best probability for qualitatively enriching our understanding of the precise mechanisms involved in transforming a defiant toddler into a juvenile delinquent may lie through conducting intervention research, preferably within genetically-informed designs. Such designs would make it possible to profile a child's genetic and environmental risk to test the efficacy of interventions specifically tailored to the family's and child's context (Dishion & Kavanagh, 2003). Rather than being limited to stimulating debates about the genetic or environmental basis of antisocial behavior, genetically-informed designs offer the opportunity to examine the "nature" of gene-environment interactions (Leve, Shaw, & Reiss, 2005). In the context of an experimental trial, this would allow interventionists to examine how malleable gene-environment interactions are in early childhood before child behavior becomes less impregnable to change (Reid, 1993).

In closing, prospective studies examining early childhood correlates of delinquent behavior indicate that consistent, albeit modest, associations have been uncovered between multiple facets of child and environmental attributes in early childhood and later serious antisocial behavior. Moreover, several of these factors appear to be malleable (e.g., prenatal smoking and drug use, parenting). Whether altering these factors in early childhood will prevent the development of serious offending during adolescence and adulthood remains to be seen, but initial findings from Olds' and colleagues' (1998) work indicate it is possible. Future prevention studies can corroborate whether such efforts will be evident working with more diverse and impoverished populations.

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**Appendix** Summary of studies, in order of birth year

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Cadore et al., 1995	University of Iowa Adoption study	1945–1974 (approx.)	<p>Adoptees from four adoption agencies in Iowa, who were between 18 and 47 years of age.</p> <p>For experimental group, biological parents were either drug users or had antisocial personality disorder.</p> <p>Sample was 95 males and 102 females.</p>	<p>Main effect for biological pathway (parent with Antisocial Behavior, ASB) was not found for adult ASB, but main effect for environment and adult ASB found. Prenatal exposure to alcohol did correlate with adult ASB. Interaction effect found where ASB increased when adoptee was in adverse home environment and had bio background of ASB.</p>
Nagin et al., 1997	Cambridge Study in Delinquent Development in London	1952–1953	<p>Longitudinal survey of 411 males from a working-class area in London. Most are white, Caucasian. Data collection was 8 to 32 years.</p>	<p>Greatest risk of criminal behavior occurred in children from large families born to women who began childbearing early. Looked at theories to explain – found support for poor parenting-role model (physical neglect, poor supervision) and diminished resources (inadequate income, parent separation by age 10)</p>

<p>Morash and Rucker, 1999</p>	<p>London Longitudinal Study, Philadelphia Cohort Study, National Longitudinal Survey of Youth, National Survey of Children</p>	<p>LLS: 1952–1954 PCS: 1959–1962 (approx.) NLSY: 1957–1965 NSC: 1965–1969</p>	<p>Four longitudinal data sets: London Longitudinal Study (LLS), Philadelphia Cohort Study, National Longitudinal Survey of Youth (NLSY), and National Survey of Children (NSC).  Cohort of 15,117 people born in Stockholm in 1953. People who were institutionalized, mentally retarded, or admitted to a psychiatric ward were discarded.</p>	<p>In some analyses, youth of early child-bearing mothers received more negative teacher ratings (inc. conduct disorders and delinquency) and greater penetration into the justice system. One sample (NLSY) only found effects (higher delinquency scores) for the white subsample.  Obstetric complications were not related to offending (as measured by criminal convictions at multiple ages: before age 15, 15–17, 18–20, and 21–30) in the absence of poor parenting. Pregnancy complications combined with inadequate parenting in early years increased violent and non-violent offending but crime rate in those experiencing both (50.5%) was not much higher than those just experiencing inadequate parenting (42.6%).</p>
<p>Hodgins et al., 2001</p>	<p>Population study in Stockholm</p>	<p>1953</p>	<p>614 children born on the island of Kauai and followed from prenatal period to adolescence.</p>	<p>For boys, strongest early childhood predictors of court records of offending were family instability (e.g., divorce/separation, marital conflict, and father absence) and below-average self-help skills. For girls, the strongest predictor was below-average intellectual skills.</p>
<p>Werner and Smith, 1992</p>	<p>Population study in Kauai, Hawaii</p>	<p>1955</p>	<p></p>	<p></p>

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## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Stattin and Klackenborg-Larsson, 1993	Clinic for the Study of Children's Development and Health in Stockholm	1955-1958	A longitudinal study of 212 subjects (122 males, 90 females) studied from birth to adulthood selected from a clinic in Stockholm. Males make up the study group for this study.	Boys' intelligence scores at age 3 were significantly related to future criminality as measured by official records ( $p < .01$ ). Criminality was significantly correlated with language ability, vocalization, vocal communicativeness, and language maturity. With the exception of maturity of language at age 3, relationship between language and criminality remained significant after controlling for SES. After controlling for SES, parents' involvement with child's play promoted early language ability.
Weikart, 1998	High/Scope Perry Preschool Project	1958-1959	Intervention, longitudinal study of 123 disadvantaged, African American children from families of limited education, low income, many single parents, and high risk of school failure.	At ages 3 and 4, children were randomly assigned to (1.) an active learning, preschool program and 1.5 hour home visit or (2.) a nonprogram group. By age 27, only one-fifth as many program group members had been arrested five or more times - 7% vs. 35%.
Raine et al., 1997	Danish perinatal study (Rigshospitalet in Copenhagen)	September 1959-December 1961	Prospective, longitudinal study of 4,269 males in Denmark	Significant interaction effect found between birth complications and early maternal rejection for violent crimes as measured by the Danish National Criminal Register. Aspects of maternal rejection that predisposed subjects to violence were being reared in a public institution and attempt to abort fetus. Main effect (but no interactions) was found for poor social circumstance.

<p>Raine et al., 1996; Raine et al., 1994</p>	<p>Danish perinatal study (Rigshospitalet in Copenhagen)</p>	<p>September 1959 to December 1961</p>	<p>1996 article: A sample of 397 Danish subjects. Data collected in hospital at birth, at 1 year, and follow-up at ages 17 to 19.1994 article: full sample of 4,269 males</p>	<p>1996 article: Rates of crime were particularly high in subgroup who had both biological and psychosocial risk factors. Rate was 2.0-2.5 times higher than poverty or bio. risk alone. Group that had the highest social risk actually showed lower crime rates than bio only or biosocial group. Results held true for crime records and maternal report of externalizing behavior.</p>
<p>Kandel and Mednick, 1991</p>	<p>Danish perinatal study (Rigshospitalet in Copenhagen)</p>	<p>September 1959 to December 1961</p>	<p>A subsample of children from this study, with two high risk groups due to parent psychiatric histories (n=144) and one control group (n=72). Earliest data was collected at birth.</p>	<p>1994 article: Found significant interaction effect for birth complications in combination with negative psychosocial environment (specifically early child reject) and violent crime in early adulthood. Neither risk factor alone was associated with increased violence. This interaction was not found for nonviolent crime.</p> <p>Pregnancy complications did not account for variance in offending status. Delivery complications did distinguish between nonoffenders and violent offenders, but not nonoffenders and property offenders. When controlling for SES and other factors, delivery</p>

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## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Brennan et al., 1999	Danish perinatal study (Rigshospitalet in Copenhagen)	September 1959 to December 1961	Prospective, longitudinal study of 4,269 males in Denmark	<p>complications accounted for 1.6% of the variance in violent offending. There was a significant interaction between parental psychiatric diagnosis and delivery complications with violent offending.</p> <p>Maternal smoking was a predictor of both violent and nonviolent crimes. Maternal smoking during pregnancy predicted arrests of offspring who were life-course-persistent but not adolescence-limited, even after controlling for parental criminality, maternal rejection, pregnancy and delivery complications, SES, and parental psychiatric history.</p>
Tibbetts and Piquero, 1999	Collaborative Perinatal Project (Philadelphia)	1959–1962	<p>Data was from the Longitudinal Study of biosocial Factors Related to Crime and Delinquency In Pennsylvania. Used 3 sources: Collaborative Perinatal Project (Philadelphia); records from Philadelphia public schools; records from Philadelphia police department. Population was primarily black, inner city youth in Philadelphia. Analysis included 207 offenders (144 boys and 63 girls).</p>	<p>Low birth weight was significantly, positively associated with an early age of onset (defined as police contact that resulted in arrest prior to age 14); higher SES was negatively associated with early onset. Interaction of low birth weight x SES and low birth weight x weak family structure had significant and positive effects on early onset.</p>

<p>Piquero et al., 2002; Gibson et al., 2000; Gibson and Tibbetts, 2000</p>	<p>Collaborative Perinatal Project (Philadelphia)</p>	<p>1959–1962</p>	<p>Prospective longitudinal cohort of 987 low SES, inner-city African American boys and girls who were raised in Philadelphia through age 17.</p>	<p>2002 article: Participants whose mothers smoked more than 20 cigarettes per day were more likely to be LCP (life-course persisters, both juvenile and adult convictions). Gender and SES also showed effects on LCP offending.</p>
				<p>Gibson, Piquero, &amp; Tibbetts: Maternal smoking distinguished between the LCP and AL (Adolescent Limited) group. High maternal smoking distinguished offenders from nonoffenders, after controlling for low birth weight, prenatal/perinatal complications, IQ, family adversity, SES.</p>
<p>Gibson and Tibbetts, 1998</p>	<p>Collaborative Perinatal Project (Philadelphia)</p>	<p>1959–1962</p>	<p>See above. Subsample of 832 used for this analysis (416 boys).</p>	<p>Gibson &amp; Tibbetts: Both maternal cigarette smoking and absence of a father were associated with early onset of offending. The interaction between the two variables exerted a significant effect on early onset.</p> <p>One-minute Apgar score had a significant and negative relationship with offending behavior (official police record). Maternal cigarette smoking was not significantly associated with offending behavior. Two variables were significantly (but weakly) correlated (<math>r = -.09</math>).</p>

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## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Serbin et al., 1998	Concordia Longitudinal Risk Project	1962–1972 (approx. children recruited in grades 1, 4, 7 between 1976–1978)	Longitudinal study – Concordia Longitudinal Risk Project. First followed a group of 1700 inner-city youth from low-income neighborhoods, then continued following through the birth of their children.	Interaction between the two variables was significantly associated with offending behavior. Maternal years of education significantly predicted children's CBCL scores, mothers' childhood behavior predicted child behavior when educational levels were controlled for statistically.
Rasanen et al., 1999	Northern Finland 1966 Birth Cohort	1966	Northern Finland 1966 Birth cohort – general population birth cohort followed from mid-pregnancy to age 28.	After controlling for other factors, mothers who smoked during pregnancy were twice as likely as those who did not smoke to have a son who committed at least one violent crime or more than one offense before the age of 28. Maternal smoking did not increase risk of nonviolent offenses. If multiple risk factors were present, the odds of committing violent crime increased to ninefold and recidivism up to 14-fold, but nonviolent criminal behavior increased to a much lesser degree (2.9 to 5.8)

<p>Streisguth et al., 2004</p>	<p>Fetal Alcohol Follow-up Study at the University of Washington</p>	<p>1966–1989 (approx.)</p>	<p>Clinical sample of 415 patients enrolled in study, were at least 6 years old.</p>	<p>Life history interviews conducted with parents or guardians found that 60% of FAS patients had gotten in trouble with the law, 50% had been confined in detention, jail, prison, or a psychiatric or alcohol/drug inpatient setting, 49% had participated in inappropriate sexual behavior on repeated occasions, and 35% had drug/alcohol problems.</p>
<p>Raine, Reynolds, Venables, Mednick, and Farrington, 1998</p>	<p>Mauritius study</p>	<p>1969</p>	<p>Study of all children born in Mauritius (island in Indian Ocean) in 1969. 1795 children (51.4% male). Recruited at age 3.</p>	<p>Children rated by parents as high on aggression at age 11 had higher scores on stimulation-seeking, fearlessness, and body size at age 3 (although larger body size at age 11 did not correlate with aggressiveness).</p>
<p>Stevenson and Goodman, 2001</p>	<p>London, Waltham Forest borough, study</p>	<p>1969–1970</p>	<p>A random sample of children living in London. 828 boys and girls, first data collected at age 3.</p>	<p>Behaviors at age 3 predicting adult criminality (as measured by court convictions) included soiling, daytime enuresis, activity, and management difficulties. Daytime enuresis and tantrums were related to violent convictions. Gender, social development, and total behavior score were related to convictions. When social factors were controlled for, only temper tantrums and management difficulties were still associated with adult convictions.</p>

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## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Caspi et al., 1995; Caspi et al., 1996; Henry et al., 1996	Dumedin Multidisciplinary Health and Development Study	April 1972–March 1973	Dumedin Longitudinal study. Earliest data was perinatal with a follow-up at age 3.	1995 Caspi et al. & 1996 Henry et al. articles: Lack of control temperament at both ages 3 and 5 was significantly correlated with teacher and parent reports of antisocial behavior at ages 9 and 11 and with externalizing problems at ages 13 and 15. Children rated as having lack of control temperament were more likely to be convicted of violent offenses, and temperament interacted with number of caretakers to predict nonviolent convictions.
Moffitt, 1990	Dumedin Multidisciplinary Health and Development Study	April 1972 to March 1973	Dumedin longitudinal study.	1996 Caspi et al. article: Children classified as undercontrolled (difficult) at age 3 were 2.9 times as likely to be diagnosed with antisocial personality disorder, 2.2 times as likely to be recidivistic offenders, and 4.5 times as likely to be convicted for a violent offense. Inhibited boys only were more likely to be convicted for a violent offense (but only one). Group comprised of children who were delinquent at age 13 & 15 and had Attention Deficit Disorder by age 11 were most likely to have significant motor skills deficits at age 3, greatest levels of family adversity, and lower IQ at age 5. ADD nondelinquent group did not have these increased risk factors.

<p>Moffitt and Caspi, 2001</p>	<p>Dunedin Multidisciplinary Health and Development Study</p>	<p>April 1972 to March 1973</p>	<p>Dunedin longitudinal study</p>	<p>Similar risk factors were present for males and females in both LCP (Life Course Persistent) and AL (Adolescent Limited) ASB groups. Factors (measured before age 3) that distinguished LCP from AP group at significance: mother's age, mother-child observation (deviant interaction at age 3), neurological abnormality, rated difficult to manage at age 2, and observed as under-controlled at age 3.</p>
<p>Caspi et al., 2002</p>	<p>Dunedin Multidisciplinary Health and Development Study</p>	<p>April 1972 to March 1973</p>	<p>Dunedin longitudinal study.</p>	<p>Direct effects of maltreatment or harsh physical discipline were found for rates of adolescent conduct disorder, antisocial personality disorder, and convictions for violent offenses by age 26. These associations were significantly amplified when expression of MAOA activity was low.</p>
<p>Thornberry et al., 2001</p>	<p>Rochester Youth Development Study</p>	<p>1973-1977 (approx.)</p>	<p>Ongoing panel study of adolescent development. Subjects recruited in 7th and 8th grades from census tracts with highest resident arrest rates; data collected every 6 months.</p>	<p>Children experiencing maltreatment only in early childhood were no more involved in delinquency, drug use, or externalizing behaviors. However, experiencing maltreatment in both childhood and adolescence increased risk of all outcomes. When looked at by type of abuse, childhood neglect (occurring between birth and 11)</p>

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## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Olson et al., 1997	Seattle Longitudinal Prospective Study on Alcohol & Pregnancy	1974–1976	Population based cohort of 464 children followed longitudinally from birth to 14 years.	was related to delinquency and externalizing problems in early adolescence while physical abuse had a greater effect on late adolescence. Prenatal alcohol exposure was related to adolescent self-reported antisocial behaviors and delinquent behaviors from the CBCL. Highest risks occurred when drinking was in early pregnancy and if it was binge drinking.
Aguilar et al., 2000	Minnesota Parent-child Project	1975–1977	20-year longitudinal study of high-risk population drawn from the Minneapolis Public Health Clinic	Sample classified into four groups – Never antisocial, antisocial in childhood but not adolescence, antisocial in adolescence but not childhood (AO), and antisocial in both (EOP). No significant differences by group found for any of the early temperament or neuropsychological variables. EOPs significantly more likely to have a single mother at birth, be physically abused between birth and 2 years, and show more avoidant attachments at 12 and 18 months.

<p>Appleyard et al., 2005</p>	<p>Minnesota Parent-child Project</p>	<p>1975–1977</p>	<p>Ongoing, prospective, longitudinal study investigating developmental outcomes of at-risk, urban children; subjects selected on basis of poverty status.</p>	<p>Early cumulative risk variables (birth to 64 months), including child maltreatment, inter-parental violence, and SES stress, significantly predicted externalizing problems at age 16 and continued to affect outcomes even after holding middle childhood risk constant. The number of risk variables in early childhood predicted linear increase in behavior problems. Middle childhood risks (1st to 6th grade) did not have same predictive power.</p>
<p>Yates, Dodds, Stroufe, and Egeland, 2003</p>	<p>Minnesota Parent-child Project</p>	<p>1975–1977</p>	<p>Minnesota Parent–Child Project, a 25-year longitudinal study of developmental adaptation in a sample of young mothers living in poverty and their firstborn children. Home visits conducted six times in the first year, with an additional lab visit at 12 months.</p>	<p>There were modest relations between preschool exposure to partner violence (age 18 to 64 months) and teacher and youth reported externalizing behavior at age 16. Life stress (as measured between 12 months and 3rd grade) also significantly predicted 16-year behavior problems.</p>

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## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Wakschlag et al., 1997	Developmental Trends Study	1975-1980	Developmental Trends Study - longitudinal study of 177 clinic-referred boys in Pennsylvania and Georgia. 71% white, 29% African American.	Mothers who smoked more than half a pack daily were more than 4 times as likely to have a child with Conduct Disorder (CD) than mothers who did not smoke. When other variables were controlled for, maternal smoking and maternal AS personality disorder were still correlated with CD outcomes. In a second logistic analysis, maternal smoking more than half a pack daily, maternal age, harsh discipline, and little supervision predicted CD.
Breslau et al., 1988	Cleveland Hospital study	1976	Cohort of children admitted to the neonatal intensive care unit in 1976 (Cleveland) with birthweight <1500 grams (considered Very Low Birthweight VLBW)	Very Low Birth Weight (VLBW) boys scored significantly higher than their controls on total Behavior Problems scales, including items that measured delinquent behaviors. 29% scored in the clinical range on the externalizing scale vs. 9% of controls. Teachers did not rate VLBW boys significantly higher on TRF although proportions in clinical range were different (40% vs. 22%). VLBW girls did not score significantly higher on mother or teacher report.

Rende, 1993	Colorado Adoption Project	1976 (oldest kids)	164 subjects participating as control subjects in the longitudinal Colorado Adoption Project (CAP). 91 males, 73 females.	The theory hypothesized that the sociability dimension of temperament would be related to delinquency. While other dimensions were associated with their predicted outcomes (emotionality with anxiety/depression, activity with attention problems), there was little evidence for associations between Emotionality-Activity-Sociability traits and delinquent behavior for girls or boys.
Olson et al., 2000	Bloomington Longitudinal Study	1976–1977	Bloomington Longitudinal Study – 168 subjects, primarily middle class, 57% male. First data collection at 6 months.	Early caregiver behaviors at 13 and 24 months correlated with externalizing behaviors in school age as rated by mom and teacher as well as ext. behaviors at age 17 as rated by mom and self. Toddler difficultness and resistance to control predicted school-age externalizing problems. This relationship held for resistance to control and mom's rating of difficult to manage and Ext. beh at 17.

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## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Bates et al., 1998	Bloomington Longitudinal Study (BLS) & Child Development Project (CDP)	BLS: 1976–1977/CDP: 1982–1983	Used data from two longitudinal data sets – Bloomington Longitudinal Study (BLS) (started assessing mother-child interaction at age 1 to 2 years) and the Child Development Project (CDP) (data collection not started until age 5; temperament assessed retrospectively)	There were direct main effects of temperamental resistance to control and middle childhood externalizing problems for both data sets. Interaction effects: when mothers were relatively low in restrictive control, there was a stronger relation between early temperament and later externalizing problems (per mother and teacher report) than when mothers were high in control. Conclusion was that parenting factors moderated the relationship between temperament and ext. behaviors.
Bates, Bayles, Bennet, Ridge, & Brown, 1991	Bloomington Longitudinal Study (BLS)	BLS: 1976–1977	Bloomington Longitudinal Study (BLS) – see above	For boys and girls, both difficult temperament and resistance to control in infancy predicted externalizing problems at age 8. For girls (but not boys), hyperactivity at age 3 predicted externalizing behavior. Restrictive, punitive control in mother-child interaction at 24 months predicted boys' externalizing.

<p>Fergusson and Woodward, 1999</p>	<p>Christchurch (New Zealand) Health and Development Study</p>	<p>1977</p>	<p>Christchurch health and Development Study – longitudinal study of 1265 children born in Christchurch, New Zealand during the middle of 1977. Studied at birth, 4 months, annual intervals to 16 years, and 18 years.</p>	<p>Higher maternal age was associated with consistent decreases in rates of juvenile offending and convictions. Intervening process variables largely explained the association between maternal age and later outcomes, suggesting that it is not the age of parents but the ability of parents to provide a supportive and nurturant family environment that determines child outcomes.</p>
<p>Fergusson et al., 1993</p>	<p>Christchurch (New Zealand) Health and Development Study</p>	<p>1977</p>	<p>15-year longitudinal study of a birth cohort of 1265 New Zealand children (Christchurch Health and Development study)</p>	<p>There were positive, significantly significant associations between maternal smoking (pre and post natal) and child behavior problems. After controlling for other variables, smoking after pregnancy was no longer related to behavior problems, but there were still small associations between smoking during pregnancy (20 or more cigarettes per day) and behavior scores.</p>

(continued)

## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Arsenault et al., 2002	Montreal study – boys from lowest SES schools in Montreal	1978	Longitudinal study in Montreal of 1,037 white, French-speaking boys	There was an interaction between specific obstetrical complications (DRS, those considered deadly risk situations) and psychosocial risk in predicting adolescent violent and nonviolent delinquency. DRS increased likelihood of delinquency, depending on level of family adversity. DRS also helped explain the continuity of violent behavior from ages 6 to 17.
Nagin and Tremblay, 2001	Montreal study – boys from lowest SES schools in Montreal	1978	Longitudinal study in Montreal of 1,037 white, French-speaking boys	Risk factors of low maternal education and teenage motherhood increased odds of being in the high aggression trajectory and of staying in the chronic trajectory.
Ross, Lipper, and Auld, 1990	New York Hospital study	1978–1979	87 children admitted to the neonatal intensive care unit at New York Hospital. 80% white, 18% black	When compared to a normative sample, premature boys had significantly higher behavior problems scores at ages 7 and 8. Premature girls did not differ from normative sample. Within the premature children sample, those with higher SES had lower scores for behavior problems than those with low SES.

Olds, Henderson, et al., 1998	Olds Elmira study	1978–1980	400 pregnant women were enrolled and assigned to a treatment group with home visitations from nurses or a control group with no home visitation	At age 15, those families with nurse visitation reported fewer incidents of running away, fewer arrests, fewer convictions/ violations of probation, fewer lifetime sex partners, fewer cigarettes smoked per day, and fewer days when alcohol was consumed. Parents reported fewer behavioral problems related to drug and alcohol use. The most positive results were concentrated among children born to women who were unmarried and from low-SES households.
Campbell et al., 1994; Campbell et al., 1996	Hard to manage preschoolers	1980–1983	112 subjects recruited between ages of 3 and 4 from local preschools, child care centers, and parent-referred. 69 of the 112 had elevated ratings of overactivity, inattention, and impulsivity.	1994: Boys with problem behaviors at age 4 continued to exhibit higher levels of activity, impulsivity, and externalizing behaviors at age 6. Behaviors were generally consistent across all contexts: home, laboratory, and school. 1996: Hard to manage preschool children at age 3 showed moderate continuity of behavior problems at ages 6, 9, and 13. Fifty and 48% of those with problems at age 3 showed clinically-significant problems at ages 6 and 9, respectively.

(continued)

## Appendix (continued)

Citation(s)	Study	Birth Years of subjects	Description of sample, inc. age of first prospective data	Results
Shaw et al., 2000	Pitt Mother & Child Project	1989–1992	310 ethnically-diverse, low-income boys followed from infancy to adolescence	Among boys identified at or above the 90th percentile on broad factors of externalizing symptoms at age 2, 63% remained above the 90th percentile at age 5, and 97% remained above the median. At age 6, 62% remained at or above the 90th percentile and 100% (all 18) remained above the median. There was a direct link between maternal depressive symptoms when children were 1.5 and 2 years of age and clinically-elevated reports of school-based conduct problems (CP) using the Teacher Report Form when children were age 8.
Beck & Shaw, 2005	Pitt Mother & Child Project	1989–1992	310 ethnically-diverse, low-income boys followed from infancy to adolescence	No direct effects were found between perinatal complications and youth report of antisocial behavior at age 10; however, a significant interaction emerged between family adversity and perinatal complications, such that low-income boys with high perinatal complications and family adversity were at elevated risk for antisocial behavior.

<p>Shaw et al., 2003</p>	<p>Pitt Mother &amp; Child Project</p>	<p>1989–1992</p>	<p>310 ethnically-diverse, low-income boys followed from infancy to adolescence</p>	<p>Observed harsh parenting at age 2 discriminated maternal-reported trajectories of conduct problems from ages 2 to 8 and showed significant associations with youth and teacher report of antisocial behavior at ages 11 and 12.</p>
<p>Olds et al., 2004</p>	<p>Olds Memphis study</p>	<p>1990–1992</p>	<p>1139 women were assigned to one of four treatment conditions (3 receiving some form of home visitation and 1 control group). Low income unmarried women were actively recruited – 92% of sample black, 98% unmarried, 64% were &lt; or = 18 years, 85% were at or below federal poverty level</p>	<p>At age 6, nurse-visited children were reported by their mothers to have fewer problems in the borderline or clinical range of the CBCL Total Problems scale (1.8% vs. 5.4%, <math>p = .04</math>). However, a reduction in behavioral problems on the CBCL was not corroborated by teachers' reports of child behavior.</p>
<p>Shaw et al., 2006</p>	<p>Early Steps Pilot study</p>	<p>1998–2000</p>	<p>120 toddler-age boys with socioeconomic, family, and child risk factors</p>	<p>Reductions in destructive and aggressive behavior and improvements in maternal involvement and positive, proactive parenting were found at ages 3 and/or 4 using Dishion's Family Check Up. Families characterized by a profile associated with early-starting conduct problems showed the greatest between-group improvement in conduct problems at age 4.</p>

# Longitudinal Perspectives on Adolescent Street Gangs

Marvin D. Krohn and Terence P. Thornberry

Street gangs have been of primary concern to the public, policy makers, and criminologists for well over a century. There is a very good reason for such concern: gang members contribute disproportionately to the overall level of crime, especially violent and serious offenses (Battin-Pearson, Thornberry, Hawkins, & Krohn, 1998; Curry, 2000; Curry, Ball, & Decker, 1996; Hill, Hawkins, Catalano, Maguin, & Kosterman, 1995; Howell, 2000; Huff, 1996; Klein & Maxson, 2006; Miller, 1975; Thornberry, Krohn, Lizotte, & Chard-Wierschem, 1993; Thornberry, Krohn, Lizotte, Smith, & Tobin, 2003; Thrasher, 1927). The research focus on gangs has led to important theoretical developments in the study of crime (Cloward & Ohlin, 1960; Cohen, 1955; Miller, 1958; Shaw & McKay, 1942; Thrasher, 1927) as well as being the impetus for many community-based prevention programs (e.g., Esbensen & Osgood, 1997; Howell, 1998; Kennedy, Piehl, & Braga, 1996; Klein, 1969; Kobrin, 1959; Mattick & Caplan, 1962; Miller, 1962; Thrasher, 1936).

In spite of these efforts gangs not only remain a significant problem, they have proliferated at an alarming rate in recent years. Klein and Maxson (2006) reviewed studies of gang proliferation and report that between 1980 and 1990 there was a dramatic increase in the number of large cities (100,000 population or more) that reported gang problems, increasing from 15% prior to 1980 to 70% by 1990. Gang problems spread to mid-sized and smaller cities from the mid 1980s through 1995 as well. Although there has been a slight reversal of the trend in less populated cities, the gang problem in larger cities remains stable.

With the rapid spread of gangs throughout the country, there has been an ever-increasing call for research to determine why individuals join gangs, the effects of gang membership on criminal behavior, why youth leave gangs once having joined, and the effects of gang membership on longer term life-course outcomes such as education and employment (Howell, 2000). Many of the more recent research efforts directed at answering these questions evidence a methodological shift from previous work on gangs. Earlier gang research either relied on observations of gang members during periods of membership or provided cross-sectional comparisons of gang members with non-gang members. More recent studies have introduced longitudinal panel designs to address questions concerning the reasons for and results of gang membership. The purpose of this paper is to examine the yield of longitudinal research on gangs in addressing these questions. Before doing so we briefly discuss the contributions of earlier research on gangs.

## Early Gang Research

Almost all of the early work on gangs targeted youth who were currently in a gang, interviewing them and observing their interactions within the gang structure (Bursik & Grasmick, 1995). These studies have provided a wealth of very rich descriptive information on the life of gang members. From these studies we have learned much about the structure of gangs, gang members' perceptions of why they joined gangs, their feelings toward other gang members, and their gang-related activities (Hughes, 2005).

Although the information from these studies has made significant contributions to our understanding of gangs and gang members, there are a number of methodological limitations with them. By focusing on youth after they had already joined a gang, the only information they offer on the reasons for joining is necessarily retrospective. Retrospective data have long been recognized as likely to be distorted (Yarrow, Campbell, & Burton, 1970) and can be influenced by the experience of gang membership itself. Limiting the focus of inquiry to current gang members also makes it difficult to determine the effect of membership on behavior. For example, it is not possible to determine if gang membership produces an increase in criminal behavior over pre-gang involvement in crime. Studies that follow gang members through the years in which they are gang members provide some information on this issue but even they cannot distinguish between a gang effect and an age effect. For example, an increase in criminal behavior over the years that youth are in a gang may be due to the fact that they are entering into the years when the prevalence of crime is at its peak rather than due to the effect of the gang.

Also, most of the early research did not follow gang members once they left the gang. Therefore, few studies could address the question of whether gang membership has an impact on future criminality. Nor could they examine the potential deleterious effect that gang membership has on life-course transitions and ultimately life chances. Some more recent qualitative studies of gang members have followed youth past the time when they were active gang members and have documented some of the adverse consequences of gang membership (Hagedorn, 1998; Moore, 1991).

Another common problem with early studies of gangs is the failure to include a comparison group of youth who do not join gangs. Many studies, especially observational ones, focus only on gang members and do not include subjects of similar age or background in order to determine if what is occurring in the lives of gang members is unique to them because of their membership or whether similar outcomes would occur to most youth who share similar background characteristics. Without such comparisons, it is impossible to determine if the gang is responsible for changes in behavior or other outcomes later in life (Hughes, 2005; Katz & Jackson-Jacobs, 2004).

Cross-sectional quantitative studies of gangs offer the advantage of including a comparison group with which to compare current gang members (e.g., Esbensen & Winfree, 1998; Klein, Gordon, & Maxson, 1986; Maxson, Whitlock, & Klein, 1997; Short & Strodbeck, 1965). This study design allows researchers to directly contrast the characteristics and behavior of gang and non-gang youth at similar ages and

having similar background characteristics in order to determine how they differ. From these differences, inferences can be made regarding the causes of gang membership and the effect of gang membership on behavior.

A major problem with cross-sectional studies, however, is that the temporal order of the variables is indefinite and therefore causal inferences are, at best, risky. For example, if we find that gang members have significantly poorer relations with their parents than non-gang members, we do not know whether those poor relations were a cause of or risk factor for joining the gang or if they are a consequence of being in a gang.

Qualitative studies have provided rich descriptive information on a number of issues regarding the characteristics of gang members and the processes that take place in the gang. Cross-sectional quantitative studies have added to our knowledge by identifying relationships between gang membership and a number of potential risk factors for joining a gang. However, there are a number of questions that cannot be adequately studied with either methodology. In the next section, we describe those issues and suggest how longitudinal panel analyses provide the best alternative for addressing them.

## **Advantages of Longitudinal Designs**

A longitudinal study, as the term is used in this review, selects a sample of respondents and follows them forward in time as they age. The ideal design for investigating the impact of gang membership on life-course development would have several key features. First, it would be based on a community sample representative of a clearly definable population. By focusing on a community sample, both gang members and non-members are represented to allow for inter-individual comparisons. Second, assessment of the sample should begin at ages that are prior to the typical onset of gang membership. Since gang membership is primarily a mid- to late-adolescent phenomenon, studies that start in late childhood or early adolescence are well-suited to this task. Third, the full sample would be followed for longer rather than shorter periods of time, hopefully across multiple developmental stages – e.g., childhood, adolescence, and emerging adulthood. Fourth, repeated measures would be taken across the follow-up period, at multiple points in time. Repeated measures allow for the assessment of intra-individual change as each person develops. Finally, the study would have a broad measurement space to allow for the assessment of antecedents, correlates, and consequences of gang membership.

With regard to the study of gang membership, longitudinal designs as just described, especially when compared to cross-sectional designs, enhance our ability to investigate a number of important substantive issues. In particular, we identify six issues that can be more fully and accurately studied with longitudinal data. They are:

### *1. The Identification of Risk Factors*

Identifying risk factors for gang membership is important for both theoretical and policy reasons. Theoretically, the accurate identification of risk factors

enhances our understanding of the origins of gang membership and helps structure more formal causal analyses. Practically, knowledge of major risk factors helps identify youth who may subsequently become gang members and aids in the development of intervention programs.

2. *Separating Facilitation and Selection Effects*

There is a well-established relationship between gang membership and involvement in delinquent behavior, especially serious delinquency and violence. This association has been observed in cross-sectional and longitudinal studies and studies based on surveys, direct observations, and official records (Thornberry et al., 2003). What is less clear, however, is the causal direction of this relationship. Does gang membership facilitate involvement in crime or are individuals already involved in criminal behavior attracted to the gang?

3. *Tracing the Duration of Gang Membership*

There is a commonsense notion that gang membership is a relatively stable phenomenon. That is, once an adolescent joins a street gang, he or she is likely to remain a gang member for quite a while. There is, however, relatively little research that follows representative samples of gang members over time to assess either this notion or the counter-notion, that gang membership is relatively fleeting.

4. *Separating Causes and Consequences*

Much of the work that has compared family and peer relationships among gang members to those of non-members has treated those relationships as risk factors or potential causes. With cross-sectional data, however, there is no way to determine the causal order among these variables. The observed relationship could have been due either to parental and peer variables leading to gang membership or to gang membership increasing the association with deviant others and the deterioration of the bond between the youth and the parent.

5. *Establishing Short- and Long-term Consequences of Gang Membership*

One important area of research that has been advanced by both qualitative and quantitative longitudinal studies of gangs is the impact that participation in a gang has on the life course and life chances of gang members. What gang members do while they are in a gang and the status of being a gang member appear to impact their future direction, but whether the impact of gang membership on this outcome is real or spurious is less well understood.

6. *Developmental Differences in Gang Membership*

Developmental issues regarding gang membership and its impact on behavior and future outcomes have been largely unexplored. For example, we do not know if the risk factors for joining a gang differ for youth who join at different ages or if selection and facilitation effects are different at different ages.

A better understanding of these six issues has important theoretical and practical implications and, as we show in the following pages, these issues are more appropriately examined using longitudinal rather than cross-sectional study designs. The subsequent sections address these six issues, discussing first why longitudinal data are better suited for assessing them and then reviewing the results of longitudinal studies that have examined them. Some of these questions have been addressed

rather fully, while for other questions, research is still in its infancy. We obviously focus on the former in the ensuing sections.

## **Risk Factors for Gang Membership**

In the epidemiological tradition, we define risk factors as “individual or environmental hazards that increase an individual’s vulnerability to negative developmental outcomes” (Small & Luster, 1994, p. 182). In the present case, risk factors for gang membership are attributes that significantly increase the chances or probability that a person possessing those attributes will subsequently become a gang member. Risk factors, by definition therefore, occur prior to the onset of the outcome.

Risk factors can be distinguished from other classes of concepts that also yield statistical associations with gang membership. These include causal variables which are also logically antecedent to gang membership but in addition to temporal order they exert a true causal impact. Risk factors are antecedent and may or may not be causal. Consequences are variables that occur after the onset of gang membership and may have been caused by gang membership. Correlates are variables that are contemporaneously related to gang membership but without temporal order being established. They merely co-occur with gang membership.

Causes, risk factors, correlates, and consequences will all yield a statistically significant association or correlation with gang membership. Thus, identifying risk factors, as opposed to any of these other types of variables, is less a matter of statistical analysis and more a matter of design.

Longitudinal designs with repeated measures are ideally suited to identifying risk factors. They follow the same people over time and first assess various individual and environmental hazards and then assess the onset of gang membership. With such a design it is relatively easy to see which earlier hazards are significantly related to later gang membership and which, therefore, can be considered risk factors.

In contrast, cross-sectional studies are severely challenged in their ability to identify risk factors. Since all data are collected simultaneously it is quite difficult to separate risk factors from correlates or consequences. For example, there is strong evidence in cross-sectional data that school failure is statistically associated with gang membership. Failure in school could lead youth to join gangs, that is, it would be a risk factor. But it is also plausible that gang membership leads to alienation from and failure in school, that is, school failure is a consequence of gang membership. Or, school failure and gang membership may be mere correlates, both generated by some common prior cause. Cross-sectional designs cannot logically distinguish among these possibilities.<sup>1</sup> Indeed, the very strong temporal dimension embedded in the definition of a risk factor suggests the superiority of longitudinal designs.

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<sup>1</sup> Cross-sectional data can be used to assess whether a fairly limited subset of variables are risk factors for gang membership. Namely, they can assess the status of variables that cannot change over time (e.g., being adopted in childhood) or whose onset prior to gang membership can clearly be established (e.g., the age of school entry). Although there are these exceptions, cross-sectional designs generally do not provide strong assessments of risk factors.

Even though risk factors are not necessarily causal, to properly identify risk factors, as opposed to correlates, is important for several reasons. First, absent accurate information on true causes, focusing intervention strategies on powerful risk factors is probably the most productive approach we have. Second, identifying risk factors is important to help target scarce prevention resources toward youth who are most likely to become gang members.

Turning to the empirical literature, a number of cross-sectional studies have identified correlates of gang membership. That is, they have identified attributes on which gang members and non-members differ, but, because of the cross-sectional design, they cannot determine if those attributes are antecedents, correlates, or consequences of gang membership. Reviews of this literature can be found in Thornberry et al. (2003, pp. 57–61) and Klein and Maxson (2006, Chapter 4).

In general, correlational studies show that gang membership is associated with deficits in a number of developmental domains. Although results are not entirely consistent across studies, and each study examines an idiosyncratic set of variables, these domains include neighborhood characteristics, family sociodemographic characteristics, parent-child relations, school factors, peer relations, individual traits, and prior deviance. The central question before us now is: which of these correlates are true risk factors, that is, which occur prior to gang membership?

The two most comprehensive assessments of risk factors are presented by Hill, Howell, Hawkins, and Battin-Pearson (1999) using data from the Seattle Social Development Project and by Thornberry et al. (2003) using data from the Rochester Youth Development Study. We start with these studies.

Hill et al. (1999) examined risk factors measured at ages 10–12 as predictors of gang membership between ages 13 and 18. Risk factors were drawn from five domains: neighborhood, family, school, peers, and individual characteristics. They found that “[21] of the 25 constructs measured at ages 10–12 predicted joining a gang at ages 13 to 18. Predictors of gang membership were found in all of the measured domains” (Hill et al., 1999, p. 308). The most potent risk factors are neighborhood youth in trouble and availability of marijuana; family structure, especially living with one parent and other adults or with no parents; low achievement in elementary school or being identified as learning disabled; association with deviant peers; prior involvement in marijuana use or violence; and externalizing problem behaviors. Hill et al. (1999) also found that having multiple risk factors greatly increases the chances of joining a gang.

Thornberry et al. (2003) examined risk factors measured before age 14 on the probability of joining a gang between ages 14 and 17. Because of the relatively small number of female gang members available for this analysis, we concentrate on the results for males. The key findings are presented here in Table 1.

For the male participants in the Rochester study, gang members have significantly greater deficits as compared to non-members on 25 of the 40 measured risk factors. Risk is observed in all seven developmental domains. Although many antecedent variables are related to the odds of joining a gang, there are few variables that, independently, have a very large impact on gang membership. For example, there are only three variables in Table 1 that have an odds ratio of 3 or more: experiencing

**Table 1** Risk factors for gang membership, Rochester Youth Development Study, males only

Risk Factors	Odds Ratios
<i>Area Characteristics</i>	
Percentage African American	1.59*
Percentage in Poverty	1.88**
Community Arrest Rate	1.79**
Neighborhood Disorganization	.95
Neighborhood Violence	.86
Neighborhood Drug Use	1.51*
Neighborhood Integration	.71
<i>Family Sociodemographic Characteristics</i>	
African American	2.28**
Hispanic	1.19
Parent Education	.53**
Family Disadvantage	1.39
Poverty Level Income	1.91**
Lives with Both Biological Parents	.47**
Family Transitions	1.42
<i>Parent-Child Relations</i>	
Attachment to Parent	1.02
Attachment to Child	.69*
Parental Involvement	.94
Parental Supervision	.53**
Positive Parenting	1.10
Report of Child Maltreatment	1.78*
Family Hostility	.77
<i>School Factors</i>	
Commitment to School	.64*
Attachment to Teacher	.48**
College Aspirations	1.09
Subject's College Expectations	.70
Parent's College Expectations for Subject	.64*
Math Score	.41**
<i>Peer Relationships</i>	
Delinquent Peers	1.97**
Early Dating	2.82**
Precocious Sexual Activity	1.58*
Unsupervised Time with Friends	1.41
<i>Individual Characteristics</i>	
Negative Life Events	3.25**
Depression	1.71**
Self-Esteem	.82
Externalizing Behaviors	1.98**
Delinquent Beliefs	2.15**
<i>Early Delinquency</i>	
General Delinquency	3.26**
Violent Delinquency	4.19**
Drug Use	2.49**
Age of Onset of General Delinquency	.78

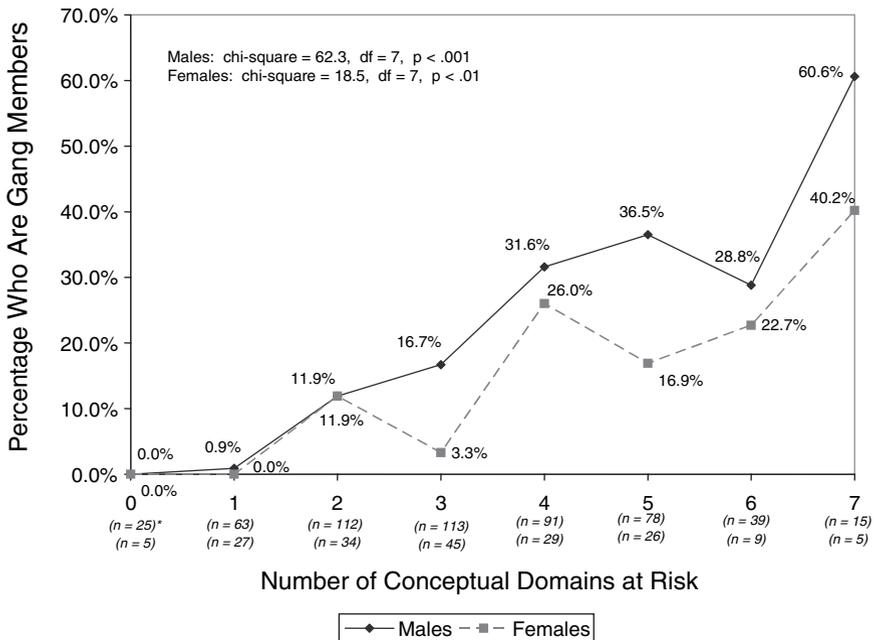
\*p < .05 (one-tailed test), \*\*p < .01 (one-tailed test).

Source: Thornberry et al. (2003), Table 4.2. Reprinted with permission.

negative life events (OR=3.25), prior delinquency (OR=3.26), and prior violence (OR=4.19).

While gang membership is not strongly related to many individual risk factors, it is strongly related to the accumulation of risk. Figure 1 presents the core results, in this case including female gang members because sample size is less of an issue for these cumulative risk analyses. For both males and females as the number of developmental domains in which risk is experienced increases, so too does the probability of gang membership. Youth, at least in Rochester, appear able to ward off the negative consequences of risk in a few domains, but, after that, the chances of gang membership increase rapidly. Hill et al. (1999) report similar results concerning the impact of accumulated risk on gang membership for the Seattle sample.

Several other longitudinal studies have identified risk factors for gang membership. Huizinga and colleagues (Esbensen & Huizinga, 1993; Huizinga, Weiher, Espiritu, & Esbensen, 2003; Huizinga, Weiher, Menard, Espiritu, & Esbensen, 1988) examined this issue in the Denver Youth Survey. They found poor parental supervision, deviant peers, non-delinquent problem behaviors, and certain indicators of school attachment and performance to be related to later gang membership. In contrast, attachment to parents, self-esteem, and attitudes toward the future were not identified as risk factors. Huizinga et al.



**Figure 1** Cumulative risk for gang membership  
 Note: Top n is for males; bottom n is for females  
 Source: Thornberry et al. (2003), Fig. 4.2. Reprinted with permission.

(1988) also report that the accumulation of risk is strongly related to gang membership.

Two studies (Craig, Vitaro, Gagnon, & Tremblay, 2002; Gatti, Tremblay, Vitaro, & McDuff, 2005) use data from the Montreal Longitudinal and Experimental Study to examine risk factors. Among the variables significantly related to gang membership are: low parental supervision, deviant peers, commitment to school, and non-delinquent problem behaviors.

Lahey, Gordon, Loeber, Stouthamer-Loeber, and Farrington (1999) examined predictors of first gang entry for males in the Pittsburgh Youth Study. Their study was restricted to African American males because of the small number of White male gang members available for analysis. In bivariate relationships, gang membership is predicted by prior conduct disorder behaviors, self-reported delinquency, and associations with delinquent peers. Gang membership is not related to household income, household structure, neighborhood crime level, or parental supervision, however.

Walker-Barnes and Mason (2001) identified ninth-graders who joined a gang during the course of that academic year. Parental warmth, parental control or monitoring, and peer deviance were all related to gang membership in the expected direction. Walker-Barnes and Mason also examined differences by race and ethnicity. In general, the parenting variables had a somewhat stronger impact for African American youth than for White or Hispanic youth. In particular, "higher levels of behavioral control and lower levels of lax and psychological control were related to decreases in gang involvement for Blacks..." (Walker-Barnes & Mason, 2001, p. 1826).

Bjerregaard and Lizotte (1995) used the Rochester data to look specifically at the impact of earlier delinquency and gun ownership on the likelihood of being a gang member. They found that prior involvement in serious delinquency and street delinquency, but not more general forms of delinquency, increases the likelihood of later gang membership. They also found that owning guns for protection, but not for sporting purposes, increases the chances of joining a gang.

One of the most thorough reviews of the risk factor literature was conducted by Howell and Egley (2005). They identified risk factors in five major domains or ecological levels. The significant risk factors to emerge from their systematic review are presented in Table 2. These results highlight the multitude of risk factors in the backgrounds of gang members and the extensiveness of risk across domains. The core finding of accumulated risk is clearly evident in all the longitudinal studies included in their review.

## *Summary*

Several general conclusions about the investigation of risk factors for gang membership appear warranted. First, there are only a relatively small number of longitudinal studies that have investigated this issue. There are even fewer studies that have used the same set of risk factors so there are few replicated results. Given the importance, both for theory and prevention, of understanding the antecedents of

**Table 2** Risk factors for gang membership in prospective longitudinal studies\**Community/neighborhood risk factors*

Availability/ perceived access to drugs (Hill et al., 1999)  
 Neighborhood youth in trouble (Hill et al., 1999)  
 Community arrest rate (Thornberry et al., 2003)  
 Feeling unsafe in the neighborhood (Kosterman et al., 1996)  
 Low neighborhood attachment (Hill et al., 1999)  
 Neighborhood residents in poverty or family poverty (Hill et al., 1999; Thornberry et al., 2003)  
 Availability of firearms (Bjerregaard & Lizotte, 1995; Lizotte et al., 2000; Lizotte et al., 1994; Thornberry et al., 2003)  
 Neighborhood disorganization (Thornberry, 1998; Thornberry et al., 2003)  
 Neighborhood drug use (Thornberry et al., 2003)

*Family risk factors*

Family structure (Hill et al., 1999\*\*; Thornberry et al., 2003)  
 Family poverty (Hill et al., 1999; Thornberry et al., 2003)  
 Family transitions (Thornberry et al., 2003\*\*\*)  
 Family financial stress (Eitle et al., 2004)  
 Sibling antisocial behavior (Hill et al., 1999)  
 Low attachment to parents/family (Eitle et al., 2004; Thornberry et al., 2003)  
 Child maltreatment (Thornberry et al., 2003)  
 Low parent education level (Thornberry et al., 2003)  
 Parent proviolent attitudes (Hill et al., 1999)  
 Family management: low parent supervision/control/monitoring (Hill et al., 1999; Lahey et al., 1999\*\*\*\*; Thornberry et al., 2003)  
 Teenage fatherhood (Loeber, Farrington, Stouthamer-Loeber et al., 2003)

*School risk factors*

Low achievement in elementary school (Craig et al., 2002; Hill et al., 1999)  
 Negative labeling by teachers (as either bad or disturbed) (Esbensen et al., 1993)  
 Low academic aspirations (Bjerregaard & Smith, 1993; Hill et al., 1999; Thornberry, et al., 2003)  
 Low school attachment (Hill et al., 1999)  
 Low attachment to teachers (Thornberry et al., 2003)  
 Low parent college expectations for subject (Bjerregaard & Smith, 1993; Thornberry, et al., 2003)  
 Low degree of commitment to school (Thornberry et al., 2003)  
 Low math achievement test score (Thornberry et al., 2003)  
 Identified as learning disabled (Hill et al., 1999)

*Peer group risk factors*

Association with peers who engage in delinquency or other problem behaviors (Bjerregaard & Smith, 1993; Bjerregaard & Lizotte, 1995; Eitle et al., 2004; Hill et al., 1999; Lahey et al., 1999\*\*\*\*)  
 Association with aggressive peers (Craig et al., 2002; Lahey et al., 1999\*\*\*\*)

*Individual risk factors*

Violence involvement (Hill et al., 1999; Thornberry et al., 2003)  
 General delinquency involvement (Curry, 2000; Hill et al., 1999; Thornberry et al., 2003; Esbensen & Huizinga, 1993)  
 Aggression/fighting (Craig et al., 2002; Lahey et al., 1999\*\*\*\*)  
 Conduct disorders (Lahey et al., 1999)  
 Externalizing behaviors (disruptive, antisocial, & other conduct disorders) (Craig et al., 2002; Hill et al., 1999)  
 Early dating (Thornberry et al., 2003)  
 Precocious sexual activity (Bjerregaard & Smith, 1993; Thornberry et al., 2003)  
 Antisocial/delinquent beliefs (Hill et al., 1999; Thornberry et al., 2003)  
 Hyperactive (Craig et al., 2002; Hill et al., 1999)  
 Alcohol/drug use (Thornberry et al., 2003; Bjerregaard & Smith, 1993; Thornberry et al., 1993; Hill et al., 1999)

Early marijuana use and early drinking (Hill et al., 1999)

Depression (Thornberry et al., 2003)

Life stressors (Eitle et al., 2004; Thornberry et al., 2003)

Poor refusal skills (Hill et al., 1999)

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\*Race/ethnicity and gender are excluded.

\*\*The Social Development Research Group study compared three family structures: no parents in home, one parent only, and one parent plus other adults. The later structure was the strongest predictor.

\*\*\*This risk factor predicted stability of gang membership.

\*\*\*\*Significant effects were observed only in early adolescence.

Source: Howell & Egley (2005), Table 1. Reprinted with permission.

gang membership this is indeed unfortunate. One high priority for future study, and a relatively easy one given the bivariate nature of most risk factor analyses, would be more coordinated replication of these results across studies.

Second, that said, across the longitudinal analyses that have been conducted there are several risk factors that stand out as being of primary importance. They are involvement in prior delinquency and related problem behaviors, low parental supervision, and involvement in deviant peer networks. Less consistently, some aspects of poor school attachment and/or performance, and experiencing negative or stressful life events are also important.

Third, there are several variables that are often proposed as risk factors for gang membership that enjoy little, if any, empirical support from longitudinal studies. They include family poverty and family structure, self-esteem, affective bonds with parents, and neighborhood crime. These findings remind us of the importance of basing theory and policy on empirically based observations and not supposition. They also remind us that not all aspects of a particular developmental domain need be equally related to an outcome. For example, in the area of the family, strong parental supervision and monitoring is consistently found to reduce gang membership, but affective ties are not related to gang membership. Zeroing in on the more central aspects, rather than adopting a blanket approach, is crucial for effective intervention.

Finally, as with many other problem behaviors, gang membership does not seem to be a product of a few central risk factors; none exerts a massive impact on the likelihood of being a gang member. But, the accumulation of risk is strongly related to the chances of becoming a gang member. Gang members have multiple deficits in multiple developmental domains, each one of which contributes in a small, but statistically significant, way to the chances of being a gang member.

## **Selection vs. Facilitation**

As noted earlier, there is no dispute about the association of gang membership and high rates of criminal involvement: gang members have much higher rates of crime than non-members. There is a dispute, however, about the interpretation of this relationship. Thornberry et al. (1993) identified three general models that could account for the strong statistical association between gang membership and high rates of crime.

The first is a “kind of person” model they labeled the *selection model*. A selection model argues that adolescents with a strong propensity for delinquency and violence seek out or are recruited into street gangs. They are likely to engage in delinquency regardless of their status as a gang member. Indeed, the observed statistical relationship between gang membership and delinquency is spurious, caused by some prior common cause. This model is most consistent with control theories of delinquency, especially those presented by Hirschi (1969) and Gottfredson and Hirschi (1990).

The second model identified by Thornberry et al. (1993) is the *facilitation model*. This is a “kind of group” model. Gang members do not have a higher propensity for delinquency and violence than non-members and, absent joining a gang, would not have higher rates of delinquency. When they join a gang, however, the normative structure of the gang along with group processes and dynamics facilitates increased involvement in delinquency. In this case, the delinquency of gang members should increase during periods of gang membership and be lower both before and after that period. This model is most consistent with learning theories (Akers, 1998) and life-course theories (Thornberry & Krohn, 2003).

These two views are not logically contradictory and both processes can occur. Thornberry et al. (1993) labeled this mixed model the *enhancement model*. Adolescents who are already involved in delinquency are most apt to join a gang (selection) but, after joining, their delinquency is likely to increase significantly (facilitation).

Although the enhancement model is quite plausible, it is not as interesting as the other two since the contrast between the first two approaches yields opposing hypotheses. Under the selection model, gang members would have higher rates of delinquency than non-members before, during, and after periods of membership. Also, among gang members, intra-individual change would not be systematically related to gang membership; if the impact of gang membership on delinquency is truly spurious (Gottfredson & Hirschi, 1990) an individual’s rate of offending should not change as a function of gang membership. In contrast, under the facilitation model, gang members would have higher rates of delinquency than non-members only during periods of membership; before and after the groups would not differ. Also, the facilitation model predicts that intra-individual change is systematically related to gang membership; if gang membership is truly causal, an individual’s rate of offending should increase when they become a gang member and decrease after they leave the gang.

Fully testing these competing approaches is impossible absent a true experimental design. Nevertheless, longitudinal studies that follow individuals across time offer the strongest feasible approach to examining them. The essence of the contrasting hypotheses just presented is temporal; in one case (selection) there should be no intra-individual change in delinquency as a function of gang membership, in the other (facilitation) there should be. Longitudinal studies with repeated measures are designed to capture intra-individual change and therefore assess this type of hypothesis.

Longitudinal designs have another advantage in this regard. By following the same individuals across time, each respondent acts as his or her own control and helps bring stable attributes under control (Farrington, Ohlin, & Wilson, 1986). Cross-sectional designs are limited to cross-person analyses and therefore can only

statistically control for other variables. For example, if gang members have higher rates of delinquency than non-members that may be because males are more apt both to be gang members and to be delinquent. If an individual's delinquency increases during periods of membership and then declines, that cannot be because of being male; the individual was male before, during, and after being a gang member. In general, longitudinal designs help control for stable characteristics, although time-varying characteristics remain a threat to validity.

Finally, longitudinal studies that are based on community samples with both gang members and non-members followed over time strengthen our ability to test these hypotheses. In particular, they can compare the delinquency of gang members to non-members at the same point in time, relative to periods of active membership for the gang members. The selection model hypothesizes that the gang members will always have significantly higher rates of delinquency than non-members; the facilitation model hypothesizes that the gang members will have higher rates than non-members only during the period of their active membership.

In sum, longitudinal designs that follow individuals across time offer many advantages over cross-sectional designs for testing causal hypotheses. While not as definitive as those from a true experiment, longitudinal results are far superior to those from cross-sectional data.

### *Initial Studies*

Early studies of the gang facilitation effect focused on relatively simple analytic strategies comparing rates of criminal involvement for gang members and non-members over time. For example, the first assessment of these models using longitudinal panel data (Thornberry et al., 1993) relied on the Rochester Youth Development Study to compare gang members to non-members at three consecutive years, from when the respondents were 15 years of age until they were 17 years of age. Two types of comparisons were made: across time and across group. The first examined whether the delinquency of gang members changed as a function of their active gang membership. The second analytic strategy compared the gang members to non-members at each annual time point. Thornberry et al. (1993) conducted the analysis for five outcomes: general delinquency, violence, property crimes, drug use, and drug sales. They were also able to examine transient gang members, those who were members for no more than a single year, and more stable gang members, those who were members during at least two of the years. The analysis was limited to male respondents.

The results are quite consistent with the facilitation model. Focusing on violent delinquency where the patterns are clearest, Thornberry et al. (1993) found that rates of violence increased when the boys joined the gang and decreased when they left it. Also, gang members had significantly higher rates than non-members typically only during periods of active membership. The same basic pattern was observed for general delinquency, drug use, and drug sales. The only exception was for property crimes where none of the hypothesized models applied: "...gang membership seems

to have little effect on the frequency of property crimes” (Thornberry et al., 1993, p. 80).

Bjerregaard and Lizotte (1995) also used data from the Rochester project to examine the impact of gang membership on patterns of gun ownership among members of the Rochester Youth Development Study. The analysis focused on later adolescence, roughly ages 16 to 18, and is limited to the male respondents because of the very low rate of gun ownership and use by adolescent females. The study distinguished between the ownership of guns for sporting purposes and for protection or illegal purposes.

Prior to joining a gang, gang members do not have significantly higher rates of protection gun ownership than non-members, nor are they more likely to engage in gun delinquency. Once in a gang, however, the rates of these two behaviors increase, only to fall after they leave the gang. For example, 30.9% of current gang members own a gun for protection as compared to 23.1% of future members and 13.2% of past members. Comparable percentages for gun delinquency are 13.6% versus 2.6% and 0%. These results, as well as multivariate logistic regressions, suggest that while there is a slight elevation in illegal gun involvement prior to membership, there is a substantial increase in involvement during the period of membership. Interestingly, there are no differences across the four groups – non-members, future, current, and past members – in terms of gun ownership for sporting purposes.

Empirical assessments of these competing conceptual models have also been conducted in several other longitudinal studies. Esbensen and Huizinga (1993) used data from the Denver Youth Survey and examined street offending, “serious crimes that occur on the street and are often of concern to citizens and policymakers, alike” (1993, p. 571). They were able to examine the impact of gang membership on behavior over a four-year period. Esbensen and Huizinga report results that are most consistent with the enhancement or mixed model. Involvement in street offending is considerably higher during periods of gang membership, than before or after. Nevertheless, gang members have a generally higher prevalence of street offending than the non-gang members, with some evidence of escalation in the year immediately prior to joining. Similar patterns were observed for serious offenses and illicit drug use, as well as when individual offending rates, instead of prevalence rates, are used as the indicator of delinquent involvement. Overall, in the Denver data there is some evidence of selection processes since prior delinquency is a risk factor for gang membership, but there is a stronger facilitation effect since the highest delinquency rates for the Denver gang members were observed during periods of active membership.

Hill et al. (1996) present data from the Seattle Social Development Project that are also generally consistent with the facilitation model. For gang members, violent delinquency is only slightly elevated in the year prior to active membership but once the adolescent joins the gang, violence increases substantially. After leaving the gang, rates of violence return to baseline. Interestingly, a somewhat different pattern is observed for drug sales in the Seattle sample. Involvement in drug sales increases substantially when adolescents become gang members but it remains high even after the individual leaves the gang. The latter pattern is not consistent with a selection model but it does suggest that the facilitative process of the gang may have

contemporaneous effects for some behaviors, e.g., violence, and both contemporaneous and lagged effects for others, e.g., drug sales.

Zhang, Welte, and Wieczorek (1999) examined these issues in a set of regression models using data from the first two waves of the Buffalo Longitudinal Survey of Young Men. Support for the selection model was somewhat mixed: prior delinquency was related to gang membership but prior drug use was not. For both behaviors there is some support for the facilitation model, however. Current gang members report marginally higher levels of delinquency than non-members ( $p < .055$ ) and significantly higher levels of drug use. Zhang et al. (1999) also found an interesting interaction between current gang membership and delinquency: "current gang membership had a relatively stronger effect on delinquency for youths who were classified in the low level of prior delinquency" (Zhang et al., 1999, p. 9). A similar interaction effect was observed for drug use. Thus, in the Buffalo data, the gang has a stronger impact on delinquency and drug use for those without a history of engaging in these behaviors as compared to those who had already initiated the behaviors.

Several analyses of this issue have been conducted using data from the Montreal Longitudinal and Experimental Study, an entirely French-speaking sample selected from low SES areas of Montreal (Tremblay, Vitaro, Nagin, Pagani, & Seguin, 2003). Early results were reported by Thornberry (1998) and Gatti, Vitaro, Tremblay, and McDuff (2002), but the fullest assessment is presented by Gatti et al. (2005). They examined the facilitation and selection effects at ages 14, 15, and 16 for four offense types – person offenses, property offenses, drug use, and drug sales – and for transient versus stable gang members.

For crimes against the person and for property crimes, the facilitation model appears to describe the behavior of the transient gang members while the enhancement model appears to describe the behavior of the stable gang members. The facilitative impact of the gang on property crimes in this Canadian sample differs from that found in Thornberry et al. (1993). Patterns of drug use and drug sales are somewhat less distinct in the Montreal sample. There is a tendency for the level of drug involvement to increase with the onset of gang membership. For example, in all of the six available comparisons (Gatti et al., 2005, Tables 6 and 7) drug sales and drug use increase during the first year of gang membership as compared to the prior year. Drug involvement remains high after periods of active membership, however, a finding similar to that reported by Hill et al. (1996).

Gatti et al. (2005) also examined the impact of current gang membership on a measure of total delinquency after they controlled for seven major risk factors for gang membership and delinquency, as well as current levels of delinquent friends. At all three ages current gang membership exerted a strong and significant impact on delinquency. Gatti et al. conclude that:

The higher delinquency rates among gang members are largely linked to the experience of the gang itself, rather than to the social deficiencies that characterize its members, and that the apparent effect exerted by the gang is specific and goes beyond simply having delinquent friends.

Gordon et al. (2004) used data from the Pittsburgh Youth Study to examine these issues. They found stronger support for a selection effect than most of the other

longitudinal studies. But even for this sample there are noticeable facilitation effects: "...we replicate prior findings of a substantial increase in drug selling, drug use, violent delinquency and property delinquency when boys are active gang members" (p. 78). They also report that these forms of delinquency decline after the boys leave the gang. Overall, the pattern of the Pittsburgh results is most consistent with an enhancement model.

The first European study using longitudinal data to examine these issues was conducted by Bendixen, Endresen, and Olweus (2006) using a sample from Bergen, Norway (see Olweus, 1993, for a general description). Bendixen et al. (2006) analyzed general antisocial behavior and violence at three time periods covering ages 13 to 16. They also examined the extent to which gang effects differed by gender.

For general antisocial behavior, which covered relatively minor acts of delinquency that focused on theft and vandalism, the Norwegian data are most consistent with the enhancement model. There are moderate-sized selection effects since gang members have higher rates of antisocial behavior than non-members prior to joining the gang. There are also moderate-sized facilitation effects as antisocial behavior for gang members is highest during periods of active membership. In all comparisons, antisocial behavior increases in the year of joining a gang and decreases the year after leaving the gang (Bendixen et al., 2006, Table 2). For violent delinquency, Bendixen et al. (2006) report a small selection effect and a large facilitation effect.

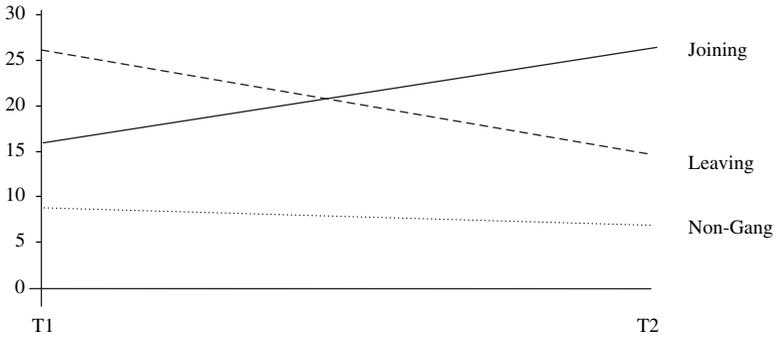
The size of the gang facilitation effect can be seen in Figure 2 reprinted from Bendixen et al. (2006). There is relatively little change in either general delinquency or violence from one time period to the next for the non-members. For the gang members however, there are substantial changes evident as a function of membership status. When an adolescent joined a gang, delinquency and violence increased substantially; when an adolescent left a gang, these behaviors declined substantially.

In the cross-time models just summarized, Bendixen et al. (2006) also included a time-by-sex interaction term. In general, the facilitative effect of gang membership on behavior was stronger for boys than for girls.

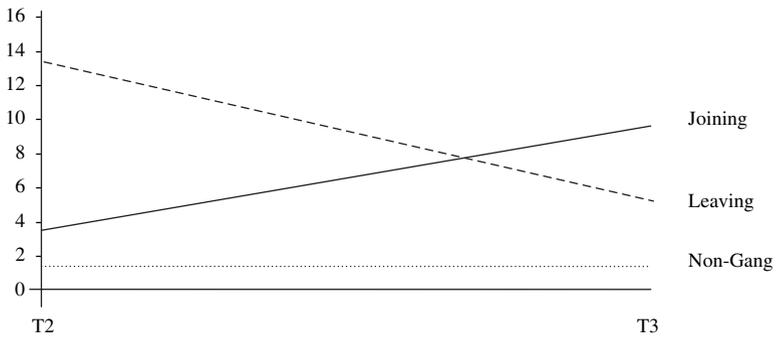
### ***More Recent Investigations***

Following these initial investigations, researchers have begun to use more sophisticated analytic strategies to see if the facilitation effect generally noted in those studies holds up under closer scrutiny. Thornberry et al. (2003) provide a more comprehensive investigation of these issues than in their original analysis (Thornberry et al., 1993). First, they examined the interplay of gang membership and delinquency across four, instead of three, years. Second, they held six major risk factors for delinquency, including prior delinquency, constant in multivariate models. Finally, they estimated a random effects model, which also included the six risk factors, to control for unmeasured population heterogeneity. All of these analyses suggest a strong facilitation effect and a rather modest selection effect:

a. General Delinquency



b. Violent Delinquency



**Figure 2** The impact of joining and leaving a gang on a) general delinquency and b) violent delinquency; Bergen, Norway Study  
 Source: Bendixen et al. (2006)

Net of the impact of family poverty, parental supervision, commitment to school, association with delinquent peers, negative life events, prior deviance, and unobserved population heterogeneity, [current gang membership is] statistically significant in predicting general delinquency, violence, drug use, and drug sales in all equations.

Hall, Thornberry, and Lizotte (2006) used the Rochester data to examine whether the impact of gang membership varies by level of neighborhood social disorganization. That is, does gang membership have a greater effect on delinquent behaviors for youth residing in areas with below-average levels of disorganization or for youth residing in more highly disorganized areas?

Given the lack of prior research on this question, Hall et al. (2006) point out that any of three models is possible. First, the gang facilitation effect could be greater in disorganized areas because of the lower levels of social control and

protective factors in those areas. Second, the effect could be greater in more organized areas because youth from these areas are exposed to fewer risk factors in general so the impact of the gang may be more pronounced. Third, the potency of the gang effect may influence delinquency regardless of the level of area disorganization.

For general delinquency, violent delinquency, drug use, and drug sales, the results clearly supported the third, or null model. Of the 16 gang membership-by-neighborhood interaction terms (4 offense types x 4 years), 12 are not statistically significant and the other four are inconsistent, one supporting the first model and three supporting the second. Hall et al. (2006) conclude: "Overall, gang membership facilitates problem behaviors in both neighborhood contexts and does so at a similar magnitude" (p. 59).

Several studies have used the trajectory models developed by Nagin (1999; Nagin & Land, 1993; see Piquero, this volume) to examine the gang facilitation effect. These models, by tracing different trajectories of behavior over time, allow analysis to focus on relatively homogeneous offending groups thereby providing "a statistical basis to control for persistent unobserved individual differences that predispose individuals to follow a specific trajectory" (Lacourse, Nagin, Tremblay, Vitaro, & Claes, 2003, pp. 185 and 186). Lacourse et al. (2003) used the same Montreal data analyzed by Gatti et al. (2005). In this study, however, they started by identifying developmental trajectories of gang membership over a seven-year period covering ages 11 to 17.<sup>2</sup> Three trajectory groups emerged: adolescents who were never a gang member during this time (74% of the sample); a childhood onset group (13%) where the probability of gang membership was high from 11 to 14 and then dropped off; and, an adolescent onset group (13%) where the probability of gang membership was low at 11 and 12 and then escalated considerably to a peak at ages 15 and 16.

To test the gang facilitation effect they first hypothesized that patterns of violent delinquency should track the gang trajectories. That is exactly what they observed (Lacourse et al., 2003, pp. 190 and 191). For each gang trajectory group, violence is elevated at precisely the ages when gang membership is most prevalent.

Lacourse et al. (2003) then examined whether movement into and out of a gang was associated with increases and decreases in violence as the facilitation model predicts. Importantly, they conducted this analysis within trajectory groups to further control for unobserved heterogeneity. At all ages for all trajectory groups, the results are consistent with the facilitation model: "Transitions into a [gang] are associated with increased violent behaviors, and transition out of a [gang] is associated with decreased violent behaviors" (Lacourse et al., 2003, p. 193). For the

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<sup>2</sup> Gang membership is based on the responses to the following question: "... were you part of a group or a gang that did reprehensible acts." This is the same measure Gatti et al. (2005) and Thornberry (1998) used in their analyses of gang effects in the Montreal data. Although Lacourse et al. (2003) refer to this as "delinquent group membership," to be consistent with the other studies that used this measure we refer to trajectories of gang membership.

trajectory group with childhood onset there is some evidence of a selection effect and Lacourse et al. (2003) conclude that the enhancement model is most descriptive of their behavior but that the facilitation model is most descriptive of those who join gangs in adolescence.

In an interesting analytic reversal, Hill, Chung, Guo, and Hawkins (2002) first estimated trajectories of violent behavior from ages 13 to 18 and then examined whether gang membership facilitates violence within trajectory groups. They identified four groups characterized by different patterns of violence: non-offenders, desistors, late escalators, and chronics. They then entered gang membership as a time-varying covariate to see if, within trajectory groups, violence changed as a function of active gang membership. For the three offending trajectories, but not the non-offending group, violence increased when the youth joined the gang and decreased when they left the gang. This held at all time points and for both transient and stable members.

The facilitative effect of gang membership was stronger for the desistor and late escalator groups than it was for the chronic offender group. Indeed, in the year(s) they were active members, the members of the first two groups have rates of violent delinquency that were as high as those of the chronic offender group. In the other years, their non-active years, they were considerably lower.

Haviland and Nagin (2005) present the most sophisticated analysis to date of the selection and facilitation models. In an effort to increase the confidence we can place in causal inferences drawn from longitudinal survey data, they combined two recent advances in statistical modeling. The first is the trajectory method developed by Nagin (1999) that creates groups or classes of adolescents who are relatively homogeneous with respect to violent offending. The second are propensity or balance models (Rosenbaum, 2002; Rosenbaum & Rubin, 1983) that create as much balance as possible on covariates, including lagged measures of the outcome, between those who experience a “treatment” and those who do not. The uniqueness of the Haviland and Nagin (2005) approach is that the balancing scores are applied within the relatively homogeneous trajectory groups to minimize differences on the lagged outcome (and other covariates) between the treated and the untreated. This approach provides a much better approximation of experimental conditions than traditional methods for analyzing longitudinal data.

Haviland and Nagin used the Montreal data (Tremblay et al., 2003) in their investigation. They estimated trajectories of violent delinquency from ages 11 to 13 and then observed the impact of joining a gang at age 14, the “treatment”, on subsequent violence. Within trajectory groups there is little if any evidence of selection effects. That is, the gang members do not differ from non-members on prior violence. There is, however, evidence of a facilitation effect in all three trajectory groups; adolescents who join a gang experience significant increases in subsequent violence. Interestingly, “for individuals in the chronic trajectory, who were already heavily engaged in violent delinquency, the point estimate for the increase is more than twice as large as that for low and declining trajectories” (Haviland & Nagin, 2005, p. 14). This is the opposite interaction to that reported by Zhang et al. (1999) and by Hill et al. (2002).

## **Summary**

Since Thornberry et al. (1993) introduced the gang facilitation model, several longitudinal studies have examined it. They have used different data sets covering different sites, time periods, and countries, different measures of gang membership, different analytic strategies, and samples with different characteristics. Despite these differences, the uniformity of results is impressive.

First, there is no evidence that is supportive of a pure selection model as suggested by control theories (e.g., Gottfredson & Hirschi, 1990; Hirschi, 1969). That is, no study finds that gang members have uniformly higher rates of delinquency and related problem behaviors as compared to non-members.

Second, all studies find that delinquency varies as a function of gang membership status, a result consistent with a gang facilitation effect. That is, delinquency almost universally increases when adolescents join a gang and the greatest differences between gang members and non-members are observed during the gang members' period of membership. Also, delinquency typically declines after the member leaves the gang, with the exception of drug sales which appears to remain elevated.

Third, some studies (e.g., Esbensen & Huizinga, 1993; Zhang et al., 1999) also find evidence of a selection effect in addition to the facilitation effect. This pattern of results is most consistent with the enhancement or mixed model.

Overall, perhaps the safest conclusion to draw is that there is a minor selection effect, a major facilitation effect, and no evidence consistent with a pure selection model. The weight of the evidence suggests that street gangs do facilitate or elicit increased involvement in delinquency, violence, and drugs. There is no evidence to the contrary and abundant evidence in support of this view. These results greatly expand our understanding of the interplay between street gangs and delinquency, an expansion in knowledge that would not have been possible without longitudinal data on gang members and non-members.

## **The Duration of Gang Membership**

There is a general notion that once youth join a street gang they remain members for relatively long periods of time. In part, this view has been generated by popular culture and the mass media. For example, the lyrics in *West Side Story* claim that:

Once you're a Jet, you're a Jet all the way,  
From your first cigarette to your last dyin' day.

In part this view is also generated by observational research that often focuses on traditional gangs in large cities with a long history of street gangs, like Chicago and Los Angeles (Thornberry & Porter, 2001). While the implied stability may be reflective of gang membership at the extreme end of the gang distribution, it may or may not represent the full range of street gangs.

Longitudinal studies, especially those based on community samples, are ideally suited for an examination of this issue. First, if the sample is representative of its locale, the gangs that the respondents belong to will be representative of the gangs that are found in that locale. Second, since the respondents are followed over time with repeated assessments of their gang involvement, direct estimates of the stability or the fluidity of gang membership can be obtained. Related issues, such as whether gang members join, leave, and re-join a gang or whether they move from one gang to another, can also be measured.

Thornberry et al. (2003) found that gang membership is quite fluid and transitory. Half of the male (50.4%) and two-thirds of the female gang members (66.0%) report being members of the gang for one year or less. In contrast, only 21.6% of the boys and 5.0% of the girls report being a gang member for 3 or 4 years. Moreover, very few of the gang members report joining a gang, leaving it, and then re-joining it or another gang. The predominant pattern is to join a gang, stay for a while (typically less than a year), and then leave the gang world. At least this is the pattern in Rochester.

Esbensen and Huizinga (1993) report very similar patterns in Denver. Over a four year period they found that of the 90 youths who reported being a member of a gang, 67% were members for only one year while only 3 percent belonged for all four years. Interestingly, when asked what role they expected to have in the gang in the near future, 60% reported that they would not want to be a member of the gang in the future.

The findings from the Pittsburgh Youth Study confirm the general patterns observed in both Rochester and Denver. Gordon et al. (2004) report 48% of the male gang members were in a gang for only one wave of data collection and 25% for only two waves of data collection.

The lack of stability in gang membership among youth in the Rochester, Denver, and Pittsburgh studies may be because all three research sites are characterized as emerging gang cities. That is, these cities did not have a long-standing tradition of gang behavior; rather the gang problem became recognized in the 1980s around the time that the three studies began. However, studies that have been done in more traditional gang cities also report that gang membership is a relatively temporary phenomenon among a majority of youth who participate in a gang (Hagedorn, 1998; Klein, 1971; Short & Strodtbeck, 1965; Vigil, 1988; Yablonsky, 1962).

## **Short- and Long-Term Consequences of Gang Membership**

Over the past thirty years there has been an increasing recognition that behavior is constantly evolving as actors age (Baltes, 1987; Baltes & Brim, 1982). Behavior initiated during adolescence can have important consequences for successful entry into adult roles and responsibilities. The way actors navigate the transition to adulthood can, in turn, have an important and longlasting impact on their life chances. The life-course perspective recognizes that as people move along trajectories, they make (or fail to make) transitions such as completing their education, getting married, or

finding a job (see Siennick & Osgood, this volume). The success in making those transitions, for example, in completing one's education, is likely to have a significant impact on life chances. Disruption in or failure to complete major transitions will adversely affect subsequent development.

There is a growing body of research that finds that involvement in delinquent or drug-using behavior increases disruption in transitions along a number of important trajectories. Adolescents involved in delinquent behavior are more likely to drop out of school (Fagan & Pabon, 1990; Kaplan & Liu, 1994; Krohn, Thornberry, Collins-Hall, & Lizotte, 1995; Mensch & Kandel, 1988), to become pregnant or impregnate someone else or become a teenage parent (Newcomb & Bentler, 1988; Smith, 1997; Thornberry, Smith, & Howard, 1997), and to be unemployed in their early adult years (Caspi, Wright, Moffitt, & Silva, 1998; Kandel, Chen, & Gill, 1995; Kandel, Davies, Karus, & Yamaguchi, 1986; Newcomb & Bentler, 1988). Since gang members are typically more involved in delinquent activities than non-gang members, it is reasonable to expect that being a member of a gang during adolescence will be associated with disrupted transitions from adolescence to adulthood and, ultimately, will adversely impact life chances. But there is relatively little direct evidence about the extent to which gang membership itself, over and above delinquent behavior, contributes to disorder in the life course.

To adequately examine the impact of gang membership on subsequent life-course transitions, it is necessary to follow former gang members over time and to compare them with non-gang members over that same period of time. Longitudinal panel studies are well suited to this task. They can determine if gang members, as opposed to similarly situated non-gang members, are more likely to have disorderly transitions such as dropping out of school and teenage parenthood. They can also examine the impact of such disorderly transitions on longer-term outcomes and determine if disorderly transitions mediate the relationship between gang membership and problematic outcomes in young adulthood. And, they can examine these issues controlling for levels of offending.

An added benefit of longitudinal studies that collect information at regular and relatively short intervals (e.g., one year or less) is their ability to identify short-term and more stable gang membership. Stability in gang membership may be expected to reflect greater commitment to the gang and the behavior and values represented therein. Hence, stable gang membership is expected to have an even greater impact on the life course than is short-term gang membership.

This issue is arguably one of the most important ones for gang researchers to address because of the long-term implications of the answers found. Yet, there has been surprisingly little research on the impact of gang membership on life-course transitions. As early as 1971 Malcolm Klein observed, "Though the need is great, there has been no careful study of gang members as they move on into adult status" (1971, p. 136), a sentiment echoed by Hagedorn (1998) and Decker and Lauritsen (1996). Even as late as 2001, Levitt and Venkatesh stated that, "Little is known, however, about the long-run impact of adolescent street gang involvement on adult outcomes" (2001a, p. 1).

Some information about the impact of gang membership on life-course transitions has been generated by ethnographic studies that incorporated interviews in the

design. For example, Hagedorn (1998) reinterviewed a sample of gang members originally studied as adolescents when they were in their early 20s. Of all male gang members, only a third had a high school diploma and about the same number were working. The rate of high school graduation for female gang members was about the same as male gang members. Almost all of the young women were mothers (88%) by their early 20s, with about 58% on welfare.

Moore (1991) found similar results in her ethnographic study. Only 40% of former gang members were employed as young adults. Female gang members had high rates of early parenthood and were more likely to be responsible for raising those children than were male gang members. Neither Hagedorn nor Moore had comparison groups; therefore, they could not control for factors other than gang membership that might have caused these outcomes.

Levitt and Venkatesh (2001a,b) present data that suggests that gang membership might not have a direct effect on some problematic outcomes once other background characteristics are controlled. In 1990 they began an ethnographic study on a sample of 118 youths aged 16–26 that resided in one public housing complex in a disadvantaged neighborhood of Chicago. Of the 118 youth in the sample, 38 were active gang participants. Ten years later, they interviewed 94 of the original sample. In their initial study (2001a), they found that gang members obtained less education, had higher rates of arrest and incarceration, and earned a greater percentage of income from illegal sources than did non-gang members. However, once background factors such as GPA and drug use among their guardians were controlled, the effect of gang membership was not a significant predictor of high school graduation, being currently employed, or being currently incarcerated. Gang membership remained a significant predictor of ever having been incarcerated and the percentage of income from illegal sources. Levitt and Venkatesh (2001b) also report that once controlling for years of education and years incarcerated as well as a number of additional background variables, the effect of gang membership on illegal income is not significant. These findings suggest that gang membership is indirectly related to negative outcomes because membership results in less education and more years of being incarcerated which, in turn, affect the source of income in young adulthood. In spite of their limited sample size, their findings are suggestive of an important impact of gang membership.

Thornberry et al. (2003) provide the most extensive examination of the impact of gang membership on life-course transitions, following the sample in the Rochester Study from age 13 through age 22. Prospectively, they examined whether those youth who were gang members at any time during the teenage years were more likely to experience problematic transitions to adulthood including dropping out of school, early nest-leaving, early pregnancy, teenage parenthood, unstable employment (as young adults), cohabitation, and being arrested in young adulthood than were those youth who did not join a gang.

For males, Thornberry et al. (2003) distinguished between short-term gang members and stable gang members. Short-term members were more likely to impregnate a girl and to cohabit than were non-members. Stable gang members were more likely to drop out of school, impregnate a girl, be a teenage parent, experience unstable employment, and cohabit than were non-members. Because of the limited time in a

gang for most females, it was not possible to differentiate between short-term and stable gang members. However, being a gang member was significantly related to all of the problematic transitions except for cohabitation. For both males and females, gang membership was also significantly related to a variable measuring the total number of problematic transitions experienced.

Thornberry et al. (2003) examined whether controlling for eight background variables, including prior delinquency, would eliminate the significant relationship between gang membership and each of the transitions. For males, stable gang membership remained significant for all the problematic transitions except for early nest-leaving. For females, gang membership was significantly related to early pregnancy, teenage parenthood, and unstable employment even after controlling for the other eight variables.

Finally, they examined whether gang membership in adolescence increased the probability of being arrested as a young adult. For males, they found that stable gang membership was significantly related to adult arrests even after controlling for the mean number of problematic transitions and the other eight control variables. Gang membership remained a significant predictor of female adult arrests as well.

In the first investigation of long-term consequences of gang membership, Krohn, Lizotte, Thornberry, Hall, and Chu (2006) examined the impact of adolescent gang membership on several outcomes at age 30. They used the male gang members of the Rochester sample and compared non-members to short-term and stable gang members.

The bivariate results indicate that stable gang members have significantly higher rates of unemployment and welfare receipt than either the non-members or the short-term members. Interestingly, the latter two groups are not significantly different from one another. In terms of criminal outcomes, both the short-term and stable gang members have significantly higher rates of self-reported crime, carrying a weapon, and being arrested. Multivariate models suggest that for employment and welfare the impact of adolescent gang membership is indirect, mediated by dropping out of school and unstable employment during the person's early 20s. For crime and arrest, the impact tends to be mediated by earlier delinquency. Interestingly, the impact of gang membership on weapons carrying is largely unmediated by these variables.

The results from the Rochester Study, along with results from ethnographic research, make a convincing case for the serious consequences of being a gang member on life-course transitions. With the increasing availability of longitudinal data, these analyses can be replicated to determine if these relationships hold for other research sites.

## **Future Directions: Examining Developmental Differences**

Of the topics we identified in the introduction that could best be addressed with longitudinal data, one has received virtually no empirical attention, namely, the impact of developmental stage on the causes and consequences of gang membership.

Theoretical approaches have increasingly emphasized that the cause of crime may vary depending on the age at which one is trying to account for it (e.g., Farrington, 2005; Moffitt, 1993; Thornberry, 1987; Thornberry & Krohn, 2005). For example, the role of the family appears to be more important for youth at earlier stages of development rather than in the later adolescent years. Is this also true for gang membership? Or, relatedly, do the self-reported reasons for joining a gang change depending on the age of new members? For example, it may be that younger teenagers are more apt to join because of the influence of a friend or older family member whereas those who join at an older age may be seeking the thrill of engaging in dangerous behavior, looking to profit from their membership, or simply trying to protect themselves.

Moreover, developmental stage can have important implications for the impact of antisocial behavior on the persistence and seriousness of future criminal behavior (Krohn, Thornberry, Rivera, & LeBlanc, 2001; Loeber & Farrington, 2001; Moffitt, 1993). Several theories suggest that there are distinct differences among offenders who start offending at different ages. Moffitt (1993) and Patterson, Capaldi, and Ban (1991) offer typological models of early and late starters. Early-starter or life-course persistent offenders begin antisocial behavior at young ages and their criminal careers are hypothesized to be more persistent, involving serious criminal behaviors. Late-starter or adolescence-limited offenders begin at an "age-normative" stage during their early teenage years. Their behavior is hypothesized to be less serious and they mature out of criminal behavior as they enter their young adult years. Life-course theories, like Thornberry and Krohn (2005) and Sampson and Laub (1993), offer a more age-graded approach. While there is a positive association between earlier onset and the duration of careers, the link between early onset and persistence is not inevitable as portrayed in the typological theories; persistence is largely produced by later patterns of life-course development.

These developmental distinctions regarding age of onset raise interesting questions when applied to gang membership. Are those who start offending early more likely to become gang members? Given the prediction that they are more likely to commit serious crimes and given that gang members are also more likely to commit serious crimes, it is reasonable to anticipate that those who start offending earlier are more likely to become gang members. It is also reasonable to expect that they will commit crime over a longer period of time. This should be true even if the level of their participation in crime is reduced when they leave the gang. A corollary of this hypothesis would be that early-onset offenders who join the gang would be more likely to be stable gang members, who are more likely to have adverse outcomes than non-stable gang members (Thornberry et al., 2003).

There may also be an association between gang membership and the longer duration of careers for those who start offending earlier. Not all early starters persist (Thornberry & Krohn, 2005) and if duration is produced by later life-course experiences, gang membership may be a particularly salient experience. If earlier-onset offenders are, in fact, more likely to join gangs, and if gangs really do facilitate delinquency (Thornberry et al., 2003), then one reason some earlier-onset offenders are more persistent may be because they become involved in gangs. That is, the gang experience enhances their other deficits to help perpetuate their careers.

It is also interesting to contemplate the impact of the age at which youth join gangs on subsequent behavior and life chances. On the one hand, joining at a younger rather than later age may have the same impact that early offending has on subsequent behavior, increasing the probability of a criminal career that is of longer duration and involves more serious criminal behavior. Also, joining at a young age may increase the probability of being in the gang for a longer duration, which we know will embed youth in a criminal career.

On the other hand, joining a gang at a younger age may be more transitory in nature and as youth become more mature, they may realize that gang membership is not in their interest. That is, joining at a younger age may actually lead to more instability in gang membership and hence less embeddedness in a criminal career. Joining in later teenage years may represent a more deliberate choice on the youth's part that involves greater commitment to the gang and the behaviors that are part of gang life.

We know of no research that specifically looks at these alternative possibilities. However, based on research on the age of onset of criminal behavior and research on the importance of considering developmental stages, we think this is an important question that should be addressed with longitudinal data. Developmental insights have improved our understanding of delinquent behavior and of effective interventions, and they are likely to do so for the study of gang membership as well. This should be a high priority for future longitudinal investigations.

## **Conclusion and Policy Implications**

Youth gangs continue to be a serious problem in the United States. Over the past twenty years they have proliferated to new cities, grown in numbers, and increased the level of violence. The scientific community has responded to this crisis with an ever-increasing number of studies focusing on gangs. Along with a continuation of the very strong qualitative research tradition on youth gangs, a growing number of quantitative longitudinal panel studies have examined issues ranging from the risk factors involved in joining a gang to the long-term consequences of gang involvement in young adulthood. These studies complement what we have learned from the qualitative research tradition and offer certain methodological advantages over those studies.

We have identified a number of those methodological advantages including the inclusion of a community sample allowing for a comparison of gang members and non-gang members, the prospective identification of risk factors for joining a gang, the ability to identify intra-individual change in behavior as each person develops, and the focus on the consequences of gang membership into early adulthood. These features of longitudinal panel studies allow for examination of both the impact of life-course events on gang membership and the impact of gang membership on life-course events and outcomes. Qualitative gang research typically does not follow gang members over extended periods of time because of the time and cost involved and cannot compare what happens to gang members versus non-members.

Although longitudinal studies of gang members have these advantages, they also have serious limitations. They focus on individual development and do not study gangs as the unit of analysis, following the group over time. They also do not take neighborhood context and organization into full account. Moreover, most longitudinal studies have been conducted in emergent gang cities where gang structures are primarily of the compressed type (Klein & Maxson, 2006).

Nevertheless, these studies have added to our understanding of gangs. The question that has received the most attention is whether the high rate of delinquent behavior among gang members is due to gangs selecting highly delinquent individuals who continue to commit crime at a high rate, or due to gangs facilitating higher rates of delinquency. Based on the findings from this research, there is little support for a pure selection model. Rather, it appears clear that gangs facilitate delinquent behavior. These findings, which have important theoretical and practical implications, could only have been determined by having data on gang members and non-members before, during, and after periods of gang membership.

Longitudinal research can substantially improve our understanding of why some youth join gangs while others do not. The few studies that have examined this issue have identified a number of risk factors and, as importantly, identified theoretically plausible factors that are not prospectively related to gang membership. The most striking finding from these risk factor studies concerns the accumulation of risk; gang members have serious deficits in multiple developmental domains. Understanding these multiple risk factors is essential for programs designed to prevent gang membership.

From the longitudinal research studies, we are also beginning to appreciate the collateral damage that gang membership has on a person's life course and life chances. Gang membership is related not only to elevated crime rates but to a number of problematic transitions in the life course that decrease the likelihood of success in the conventional arena. The research on this issue is very limited and these early findings need to be confirmed. In addition, several longitudinal studies continue to follow their samples well beyond the very early adult years, providing an opportunity to examine the long-term impact of gang membership.

### ***Policy Implications***

The policy implications of this research raise an interesting conundrum. Gang members commit the lion's share of serious delinquency and the gang itself appears to elicit that behavior. Gang membership also creates serious disruption in the life course, which imposes substantial individual and societal cost. It seems abundantly clear that preventing gang membership and reducing its consequences are fundamentally important policy objectives.

However, at the present time, no known gang reduction program – either prevention, intervention, or suppression – has acceptable scientific evidence of its effectiveness (for reviews, see Klein & Maxson, 2006; Thornberry et al., 2003). Thus, in the arena in which we need the most help, we have the fewest resources.

In light of this, we offer the following suggestions:<sup>3</sup> First, some evidence-based programs have been shown to reduce delinquency and violence for serious offenders. The Blueprints Program (Elliott & Mihalic, 2004) offers a compendium of effective programs that meet stringent evaluation criteria. Until effective gang-focused programs are developed, we recommend a more indirect approach; use gang membership as a marker to enroll gang members in these programs.

Second, we should encourage the developers of those effective programs to collaborate with gang experts in order to tailor the most appropriate programs to the specific needs of gang members. We must recognize these as new programs, however, and evaluate them rigorously.

Third, we should identify the most promising direct gang intervention programs and implement them under controlled conditions with careful evaluations. Some of them may well work, but we often do not know if they do because of poor evaluations. Again, better evaluations are the key to progress.

The longitudinal studies have pointed to the centrality of street gangs and gang members in understanding the origins of serious delinquency. We must now take that knowledge base and use it to develop more targeted and effective interventions.

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<sup>3</sup> A fuller discussion of these issues can be found in Thornberry et al. (2003: Chapter 10; see also, Klein & Maxson, 2006).

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# A Review of Research on the Impact on Crime of Transitions to Adult Roles

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For centuries, criminologists have observed that most criminal offenses show a sharp rise in prevalence during adolescence, followed by a relatively rapid decline in the early twenties (Hirschi & Gottfredson, 1983; Quetelet, 1984 [1833]). The dramatic decrease in criminal behavior during young adulthood, and increasing scholarly interest in desistance more generally, have led researchers to work to uncover the processes behind age-linked change in offending. Because the drop in offending occurs during the same period of the life course in which many individuals adopt adult roles, role transitions have received growing attention as potential explanations for desistance.

Although the late teen years and early twenties are characterized by marked shifts in everything from marital and work status to living arrangements, in early searches for potential mediators of the age-crime relationship, some role transitions received more attention than others. The search for a “marriage effect” was especially prominent and yielded mixed evidence. While early interviews (e.g., Knight & West, 1975) and most quantitative research in the 1970s and 1980s provided little evidence that marriage per se was linked to desistance (Wright & Wright, 1992), Sampson and Laub (1990) found that marital attachment did predict declines in offending. There were also some early indications that having children may be relevant for crime, but the tight link between parenthood and marriage made separating their distinct influences difficult (Rand, 1987). Most early studies of the employment-offending link were at the aggregate level of analysis (e.g., Glaser & Rice, 1959), but Farrington, Gallagher, Morley, Ledger, and West (1986) also found that young males committed more offenses during periods of unemployment than they did while they were working. In the early 1990s, Sampson and Laub’s (1993) assertion that adult experiences should matter for adult offending sparked additional interest in the relationships between role transitions and crime.

Although these studies suggest that spouses, parents, and workers may offend less than do their counterparts who do not hold these roles, they have little direct bearing on whether role transitions can explain the age-crime curve. Role transitions will account for the relationship between age and crime to the degree that they are strongly associated with offending and that their own age trends match the age trend in offending (Hirschi & Gottfredson, 1985; Osgood, 2005). Many early studies had limited potential for examining these issues due to small sample sizes or research designs that confounded age, period and cohort effects.

Recently, researchers have had access to many prospective longitudinal studies that hold the promise of clarifying these role-crime relationships. The life-course perspective has also led scholars to consider a broader array of life changes in young adulthood, including changes in school status and living arrangements. Such factors, in combination with advances in statistical methods for examining the impact of life events, have enabled scholars to better address these key questions: Are transitions into adult roles associated with desistance, and if so, do they explain the relationship between age and crime? In the present paper, we review the theoretical reasons why role transitions may affect offending, some ways in which choices of samples or statistical methods might affect our conclusions about role transitions and crime, and recent evidence on the extent to which various roles, specifically marriage, parenthood, student status, employment, and living arrangements, are linked to desistance and the age-crime curve.

## **Why Might Transitions into Adult Roles Affect Offending?**

In many theoretical perspectives, adult roles are portrayed as being incompatible in some way with deviance and offending. For example, Sampson and Laub (1990) proposed an age-graded theory of social control in which individuals' social investments in conventional roles serve to restrain them from crime. The specific roles that serve this function vary across the life course, and Sampson and Laub concentrated on adulthood and roles such as marriage and work. In this view desistance occurs through the gradual development of "stakes in conformity" and increasing potential costs to criminal behavior. People who become attached to their conventional roles will be reluctant to jeopardize these roles by offending, and they will be further restrained from crime by the sense of obligation and responsibility that accompanies these roles (Laub & Sampson, 2003). Under this framework, whether or not holding a role reduces crime depends on the strength and quality of the social bonds engendered by the role. This means that any individuals who adopt adult roles without increasing their attachment to conventional society should not show decreases in offending.

In other frameworks, entry into adult roles is hypothesized to restructure individuals' lives in ways that reduce the chances that they will have opportunities to offend. For Osgood, Wilson, O'Malley, Bachman, and Johnston (1996), a key aspect of this restructuring involves the impact of role transitions on the activity patterns that previously encouraged offending. They argue that time spent in unstructured socializing is positively associated with crime and deviance because that is when situations conducive to offending are especially prevalent (Osgood et al., 1996). Therefore any role transition that reduces unstructured socializing should reduce offending as well. This mechanism is visible in work by Shover (1996, p. 98), who describes the incompatibility of the "good times" sought by offenders and the restrictions of adult roles: "[f]ew men past young adulthood can spend their nights drinking and playing in bars and routinely arise and go to work just a few hours later." Also point to reductions in unstructured time with peers as an

avenue by which entry into adult roles can lower offending (e.g., Laub & Sampson, 2003, p. 135).

Warr (1998) offers an alternative interpretation for reductions in crime stemming from decreases in time with friends upon entering adult roles. He proposes that when roles like marriage reduce time spent with deviant friends, deviant peer influence is reduced, and this declining influence explains the link between role transitions and desistance. In this reasoning, the reduction in crime will be specific to those with deviant peers. Both types of peer effects are supported by a recent study by Haynie and Osgood (2005) which found evidence of influence from deviant peers, but also evidence that unstructured socializing increases offending, whether or not one has deviant peers. There are thus multiple ways in which changes in daily habits and activities brought about by role transitions might lead to desistance.

Under these frameworks, role transitions themselves lead to change in offending. In contrast, the decrease in crime may depend on an orientational change or identity shift that must precede the role transitions. Shover (1996) describes the mindset of the ordinary offender as characterized by a focus on the enjoyment of good times, a lack of concern for obligations that are not immediate, and the avoidance of work and other restrictive routines. Giordano, Cernkovich, and Rudolph (2002) propose that before desisting, offenders first experience a cognitive shift toward openness to changing these behavioral patterns; they will then be able to use exposure to subsequent life experiences—including role transitions—to create conventional lifestyles for themselves. Many of the offenders interviewed by Giordano and colleagues (2002) had come to view their deviant lifestyles as undesirable or unviable, and they constructed alternative selves and social lives as they disentangled themselves from those lifestyles. Transitions into adult roles then served as “hooks for change” (Giordano et al., 2002, p. 992) that gave offenders opportunities and resources that aided them in the difficult task of desistance. From this point of view, the contribution of transitions into conventional adult roles is contingent on cognitive changes, and these roles are more enabling rather than they are directly causal.

What if the relationships of role transitions to offending reflect the spurious contribution to both of stable preexisting characteristics, rather than effects of the roles themselves? Gottfredson and Hirschi (1990) argue that individuals do not “accidentally” or randomly enter states like marriage or stable employment. Individuals who find the constraints of these conventional roles unacceptable are the same individuals who tend to show high rates of offending, namely, people who lack self control. This line of reasoning suggests that any role-crime relationships may be due to selection effects, and that role transitions themselves may not lead to change in offending.

## **Conceptually Important Features of Studies**

There are thus many reasons why role transitions and offending might be related, and many different methods have been used to determine the extent and nature of these relationships. Might a researcher’s choice of research methods affect

substantive conclusions about role effects on crime or about the theories that predict these effects? It is useful to consider studies of role transitions and crime in light of two types of issues. The first, which we address in this section, concerns features of conceptual importance in that they affect the substantive inferences that can be drawn from studies. The subsequent section discusses methodological issues critical to the reliability and validity of findings and to the justification of causal inferences.

### *Age Range*

The age range covered by a study must be considered in interpreting its results because age is closely tied to the rates of transitions to adult roles and to the societal reactions to those transitions. For instance, according to Fussell and Furstenberg's (2005) analyses of 2000 U.S. census data, 3% of both white and black men are married at age twenty, and 64% of white men and 44% of black men are married at thirty. Thus a study would have to cover an even broader age span to capture the large majority of transitions to marriage in both groups, and studies of shorter periods will certainly miss a large portion of the eventual transitions.

The specific age range included in a study may be consequential because there are reasons to suspect that the nature and meaning of role transitions differ depending on whether they occur early, at a normative age, or much later. Entering full time employment before completing high school is likely to preclude the advanced education that would enhance future job prospects, for example, while teen parenting brings obligations and responsibilities before social and financial resources are in place. Also, there is a broad, though loose, consensus about the ages at which role transitions are most appropriate (Furstenberg, Kennedy, McCloyd, Rumbaut, & Settersten, 2004). As a result, "off time" transitions may bring reactions from others that could counter crime-reduction benefits through processes such as labeling (Lemert, 1972), negative emotions (Agnew, 2001), or rejection by conventional peers (Sutherland & Cressey, 1955).

The degree of consistency of role transition effects across ages has great implications for both policy and theory. For instance, policies to support marriages, parenting, or employment among former prisoners by means such as training, counseling, parole decision-making, or financial incentives will be considerably more useful and easier to justify if their benefits are not limited to certain ages. Consistent effects across a broad age range would match straightforward versions of the theories discussed above, while effects limited to a specific age range would call for more complex interpretations combining multiple theories or involving the interplay of competing forces.

Regardless of the specific age range covered, the duration of a study has additional consequences for the information it can produce. A study covering no more than a few years may add to our understanding of the short-term impact of a role transition, but it will be uninformative about the longevity of those effects or their evolution over time. For example, using the Glueck and Glueck (1950) long term follow-up data, Laub, Nagin, and Sampson (1998, p. 237) were able to examine the

progress of the relationship between marriage and crime over time, which led them to conclude that “[t]he effect of a good marriage takes time to appear, and it grows slowly over time until it inhibits crime.” Extended longitudinal studies are needed to reveal these kinds of relationships.

### *Study Era*

Elder (1994) suggests that the historical context can have dramatic effects on the life pathways that are available to individuals and on their reactions to turning points. Will our conclusions about role effects on crime differ depending on the era of the study? In other words, are findings based on “old” data informative about the experiences of modern cohorts? Consider the marriage effect. Laub and Sampson (2003) suggest that marriages in the United States in the 1950s may have been characterized by especially high levels of informal social control, as gender roles during this era encompassed very specific role obligations for husbands (who were to provide economic support) and wives (who were to manage the household and their husbands’ affairs). If these qualities are the mechanism behind the effect of marriage, and they are less characteristic of modern marriages, then we find may a weaker marriage effect among contemporary samples.

In addition, recent changes in the timing and sequencing of role transitions (Shanahan, 2000) may mean that we must wait to observe role effects among respondents who are currently in their early twenties. For instance, the growth in non-marital child bearing means that pregnancy is less likely to lead to marriage today than in earlier decades, especially among serious offenders (Giordano et al., 2002). Upon finding no effect of romantic partnerships on desistance, Stouthamer-Loeber, Wei, Loeber, and Masten (2004) suggest that a “good marriage effect” may not yet be visible among respondents in the Pittsburgh Youth Study, who are only now reaching their thirties. These authors also note that the rate of serious and persistent delinquency among their respondents may be unusually high, because the sample was interviewed during an upsurge in violent crime in Pittsburgh (Stouthamer-Loeber et al., 2004). This illustrates how fluctuations in base rates of both offending and role transitions affect the nature of the data collected.

In contrast, Laub and Sampson (2003) argue that these changing base rates do not necessarily have any bearing on the nature of any role effects on offending, and they fully expect that the patterns revealed in their own research will be generalizable to other eras and places. They also point out the inevitable fact that the “modernity” of any study’s participants and design necessarily will be capped by the participants’ current ages: We can only learn about the consequences in middle-age of early adult role transitions that happened at least a couple of decades earlier. This means that researchers interested in long-term patterns of offending through a major portion of the life course must rely on data from cohorts born in a prior era. The answer to this dilemma is not to criticize studies for being out of date but rather to insure the continuity of sound, long-term studies of cohorts from each decade to insure that in

the long run we will have the data needed to determine the magnitude, nature, and sources of any differences across eras in the effects of role transitions on crime.

### *General Versus Selected Populations*

Another conceptually important distinction among studies of role transitions and crime is whether the sample is drawn from the general population or a high risk group, such as people who have been convicted or incarcerated for previous crimes. Studies of high risk or offender populations have predominated in this area of research (e.g., Horney, Osgood, & Marshall, 1995; Piquero, Brame, Mazerolle, & Haapanen 2002; Uggen, 2000), and discussions of desistance have emphasized the need to concentrate on people with substantial histories of offending (e.g., Bushway, Piquero, Broidy, Cauffman, & Mazerolle, 2001; Laub & Sampson, 2001). We would agree that studying such groups is appropriate both because they are central to policy concerns and because it is essential that we insure that our theories are relevant to the most serious forms of offending. We would also argue, however, that it is important to complement this research with studies of general populations, and here we disagree with Laub and Sampson's (2001, p. 10) contention that "criminologists should . . . not spend much time or energy studying termination and desistance for low-rate offenders," and their recommendation that scholars focus mainly on "termination and desistance . . . among those who reach some reasonable threshold of frequent and serious criminal offending." Instead, we believe that there is a great deal to be gained by studying declines from all types and levels of offending and from conducting studies of both selected and general population samples and comparing the findings they yield.

The primary theories linking role transitions with crime are not specific to any specific type of offenses or group of offenders, so they predict that the findings will match across general and high risk populations. Whether the findings do match is an empirical question, and if they do not, the divergence between types of studies would give direction to the search for better explanations. Furthermore, even if crimes meriting long prison sentences are rare in general population samples, lesser offenses such as shoplifting, writing bad checks, and minor assaults have considerable societal costs precisely because they are so common. Finally, the age-crime curve, the fundamental fact underlying the study of crime and the life course, applies to both high and low level offenders (Hirschi & Gottfredson, 1983), and we cannot limit our attention to either group alone if we wish to explain it.

### **Methodological Issues in Assessing the Effects of Role Transitions**

In this section we review a variety of important methodological issues for successful research on role transitions and crime, dividing them into issues concerning measurement and research design and issues in statistics and data analysis. Researchers

face sizable methodological obstacles in seeking to determine the impact on crime of transitions into adult roles, and here we discuss some important issues that must be addressed and the major approaches available for doing so. Considerable progress has been made on these issues in recent years, and ongoing methodological research promises to bring us not only improved techniques of research design and analysis but also a clearer understanding of the proper use and interpretation of key methods.

## ***Measurement and Research Design***

### **Cross-sectional Versus Longitudinal Designs**

Cross-sectional studies in which information on both role statuses and offending is collected at a single time point can demonstrate whether individuals who hold adult roles tend to offend less than do individuals who do not hold those roles. For example, Arnett (1998) examined the past-year frequency of substance use and dangerous driving among people who were married and unmarried parents and non-parents at the time they were surveyed. The major drawback of such cross-sectional designs is that they provide little reassurance that group differences in offending indicate the effects of role transitions rather than preexisting differences between people who did and did not enter those roles. For example, while the observation that married people commit less crime may reflect an actual effect of marriage, it also may reflect the fact that individuals who are unlikely to marry are also likely to offend. Without longitudinal data, a researcher is limited to testing and controlling for differences on measured variables that can be presumed to have preceded the role transition. For instance, Arnett (1998) included respondents' age, gender, education, and parental education as covariates in his models. Yet such controls are adequate only to the degree they can fully account for prior differences in offending, which cannot be tested using cross-sectional data. Accordingly, most of the research on role effects has drawn on longitudinal data, where one or more panels of respondents are assessed repeatedly over some period of time, and our review will concentrate on such studies. (In a later section we discuss data analytic issues for addressing this selection problem.)

### **Frequency of Measurement: Panel Surveys, Archival Data, and Life History Calendars**

A key feature of longitudinal studies of role transitions is the frequency with which both crime and role transitions are measured. This frequency places a limit on the potential to identify the sequence or timing of changes in roles and changes in offending. For instance, a panel survey measuring both offending and parenthood once every two years can only determine the causal ordering between the two if the effect has a causal lag of at least two years. If the lag is shorter, in this research design they would appear simultaneous. In contrast, archival data have the potential

to yield very precise information on timing of crime and role transitions, with arrest records providing dates of offenses and birth registries providing dates of births. Those benefits may be elusive, however, given limited access to such databases in the United States. (For a contrast in another country, see Blokland & Nieuwebeerta, 2005.)

Another means of increasing the frequency of measurement is to use a life history calendar as an aid to recall in order to gather more fine-grained information on timing as part of a survey (Freedman, Thornton, Camburn, Alwin, & Young-DeMarco, 1988). Life history calendars provide a structured framework for having respondents retrospectively report when various events occurred during a reference period, typically the preceding one to five years. For instance, using a “crime calendar” which broke respondents’ past three years into monthly intervals, Horney and colleagues (1995) asked recently incarcerated offenders in Nebraska to report the months during which they committed various offenses. Then using a similar “event calendar”, they determined which months the respondents were attending school, working, living with a wife or girlfriend, and using various substances. In describing their long-term follow-up of the Gluecks’ original sample of delinquent youths, Laub and Sampson (2003) explain that life-history calendars helped clarify the timing, sequence, and duration of events in respondents’ lives. In this way, efforts to improve recall for roles and behaviors of interest can reduce measurement error and help researchers gain purchase on the causal ordering of life events.

### **Self-Reported Versus Official Delinquency**

Role transitions and changes in offending have been studied using official arrest records (e.g., Blokland & Nieuwebeerta, 2005), survey respondents’ self-reports of their offending (e.g. Warr, 1998), and even offenders’ self-reports of their arrests (e.g., Uggen, 2000). Still, few available datasets contain information on both self-reported and officially recorded offending, and each form of information has strengths and weaknesses. Official data have been criticized for reflecting official responses to or management of crimes as much as they reflect actual offending behavior (Short & Nye, 1957). Official data also clearly underestimate the actual prevalence of offending (Laub, 1997; Short & Nye, 1957), indicating that these data may give an inflated impression of how many offenders have fully desisted from crime.

A common weakness of self-report measures is that they can over-emphasize minor and perhaps even trivial offenses, revealing little about more serious offending (Hindelang, Hirschi, & Weis, 1979). Elliott (1994) has demonstrated, however, that this problem can be overcome by limiting analyses to more serious self-report items and by gathering more detailed information about offenses. There is considerable evidence supporting the overall reliability and validity of self-report measures, though there are also indications of differential validity that could result in biased estimates of important relationships such as race and gender differences (Junger-Tas & Marshall, 1999).

The balance of strengths and weaknesses of official and self-report measures of offending implies that the field is best served by conducting studies of both types in order to determine whether findings replicate across them. Our conclusions about role effects on crime will be much more straightforward if these effects are visible regardless of the measure of the dependent variable. Replicating findings on role effects across measures also provides reassurance that results truly characterize crime in general rather than the biases of a specific measure or the particular types of crime a measure emphasizes. Still, each type of measure is valuable for addressing some issues that the other is not. For instance, even net of self-reported offending, justice system contacts appear to have independent effects on life trajectories (Bernburg & Krohn, 2003; Hagan & Palloni, 1990), giving us good reason to be concerned with predicting arrests and spells of incarceration. As always, the preferred research design must be dictated in part by the research question.

## *Statistics and Data Analysis*

### **The Problem of Selection**

Transitions into adult roles such as gaining full-time employment, becoming a parent, and moving away from one's parents' home do not happen at random in a vacuum, and researchers are rarely, if ever, in a position to assign some people to experiencing these events and others not. Many factors including prior experience, personality, and life circumstances are likely to contribute to role transitions, and any of those factors might also affect crime. Accordingly, the most important and difficult methodological issue in research on the effects of role transitions is the possibility of selection: what appear to be the effects of a role transition actually may be consequences of factors that led to the transition. Gottfredson and Hirschi (1990) illustrate this logic in their argument that role transitions such as marriage and employment are associated with lower crime rates only because both are caused by high self control.

A variety of pre-existing individual characteristics could contribute to both role transitions and crime. With regard to demographic factors, for instance, there are well established race, gender, and social class differences in the rates and/or ages at which people complete their educations, begin full-time employment, get married, and become parents (Fussell & Furstenberg, 2005), and these same factors are known correlates of criminal behavior. Personality traits such as self control, sensation seeking, and extroversion are another class of variables that could contribute to both offending and role transitions (Gottfredson & Hirschi, 1990; Miller & Lynam, 2001). There are, of course, many external features of people's lives that could also be relevant, such as peer groups, parents' resources, neighborhood characteristics, and local economic conditions.

More complex selection effects are suggested by the emphasis on cognitive change in the desistance process portrayed by Shover (1996) and Giordano and colleagues (2002). In this view, any effect of a role transition such as marriage is

subsequent to the emergence of a cognitive openness to change that spurs interest in both marriage and reform. The subsequent decision to marry then matches the classic rational choice conception of selection from economics in which people choose a treatment because they expect it will enhance their standing on an outcome (Winship & Morgan, 1999). A similarly difficult pattern would be a reciprocal effect in which current offending affects role transitions. For instance, ongoing offending might make a person a less desirable marriage partner or interfere with holding down a job.

Whatever its source, selection produces spurious association between a role transition and crime, presenting the methodological challenge of excluding that spurious association from the estimate of the events' impact. Studies of role transitions and crime are almost always observational, meaning that researchers seek to infer the effects of role transitions from patterns of association that arise naturally in the lives of the sample being studied. The challenge of estimating casual effects is greater in such observational studies than in either true experiments, where researchers randomly assign people to different role transitions through some sort of intervention, or natural experiments, in which systematic differences in rates of a role transition arise through external forces such as a policy change or natural disaster.

## Methods for Addressing Selection

### Regression Adjustments

Statistical controls via regression analysis provide the simplest and most common means of adjusting for factors that might produce spurious association. To use these adjustments, one simply estimates a regression model that includes as explanatory variables not only the role transitions of interest, but also measures of the potential sources of spuriousness, such as demographic characteristics and prior measures of offending, personality traits, and the social environment. If the model is properly specified, so that it accurately characterizes the relationships between the outcome and the confounding variables, then this analysis will eliminate the contribution of those variables.

This regression or covariance correction has two weaknesses, one technical and the other more fundamental. The technical weakness is that the greater the differences between people who do and do not experience the role transition, the more heavily this correction relies on the linearity, additivity, and homogeneity of relationships assumed by basic regression models and the less potential for testing and correcting those assumptions (Winship & Morgan, 1999). For instance, this method is not a plausible means of adjusting for the educational difference between teen mothers and women who have no children by age 26 because those two groups likely have very different ranges of education, with high school dropout common and post graduate degrees rare for teen mothers and the opposite for women who delay child-bearing. A regression adjustment for this difference in education makes the untestable assumption that the relationship of education to crime is identical for these two groups, even across the levels of education where they do not overlap.

A more fundamental weakness, however, is that this regression approach adjusts only for those variables that are measured and included in the analysis, which is often referred to as adjusting for observables but not unobservables. If reliable variance in crime remains unexplained, as is always the case, there may be omitted variables that influence both crime and the role transition and that might, therefore, account for the association between them.

### Propensity Matching

Propensity matching (Rosenbaum & Rubin, 1983; Winship & Morgan, 1999) provides a means of resolving the technical shortcoming of regression controls for selection. Propensity scores come from an analysis relating all of the control variables to the role transition of interest, such as marriage or parenthood, thereby capturing in a single score (the fitted probability) the association between the entire set and the role transition. Using those scores to match people who experience the role transition with people who do not yields groups with comparable distributions on the control variables, thereby removing group differences on those variables without making problematic assumptions about their relationships with the outcome. The application of propensity matching is an important focus of current research on role transitions and crime, and important advances are being made. Both King, Massoglia, and MacMillan (2007) and Sampson, Laub, and Wimer (2006) provide strong examples of the use of propensity score analysis in investigations of the effect of marriage on crime.

Though there is much to recommend propensity matching, its function is solely to remove differences between the groups on a given set of control variables. Propensity matching does not address the deeper problem of selection on unobserved variables.

### Analyses of Within-Individual Change

When longitudinal data are available for both a role status and crime, limiting analyses to within-individual change over time provides a readily available means of controlling for one broad class of potential unobserved confounding variables. Analyses of within-individual change gauge the impact of a role transition by comparing individuals' crime rates after the role transition to their own crime rates before the transition, relative to change over the same age span for individuals who do not undergo the role transition (Osgood, 2005). In criminology, Horney and colleagues (1995) first demonstrated this statistical approach in their analyses of the relationship between offending and changing life circumstances in monthly event-calendar data for a sample of men entering prison. This approach has since been used by Piquero, MacDonald, and Parker (2002) in analyses of offending among young men paroled from the California Youth Authority, by Laub and Sampson (2003) in their more recent analyses of long-term criminal careers for the Gluecks' sample,

and by Blokland and Nieuwbeerta (2005) in their study of both officially processed offenders and a general population sample in the Netherlands, among others.

Focusing on within-individual comparisons brings the advantage of automatically controlling for all individual characteristics that do not change over time, whether or not measures of those characteristics are available. Because an individual's gender, race, early childhood experience, and presumably many personality traits and abilities remain the same before and after the role transition, such factors cannot account for any change systematically associated with that event. In this way, analyses of within-individual change provide a much stronger control for selection processes than the standard regression or covariance adjustment for prior measures of an outcome (Allison, 1990). The primary limitation of analyses of within-individual change is that they do not address selection or spuriousness due to unmeasured variables that also vary over time, but measured time-varying variables can be controlled as covariates or through propensity scoring techniques.

Several statistical approaches serve the purpose of limiting analyses to within-individual change. This feature is inherent in fixed effects panel models (Johnson, 1995) and analyses of change scores (Allison, 1990). Random effects or multi-level panel models also can be limited to within-individual change, either by controlling for individual means over time on the role transition measure or by transforming the role transition measure to deviations from those individual means (Raudenbush & Bryk, 2002, pp. 134–142).

Longitudinal data are especially valuable for research on the impact of role transitions because they illuminate whether differences between people who did and did not undergo a transition reflect change associated with that event or a continuation of prior differences. The standard longitudinal panel model provides all the information needed for within-individual analyses, namely, repeated measures of both role transitions of interest and offending. Within-individual analyses make excellent use of this information to substantially reduce selection effects, and they do not carry a heavy burden of implausible or untestable assumptions. In our view, analyses of within-individual change provide a valuable means of strengthening analyses on the effects of role transitions on crime that merits much wider use in this field of research.

### Experiments: True, Quasi, or Natural

Of course the strongest method for eliminating the threat of selection is the random assignment experiment, which inherently reduces differences between groups to chance levels for all other variables, whether measured or not. Though we do not expect experiments ever to be a mainstay of research on role transitions, random assignment to life transitions does sometimes occur. A notable example is Uggen's (2000) research on the effects of employment, based on an experiment in which offenders and other at-risk individuals were randomly assigned to an intervention in which they were offered minimum wage jobs or to a control group.

Experiments randomly assigning individuals to marriage or parenthood seem unlikely, but it may be possible to identify policy changes or essentially random

events that create natural experiments or strong quasi-experiments. For instance, Hotz, McElroy, and Sanders (2005) reasoned that miscarriages happened essentially at random and thus present a natural experiment on the impact of becoming a parent by creating a group of non-parents who are comparable to new parents. Perhaps some states will enact policies that would alter tax incentives for marriage among the poor while others do not.

Because the presence versus absence of these policies would be exogenous to individuals' role transitions, it may constitute an instrumental variable appropriate for analyses that address selection on unobserved factors (Winship & Morgan, 1999). Instrumental variable analyses depend on identifying measures that influence the role transition but do not directly influence crime, an assumption that is difficult to justify with passive observational research designs. For this reason current methodological standards largely limit the use of instrumental variable models to policy changes and natural experiments (Angrist & Krueger, 2001).

Though we do not foresee that true, natural, and quasi-experiments will ever dominate research on life transitions and crime, they are an important adjunct to the more common passive observational methods. We encourage researchers to seek opportunities to take advantage of these research designs.

## **What do we Know About Role Transitions and Offending?**

With the growing popularity of more sophisticated methods for examining the effects of time-varying covariates, research activity surrounding life course transitions and criminal offending has increased. This upsurge in scholarship has clarified some role-crime relationships and revealed intriguing caveats about others. Below we review the recent evidence on the relationships of marriage, parenthood, student status, employment, and living arrangements with adult offending. Unless otherwise noted, the studies reviewed drew on longitudinal data, which means that these investigators had data essential to accounting for pre-existing differences in crime between individuals who did and did not enter the examined role status.

### ***Marriage***

Of all of the role transitions examined to date, marriage appears to have the largest and most consistent effect on offending. Cross-sectional analyses and comparisons of groups of offenders who do and do not marry have linked marriage with reductions in everything from criminal convictions (Farrington & West, 1995) to traffic offenses and heavy drinking (Arnett, 1998). Warr's (1998) analyses of two waves of data from the National Youth Survey revealed that marriage is associated with decreases in both time spent with peers and the number of delinquent friends, and these changes in peer relations are in turn associated with declines in offending. Sampson and Laub's (1990) earlier longitudinal analyses of data from the Gluecks'

study of official delinquents and a non-delinquent comparison sample demonstrated that marital attachment is negatively associated with crime and deviance net of prior arrests. Later, Laub and colleagues (1998) conducted additional analyses of the Glueck data comparing latent classes of offenders—a strategy that at least partially controls for stable individual differences—and found that offenders who entered good marriages showed initial increases in arrests but marked later decreases. Subsequent within-individual analyses (Laub & Sampson, 2003) confirm that the link between marriage and offending is not due to stable between-person differences. Blokland and Nieuwebeerta (2005) found a beneficial within-individual effect of marriage on convictions among low- and moderate-rate officially processed offenders in the Netherlands, although the effect was not present for high-rate or sporadic offenders or for self-reported offending among a general population sample. Using monthly data on newly convicted offenders in Nebraska, Horney and colleagues (1995) similarly found that the marriage effect remains visible when criminal offending by married individuals is compared to their own levels of offending during times when they were not married. Conclusions about the causality of the marriage effect may be further reinforced by additional findings from these within-individual analyses, which suggest that just as marrying reduces the odds of offending, divorcing or separating increases these odds (Horney et al., 1995).

There are, however, caveats to the beneficial effects of stable romantic partnerships. For example, given the apparent robustness of the marriage effect, it is somewhat surprising that studies employing within-individual analyses have found that individuals commit more crimes while cohabiting with romantic partners (Horney et al., 1995; Piquero, MacDonald et al., 2002). This may explain why Stouthamer-Loeber and colleagues (2004), who examined the effects on crime of living with a partner whether married or not, found no overall effect of romantic partnership on offending.

Furthermore, in line with Sampson and Laub's (1993) predictions about the importance of relationship characteristics in encouraging desistance, some studies have revealed that marriages to criminal spouses may fail to reduce crime, or even result in increased offending. For example, Osborn and West (1979) used data from the Cambridge Study in Delinquent Development to show that among males who had official records at the time of their marriages, the prevalence of reconviction was higher for those whose wives had official records than it was for those whose wives did not have criminal records. Similarly, analyzing data from a six-year follow-up of Iowa high school students, Simons, Stewart, Gordon, and Elder (2002) found that grown delinquent youths tended to choose antisocial romantic partners, and that involvement with an antisocial romantic partner was positively related to young adult offending even net of prior delinquency. Giordano and colleagues (2002) also question whether the strength of the marital bond is as important as are the partner's own characteristics and conventionality.

The marriage effect may also be contingent on entry into other adult roles. For example, Giordano and colleagues (2002, p. 1013) found that attachment to spouse alone did not predict criminal involvement for males or females, but the attainment of a complete adult "respectability package" of marriage and full-time work was associated with desistance. Using within-individual analyses of data from

a sample of California Youth Authority parolees, Piquero, Brame and colleagues (2002) found that a similar combination of “stakes in conformity” in the form of marriage and full-time employment was negatively related to nonviolent arrests among males. These findings suggest that simultaneous commitment to multiple conventional lines of action may bring about the largest reduction in offending.

### **Nature of Evidence on Marriage and Cohabitation and Offending**

How strong is the evidence base for concluding that marriage reduces crime? Most studies on the subject, whether cross-sectional or longitudinal, have found that marriage seems to suppress offending. This “marriage effect” is visible in within-individual analyses of high risk populations using both self-report measures (e.g., Horney et al., 1995) and arrest data (Piquero, MacDonald et al., 2002), and among general populations using self-report data (e.g., Warr, 1998). Furthermore, using the Glueck data, Laub and colleagues (1998) have demonstrated that the decline in offending occurs after, rather than before, the point of marriage, and that the effect appears gradually over time. Two recent studies using propensity score approaches also report robust effects of marriage on reductions in crime. King and colleagues (2007) applied propensity score matching to self-reported offending among National Youth Survey respondents, and Sampson and colleagues (2006) used propensity score weighting in analyzing arrests among the Gluecks’ sample. In sum, there is substantial convergence in findings of a marriage effect across measures of crime, type of population studied, a sizable age range, very different study eras, and statistical approaches.

### **Explaining the Relationship Between Marriage and Crime**

Why does marriage appear to reduce offending? Warr (1998) found that reductions in time spent with friends and in friends’ deviance mediated the marriage-offending link, suggesting that changes in peer influence are behind the effect. In contrast, Sampson and Laub (1990) argue that when people have strong attachments to social roles such as marriage, they will be unwilling engage in crimes that would jeopardize those roles. Using the Gluecks’ longitudinal data on delinquent and non-delinquent males, they defined strong marital attachment as warm feelings toward one’s wife, a sense of compatibility with her, and the assumption of financial and emotional marital responsibilities. They found that such marital attachment was more closely linked to reductions in offending than was marriage per se (Sampson & Laub, 1990). Although these studies point to potential explanations of the marriage effect, researchers rarely have data that allow strict tests of theory. For example, Laub and Sampson (2001, p. 47) note that both their 1993 analyses and Warr’s (1998) analysis provide “no way to distinguish between differential association and routine activity or opportunity explanations of the marriage effect.” Often, scholars relying on secondary data are limited in either the available measures of potential mediating variables, or in the number of waves of data at their disposal—which

means they cannot test theories using within-person analyses or other strategies that minimize the effects of selection.

### *Parenthood*

Many theoretical perspectives anticipate a “parenthood effect” on crime, either because new parents will become attached to and invested in their children (as in social control theory) or because parental obligations will bring reductions in the time they spend in unstructured socializing (as in Osgood et al.’s application of routine activities theory) or in the amount of time they spend with deviant peers (as in Warr’s examination of role transitions and deviant peer influence). Studies of parenthood and criminal offending indicate that if having children is linked to desistance, it may be a more important factor for females than for males. Graham and Bowling’s (1995) cross-sectional survey data indicated that the odds of desistance were three times as high for females who stayed at home most nights to look after their children, in comparison to the odds for those who did not spend their evenings in this way. In contrast, males who similarly took responsibility for childcare did not have higher odds of desistance. Uggen and Kruttschnitt (1998) found that female offenders who had children at the time they began participation in the National Supported Work Demonstration Project had lower risks of subsequent illegal earnings than did female offenders without children, although the pattern was not present for illegal earnings among males or for arrests among either males or females. In contrast, Warr’s (1998) cross-sectional analysis of data from a nationally representative sample (the National Youth Survey) found no differences in crime between unmarried parents and non-parents and between married parents and non-parents. Blokland and Nieuwebeerta’s (2005) within-individual analyses revealed no consistent effects of parenthood on crime among an officially processed offender sample or a general population sample in the Netherlands. Finally, Giordano and colleagues (2002) found that attachment to children did not predict criminal involvement for males or for females, although the overall parenthood effect was in the expected direction and the modest sample size of 197 provided little statistical power for detecting this effect.

Why do children not appear to play a larger role in desistance? Giordano and colleagues (2002) suggest that the mere presence of a potential “hook for change” is not sufficient to bring about meaningful change in offending, and the effect of parenthood in particular may be contingent on a conscious movement away from a criminal identity. While spouses can play an active role in the desistance process by monitoring and limiting behavior, the effect of children on crime may depend largely on the offender’s own agency and willingness to embrace parenthood as a turning point (Giordano et al., 2002). Interestingly, the sole effect of children found by Uggen and Kruttschnitt (1998) was found net of self-reported illegal opportunities. While Graham and Bowling’s (1995) findings suggest that the amount of time spent on childcare may explain any gender difference in the parenthood effect, there appears to be a marked “lack of inevitability of a child effect” (Giordano et al., 2002,

p. 1038), and simple reductions in criminal opportunities may not be a sufficient mechanism to lead to desistance.

### **Nature of Evidence on Parenthood and Offending**

While Uggen and Kruttschnitt's (1998) findings indicate that parents may be more likely to take advantage of "hooks for change" such as jobs, few studies suggest that having children reduces offending. Still, it may be premature to conclude that there is no effect of parenthood on crime. Parenthood has received much less research attention than has marriage as a potential explanation for reductions in crime in young adulthood, and most of the few existing studies have examined cross-sectional differences in offending between parents and non-parents (e.g., Graham & Bowling, 1995; Uggen & Kruttschnitt, 1998; Warr, 1998), rather than examining whether individuals show changes in offending upon becoming parents (for an exception, see Blokland & Nieuwbeerta, 2005).

Furthermore, whereas researchers studying role transitions such as marriage may be able to assume that individuals enter those roles intentionally and purposefully, this may not be as true of the parent role. Some of the quantitative studies of parenthood and crime have examined the impact of simply having had a child, rather than the impact of living with that child or otherwise showing more active involvement in the parent role (e.g., Blokland & Nieuwbeerta 2005; Warr, 1998). Perhaps especially for fathers, this definition of parenthood may not necessarily imply even minimal commitment to or investment in the role. Such conceptual issues may help explain why the few studies on this topic have failed to find benefits of parenthood for offending.

Giordano and colleagues' (2002) similar assertion that roles like parenthood do not have the same meaning for all offenders indicates that more useful work on this topic might focus not on parenthood itself, but rather on the mechanisms by which we might expect parenthood to have effects on crime. For example, although Laub and Sampson (2003) found that parenthood added little to the effect of marriage on crime among the Glueck men, the men's narrative accounts of their desistance pointed to the sometimes dramatic changes in routine activities that followed the birth of a child. Any such effects might be limited to the first few years of parenthood, as very young children appear to have the greatest impact on their parents' schedules (Osgood & Lee, 1993). Thus, we encourage further research on the topic using longitudinal data and measures of possible mechanisms of any parenthood effect.

### ***Student Status***

Although many desistance researchers have focused on the school-to-work transition, employment has been much more prominent in this line of research than has student status. The findings of the few studies that have examined changes in

offending upon school entry and exit suggest that enrolling in school may suppress criminal behavior. O'Connell (2003) found that previously incarcerated drug offenders who were attending school were less likely to use drugs or to be rearrested than were those who were not enrolled in educational programs. Uggen and Kruttschnitt (1998) similarly found that for both males and females, being a student was negatively associated with the risk of arrest, although student status was not related to the risk of illegal earnings. The general notion that students commit less crime is further supported by the results of within-individual analyses by Horney and colleagues (1995), who found that individuals showed lower odds of offending during periods when they were attending school than they did during periods when they were not students. Also using within-individual analyses, Blokland and Nieuwbeerta (2005) found that studenthood was the only role status that seemed to suppress offending among a general population sample in the Netherlands.

### **Nature of Evidence on Student Status and Offending**

Although there is little research on schooling and crime in the transition to adulthood, there seems to be a negative relationship between being a student and offending. Even when respondents are "used as their own controls", as in the within-individual analyses of Horney and colleagues (1995), students appear to offend less than do non-students. What we do not yet know is if this beneficial effect of being a student is consistently visible in general population samples; most previously published work—the studies of Uggen and Kruttschnitt (1998), Horney and colleagues (1995), and O'Connell (2003)—has drawn on data from institutional samples. Blokland and Nieuwbeerta (2005) did recently find a beneficial within-individual effect of student status among a nationally representative Dutch sample. Given recent increases in post-secondary enrollments, the relationship between student status and offending in the general population warrants further research attention.

### ***Employment***

Findings from a number of studies suggest that entry into full-time employment reduces offending, although effects are often limited to certain types of offenders or offenses. O'Connell (2003) found that employed offenders showed reductions in drug use even when aggression and risk-seeking were controlled, although work did not appear to affect arrests. There is some evidence that for youths making the transition to adulthood, it is not unemployment specifically, but rather idleness that is associated with increases in offending. Stouthamer-Loeber and colleagues (2004) found that those serious juvenile delinquents who were neither in school nor working in young adulthood were more likely to persist in offending than were their counterparts who were not idle. While this finding suggests that the presence of some role statuses may compensate for the absence of others, this would appear

inconsistent with the finding that a “package” of employment and marriage may be especially relevant for desistance (Giordano et al., 2002).

Employment is more readily subject to experimental manipulation than role transitions like marriage and parenthood. Using data from the National Supported Work Demonstration Project, Uggen (2000) found that offenders ages 27 and older who were randomly assigned to minimum-wage jobs showed decreased risks of arrest and illegal earnings, but the effect was not present for younger offenders. This result suggests that employment may have “real” effects on offending, at least for some demographic subgroups. Despite this finding, though, more often than not experimental work-related programs have no notable effects on crime, perhaps because they tend to be brief and narrow in focus, and because they do not address participants’ lack of motivation (Bushway & Reuter, 1997). Bushway and Reuter (1997) suggest that the most successful programs are those that target older ex-offenders because unlike their younger and less mature counterparts, older participants tend to be ready to settle down. This reinforces the notion that some life events may become turning points only when offenders actively use them toward that end (Giordano et al., 2002).

Many studies featuring within-individual analyses have revealed decreases in offending upon entry into employment, but just as in studies using different analytical strategies, work appears to “work” only for certain subgroups of offenders, or certain types of crime. Horney and colleagues (1995) found that offenders who worked full-time showed lower odds of assault, yet higher odds of property crime. Furthermore, although Piquero, MacDonald, and colleagues (2002) found that white parolees who were employed full-time were less likely to have subsequent violent arrests than were their nonworking counterparts, they found no effects of work for nonwhite parolees or for nonviolent arrests. Laub and Sampson (2003), though, drawing on data from the Gluecks’ sample, found that unemployment was associated with multiple forms of offending, ranging from predatory crime to alcohol and drug crime.

Just as with marriage, there is some evidence that offending is more closely linked to job attachment or quality than it is to employment per se. Sampson and Laub (1990) report that job stability reduces the chances of persistent offending. Using path analysis, Simons and colleagues (2002) found that job attachment was negatively related to offending among males, although the relationship was not present for females. Cernkovich and Giordano (2001) found that individuals who were more satisfied with their financial and educational situations were less likely to offend even net of prior offending, although the relationship was present only for never-institutionalized respondents. Uggen (1999) found that, among National Supported Work Demonstration Project participants who found their own employment outside of the project, the quality of offenders’ jobs predicted both their economic and their non-economic crime. Uggen was able to account for selection into employment by modeling job entry as a function of observed social and demographic covariates. Although Uggen found job quality effects net of these same covariates, it is possible that the respondents who were most likely to enter high quality rather than low quality jobs were also the most likely to show reductions in offending. Whether job attachment and job quality effects are due to selection remains an

empirical question. See Uggen and Wakefield's chapter of this volume (2008) for an extensive review of the evidence on the relationship between employment and crime.

### **Nature of Evidence on Employment and Offending**

More often than not, studies of employment and crime have found that work suppresses offending. Although most of these studies have drawn on data from samples of convicted offenders, Simons and colleagues (2002) and Cernkovich and Gordanano (2001) have found similar results using small non-institutional samples. Still, when work effects are found, they typically depend on something else, such as age (Uggen, 2000), race (Piquero, MacDonald et al., 2002), or type of offense (Horney et al., 1995), and this "something else" is rarely consistent across studies. Although contingent on characteristics of the offender or the offense, these work effects have been found in experimental designs (Uggen, 2000) and within-individual analyses (Horney et al., 1995; Piquero, MacDonald et al., 2002), suggesting that they are not artifacts of selection.

Why would employment reduce offending for some individuals and not others? Uggen and Wakefield (2008) suggest that the effect of employment on crime may depend on the characteristics of the job, and that variation in job quality across demographic subgroups may explain the inconsistent work effects found in previous research. We believe that work effects have been found often enough to make employment and its mechanisms of action on crime important areas for future research.

### ***Living Arrangements***

Although marriage and employment, and to a lesser extent parenthood and student status, have received increasing attention as potential explanations for the age trend in crime, the considerable "demographic action" of the young adult years (Rindfuss, 1991) encompasses other, less examined life transitions as well. Living arrangements in particular are highly variable during this period of the life course, and although residential moves often accompany other role transitions such as marriage, many times they do not (Goldscheider & DaVanzo, 1985). If changes in living arrangements are one of many possible transitions that occur during this same age range, might they explain a portion of the decline in offending?

Few studies have examined the relevance of living arrangements for deviance independent of other roles, and the existing evidence is mixed. For example, while Newcomb and Bentler (1987) found that living in a dormitory was associated with a decreased likelihood of substance use, Bachman, O'Malley, and Johnston (1984) found an increase in drug use by those who move from their parents' homes to living in dormitories and similar arrangements. Students who live independently in off-campus housing or in dormitories have been found to drink more frequently and to

engage in more frequent binge drinking than do students who live with their parents (Harford & Muthen, 2001), although this may be more true of males than it is of females (Valliant & Scanlan, 1996). These studies all examined living arrangements and alcohol and drug use; to our knowledge, no studies have examined the relevance of living arrangements for criminal offending.

### ***Prior Studies' Research Methods and Substantive Findings***

The studies reviewed above represent a wide range of choices in research design and analytical methods. They have covered individuals born in the 1920s and 1930s (Laub & Sampson, 2003; Sampson & Laub, 1990, 1993), the 1950s and 1960s (Horney et al., 1995; Piquero, Brame et al., 2002; Piquero, MacDonald et al., 2002; Warr, 1998) and the 1970s and early 1980s (Blokland & Nieuwbeerta, 2005; Simons et al., 2002; Stouthamer-Loeber et al., 2003). Most have tracked respondents to their early to late twenties (Giordano et al., 2002; Horney et al., 1995; Piquero, Brame et al., 2002; Piquero, MacDonald et al., 2002; Simons et al., 2002; Stouthamer-Loeber et al., 2003; Warr, 1998), but some address much longer periods of the life course (Blokland & Nieuwbeerta, 2005; Laub & Sampson, 2003). Some draw on data from high-risk or institutionalized samples (Blokland & Nieuwbeerta, 2005; Giordano et al., 2002; Horney et al., 1995; Piquero, Brame et al., 2002; Piquero, MacDonald et al., 2002; Uggen, 2000), and others draw on data from general population samples (Blokland & Nieuwbeerta, 2005; Warr, 1998). Many use self-report measures of offending (Giordano et al., 2002; Horney et al., 1995; Simons et al., 2002; Stouthamer-Loeber et al., 2003; Warr, 1998), but some rely on official arrest records as well (Blokland & Nieuwbeerta, 2005; Sampson & Laub, 1990, 1993). Researchers increasingly have employed within-individual analyses (Blokland & Nieuwbeerta, 2005; Horney et al., 1995; Laub & Sampson, 2003; Piquero, MacDonald et al., 2002) and other techniques to minimize selection biases such as propensity score methods (King et al., 2007; Sampson et al., 2006) and experiments (Uggen, 2000).

Readers may wonder whether substantive conclusions regarding role transitions and criminal offending do in fact vary across the methodological distinctions we raised in the first sections of this chapter. We believe that this remains an open question for two reasons. First, the total body of research on roles and crime is still relatively small, and the number of studies representing each potentially important methodological distinction is even smaller. Second, studies tend to feature certain combinations of methodological features, making it difficult to tease out the implications of any single feature. For example, while official records were popular sources of crime data for studies of earlier birth cohorts, studies of later birth cohorts have tended to rely on respondents' self-reports of their offending.

In the extant research on roles and crime, the marriage-crime relationship appears robust to choices of research methods, and variations in the work-crime relationship are not clearly patterned after variations in study method. There simply are too few studies of other role transitions to determine whether their "effects" are visible only

in studies with certain design features or analytical strategies. Further research on roles and crime may increase our knowledge not only of this substantive topic, but also of the broader implications of conceptual and methodological choices in research design.

## **Implications for Policy**

If role transitions do cause declines in offending, then policies that encourage or facilitate offenders' entry into adult roles may have crime prevention benefits. For example, prison visitation policies may be structured to preserve inmates' interpersonal bonds, training programs may promote entry into stable jobs, or reduced eligibility restrictions for education loans may allow offenders to reenroll in school. It may be more difficult to provide these institutional supports for offenders who are not under criminal justice supervision, although measures such as universal family education programs can reach offenders in the general population as well.

We are sympathetic to the fact that there are parties to role transitions besides the offender, including employers, children, and (often noncriminal) spouses. Given the potential harms to these parties, in the absence of more conclusive evidence of beneficial effects of work and parenthood we are reluctant to offer a blanket prescription for policies that encourage all of these role transitions for all offenders. A safer and possibly more effective approach would entail focusing on the mechanisms by which role transitions have their effects. Unfortunately, these mechanisms are not yet well understood. While we await further research, we believe that there is enough evidence on role transitions and crime to suggest that society should not create additional obstacles to family life, work, and school enrollment among offenders who want to participate in these institutions. Even if role transitions themselves are not the key to reducing offending, and conventional orientations, maturity, or attachment to society matter more, by restricting offenders' access to these roles and institutions we risk worsening the "us/them" divide and ultimately weakening offenders' motivation for reform.

## **Conclusions**

After decades of research, we can say that some transitions into adult roles are associated with reductions in criminal offending. Declines accompanying marriage have been the most consistently found across different samples, historical eras, and analytic methods. Also, more often than not, research has revealed that individuals offend less when they are in school or when they are working than they do when they do not hold these roles; we will be interested to see whether future research confirms these findings. In contrast, parenthood is not consistently linked to crime. This may seem surprising, and it should prompt scholars to consider why one would

expect an effect of parenthood, and what the absence of such an effect might mean for the theories relating all role transitions to crime.

We discussed several plausible theoretical accounts of connections between role transitions and crime, and research in this area will be much more useful if it not only establishes the effects of transitions but also determines why those effects arise. Tittle and Meier (1990) argue that research on the elusive relationship between socioeconomic status and crime might become more useful if it led scholars to more closely examine the factors thought to produce that association. Perhaps the same will be true for the weak or highly conditional associations of crime with parenthood and employment. In order to establish that a particular factor explains the relationship between an adult role and offending, researchers must compare the role-crime relationship both before and after controlling for the potential mediator. Unfortunately, researchers rarely have data on adult roles, criminal offending, and potential mechanisms of role effects all at once. As a result, we still know little about *why* role transitions such as marriage are linked to crime, and why differentiating features of roles such as their timing or quality seem to matter. Assessing explanations for the effects of role transitions should be a high priority in future research.

Research on crime in the transition to adulthood would also benefit from systematic attention to the definition of role transitions. For instance, though the biological facts defining parenthood may be obvious, it is less clear that such a definition is relevant to crime. Our theories would not seem to predict that parenthood, marriage, and employment would reduce crime for a mother who gave up her child for adoption, a husband who did not reside with his wife, and an employee who worked sporadically and changed jobs frequently. Exploring alternative definitions of these roles could prove useful for clarifying which features of the role transitions are the source of crime reductions, which in turn could advance the search for explanations of those effects.

Another potentially productive avenue for future research would be to explore whether the particular combination of adult roles a person occupies is related to crime in ways that go beyond the separate contributions of the individual roles. Macmillan and Eliason (2003) argue that viewing each role in isolation could be misleading and that the roles are not independent but rather highly interdependent, not only at any given age but also within people's lives over time. In this vein, Osgood, Ruth, Eccles, Jacobs, and Barber (2005) used latent class analysis to identify six groups with distinct role combinations at age 24. They found both that those groups differed in many domains of life at that age, and that membership in the groups was strongly related to a variety of background characteristics such as parents' education and income. There is some indication that combinations of roles matter for crime in Giordano and colleagues' (2002, p. 1013) finding that a "respectability package" of multiple roles is more important for crime than are the separate components of the package, and in Stouthamer-Loeber and colleagues' (2004) finding that in some circumstances that presence of one adult role can compensate for the lack of another.

In addition to these new directions for future research, it is important simply to have more studies that expand our base of knowledge about adult role transitions and crime. Even with the considerable growth of research in this area, there are still

relatively few studies that are strong in the sense of having a sizable sample that is followed for several years or assessed frequently (e.g., Horney et al., 1995), obtaining repeated measures of both crime and role statuses, and using strong statistical methods (e.g., within-individual analysis, experiment, or well-justified instrumental variable). Because most criminological panel studies that have followed respondents to young adulthood have collected data on role transitions, researchers could usefully add to our knowledge base just by applying advances in statistical modeling to existing datasets.

Still, we encourage scholars to move beyond basic replications of prior findings on role-crime relationships, and to instead work to advance our knowledge of other under-explored topics while simultaneously examining whether prior studies' findings hold across data sources. For example, few studies have related cohabitation, student status, parenthood, and living arrangements to crime. Though reductions in crime have been found to accompany marriage in many studies, we know relatively little about whether that relationship varies with age, duration, era, or qualities of the marriage. More research is also needed to sort out the inconsistencies across studies in the factors associated with the presence and absence of an effect of employment on crime. Replications of prior findings will help us draw broad conclusions about role-crime relationships, but these studies could potentially contribute far more. For many topics this may require new data collection efforts, but we suspect that the creative use of existing data could be quite informative.

Finally, we suggest that researchers also turn their attention to the phenomenon that motivated attention to adult role transitions and crime in the first place: the age-crime curve. Criminologists became interested in role transitions because those transitions become common at ages when the crime rate is falling. It seems odd, therefore, that few criminologists have assessed the degree to which controlling for role transitions accounts for the overall reduction in crime. To our knowledge, the only published study that has done so is Blokland and Nieuwebeerta's (2005) recent long-term study of offenders in the Netherlands. They found that, though role transitions are significantly associated with offending, those transitions account for only a modest portion of the overall age trend in offending. This result appears to be due in part to the limited strength of the associations of roles and crime and in part to the role transitions becoming common only after considerable declines in offending. These findings need to be replicated with other data sources, but they suggest both that we need to learn more about when and how role transitions are important for crime and that we should consider a broader range of potential causes of crime during this important and interesting period of the life course.

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**Part III**  
**Intervening in the Life-Course of Crime**

# What have we Learned from Longitudinal Studies of Work and Crime?

Christopher Uggen and Sara Wakefield

**Abstract** This review paper considers the connection between employment and criminal behavior. We first examine theories that suggest a link between work and crime at different life course stages. Next, longitudinal studies and statistical approaches to specifying the relationship are discussed. Results of existing studies are organized into discussions of work intensity and adolescent delinquency, job characteristics and crime, and unemployment and crime rates. We then offer a more focused discussion of ex-offenders and reentry. The paper concludes with a brief summary of what has been learned, suggesting that investments in longitudinal investigations have yielded important new knowledge about when and how work matters for crime and delinquency.

Employment has long been viewed as a solution to problems of crime and delinquency. In this chapter, we evaluate this longstanding faith in work as a means to prevent delinquency among adolescents, mitigate the connection between poverty and crime, and reduce recidivism among previously active criminal offenders.

In 2003, roughly 6.9 million Americans were under some form of correctional supervision (Bureau of Justice Statistics, 2004). Each year, more than 600,000 inmates join four million probationers and 750,000 parolees already under community supervision (USDOJ, 2004a,b). How well these former inmates, probationers, and parolees fare once they return to the community is of central concern to criminologists, corrections officials, and policy makers. In addition to those currently involved in the legal system, a large group of adolescents are at risk for delinquency involvement that may lead to more serious crimes as young adults. Finally, to the extent that crime is related to the availability of quality legal work, the number of persons in conditions of poverty or unemployment is likely to be related to the creation of new offenders. The U.S. Census Bureau estimated that 12.5 percent of Americans live in poverty and 6 percent were unemployed in 2003 (Bureau of Labor Statistics, 2004; US Bureau of the Census, 2004). Though these populations overlap with those under correctional supervision, these estimates suggest a potentially large number of Americans “at risk” for criminal involvement. In this paper, we evaluate the extent to which beliefs about work as a crime prevention tool are supported by social scientific evidence.

The swelling percentage of state and federal budgets devoted to policing and corrections underscores the need for policy makers to access solid social scientific evidence on the determinants of crime and recidivism. In response to these needs, social scientists have long focused on work as a key determinant of desistance or movement away from crime. Employment is a natural focus for social scientists and policy makers, as it is more easily manipulated in policy interventions than other important social influences (such as marriage or friendship networks), it is a social role of major importance, and it reduces the economic attraction of crime for potential offenders.

Just as employment has been a common site of research for criminologists, studies that exploit longitudinal data have also been a natural choice for measuring within- and across-person changes in crime over time. In this paper, we first explore why work is likely to affect crime and recidivism, paying particular attention to the most important dimensions of employment for crime reduction. We next briefly review a range of statistical innovations useful for studying work and crime with longitudinal data. We then describe classic research on work and crime utilizing cross-sectional evidence and link these studies to more recent results from longitudinal studies. Specifically, we summarize results from studies of work and crime among adolescents, ex-offenders, and other populations “at-risk” for crime, as well as providing a brief review of research on aggregate-level trends in crime and macroeconomic conditions. Finally, we conclude by asking whether longitudinal studies are worth the considerable time and expense they require, as balanced against the knowledge they have yielded to date.

## **Why Study Work?**

Why might work be related to criminal offending? Classic research in criminology is suggestive of a variety of mechanisms linking work and crime. We begin by discussing the remunerative qualities of employment. At its most basic, paid work provides legal income for potential offenders.

### ***Economic and Rational Choice Theories***

Economic or rational choice theories of crime suggest that income earned from legal employment will reduce the attraction of offending for financial gain (Becker, 1968; Cornish & Clarke, 1986; Ehrlich, 1973; Freeman, 1992). In classic economic theory, choice is the central mechanism linking work and crime. Beyond providing financial incentives for conforming, legal work may also increase the costs of crime. The possibility of arrest may serve as a greater deterrent for employed offenders relative to those who are not employed because arrest and concomitant punishment may result in the loss of a valued job (see, e.g., Sherman & Smith, 1992).

### ***Structural Strain and Differential Opportunity Theories***

Structural strain theories (Merton, 1938) suggest that crime results when legitimate pathways to economic and social success are blocked. Similarly, one variant of differential opportunity theory argues that access to illegitimate as well as legitimate opportunities varies considerably across persons (Cloward & Ohlin, 1960), with each person positioned along two opportunity structures, one involving legitimate work and the other illegal opportunities. These theories place the *relative* gains available from legal and illegal work at center stage. Beyond the mere presence or absence of employment, studies in this tradition emphasize the quality of employment in relation to crime. Investigations have examined the impact of income inequality (Blau & Blau, 1982), concentration in the secondary labor market (Crutchfield, 1989; Crutchfield & Pitchford, 1997), the stability of employment (Sampson & Laub, 1993), and its overall quality (Uggen, 1999) on criminal behavior. Trends in area crime rates have also been linked to trends in macroeconomic conditions in a number of studies (Allan & Steffensmeier, 1989; Britt, 1997; Massey & Denton, 1993; Morenoff & Sampson, 1997; Sampson, 1987; Wilson, 1996).

### ***Social Control and Bonding Theories***

Other theories of crime do not assign a causal role to employment in itself, but to the social bonds that employment creates for workers. Social control or bonding theories describe the bonds that work engenders as the central mechanism linking work and crime. Travis Hirschi's (1969) social control theory argues that commitment to conventional lines of action (such as work) and involvement in legal work among young adults is associated with fewer delinquent acts. Young working adults thus have a "stake in conformity" that renders crime less attractive (Briar & Piliavin, 1965; Toby, 1957). Among adolescents who work, Sheldon and Eleanor Glueck have shown that delinquents tend to work in jobs with less supervision relative to non-delinquents (Glueck & Glueck, 1950). Social interaction at work is also likely to increase the "informal social controls" to which potential offenders are subject (Sampson & Laub, 1993), and connections made through work may replace deviant peer networks with law-abiding friends. Thus, crime and work are related to the extent that work exerts social control over potential offenders and creates pro-social bonds for young adults.

### ***Routine Activities***

Finally, routine activities theories shift the emphasis from the individual effects of work to the structural impact of employment on everyday life (Cohen & Felson, 1979). Osgood, Wilson, O'Malley, Bachman, and Johnston argue that this approach

“shifts attention away from the personal histories of offenders toward the dependence of crime on opportunities presented by the routine activities of everyday life” (1996: 635). The routine activities approach also anticipates differing effects of employment on crime. For example, unemployment may reduce crime by decreasing the numbers of hours people spend outside of their homes (thereby allowing them to protect their homes from burglary). Alternatively, those who are employed have fewer hours to devote to crime themselves.

### *Self-Control Theories*

Though numerous theories of crime anticipate a “real” relationship between crime and employment (whether positive or negative), others argue that such a finding may be spurious due to common or correlated causes. Gottfredson and Hirschi (1990) maintain that many of the putative connections between crime and employment are the result of selection bias. In their view, criminals and non-criminals are differentiated primarily by their levels of self-control, with offenders having far less of it than non-offenders. According to this view, low self-control predicts crime over time as well as the likelihood of finding and maintaining high-quality employment. Thus, non-criminals self select into more and better employment opportunities. From this viewpoint, statistical associations or relationships between employment and crime are likely the result of unmeasured variation in levels of self-control.

### **Work, Crime, and the Life Course Perspective**

While most classic theories of crime suggest that employment may reduce crime, more recent investigations have shown greater complexity in the relationship. Life course theories suggest that the effects of employment on crime or recidivism are age-graded and contingent upon particular stages within the life course. For example, some types of work may reduce crime only for some types of offenders (see, e.g., Blokland & Nieuwebeerta, 2005). Moreover, the work-crime relationship may be dependent on age, gender, marital and parental status, and a host of other life course contingencies. Travis Hirschi (1969) argues that while commitment and involvement in work is beneficial for young adults, *over-involvement* in work at a young age may be detrimental. More recent research has supported this argument; adolescents who are over-invested in work relative to school are more likely to engage in delinquency (Bachman & Schulenberg, 1993; but see Paternoster, Bushway, Brame, & Apel, 2003). Similarly, in a study of recently released inmates, drug addicts, and high school dropouts, Uggen (2000) finds significant effects of work *only* for offenders age 26 or older. It is likely that family connections also play a role in conditioning the effect of employment on crime (Uggen, Wakefield, & Western, 2005). The presence of a spouse or child may intensify the positive effects of employment (Laub & Sampson, 2003; Sampson & Laub, 1993).

The life course perspective naturally lends itself to longitudinal analyses of work and crime. Life course models using longitudinal data distinguish among individuals in the effects of work on crime as well as compare the effects of work on specific individuals over time. In the next section of the paper, we outline the common statistical approaches and methodological innovations aimed at overcoming confusion regarding the temporal ordering of causal effects and selection into work and crime. We also summarize findings from studies of work and crime at the individual and aggregate levels. In particular, we highlight innovations that use longitudinal data to discriminate among the causal mechanisms described above, as well as methods that attend to problems of selectivity into employment and crime, and concerns about causal ordering and spuriousness.

## **Longitudinal Studies of Work and Crime**

The reliance of early studies on cross-sectional data has rendered them better-suited for describing correlations between work and crime than for drawing causal inferences. While useful, studies using cross-sectional data are unable to test some of the most complex issues involving work and crime, such as temporal order, differential selection into employment, reciprocal effects between work and offending, and elaboration of causal mechanisms (Thornberry & Krohn, 2003). First, when work and crime are measured at the same time, the analysis is unable to adequately describe which variable is the cause and which variable is the effect. Second, in cases in which a significant association is detected between work and crime, cross-sectional data offer no way of determining whether those least likely to commit crime are also those who select into employment opportunities (e.g., Gottfredson & Hirschi, 1990). Third, it is likely that crime and work are reciprocally related (Hagan, 1993; Thornberry & Christenson, 1984). Finally, the causal processes predicted by major theories of crime are most easily tested when measures of deviance in addition to various demographic characteristics can be measured prior to beginning employment. In response to these difficulties, social scientists have increasingly turned to longitudinal designs in order to adequately measure and test these competing arguments. We review these longitudinal studies below, summarizing some of the major research efforts in this area in the chapter Appendix.

Classic cohort studies heralded a wave of longitudinal research on crime. Sheldon and Eleanor Glueck followed 500 delinquent boys who were matched with a control group to analyze the impact of family, work, and attachment on delinquent outcomes (Glueck & Glueck, 1930, 1937, 1943). Sampson and Laub, (1993; Laub & Sampson, 2003) updated these data, and applied modern statistical analyses to develop a social control theory of crime which focuses on the social bonds of work. Wolfgang, Figlio, and Sellin's analysis of a cohort of men born in 1945 in Philadelphia (1972) refocused attention on individual careers in crime and found a positive relationship between spells of unemployment and arrest. Farrington and West's Cambridge Study in Delinquent Development (Farrington, 1986; West & Farrington, 1977) followed 411 boys from London from the age of eight. In 1976, the National Youth

Survey began following over 1,700 adolescents who are now between 39 and 45 years old (Elliott, Huizinga, & Ageton, 1985).

These classic studies have given way to more recent large-scale longitudinal studies (e.g., Thornberry & Krohn, 2003) as well as a host of smaller, community studies (e.g., Mortimer, 2003) and improved study designs. Classic longitudinal studies generally selected individuals from a birth cohort and followed them for a number of years, collecting multiple observations on crime, work, and other important events. This design has been criticized for its inability to distinguish between age, period, and cohort effects. It also tends to be costly and is often plagued by problems of selective attrition (see Farrington, Ohlin, & Wilson, 1986; Tonry, Ohlin, & Farrington, 1991 for a detailed discussion). In response, researchers have adopted accelerated longitudinal designs which follow several cohorts over a period of years (Sampson, Morenoff, & Raudenbush, 2002; Tonry et al., 1991). Accelerated designs allow researchers to distinguish cohort and period effects and tend to be less costly because the data collection time is shortened.

Data from these recent studies have been used to test increasingly complex hypotheses about how employment influences crime at the individual and aggregate levels. Yet, longitudinal studies tend to be much costlier than smaller, cross-sectional analyses and many have claimed that they are not worth the expense. In an especially strong review, Hirschi and Gottfredson argue that the costs of longitudinal research substantially outweigh its benefits, noting that the “design has been oversold to criminology at high substantive and economic costs” (1986: 582; see also Gottfredson & Hirschi, 1986). Since longitudinal studies remain an expensive way of collecting data, it is necessary to take stock of their findings and justification for their continued use.

## **Statistical Approaches to Measuring the Relationships Between Work and Crime**

Though experimental research remains the gold standard for evaluating employment and crime (Campbell & Stanley, 1966), programs of this kind are relatively rare. In the absence of random assignment to work, analysts have adopted numerous statistical correction techniques to account for differences across persons in order to estimate “true” employment effects. A major concern in analyses of employment concerns the non-random selection of persons into jobs and the impact of prior acts of deviance on the probability of both getting a job and committing more crime. If an analysis shows a relationship between employment and crime for any one individual, this in and of itself is not strong evidence of an employment effect. This is especially true in studies of offenders as those with prior criminal experience may be least likely to select into legal employment (Freeman, 1997; Pager, 2003; Western, 2002). A wealth of research has demonstrated that people who don’t work are systematically different than those who do (just as offenders may be systematically different from non-offenders) and analysts have developed a number of statistical techniques to deal with this problem of selectivity into employment.

## **Cross-Sectional Approaches to Work and Crime**

### ***Covariate Adjustment***

Covariate adjustment refers to attempts to name and measure all factors associated with crime that could be plausibly influence selection into work as well. For example, OLS regression approaches that include “controls” for age, gender, race, or social class will adjust work effects for these factors. Of course, other important variables may be omitted, such as ambition or motivation. Covariate adjustment is a common statistical method used by researchers using cross-sectional data to attempt to account for the characteristics that account for criminal involvement as well as employment. While useful, this approach is highly dependent upon researchers choosing the “right” variables to control for and, when used with cross-sectional data, does little to advance knowledge on the causal ordering of work and crime.

## **Longitudinal Approaches to Work and Crime**

### ***Lagged Dependent Variables***

Longitudinal data is useful for the selectivity problems described above as it often includes multiple measures of crime and employment over time. While work may influence crime, analysts have also shown that crime influences later work experiences (Caspi, Wright, Moffitt, & Silva, 1998; Hagan, 1993). Utilizing multiple measures of work and crime allows analysts to estimate the effect of work on crime, net of prior criminal acts (e.g., Huiras, Uggen, & McMorris, 2000). Lagged dependent variable models generally predict crime at time 3 using work at time 2 and crime at time 1 as covariates. By including a prior crime measure, or a “lagged” dependent variable, such approaches reduce the influence of stable factors that may be driving both processes (though time-varying factors related to both work and crime remain a threat to analyses of this type). This lagged dependent variable approach represents a substantial advance over covariate adjustment alone. It therefore leads to stronger tests of employment effects and firmly establishes temporal sequencing.

### ***Selection Models for Across-Individual Comparisons***

When studying the effects of employment conditions on crime, analysts are limited to a “working” subgroup that may not be representative of the entire sample. Put simply, analyses of work hours, wage rates, or job quality are complicated by the fact that not everyone works and that access to “good” jobs is not randomly distributed across the population. This problem is exacerbated in a sample of former or current

offenders as this group is especially likely to be unemployed. Heckman (1976, 1979; see also Winship & Mare, 1992) provides a two-step method for correcting for sample selectivity. One first estimates a selectivity coefficient with a model predicting entry into employment. This produces a selectivity coefficient which is then included as a regressor in the second stage of analysis which might predict crime or recidivism. The results of the second stage of the analysis allow researchers to partially control for any observed employment effects by accounting for the fact that not everyone works (e.g., Paternoster et al., 2003; Uggen, 1999; Warren, LePore, & Mare, 2000).

A related method, propensity score matching, also uses a two-step procedure to correct for sample selectivity (Rosenbaum & Rubin, 1983; see also Harding, 2003 for an example on differential selection into neighborhoods and later outcomes and Morgan, 2001 for an example on selection into schools). Analysts first predict entry into employment using demographic information, prior labor market experience, or other expected predictors of employment. Workers and non-workers are then “matched” on the resulting propensity scores (with those who have no close match in the sample dropped from the analysis) and a model of crime is then estimated. Propensity score matching models ensure that, to the extent possible, researchers are making an “apples to apples” comparison of workers with similarly-situated non-workers. Estimated work effects on crime can therefore be more reliably attributed to employment.

Researchers may also use endogenous switching regression models, which estimate the effects of being on one “work track” versus another (Mare & Winship, 1988; Winship & Mare, 1992). For example, offenders may be more likely to work in the secondary labor market (consisting primarily of low-skill, low-wage jobs) relative to the primary labor market (consisting of jobs with higher wages, educational requirements, and more stability relative to the secondary labor market) (Crutchfield & Pitchford, 1997; Western, 2002). A switching regression would reallocate primary sector workers to the secondary sector and re-estimate work effects on crime. An analyst interested in the effect of arrest or criminal punishment on later wages (e.g., Western, 2002) may further suspect that the effect of arrest on wages is unlikely to be the same across these two markets because arrest also substantially predicts in which part of the labor market offenders are likely to work. The endogenous switching regression approach simultaneously predicts the effect of arrest on wages while also accounting for the sector of the labor market each worker is in.

### ***Selection Models for Within-Individual or Within-Area Comparisons***

The methods described above are typically used when comparing across offenders and non-offenders with respect to some other variable, such as work status, job quality, or number of hours worked. An alternative approach often used in conjunction with longitudinal data is a within-person (or within-area) change model.

Within-person change models ask whether (and under what conditions) people are offending during times in which they are working (or not working). Pooled cross-sectional time series designs, such as fixed and random effects models, relate within-individual changes in employment status to crime (or vice versa), while controlling for all stable within-individual characteristics (see Bushway, Brame, & Paternoster, 1999 for a detailed comparison of random and fixed effects models; Paternoster et al., 2003; Uggen & Thompson, 2003). A related method linking between and within-person models is hierarchical linear modeling in which a within-person fixed or random effects models is first estimated. The parameters estimated from the within-person change model may then be used as the dependent variables in the between-person model (Bryk & Raudenbush, 1992; Osgood et al., 1996). Both the pooled cross-sectional time series and hierarchical approaches allow researchers to examine the effects of work on crime while accounting for individual differences in criminal propensity (e.g., Gottfredson & Hirschi, 1990).

## **Results from Longitudinal Studies of Work and Crime**

The next section of the paper reviews research on work and crime using longitudinal data. We focus not only on the presence or absence of employment, but also on the important aspects of employment such as work intensity or hours, work environment, and labor market sector that may be related to crime.

### ***Studies of Adolescents and Young Adults in the General Population***

#### **Work Intensity and Delinquency**

Early criminological theory often suggested that adolescent work experiences would be beneficial by providing income for extracurricular activities, increasing supervision of adolescents, and providing work experiences that would be valuable in adulthood. Empirical research, however, has shown work to be most beneficial to adults (e.g., Sampson & Laub, 1993; Uggen, 2000; Wright, Cullen, & Williams, 2002). For juveniles, a number of studies have found negative effects of work experiences, particularly those described as *intensive* (usually measured as working 20 or more hours per week) (Bachman & Schulenberg, 1993; Staff & Uggen, 2003; Wright & Cullen, 2000; Wright et al., 2002; but see Johnson, 2004 for evidence of race differences in the effect of intensive work). Whereas the adoption of a prosocial identity centered around work may foster desistance in adults (e.g., Matsueda & Heimer, 1997) among adolescents, valuing intensive work roles over school roles often results in decreased educational performance, attainment and aspirations (Bachman & Schulenberg, 1993; Mortimer & Finch, 1986; Steinberg & Cauffman, 1995; Steinberg & Dornbusch, 1991). Too much work at too early an age may also encourage a precocious transition to adult roles (e.g., Hirschi, 1969;

Krohn, Lizotte, & Perez, 1997; Rindfuss, Swicegood, & Rosenfeld, 1987). Though some work experience is in many ways beneficial to adolescents, too much work appears to increase delinquency (D'Amico, 1984; Marsh, 1991; Mortimer & Finch, 1986; Shanahan Shanahan, Finch, Mortimer, & Ryu, 1991; Steinberg & Dornbusch, 1991; Steinberg, Fegley, & Dornbusch, 1993).

Empirical research on adolescent employment and delinquency emphasizes not only the presence or absence of employment and the number of hours worked, but also how work hours are spaced out over time. Mortimer (2003) describes adolescent work in terms of its intensity as well as its duration. Adolescents may engage in work of low duration and low intensity (such as babysitting), low duration and high intensity (for example, a full-time summer job), high duration and low intensity (a regular job less than 20 hours per week), or high duration and high intensity (a regular job more than 20 hours per week). Mortimer's analysis of the effects of work on problem behaviors and alcohol abuse suggests that low intensity work of substantial duration ("steady" work) is most advantageous for a variety of outcomes, as well as the overall transition to adulthood (2003; see also Staff, 2004).

Cross-sectional studies on adolescent work intensity have been subject to many of the criticisms described above. Perhaps adolescents who work intensively are different from adolescents who do not in ways that are systematically related to delinquency (Gottfredson & Hirschi, 1990; Paternoster et al., 2003; Warren et al., 2000). Adolescents who work intensively may be less invested in school to begin with and more likely to engage in delinquency even in the absence of intensive work (Greenberger & Steinberg, 1986). Adolescents experiencing difficulty in other arenas, such as school or family life, may also seek out intensive work. In a 1993 study, Hagan and Wheaton show that youth who are experiencing trouble at home may marry or become parents early as a way of escaping their adolescence. Intensive work may also be sought out as a way to precociously enter adult roles. In short, there are a multitude of reasons to suspect that adolescents who are working intensively are both systematically different from those who are not and more likely to be delinquent even in the absence of work.

Empirical research has established the ways in which intensively working adolescents differ from their peers. Using data from the Monitoring the Future study, a nationally representative survey of high school seniors with annual follow-ups for a subset of respondents, Bachman and Schulenberg (1993) show that intensive work (working 20 or more hours per week) is positively correlated with potentially harmful or delinquent behaviors (smoking, drinking, and drug use, aggression, theft, victimization, trouble with police), even while controlling for prior deviance. Moreover, students with poor educational success are most likely to work intensively later on in high school. Steinberg et al. (1993) also exploit longitudinal data to show that adolescents who work intensively are in fact different from those who do not. Intensive workers were less engaged in school and least supervised by their parents prior to working intensively.

Though research supports a self-selection or propensity component in the effect of intensive work on delinquency (see especially Apel et al., 2007; Apel, Paternoster, Bushway, & Brame, 2006; Paternoster et al., 2003), longitudinal research has also established that working intensively also has an independent

effect on educational attainment, performance, and delinquency, even when prior school difficulty and problems at home are controlled. Ploeger (1997) used the National Youth Survey described earlier and reported that work was associated with a number of delinquent or problem behaviors for adolescents (substance use, alcohol use, and aggression), even after controlling for prior levels of delinquency. A number of other studies using longitudinal data have further clarified the relationship between work intensity and delinquency while controlling for measures of prior deviance and differential selection into work (McMorris & Uggen, 2000; Mortimer, Finch, Ryu, Shanahan, & Call, 1996; Staff & Uggen, 2003; Steinberg & Dornbusch, 1991).

### **Employment Characteristics and Delinquency**

Numerous studies have identified particular characteristics of jobs that might account for the negative impact of work intensity on crime. In general, observers remark on the overall poor quality of adolescent employment opportunities for social capital and skill development (Wright & Cullen, 2004; but see Staff & Uggen, 2003). In addition, Ploeger (1997) notes that adolescents who work are more likely to come into contact with older, delinquent peers at work and are thus exposed to more opportunities for delinquency. Osgood (1999) and Osgood and Anderson (2004) argue from the routine activities perspective that working intensively substantially increases the amount of unstructured time adolescents spend with peers, thereby increasing their opportunities for delinquency.

Recent research has examined the relationship between delinquency and the types of jobs in which adolescents typically work (Shover, 1996; Staff & Uggen, 2003; Wright & Cullen, 2000). Using cross-sectional data, Wright and Cullen (2000) find no relationship between work environment and delinquency but a significant association between adolescent employment, contact with older delinquent peers, and increased delinquency (see also Ploeger, 1997). Similarly, using a prospective, community study of adolescent development (Mortimer, 2003), adjusting for sample selection into work (Heckman, 1976, 1979), and including measures of prior delinquency, Staff and Uggen (2003) found that some types of employment in adolescence reduced delinquency while others appeared to increase it. Potentially problematic jobs are characterized by autonomy, high wages, and status among peers. Better jobs from a delinquency-reduction perspective, are those most compatible with educational obligations, those offering numerous opportunities to learn new skills which could be used in other jobs, and those unlikely to include substantial contact with delinquent peers. A related study using the same community survey found that workers in jobs that fit their long-range career goals are less likely to commit workplace crime, even after controlling for prior acts of workplace deviance and general crime (Huiras et al., 2000). Job quality appears to be important for young adults as well as adolescents. Using lagged dependent models and Heckman-style corrections for sample selection to account for prior crime, work, and background factors, Wadsworth (2006) finds that job quality, more than income, is related to reduced crime.

All in all, the relationship between work and crime is complex for adolescents and many of the complicating questions have been at least partially resolved through the innovative use of longitudinal data. The use of sample selection corrections and inclusion of measures of prior deviance have helped better describe the selection of adolescent into work, work hours, and work environments. In sum, work can be beneficial for adolescents insofar as work hours are moderated, work does not detract from age-appropriate social roles (in school and within the family), and does not include significant associations with older, delinquent peers. Recent work comparing covariate adjustment, lagged dependent variable, and pooled cross-sectional time series models by Paternoster et al. (2003), however, challenges even the formerly secure finding that high work intensity increases crime among adolescents, raising further questions about our abilities to adequately control for selection into work. Moreover, though research supports similar deleterious effects of work for boys and girls (Heimer, 1995), Johnson (2004) finds the positive effect of intensive work on delinquency is most applicable to white youth (see also Newman, 1999).

Paternoster and colleagues examine intensive working with a random and fixed-effect analysis and propensity-score matching (see also Brame, Bushway, Paternoster, & Apel, 2004) and trajectory based (Apel et al., 2007) models. The results of these models suggest that selection into intensive work is responsible for much of the earlier observed relationship between intensive working and crime. In the most rigorous statistical attempt to date to address sample selection issues, Paternoster et al. (2003) use the National Longitudinal Survey of Youth and find no effect of intensive work on dichotomous indicators of substance use and delinquency, net of other relevant covariates (see also Brame et al., 2004). Currently, research on adolescent employment and delinquency leaves a number of open questions regarding the impact of job characteristics, group differences, and selection effects in need of resolution, most likely with longitudinal data and replicated across several data sources.

## *Studies of the General Population*

### **Unemployment and Crime: Aggregate-Level Research**

Beyond work and crime relationships at the individual-level, crime rates are likely to be influenced by labor market conditions and the unemployment rate. Contrary to work effects at the individual-level on adolescent delinquency, aggregate-level research suggests that unemployment is positively associated with crime and delinquency for young adults. Allan and Steffensmeier (1989) show that both unemployment and underemployment of young adults is positively associated with crime (see also Shover, 1996; Sullivan, 1989). Similarly, Crutchfield (1989) shows that an abundance of secondary labor market jobs is associated with higher crime rates. In an analysis of 16 to 24 year-old males, Freeman and Rodgers (1999) show that crime fell in areas with the largest declines in unemployment.

The relationship between crime and macroeconomic conditions is the subject of debate and represents an area in which causal order and process is the source of much disagreement. Criminological theory offers competing predictions on the direction of the relationship between unemployment and crime. Economic choice and opportunity theories suggest that unemployment will cause more crime as financial need rises and potential offenders are unable to meet their needs with income from legal work (Cantor & Land, 1985; Cloward & Ohlin, 1960; Ehrlich, 1973; Greenberg, 1985). Alternatively, routine activities theories (e.g., Cohen & Felson, 1979) suggest that crime may fall during times of economic downturn when more people remain home during the day (reducing home burglaries) and spend less time outside the home engaging in leisure activities at night (reducing their chances of victimization). Additionally, both processes may be operating simultaneously, resulting in no observed relationship between unemployment and crime. Cantor and Land (1985) make just such an argument in that contemporaneous unemployment is likely to decrease opportunities for crime but lagged unemployment is related to increased motivations to commit crime (see also Britt, 1994, 1997).

In a review of available research, Chiricos (1987) reports inconsistent results regarding unemployment and crime (see also Land, Cantor, & Russell, 1995). Few studies found the expected positive relationship between unemployment and crime at the national level. Results are more consistent at lower levels of aggregation, most likely owing to the more homogenous populations in city and county units. In most studies, unemployment is positively related to crime, though it more strongly influenced property crime relative to violent crime. Many analyses using cross-sectional data yield results in which the causal order between unemployment and crime is unclear. Unemployment may cause crime, crime may cause unemployment (Caspi & Moffitt, 1995; Hagan, 1993; Thornberry & Christenson, 1984), or the two may be reciprocally related. In order to more clearly differentiate the temporal order of crime and unemployment, numerous studies have used time series data across a number of geographic areas and included both lagged and contemporaneous measures of unemployment. This strategy is analogous to the within-person change models discussed earlier in that they allow for researchers to control for time-stable characteristics of an area while also establishing the temporal order of crime and unemployment.

Using lagged and contemporaneous measures of unemployment, Britt (1994, 1997) and Cantor and Land (1985) find a negative effect of unemployment on crime but a positive lagged unemployment effect. Employment of poor quality is also positively related to crime. Researchers have also noted a potential ecological fallacy in aggregate studies of unemployment and crime. Though aggregate-level research has demonstrated a positive relationship between lagged unemployment rates and crime rates and a negative relationship between contemporaneous unemployment rates and crime rates, these results do not show that unemployed individuals commit more crime than the employed as a result of economic downturns. Individual-level studies using the Cambridge Study of Delinquent Development (West and Farrington) and the National Longitudinal Survey of Youth (1979) have shown that this is in fact the case. Farrington, Gallagher, Morley, Ledger and West (1986) found increased criminal involvement among young adults during times of unemployment.

Crutchfield and Pitchford (1997) show that youths employed in the secondary labor market are more likely to commit crime relative to those in more high quality, stable jobs. Crime among secondary labor market workers was especially high in areas of high secondary labor market concentration (Crutchfield & Pitchford, 1997). Using both area and individual-level variables, Bellair, Roscigno and McNulty (2003) link greater opportunity in the low-wage labor market to increases in violent crime among adolescents.

The routine activities approach has also been validated at the individual-level. Osgood (1999) and colleagues (1996) use fixed-effect within-person change models to show that young adults who spend relatively large amounts of unstructured time with peers are more likely to engage in crime. Similarly, Fergusson, Horwood, and Woodward use a fixed-effects specification to link spells of unemployment to increases in crime and substance use among young adults (2001). Finally, also using a fixed-effects model, Uggen and Thompson (2003) find a positive effect of local unemployment rates on illegal earnings, but this effect is reduced to non-significance when individual employment characteristics are included in the models.

Overall, results from area studies of macroeconomic conditions and crime suggest that unemployment has a lagged and a contemporaneous effect on crime. Additionally, concentrated employment opportunities of low quality, so-called underemployment, is also associated with increased crime, even when selection into work and other background characteristics are controlled.

## *Ex-Offenders, Current Offenders, and “At-Risk” Populations*

### **Work and Crime Among Former Offenders**

The effect of employment on crime is especially important to practitioners working with ex-offenders or other groups deemed to be at high risk for crime. Offenders with prior criminal histories may commit more crime in the absence of quality, legal employment as they are most likely to possess “criminal capital” (Hagan, 1993). Ex-offenders are most likely to experience short spells of employment, supplemented by short spells of illegal work (Cook, 1975; Fagan, 1995). Offenders are typically less skilled than other workers, less educated, and experience high levels of discrimination in the labor market as a result of their criminal history or race (Pager, 2003). Offenders may earn more from illegal work than legal work for many types of crime (Freeman, 1992, 1997; Freeman & Holzer, 1986; Grogger, 1995; Wilson & Abrahamse, 1992).

The special problems of reentering ex-offenders, the poorly educated, or other at-risk populations has been the focus of much of the experimental research on crime and employment, where results have been decidedly mixed. England’s APEX program and Michigan’s Comprehensive Offender Manpower Program and Transitional Aid Research Project provided job placement and counseling

for ex-offenders and found no difference in recidivism rates across treatment and control groups (Berk, Lenihan, & Rossi, 1980; Soothill, 1974). On the other hand, the National Supported Work Demonstration reported very weak effects of work on crime (Piliavin & Gartner, 1981). Supported Work randomly assigned ex-offenders, ex-addicts, youth dropouts, and AFDC recipients to subsidized employment. A reanalysis of the Supported Work data found that the effects of work on crime were age-graded, with work reducing recidivism only for older participants (Uggen, 2000). Evaluations of the Job Corps program, using random assignment and matched comparison designs, provided intensive job training and placement and reported reduced arrest rates and higher wages for those who completed the program (Cave, Doolittle, Bos, & Toussaint, 1993; Schochet, Burghardt, & Glazerman, 2000).

In a review of experimental evidence on work and crime, Bushway and Reuter (1997) conclude with a point we discussed earlier; providing employment to offenders and at-risk groups works only for some kinds of offenders in some situations. The available evidence suggests that residential job training programs (such as Job Corps) are useful for preventing arrest among high school dropouts and that providing employment opportunities is especially helpful for older offenders (Uggen, 2000). It is also likely that the null effects of employment on crime noted in some experimental and quasi-experimental research designs are related to the types of employment opportunities offered to participants. These jobs are generally of low quality and training programs may not do enough to overcome pre-existing deficits in education, job skills, and work experience to reduce crime to any great degree (Bushway & Reuter, 1997). Using a statistical correction for selection into employment with the Supported Work sample, Uggen (1999) finds that, as with adolescents, jobs of high quality are associated with less crime (see also Crutchfield & Pitchford, 1997; Shover, 1996). Thus job programs for those at-risk for crime (ex-offenders, drug addicts, youth dropouts) may be more successful when they attend to human capital deficiencies and offer a path into high quality employment.

Beyond the deficits restricting the job opportunities of offenders, a substantial research literature has also documented the strong effects of criminal punishment on later employment. Using the National Longitudinal Survey of Youth, a fixed effects model, and a comparison of subgroups, Western (2002) shows that incarceration reduces later earnings and employment opportunities by disrupting connections with potential employers (e.g., Granovetter, 1973; Hagan, 1993). Incarceration reduces human capital because it diminishes work experience. Pager (2003) documents significant labor market discrimination against those with a criminal conviction (see also Bushway, 1998). Punishment may also intensify the forces pushing offenders into unemployment and low quality work and make recidivism more likely. There is some support for the idea that criminal justice interventions may be more effective among offenders with a stable work history. Employed sex offenders may be more likely to respond to treatment (Kruttschnitt, Uggen, & Shelton, 2000) and the impact of arrest in domestic violence cases may be partially dependent on the employment status of the offender (Sherman & Smith, 1992).

## Conclusion

### *Taking Stock: What do we know?*

Longitudinal studies of crime have yielded several empirical generalizations concerning the effects of employment. The longitudinal design has also allowed researchers to control for a number of confounding influences including differential selection effects and prior levels of offending. A number of studies also describe important features of employment that condition the relationship between work and crime. First, employment effects are likely to be age-graded, with intensive work causing disruptions in adolescent development and the provision of a basic job opportunity especially beneficial among older criminal offenders. Second, criminal justice interventions tend to undermine the employment opportunities of those punished even though employed offenders may be most amenable to treatment interventions. Finally, employment quality may be more important for crime reduction than the simple presence or absence of a job, as many of those at high risk for crime are likely to also have substantial opportunities in the illegal labor market open to them.

Despite all the research suggesting work and crime are related and the important methodological complications involved in measuring this relationship, longitudinal data have been underutilized and are often analyzed cross-sectionally. Moreover, the differences across longitudinal studies depending on the varying methods used to correct for sample selection also underscore the need for increased experimental designs that include true random assignment to employment. Loeber and Farrington (this volume) offer a compelling argument and a study design for longitudinal data collections that include experimental treatment evaluations (see also Tonry et al., 1991). They outline numerous threats to validity in non-experimental designs that are only partially overcome by the corrections described in this paper, such as interpreting causal effects that are actually the result of history and maturation and the confounding effects of testing and instrumentation (Tonry et al., 1991: 35–36).

More research that combines a longitudinal design with random assignment is needed because we also suspect that the effects of employment are potentially contingent on other social roles, such as marriage, parenthood, or community involvement (Uggen, Manza, & Behrens, 2004; see also Siennick & Osgood, this volume). Married offenders may have an extra incentive to remain in legal work (Sampson & Laub, 1993), involvement in legal work may also cement bonds between offenders and their children (Edin, Nelson, & Paranal, 2001), and community involvement in conjunction with legal work may enhance prosocial identity development (Maruna, 2001; Matsueda & Heimer, 1997).

Longitudinal research in employment and crime has surely advanced knowledge beyond that available from cross-sectional studies. While few empirical generalizations have been firmly established in the literature, longitudinal studies have helped isolate the areas of greatest consensus and controversy. They have also “raised the bar” for non-experimental designs to more rigorously account for the selection processes that place criminals and non-criminals into different employment statuses.

Further longitudinal analyses can help reveal how changing life circumstances, such as employment characteristics, are linked to changes in crime and recidivism.

### ***Continuing Methodological Challenges, Complexity, and Public Policy Implications***

We began this paper by commenting on the prevailing faith by criminologists, policy makers, and the general public regarding the relationship between work and crime. Our review of scientific evidence on this question suggests that both optimism and caution are warranted. Overall, the research literature demonstrates a complex relationship between work and crime at the aggregate and individual levels of analysis. Work is important for *some* groups, at *particular* life stages, and is more consequential in some areas relative to others – thus *when* and *where* work opportunities occur in the lives of at-risk adolescents, former offenders, or in particular neighborhoods is of most consequence.

We note that one of the most firmly established findings in the area, the positive relationship between work intensity and delinquency, has recently come under challenge from analysts using methods that elegantly account for the selection of young people into jobs (e.g., Apel et al., 2006; Paternoster et al., 2003). Recent research also suggests that analysts in the future ought to pay greater attention to the attributes of employment opportunities for adolescents as opposed to merely the presence and amount of work. Employment that is compatible with adolescents' school roles and career aspirations may be useful, even if those jobs require significant investments of time. Overall, the research literature on adolescent employment and delinquency suggests that hard and fast limits on the availability of employment or hours worked per week may be too simplistic.

The life course perspective and research on those returning to the community from correctional supervision also result in more complexity in the relationship between work and desistance from crime. This literature demonstrates the particular responsiveness of older offenders to employment opportunities, even those of relatively low quality. Unfortunately, many job programs currently have age limits that restrict program participation to those under the age of twenty-five (for example, Job Corps).

Providing employment opportunities for ex-offenders is no panacea, however, and evaluation of job programs for former inmates suggests that policy makers may need to re-conceptualize the definition of program success and lower their expectations for what work can (and cannot do). For example, the National Supported Work Demonstration reduced crime among those who received jobs, but did nothing to reduce substance abuse. As we have demonstrated in this paper, employment is more than a mere job. Beyond income, work connects adolescents to their peers (both delinquent and "straight"), offers informal social networks that may conflict with crime, and provides ex-offenders with pro-social roles. All of these aspects of work may reduce crime among ex-offenders; at the same time, employment also provides an income with which to sustain prior substance abuse.

The complexities described above are particularly important to consider given that employment represents one of the few areas in which governments, schools, prisons, or communities are able to intervene significantly in the lives of potential and current offenders. For example, while we cannot provide individuals with spouses, we are able to increase the chances that they will become employed after leaving prison by enhancing their skills through expanded educational or training initiatives in prison. Such policies would represent a shift in current focus, but would also represent a significant cost to the public. Solid policy guidance from longitudinal studies is sorely needed in an era of increasing imprisonment rates. While we believe that the literature reviewed above supports the idea that employment can reduce crime, it also suggests that the relationship is quite complex, varying across time, space, and individuals. In light of the high crime and imprisonment rates in the United States, the salience of employment to criminal offenders, and the continuing political viability of jobs programs, further investment in longitudinal and experimental research on the relationship between work and crime is clearly merited.

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**Appendix** Key findings by study and method

Study (place, time, n)	Age, race, gender	Cites	Method	Findings
1 National Longitudinal Survey of Youth (NLSY) 1997	12–16 in Wave 1; white, black, Hispanic; male and female	Apel et al. (2006)	Random effects Poisson	Intensive work increases delinquency and drug abuse but transition to intensive work does not lead to an increase in either.
		Wadsworth (2006)	Lagged dependent variable models, Heckman-style selection correction	Quality of job has stronger influence on economic and noneconomic crime than income, job stability, education, and other background factors.
		Patemoster et al. (2003)	Random-effect probit, fixed-effect logit regressions, propensity-score matching model	No effect of intensive work on dichotomous indicators of substance use and delinquency; selection into intensive work is responsible for much of relationship between intensive work & crime
		Western (2002)	Fixed effects model, comparison of subgroups	Incarceration reduces later earnings & employment opportunities via disrupting connections with potential employers
		Wright et al. (2002)	OLS regression, path models	Work most beneficial to adults, potentially detrimental to youth
		Crutchfield and Pitchford (1997)	OLS regression	High quality, stable jobs = less crime

2	National Youth Survey (NYS); n=1,700; 1976-	Adolescents now 39-45 yrs. old	Ploeger (1997)	OLS regression	Pos. rel. btwn work & crime, esp. w/drugs & alcohol, selection bias
3	Youth Development Study (YDS); St. Paul, MN; n = 1,000; 1988-	African-American, Hispanic, White; high school seniors	Bushway (1998)  Staff and Uggen (2003)	Differences in differences  Regression w/statistical controls, lagged dependent variables, other indicators of prior deviance, hazard rate for selection to employment based on a Heckman sample selectivity model	Arrest can lead to minor problems in labor market beyond current or past crimes  Some types of employment in adolescence decrease delinq. while others increase it
	Huiras et al. (2000)		Static score regression	Career stakes & job satisfaction exert independent effects even when prior deviance statistically controlled	
	McMorris and Uggen (2000)		Static score regression models	Long work hours increase adolescent drinking	
	Mortimer (2003)		Regression analyses; qualitative interviews	Low intensity, steady work is most advantageous	
	Mortimer et al. (1996)		Probit, LIMDEP	High intensity work increases adolescent drinking	

(continued)

## Appendix (continued)

	Study (place, time, n)	Age, race, gender	Cites	Method	Findings
4	Gluecks; n = 500; Boston, MA; 1930-	Boys 7-32, 7-70 white males	Sampson and Laub (1993)	Regression, probability models, logistic regression, event history analysis	Crime and work are related to the extent that work exerts social control over potential offenders and creates prosocial bonds for young adults.
5	Cambridge Study in Delinquent Development, n = 411; London; 1961-1981	Boys 8-46; white males	Farrington et al. (1986)	Life history interviews; hierarchical statistical models of change (Poisson)	Bonds to work encourage desistence among adults (informal social control)
6	Philadelphia birth cohort 1945; n = 9,945	Men born in 1945	Hagan (1993)	Poisson, GLIM	Crime rates higher during periods of unemployment in subjects' lives
7	Monitoring the Future; n = 70,000; U.S.; 1985-1989	High school seniors	Wolfgang et al. (1972)	Poisson regression, logit models, path models	Criminal embeddedness influences later unemployment
			Thomberry and Christensen (1984)	Probability models; matrix tests	Positive relationship between unemployment & arrest
			Bachman and Schulenberg (1993)	Reciprocal causal model	Employment & crime mutually influence each other over life span
			Osgood et al. (1996)	Multiple classification analysis	Intensive work (20+ hrs/wk) positively correlated with potentially harmful or delinq. behavior
				Fixed effects panel model	Unstructured social activities increase crime, drug/alcohol use, dangerous driving

8	National Supported Work Demonstration; 1975-1977; n = 3,000+	Mean 25 yrs old, 92% male, 76% African-American	Piliavin and Gartner (1981) Uggen (2000)	Regression, probability models, Tobit models Event history analysis	Weak effects of work on crime for older offenders Significant effects of work only for older offenders
			Uggen and Thompson (2003)	Pooled cross sectional time series analysis	Criminal earnings are sensitive to embeddedness in conforming work and family relationships, criminal experience, and the perceived risks and rewards of crime
9	National Longitudinal Study of Adolescent Health; n = 90,000; U.S.; 1994-1996, 2001-2002	White, African-American, Cuban, Chinese	Uggen (1999) Johnson (2004)	Two-equation probit models Conditional change models	High job quality decreases likelihood of criminal behavior among group of high risk offenders Effect of intensive work on delinquency is most applicable to white youth
10	Wisconsin & N. California high schools; n = 1800; 1987-1989	10 and 11th grade students; male and female; White, African-American, Asian, Hispanic	Bellair et al. (2003) Steinberg et al. (1993)	Logistic regression, HLM MANOVA, ANCOVA	Low-wage, service sector employment opportunity increases likelihood of violent delinquency. Somewhat mediated by school achievement and attachment Intensive workers (20+ hrs/wk) are less engaged in school and less supervised by parents prior to working intensively

(continued)

## Appendix (continued)

	Study (place, time, n)	Age, race, gender	Cites	Method	Findings
11	Dunedin multidisciplinary health and human development study, n = 1,000, 1972	13-, whites, males and females	Caspi and Moffitt (1995)	Various categorical and continuous statistical approaches	Unemployment may cause crime, crime may cause unemployment
12	Bremen, Germany, n = 1660, 1989	9th–10th grade students, males and females	Caspi et al. (1998)	Life History Calendar; tobit models	Failure to account for prior social, psychological, and economic risk factors may lead to inflated estimates of the effects of unemployment on future outcomes
13	Denver Youth Study, n = 1528, 1988	Ages 7–15, White, African-American, Hispanic, males and females	Huizinga, Schumann, Ehret and Elliott (2003)	Cross tabs, logistic regression, odds ratios	Juvenile sanctions are related to reduced chances for stable, skilled jobs
13	Denver Youth Study, n = 1528, 1988	Ages 7–15, White, African-American, Hispanic, males and females	Huizinga et al. (2003)	Cross tabs, logistic regression, odds ratios	Juvenile sanctions are related to increased unemployment
14	Dutch national crime survey (NSCR), n = 2,951 & Criminal Career and Life-Course Study, n=4,615, 1977	12–72, 15–30	Blokland and Nieuwbeerta (2005)	Semi-parametric group-based models	Life circumstances (including employment) influence criminal behavior but differ across offender groups

15	Christchurch Health and Development Study, n = 1265, 1977-	12-21, white males and females	Fergusson et al. (2001)	Fixed effects regression	Exposure to unemployment significantly associated with increased risks in crime and substance use
16	Rochester Youth Development Study, n = 1000, 1987	13.5-22 years old, males, females, white, African-American, Hispanic	Bernburg and Krohn (2003)	Logistic regression	Police and juvenile justice interventions positively and significantly affect periods of unemployment in adulthood; educational attainment mediates some of the effect

# The Effect of Arrest and Justice System Sanctions on Subsequent Behavior: Findings from Longitudinal and Other Studies

David Huizinga and Kimberly L. Henry

## Introduction

The purpose of this chapter is to summarize what is known about the effect of arrest on subsequent behavior, especially delinquent behavior, and what prospective longitudinal studies have added to this knowledge base. In addition, we examine what longitudinal studies have found about the effect of sanctions following arrest. In so doing, what policy relevant actions might be justified on the basis of current knowledge about the effects of arrest and sanctions and the opportunities for longitudinal studies to provide additional information about these effects are considered. The emphasis of the chapter is thus on what is called “specific deterrence,” how arrest and sanctions do or do not deter future criminality of apprehended individuals. However, offenders are also affected or controlled by the attitudes and behavior of the general population, which provide the context in which the effect of a specific arrest occurs. Thus, for completeness, a brief summary of information about the influence of arrests and sanctions on the population in general or “general deterrence” is also provided.

Because a focus of this chapter is on longitudinal studies, it is necessary to describe what kind of studies are included in this group. For the purpose here, longitudinal studies will refer to prospective multiple year studies that cover major portions of the life-course and that are based on probability samples of some specified general population. They are thus distinguished from most evaluation or experimental studies. The distinction being that evaluation studies (related to the effect of arrest and/or sanctions) are generally not based on probability samples of a population, but rather on assignment of targeted subsets of individuals to treatment and control groups; and, although sometimes longitudinal in design, are also often of short term duration of one, two or a few years. The longitudinal studies are also distinguished from studies based on other purposive samples or on studies that have a retrospective data collection design. Of particular importance, the review of the effect of sanctions following arrest provided here is generally limited to findings from longitudinal studies in general populations. There is a large body of literature on the effect of sanctions using samples of arrested or adjudicated youth, and a review of this literature is beyond the scope of this chapter.

It is common in describing the influence of arrest and sanctions on apprehended individuals to consider two opposing views. The first is a deterrence or punishment orientation that suggests that arrest should reduce or eliminate future offending behavior. The rationale for this view is that being arrested should indicate to the individual that the behavior is socially disapproved and sanctioned and that the arrested individual will perceive an increased risk of arrest and sanction for future engagement in the offense (and by generalization to other delinquent/criminal acts, an increased risk for these as well). This realization of increased perceived probabilities of being apprehended (and in some cases further sanctioned) is presumed to reduce future offending (see e.g., Tittle, 1969). The second view is that arrest may or will increase subsequent offending behavior by resulting in the official labeling of the arrested individuals as bad or delinquent. Such labeling may result in stigmatization and limited access to desirable roles and normal developmental opportunities and thus result in greater involvement in deviant roles that are still open to them. Such labeling may also result in changes in self-identity or self-concept so that arrestees adopt a more deviant view of themselves. Either or both of these outcomes in conjunction would be expected to increase, rather than decrease, future offending behavior (see e.g., Klein, 1986).

There are thus quite different theoretical positions on what the effect of arrest on subsequent behavior might be. However, it is also possible that there is no real effect of arrest or sanctions and that neither of these two alternative theories actually apply in most cases. The motivation and impetus to commit crime/delinquency may be embedded in fun or excitement, economic or other needs, social prestige and peer support for the behavior at the time, as well as other factors and may not depend on reflection about the potential for apprehension (cf. Eskridge, 1983). Or, even if such reflection occurs, the risk of apprehension may be outweighed by the reward and may be minimized by careful actions of the individual. In addition, in the lives of some subsets of youth, arrest is more normative and may be seen as a "rite of passage" and a common part of growing up, and arrest (and even incarceration) may not be seen as a great punishment, so that labeling, at least among friends and family, does not occur. Similarly, offenders who are arrested may obtain a more accurate perception of the probability of arrest for criminal/delinquent acts, which is usually lower than that held by the public at large. They also may learn about the operation of the justice system, with the result that beliefs about "how bad" arrest and sanctions might be become lowered and orientations and attitudes favorable to a violation of the law may develop. As illustrated by these differing orientations and rationales, views that arrest and sanctions should decrease, increase, or have little effect on subsequent crime/delinquency are all plausible.

There are several issues that surround an investigation of the effect of arrest. First, it is difficult to disentangle the effect of arrest from the effect of sanctions that may follow arrest. Some individuals are contacted by the police for a delinquent/criminal offense and released. Others are formally arrested and an arrest record created, but are informally handled by the police (e.g., lectured and released, or, if a juvenile, released to a guardian). Others may be "referred to court" where, based on recommendations of the district attorney, prosecutor, or probation department, the charges may be dropped and the individual released or the individual may be held or required

to appear in court. For this latter group, a wide variety of sanctions may be applied including, for example, dismissal, fines, community service, restitution, attendance in treatment programs, or incarceration. Given the wide range of potential outcomes from an arrest, it seems reasonable to suppose that the effect of an arrest might vary depending on the level of sanction that follows arrest.

The police and district attorney/prosecutor/probation department have substantial discretion regarding whether to forward a case within the justice system so that more severe sanctions might be applied. In addition, there are offenders who avoid detection, or at least avoid apprehension. Thus, to more appropriately assess the effect of arrest, it becomes necessary to include within the sample under study individuals who offend but have no police contact, individuals who offend and have police contact/arrest but are lectured and released, individuals who are referred to court by the police but whose cases are dismissed, and individuals who are referred to court and receive some sanction. Similar thoughts about the need to include individuals who are not punished in examinations of specific deterrence have a long history (e.g., Gibbs, 1975; Tittle & Logan, 1973). In addition, although diversion from juvenile court is no longer particularly popular (at least in the U.S.), the potential for diversion to alternative programs at either the police or court level remains as an additional processing alternative or sanction.

Given the above, three observations seem warranted. First, the question—does arrest have an effect on subsequent behavior, regardless of additional sanctions that may follow—is not an unreasonable question. Some offenders only experience police contact or arrest and are then released, so that the influence of arrest on their behavior would be missed if only those referred on in the justice system were included in an examination of the impact of arrest. Also, it may be that it is the certainty of arrest and not the severity of following sanctions that produces an effect of an arrest. Conceivably, arrest by itself may act as a deterrent to or act to amplify future delinquency/crime. (It is noted that a study that could separate the effect of simple arrest and the effect of arrest followed by different sanctions might be more informative.) Second, for similar reasons, an answer to the effect of arrest question can not be obtained by examinations of different court sanctions or levels of punishment. However, only a few studies have addressed the impact of arrest while many studies have addressed the impact of a variety of court imposed sanctions or treatments. In fact, the number of such intervention studies is sufficiently large to permit large scale meta-analyses of these studies (see e.g., Aos, Phipps, Barnoski, & Lieb, 2001; Lipsey & Wilson, 1998). Third, in the design of experimental studies of treatment efficacy, the existence of a true “no-treatment” control group is of importance (see e.g., Dunford, 2000), and may be especially so for policy implications. Similarly, the existence of a non-arrested group of offenders is important to an examination of the impact of arrest. That is, it is important that the subsequent behavior of arrestees be compared with behavior of similar offenders who are not arrested, so that the influence of arrest can be more adequately determined.

In considering the existing literature, it is also important to remember the changes in juvenile court practices over the last half century or so (for descriptions of these changes see Howell, 1997). The sanctions applied by the court from the 1960s or early 70s to those applied in the late 1980s and 1990s have changed, becoming more

severe. Thus, to the extent that the effect of arrest depends on later court sanctions, the effect of arrest *may* differ over these historical periods. Comparison of findings across these time periods must be made carefully.

In examining the effect of arrest and sanctions on subsequent delinquent/criminal behavior, it is necessary to select a measure of delinquent/criminal behavior to be used as an indicator of subsequent behavior. The two measures that have been used for this purpose are self-reported delinquency/crime and official (or self-reported) arrest information. Some studies of the impact of arrest have used official arrest data, with outcomes measuring the number of re-arrests, time to next arrest, seriousness of subsequent arrests, etc. Many of these follow a criminal career parameterization. A limitation of these studies, as indicated above, is that the future offending of those offenders who are never apprehended (or whose apprehension is not recorded) are excluded from consideration. In addition, while it is useful to know whether an arrest reduces the number of future arrests or results in never being arrested again, this unfortunately does not necessarily imply the reduction or cessation of offending behavior. This is not a new observation; the level of unrecorded or hidden crime underlies the development of self-report measures over 40 years ago (Short & Nye, 1958).

The effect of arrest and sanctions may also vary by type of offender. Not all individuals commit delinquencies/crime for the same reason. Offenders vary by age, by gender, by stage in a delinquent career, and level of seriousness and frequency of offending. Thus, the effect of an arrest might vary between first time arrestees who are low level offenders and first time arrestees who are "seasoned" serious offenders; or between individuals first arrested at age 10 and those first arrested at age 17, and so on. Arrest is also not evenly distributed in the population and its effect may be different in localities or neighborhoods where it is rare than where it is commonplace.

In considering the contribution of the longitudinal studies to knowledge about the effect of arrest and sanctions on subsequent delinquent/criminal behavior, it is interesting to note that there appears to be some compartmentalization of criminological research. One group of studies emphasizes individual development and prevention, often in social and environmental context, and another group of studies focuses on special populations, such as studies of sanctions and treatment of adjudicated or convicted persons. The former often examine personal and social characteristics that lead to arrest, but do not often consider arrest as a risk factor for future offending. The latter begin with those already caught-up in the justice system, attempting to identify strategies that will best ameliorate their illegal behavior. In addition, most of the developmental/prevention oriented studies focus on childhood and adolescence while the focus of the treatment oriented studies, although oriented toward adolescence, often involve adults as well. Rarely is there an examination in a developmental perspective that includes early delinquency/crime, arrest, justice system sanctioning, subsequent delinquency/crime and longer-term outcomes over different phases of the life course. This is, perhaps, one of the greatest potentials of the longitudinal studies.

In the following, we provide a review of what has been learned from longitudinal and some other studies about the influence of arrest and sanctions on subsequent

delinquent behavior, and about the influence of arrest on other factors that may be related to subsequent offending. In a final section we offer suggestions for policy and suggestions for the use of existing and future longitudinal studies in providing useful information about this topic. We begin with a brief summary of information about the influence of arrest on general deterrence and then examine the influence of arrest on individual offenders.

## **The Effect of Arrest and Sanctions on Subsequent Delinquent and Criminal Behavior: A Review of the Literature**

Legal sanctions, including arrest, may have both a general deterrent effect and a specific deterrent effect on criminal behavior. As a general deterrent, it is hypothesized that more aggressive policing, employing additional police, improving clearance rates, and/or creating more severe penalties for crime will generally deter people from committing crime. As a specific deterrent, it is hypothesized that arresting and placing sanctions on an individual will serve as a punishment that will ultimately deter the individual from engaging in future crime. Of course, in the most severe cases, arrest and subsequent incarceration may reduce future criminality by means of incapacitation. In the next two sections, we will review studies that have examined general and specific deterrent effects of arrest and subsequent sanctioning on delinquent and criminal outcomes. A final section provides a description of studies that have examined the impact of arrest on other factors that are assumed or have been shown to affect subsequent offending.

### ***The General Deterrent Effect of Arrest***

Empirical research on deterrence over the past 30 years or so, began with community, state, or national studies, using aggregate level data, to examine the relationship between (1) the proportion of crimes known to the police that resulted in arrest or other sanctions and (2) official crime rates (e.g., Chiricos & Waldo, 1970; Gibbs, 1968; Kobrin, Lubeck, Hansen, & Yeaman, 1972; Tittle, 1969; Tittle & Rowe, 1974). Early studies of general deterrence focused on homicide (Gibbs, 1968) and felony offenses (Tittle, 1969). These studies found an inverse relationship between the certainty and severity of punishment and subsequent homicide and felony offenses. Interestingly, however, in the Tittle study, only severity was found to be inversely related to homicide, and for other felonies severity was significant only when accompanied by higher levels of certainty. As Kobrin, et al. (1972) noted, however, the measures used in these studies were limited by the well-known problems inherent in official data (crimes known to the police are poor representations of the amount of crime) and the use of prison admissions and sentence length as measurements of certainty and severity, respectively, is quite

limited. Inclusions of the effect of other responses of the justice system such as the effect of arrest and adjudicative action are needed.

In an extensive examination of general deterrence within the State of California, Kobrin, et al. (1972) made use of the more detailed official data available for that state. This data permitted the expansion of the notion of punishment to include the entire range of criminal justice response. They developed the notion of a "sanction pattern" that measures varying levels of sanction within each of the various stages of criminal justice process—police/arrest, pretrial, conviction, and sentencing. The findings from this study of felony offenses indicated that there was a great deal of diversity among jurisdictions in their sanction patterns. However, in general, both certainty and severity, measured as an entire justice system response, were inversely associated with crime rates. More detailed analyses, however, revealed that the magnitude and components of these deterrent effects varied by population size. For larger counties (over 500,000 in population) both the police and sentencing were important contributors. For smaller counties (25,000 to 500,000 in population) police action was the overwhelmingly responsible contributor, and in counties with populations less than 25,000 police action was not significant. Given the important role of the police in counties over 25,000 (the majority of the population), it is quite interesting that between their role of apprehending suspects and their role in procuring evidence to support a decision to charge, it was the gathering of evidence ("good pinch" scores) that had the greatest effect on reduced crime rates. We have described the Kobrin et al. study in some detail because it is the first deterrence study that was expanded to include the effect of arrest and because of its suggestion to develop a system wide perspective on deterrence, a strategy that might be applied in specific deterrence studies as well.

In a review of studies focusing on the general deterrence of arrest, Sherman et al. (1997) notes that some evidence suggests that, at the community level, there is "a threshold beyond which the effect of increased arrest rates becomes evident, while no such effect is apparent below the tipping point of a minimum dosage level." He points to Tittle and Rowe's (1974) study of the relationship between probability of being arrested for a crime (as measured by arrest clearance rates—i.e., percentage of reported crimes for which the police believe that they have arrested the perpetrator) and actual crime rates in cities and counties in Florida. Their study suggested that the probability of arrest must exceed a critical level (estimated at .30), referred to as a tipping effect, before a beneficial impact on crime may be observed. However, a follow-up of these same data and of additional data from California indicated that the effect of probability of arrest on crime rates in both states was only observed in cities and counties with a population of less than 10,000 people (Brown, 1978). Chamlin's (1991) data confirms this observation; he also found that the effect of a tipping point is only observed in smaller cities.

Other studies that used national samples and appropriately controlled for relevant covariates (e.g., social, economic, and demographic variables) have also failed to find a substantial effect of clearance rates on crime in cities with larger populations (Greenberg & Kessler, 1982; Greenberg, Kessler, & Logan, 1979). However, some exceptions exist. In one study reported by Cloniger and Sartorius (1979) the effect of clearance rates on homicide and auto theft in Houston, Texas was assessed. Their

analyses indicated that both of these offenses were negatively correlated with their respective clearance rates. In a second study reported by Chamlin (1988), more arrests were associated with a reduction in robberies, but not property crimes.

The evidence concerning the effect of arrest on general deterrence is thus mixed. Some studies find an effect, others do not. And, there is evidence that the effect may vary by size of community and also some evidence that the effect may vary by type of crime. Most of the literature reviewed above is based on index crimes or felony crimes. In addition, evidence provided by Logan (1975) and Decker and Kohfeld (1985) warns that high crime rates may lower the arrest rate (rather than the reverse).

### *The Specific Deterrent Effect of Arrest*

Many studies have assessed arrest as a specific deterrent. The research in this area has also resulted in mixed conclusions, although most of the studies have reported a non-significant or even harmful impact of arrest. Many of these studies have compared the subsequent offending and/or arrest history of individuals who were arrested (and potentially further sanctioned) to similar individuals who were never apprehended for their crimes. Other studies have compared various forms of sanctioning following arrest to determine if different types of punishment have different effects. The impact of arrest is considered first followed by the consideration of the effects of other sanctions.

One of the earliest studies was conducted by Gold and Williams (1970). Using a sample from Michigan, a small number of adolescents who were charged with an offense and not charged with an offense were matched on personal characteristics, offenses, and a variety of pertinent variables to increase the likelihood that subsequent differences were due largely to the effect of the charge. Over time, the adolescents who were charged with an offense were more likely to offend again.

Farrington (1977) examined the effect of conviction among a sample of working-class boys in London (the Cambridge Study in Delinquent Development). He reported that youth first convicted prior to age 14 and those first convicted between ages 14 and 18 had higher delinquency scores at age 18 than did similar non-convicted youth. His data also suggested that among boys matched on prior self-reported delinquency, those with a previous finding of guilt were more likely to be re-convicted. Structural equation models of the London data have also found a positive relationship between conviction and subsequent delinquency. De Li (1999) in a study of status achievement found that conviction at ages 10–13 was related to delinquency at ages 14–15 and that conviction at ages 14–16 was related to delinquency at ages 18–19. Although these studies focus on conviction instead of arrest, we include them here because they are based on the full sample (not just those convicted), the conviction indicates that the individual is most likely guilty of the offense charged at arrest, and consideration of further sanctioning is not involved.

Based on a large longitudinal study of police apprehension and subsequent delinquency using an in-school sample, Thomas (1977) reported on the impact

of apprehension in 1974–75 on delinquency in 1975–76. A regression model controlling for prior delinquency, indicated that prior police apprehension was only weakly predictive of subsequent delinquency and added less than one percent to the explained variance, thus suggesting little relationship between police intervention and reductions in subsequent delinquency, but it must be noted that the effect was negative, i.e., apprehension reduced delinquency but the reduction was so relatively small, we classified this finding as being most similar to no effect.

Paternoster (1978) reported that the simple correlation between police apprehension and subsequent delinquency (from time one to time two) was 0.33, indicating that that higher subsequent involvement in delinquency was associated with a prior police contact. In a stepwise multiple regression analysis in which age, race, gender, delinquent self-image, and other control predictors were included first, apprehension by police continued to have a significant influence on subsequent delinquency. Paternoster also found that there was a difference in this relationship by social class, with the relationship being strongest for lower-class youth.

In another study, Huizinga, Elliott and Dunford (1986) examined the prevalence of arrest within different types of delinquents among youth in the National Youth Survey. As would be anticipated, they found that as seriousness and frequency of offending went up, the probability of arrest also increased. However, among serious delinquents only 10% were arrested in any year, and among serious career offenders—defined as having at least two years of continuous involvement—less than 25% were arrested. Additional analyses examined the effect of arrest on subsequent delinquency. Arrestees and controls were matched on age, gender, race, socioeconomic family status, and delinquency participation in the two years preceding arrest. They found that there were no significant differences between the arrestees and their controls in mean delinquency or mean index offending (UCR Part I offenses) in the year preceding arrest, the year of arrest, and the year following arrest. Use of a delinquency typology (non-delinquent, exploratory delinquent, non-serious delinquent, serious delinquent) also revealed that over 80% of the arrestees either increased or maintained the same level of delinquent involvement as their non-arrested matched control in the year of arrest and in the two subsequent years following the year of arrest.

Klein (1986) conducted the only true experimental test of the effect of arrest on subsequent delinquency. A sample of 306 adolescents who were apprehended by police in nine different stations in California was identified. All of the adolescents were considered by the police department as referable to further juvenile justice system processing. However, the 306 adolescents were randomly assigned to one of the following four conditions: counsel and release, referral, referral with purchase of service, and petition request. Following randomization, the adolescents were interviewed about their delinquency after nine months and re-arrest was determined after six, fifteen, and twenty-seven months. The results of the study indicated that referral to community agencies and petitioning toward juvenile court led to a greater probability of recidivism. That is, adolescents who were counseled and released were less likely to be rearrested during the observation period than adolescents who were either referred or petitioned. It is important and interesting to note

that no significant differences existed between the four conditions in rate of actual self-report offending. That is, the effect of juvenile justice system processing didn't result in involvement in more crime. Rather it resulted in a higher likelihood of being rearrested for involvement in crime.

Several investigations of the effect of legal sanctions on subsequent delinquent behavior have been conducted using data from the Denver Youth Survey, a multi-year longitudinal study of individuals who grew up in high-risk, inner-city neighborhoods in Denver, CO. Based on data from 1989, Huizinga and Esbensen (1992) reported that arrest did not lead to decreased involvement in crime. In more detailed analyses, Huizinga, Esbensen, and Weiher (1996) matched each adolescent who had experienced their first arrest with a similar control individual who had never been arrested. The adolescents were matched with regard to age, sex, ethnicity, type of neighborhood, attitude towards delinquency, peer delinquency, and prior delinquent involvement. For all types of delinquency (status, minor, and serious offenses), the delinquency of most of the arrestees was either equal to or greater than their matched control during the year after the arrestees' first arrest. They concluded that arrest and subsequent sanctioning had little effect and potentially even a harmful effect for most adolescents. In an examination of adolescent precursors to adult incarceration, Huizinga, Weiher, Espiritu, and Esbensen (2003b) also reported that, among male serious offenders, arrest as a juvenile was related to incarceration for a criminal offense as an adult and that the effect was even larger for juveniles who were arrested and incarcerated.

In a cross-national study, Huizinga, Schumann, Ehret, and Elliott (2003a) compared the effect of juvenile justice system processing on subsequent delinquent and criminal behavior among the youth of the Denver Youth Survey and the youth of the School-to-Work Study in Bremen, Germany (a high-risk sample of German youth followed longitudinally throughout adolescence and into young adulthood). The primary purpose of the comparative study was to examine similarities and difference in the two juvenile justice systems and to draw conclusions about the effects of the systems on behavior. The study was particularly interesting because of the contrasting systems. Bremen is oriented toward a more lenient, diversion focused system, while Denver's juvenile justice system takes a more punishment oriented approach. Based on a comprehensive set of analyses (that included matched-controls similar to that described above), Huizinga and colleagues concluded that in both cities arrest had little effect on subsequent delinquent and criminal behavior. Furthermore, all significant effects indicated that arrest resulted in sustaining or elevating the offending adolescent's future delinquent behavior. The study also reported that the level of sanctioning received (not arrested, dismissed/diverted, or serious sanction) had very little influence on future offending.

Another recent large cross-national longitudinal study, involving a sample from multiple schools in Victoria, Australia and Washington State in the U.S. was conducted by Hemphill, Toumbourou, Herrenkohl, McMorris, and Catalano (2006). Approximately 4000 students in the 5th, 7th, and 9th grades in both countries completed in-school questionnaires that included information about delinquency, school suspension, arrest, and a large number of risk and (appropriately defined) protective factors. Two school surveys were conducted one year apart. Using a variety measure

of delinquency (count of different kinds of offenses committed) they defined antisocial youth as those who committed two or more offenses. Using youths from the 7th and 9th grades in the first year, they examined their antisocial behavior in the second year using a logistic regression. They found that individually and, when considered jointly, both arrest and school suspension led to increases in levels of delinquency in the following year. However, in a large regression model that controlled for 29 other variables including gender, prior delinquency, other individual risk and protective factors, and risk and protective factors from the family, school, peer, and community domains, as well as country, they found that although school suspension still significantly contributed to an increase in delinquency, the effect of arrest was no longer significant (although being positively related to year two delinquency). It should also be noted that country was not a significant predictor of second year delinquency. The researchers concluded that the main finding from this study was that school suspensions increase subsequent delinquency.

Paternoster and Piquero (1995) analyzed data from high school students in the U.S. The students completed a survey in 10th grade and 11th grade. After controlling for prior use of drugs, prior involvement in other forms of delinquency, and other pertinent covariates, students who had been recently sanctioned for alcohol or marijuana use in 10th grade reported higher levels of alcohol and marijuana use in 11th grade as compared to students who were not sanctioned.

Kaplan and Damphouse (1997) used the early three years of a major longitudinal study (3,148 students in grade seven of the Houston Independent School District in 1971, who were followed in grades eight and nine) to examine the effect of social sanctions on subsequent delinquency. The measure of social sanctions combined contact with law enforcement personnel, being suspended or expelled from school, and being taken to the principal's office for punishment. Although the sanctions considered go beyond contact and arrest, the findings are included here because they come from a longitudinal study and involve an examination of the deterrence and labeling perspectives of a social sanction measure that includes arrest. Controlling for gender, race, and socioeconomic standing of the family, together with prior delinquency in a regression model, Kaplan and Damphouse found that higher levels of social sanctioning resulted in higher levels of subsequent delinquency. This study also noted that the magnitude of the effect of social sanctioning on delinquency varied by level of self-derogation, a characteristic akin to self-esteem. The effect of sanctions on subsequent delinquent behavior was reduced but still significant for those with higher levels of self derogation.

Using data from the Rochester Youth Development Study, a long-term longitudinal study in Rochester, New York, Bernburg and Krohn (2003) examined the effect of arrest (including recorded police contact) and juvenile justice intervention during adolescence on involvement in crime during adulthood among males of this study. Controlling for adolescent delinquency, race, family poverty, and educational capabilities, they found that both police arrest/contact and subsequent juvenile justice system intervention increased the level of serious crime at ages 19–20 and the level of general crime at ages 21–22, thus indicating longer term effects of juvenile arrests and sanctions. However, these effects were diminished if the individual completed high school.

The Edinburgh Study of Youth Transitions and Crime is a large multi-year longitudinal study of youth development in Edinburgh, Scotland. Controlling for prior delinquency and other relevant variables in a regression model, findings from this study indicated that as the number of offense types known to the police in one year increased, the level of delinquency reported during that year and the next year also increased (Smith, 2006). This study also found that young people caught by the police were more likely to persist in offending than those who offended at the same level but were not caught. As noted by Smith, a particularly striking finding is that the chances a juvenile will stop offending altogether are sharply reduced by contact with the police. Thus, the findings suggest that a policy of increased intervention by the juvenile justice system is unlikely to result in a reduction in juvenile offending.

Additional findings from the Edinburgh Study are provided by McAra and McVie (2007). Using propensity matched intervention and control groups, they found that between the group of youth for whom there was a "police decision to charge" (similar to arrest in the U.S.) and a matched control group there was no significant difference in the prevalence or frequency of subsequent serious delinquency in the year following the "arrest." Similarly, between the group of youth for whom their was a Reporter referral (similar to referral to court in the U.S) and a corresponding control group who were not referred, there was also no significant difference in the prevalence or frequency of subsequent serious delinquency in the year following the referral.

Smith and Gartin (1989) reported one of the few studies that found a deterrent effect of arrest. They used official police contact data from a sample of 325 male, Wisconsin residents born in 1949, who were followed until their 25th birthday. Independent of the effect of seriousness of the current offense, the number of prior arrests and index contacts, the prior rate of offending, and the remaining time at risk, arrest had a consistently negative effect on the number of future police contacts. However, the number of prior police contacts also appeared to matter, "... on the first through the third contacts, approximately 30 percent of offenders have no further contacts. For those with a fourth or fifth contact however, 20 percent are desisters, and only 10 percent of those with a sixth contact have no further contacts" (p. 99). Smith and Gartin also reported differences between individuals who were apprehended but released as compared to those who were apprehended and arrested. In their sample, 50 percent of offenders who were arrested as a result of their first contact with the police did not have any subsequent police contacts (at least through age 25); however, only 28 percent of offenders who were not arrested during their first contact with the police had no further police contacts. Smith and Gartin note that this relationship holds for the second arrest; however, the influence of arrest on recidivism is diminished at both the third and fourth police contact and the effect is reversed by the fifth contact. That is, after the fourth contact with police, the effect of arrest appeared to increase the likelihood of recidivism. While more experienced offenders tended to continue offending after apprehension, in general, arrest did appear to significantly reduce subsequent rates of offending. Smith and Gartin's analysis also suggested that novice offenders were less likely to recidivate after an arrest.

In a larger study of rational choice, Matsueda, Kreager, and Huizinga (2006) using data from the Denver Youth Survey found, controlling for a range of other variables, that as experiential risk of apprehension (indicated by the number of arrests divided by the number of self-report offenses) increased, so did the perceived chance of apprehension, and that increased perceived chance of apprehension was accompanied by a decrease in future offending. Thus, perceived certainty of arrest was associated with a subsequent decrease in delinquent behavior. However, as noted elsewhere, with current levels of apprehension, more frequent offenders have a smaller experienced probability of arrest per offense than less frequent offenders and hence may be less influenced by arrest.

Over the past few decades, there have been others who have sought to determine if the effect of arrest is moderated by one's prior arrest history. That is, if arrest is either a more or a less salient predictor of subsequent offending for people who have progressed further into their criminal careers. Lending support for a moderation hypothesis, Cameron (1964) indicated that apprehension of novice shoplifters tended to stop their shoplifting behavior. Both Thorsel and Klemke (1972) and Packer (1968) argued that justice system sanctioning is likely to be less effective among experienced criminals. Similar findings concerning arrest contingencies, that the probability of re-arrest increases consistently with increasing numbers of arrests, have also been found in analysis of individuals in the Philadelphia cohort study (Blumstein & Moitra, 1980; Wolfgang, Figlio, & Sellin, 1972). On the other hand, Klein's (1974) examination of 13 police agencies in California revealed that arrest and subsequent sanctioning was more likely to decrease the rate of future offending among experienced offenders than novice offenders.

To assist in summarizing these various findings about the effects of arrest, it is helpful to outline the findings in a table. Tables 1 and 2 list the studies, type of analyses, intervention, and subsequent outcome described above. Table 1 lists those studies that found that arrest (or related intervention) either had no effect (indicated by a O) or resulted in an increase in subsequent delinquency (indicated by a+). Table 2 lists those studies that found that arrest resulted in a decrease in subsequent delinquency (indicated by a-). As is seen in comparing the two tables, the preponderance of studies found that arrest either had no effect or increased subsequent delinquency. Interestingly, this general finding seems to hold across studies conducted over the last 35 years or so, suggesting some robustness over time.

### ***Findings About Sanctioning Following Arrest or Police Contact***

As noted earlier, many studies have examined the extent to which various forms of sanctioning following arrest differentially impact subsequent criminal offending. In the following review, findings from some of the studies that have compared major kinds of police and court alternatives in sanctioning are described. We emphasize that this is not a complete review, but focus on some studies of major alternatives in sanctioning. In addition, findings about diversion from juvenile court and intensive supervision, as well as findings about specific programs for special subgroups

**Table 1** Summary of studies finding an *Increase or No Change* in subsequent delinquency following arrest<sup>†</sup>

Study	Type of Analysis	Intervention Examined			Future Outcome		
		SRD	Police Contact	Re-Arrest	Police Contact	Re-Arrest	Adult Outcome
Gold and Williams 1970	Matched Group	+					
Thomas 1977	Model with Control Vars.	O					
Farrington 1977	Matched Group	+					
Paternoster 1978	Model with Control Vars.	+					
Huizinga et al. 1986	Matched Group	O					
Klein 1986	Experiment	O				O	
Huizinga and Esbensen 1992	Matched Group	+				+	
Paternoster and Piquero 1995	Model with Control Vars.	+					
Kaplan and Damphouse 1997	Model with Control Vars.	+					
Huizinga et al. 1996	Matched Group	+					
Bernburg and Krohn 2003	Model with Control Vars.	+					+
Huizinga et al. 2003	Type of Offender						+
Huizinga et al. 2003 (Denver Sample)	Matched Group and Other	O					+

Huizinga et al. 2003 (Bremen Sample) Smith 2006	Matched Group and Other Model with Control Vars.	Arrest	O	+
Hemphill et al. 2006	Model with control Vars.	Arrest	O	
McAra and McVie 2007	Propensity Matched Group	“Arrest” (Police charge)	O	

† + = Increase in subsequent delinquent/criminal behavior  
 O = No change in subsequent delinquent/criminal behavior  
 - = Decrease in subsequent delinquent/criminal behavior

**Table 2** Summary of studies finding a *Decrease* in subsequent delinquency following arrest<sup>†</sup>

Study	Type of Analysis	Intervention Examined	Future Outcome			
			SRD	Police Contact	Re-Arrest	Adult Outcome
Cameron 1964		Arrest			—	
Smith and Gartin 1989	Model with Control Vars.	Arrest		—		

† + = Increase in subsequent delinquent/criminal behavior

O = No change in subsequent delinquent/criminal behavior

— = Decrease in subsequent delinquent/criminal behavior

of arrestees are not described. Although a thorough consideration of diversion is beyond the scope of this review, it should be noted that Aos et al. (2001) indicate that, overall, court diversion programs result in a slight reduction in future offending in comparison to regular court processing and are less expensive. Diversion programs as practiced in the 1970s and 1980s in the U.S. had substantial variation in outcomes, but in general were as successful or more successful than regular court processing (see e.g., Dunford, Osgood, & Weichselbaum's 1982 national evaluation of diversion programs). We also do not review studies of arrest in domestic violence cases and NIJ's Spouse Abuse Replication Projects, since this topic would require a lengthy review and is beyond the scope of this chapter.

Rose and Hamilton (1970) conducted a randomized trial of adolescents in Britain. Male adolescents who had come in contact with the police as the result of a first-time, minor offense were randomly assigned to be either cautioned and released or to be cautioned and released with police supervision. The supervision lasted for a period of six months. After thirty months, the rate of recidivism for the two groups was not significantly different (26% of those with no supervision were rearrested and 27% of those with supervision were rearrested), indicating that the increased supervision had no effect on recidivism.

Thornberry (1971) using data from the Philadelphia Cohort Study found that among adolescents, as the penalty of sanctions increased across being warned and released by the police, being diverted from court, being put on probation, and being incarcerated, the more likely the offender was to be re-arrested. Wolfgang et al.'s (1972) analysis of the Philadelphia Cohort Study also indicated that although a lenient disposition does not necessarily prevent an offender from being rearrested, neither does it encourage repeat offending. They further noted, as observed by Thornberry, that the more severe the court disposition, the higher the probability of re-arrest.

Berg and colleagues (Berg, Consterdine, Hullin, McGuire, & Tyrer, 1978; Berg, Hullin, & McGuire, 1979) also conducted a randomized experiment to assess the effect of legal sanctions among adolescents in England. To test the impact of two court dispositions for adolescents who were charged with truancy, 96 adolescents were randomly assigned to either be released or released with supervision by a social

worker. Over the course of the next six months, the supervised adolescents were more often truant and committed more offenses than the unsupervised adolescents.

Schneider (1982) reviewed studies that assessed recidivism among adolescent status offenders who were punished with detention and those who were diverted. She reported that two-thirds of the studies suggested no difference between detention and diversion, while the remaining studies were equally divided between programs that showed a beneficial impact of diversion and programs that showed a harmful effect.

As noted earlier, Klein (1986) conducted a true experimental test of the effect of arrest on subsequent delinquency that in addition to arrest included other sanctions. Adolescents who were counseled and released were less likely to be rearrested over the subsequent 27 months than adolescents who were either referred or petitioned. However, as noted before, no significant differences existed between the conditions in rate of actual self-report offending.

It is interesting to note that Shannon (1980, 1985) had earlier analyzed the same data used by Smith and Gartin (1989), to assess the impact of incarceration on subsequent criminal behavior. (As described above, Smith and Gartin found that police contact reduced subsequent contacts.) However, Shannon reported that incarceration led to increased involvement in crime. Smith and Gartin (1989) state that "it is possible that punishment will reduce future offending relative to no punishment at all, but that among those who are punished, more severe punishment may lead to increased future criminality. . . thus, within the same data, evidence indicates that arrest has some specific deterrent value even when more severe sanctions do not."

As part of the evaluation of Florida's Community Control Program (a home-confinement program), Smith and Akers (1993) compared the effectiveness of home-confinement to imprisonment. Using recidivism as their outcome variables, they reported no significant differences between the two sanctions.

Espiritu and Huizinga (1999) considered the effect of court disposition on subsequent offending. They reported that the self-reported delinquency of adolescents before, during, and after a court sentence remained similar. That is, being processed through court appeared to have no effect on subsequent delinquency.

Gottfredson (1999) assessed the effect of two forms of sanctioning, probation and incarceration, on recidivism among offenders convicted of felonies in Essex County, NJ during 1976 and 1977. He reported that neither type of sentence nor length of sentence had a significant impact on recidivism.

Recently, Spohn and Holleran (2002) considered the effect of incarceration on recidivism of felony offenders living in Jackson County, Missouri in 1993. They reported that incarceration increased recidivism. That is, offenders who were sent to prison were more likely to recidivate than similar offenders who were put on probation. Furthermore, their results suggest that the harmful effect of incarceration was most robust among drug offenders.

Using data from the Rochester Youth development study, Bernburg, Krohn, and Rivera (2006) examined the effect of juvenile justice intervention following arrest on subsequent delinquency. They found that intervention significantly increased subsequent serious delinquency, largely attributable to an increased involvement in gangs and delinquent peers.

McAra and McVie (2007) found in the Edinburgh Study of Youth Transitions and Crime that the group of youth who were brought to a “children’s hearing” (somewhat similar to formally appearing in juvenile court in the U.S., but with substantial differences) were significantly more likely to report involvement in serious delinquency than youth in a propensity matched control group, and that the mean frequency of serious offending was greater in the hearings group than in the control group, although the frequency difference was not quite statistically significant. Combining this finding with the findings described earlier from the Edinburgh study about the effect of arrest, as well as other analyses, McAra and McVie observe “Taken together, our findings indicate that the key to reducing offending may lie in minimal intervention and maximum diversion: that doing less rather than more in individual cases may mitigate against the potential damage which system contact brings.”

Although there are thus many studies that find either no effect or increases in offending following the imposition of sanctions, a few studies have found that sanctions decrease future offending. Murray and Cox (1979) reported a beneficial effect of sanctioning. Employing a quasi-experimental design, they assessed the rate of recidivism among serious juvenile offenders who were either incarcerated or put on probation. While both groups demonstrated a reduced rate of recidivism after having received either an incarceration sentence or probation, the adolescents who were incarcerated showed a lower rate of recidivism. Furthermore, among those on probation, adolescents who received a more restrictive probationary program demonstrated a lower rate of recidivism.

Using a sample of men born in Denmark during the mid 1940s, Brennan and Mednick (1994) reported that recidivism was significantly lower among sanctioned arrestees as compared to released arrestees. They also assessed the effectiveness of different forms of sanctioning (fines, incarceration, and probation) and found no differential effects on recidivism. Interestingly, they reported that within offense level, individuals who received sanctions for a greater proportion of their arrests were less likely to recidivate.

Table 3 provides an outline summary of the various studies on sanctions described above. As seen in the table, most of these studies found that sanctions in comparison to no sanction resulted in either no change or an increase in subsequent delinquency and/or that increased severity of sanctions resulted in either no change or an increase in subsequent offending. Even in the study that found that some sanction was necessary to achieve reduction in recidivism among Swedish men, there were no differences in recidivism among different sanctions running from fines to incarceration.

### ***The Effect of Arrest and Sanctions on Factors Related to Subsequent Offending***

Research has shown that justice system processing not only has an effect on one’s subsequent delinquent behavior, but also may affect other areas of an

**Table 3** Summary of studies about the effects of sanctions following an arrest

Studies indicating no change or increase in subsequent delinquency			
Study	Offense	Sanction Examined	Outcome
Rose and Hamilton 1970	Minor Offense	Contact and Supervision	O
Thornberry 1971		Increasing Severity	+
Wolfgang 1972		Increasing Severity	+
Berg et al. 1978	Truancy	Supervision	+
Schneider 1982	Status	Detention	+
Klein 1986		Referred	+
		Petitioned	+
Shannon 1985		Incarceration	+
Smith and Akers 1993		Home confinement vs. Incarceration	O
Gottfredson 1999	Felonies	Probation, Incarceration	O
Espiritu and Huizinga 1999		Various Court Dispositions	O
Spohn and Holleran 2002		Probation vs. Incarceration	O
Huizinga et al. 2003		Incarceration	+
Bernburg et al. 2006	Serious Delinquency	Juvenile justice intervention	+
McAra and McVie 2007		“Court Appearance”— (Scottish Hearing)	+
Studies indicating a decrease in subsequent delinquency			
Murray and Cox 1979		Probation	—
		Restrictive Probation	—
		Incarceration	—
Brennan and Mednick 1994		No sanctions	+
		Some sanction (fine through incarceration)	O

† + = Increase in subsequent delinquent/criminal behavior

O = No change in subsequent delinquent/criminal behavior

— = Decrease in subsequent delinquent/criminal behavior

offender's life. Often, arrest and other forms of legal sanctioning lead to undesirable outcomes, including limited employment and educational opportunities, early pregnancy/parenthood, and negatively changed attitudes and beliefs. In turn, these undesirable outcomes may be anticipated to affect later offending behavior. In the following section, some of the research studies that have examined the effect of arrest and interventions on these types of non-criminal outcomes are described. It should be noted that these studies are important in specifying intervening factors that may be involved in producing the observed effects of arrest or intervention. Simply observing that arrest or legal sanctioning leads to increased or to decreased offending, when these occur, is not sufficient in itself to confirm the tenets of either labeling or deterrence theories. Both theories specify changes in other intervening variables and discovering whether change has occurred in these intervening variables is thus necessary in examining their veracity.

## Effects on Employment

Bushway (1998) noted that traditional criminological work tended to consider the effect of employment problems on crime, but more contemporary work has considered the possibility that crime is causally prior to employment problems or that the effect is at least bidirectional. Bushway points to work by Sampson and Laub (1997) which claims that people who have become involved with the justice system as a result of their involvement in crime may be formally labeled and that this negative form of labeling causes potential employers to exclude ex-offenders from potential employment.

Longitudinal data are needed in order to properly address the relationship between justice system involvement and subsequent employment because careful selection of background covariates must be included in order to ensure that the effect of arrest (and other forms of sanctioning) is not spurious. Several published studies have reported on this hypothesis. Thornberry and Christenson (1984) utilized the 1945 Philadelphia birth cohort study and found that arrest resulted in longer periods of unemployment during the following two years. They concluded that the relationship between involvement in the justice system as a result of crime and employment opportunities is at least bidirectional.

Utilizing West and Farrington's London sample, Hagan (1993) reported that self-reported delinquency at ages 18 and 19 significantly predicted the likelihood of unemployment at ages 21 and 22; however, official conviction at age 18 and 19 was not a significant predictor of subsequent unemployment. Based on his findings, he concluded that the young men's embeddedness in youthful crime was a better determinant of unemployment than official sanctioning. However, Nagin and Waldfogel (1995) analyzed the same data using a "difference in differences" fixed effects approach, assessing the intra-individual change in employment among 17–19 year-old men who were divided into distinct groups by their criminal history record and criminal involvement. Their analysis estimated the effect of a conviction on employment after controlling for the effect of their criminal involvement. Their results suggested that being convicted had a robust and negative effect on employment stability, while past criminal activity had no effect on employment stability.

Sampson and Laub (1993) utilized the Glueck's data to test a hypothesis about juvenile incarceration. After adjusting for substance use, age, race and delinquency, the length that a young man was incarcerated as an adolescent had a significant negative effect on job stability through age 32.

Bushway (1998) utilized the White, male respondents of the National Youth Survey data to assess the effect of first-time adult arrests on employment stability. Like Nagin and Waldfogel (1995), Bushway analyzed the data using a "difference in differences" fixed effects approach, splitting the sample into two distinct groups. One of the groups included men who had never been arrested and the other group included men who were arrested for the first time between 1983 and 1986. After adjusting for carefully chosen covariates (delinquency, marital status, age, and place of residence), arrest was found to decrease the time spent employed during the year by 10.78 weeks. However, no effect on arrest was found for job stability (staying at one job for at least 40 weeks during the year).

Tanner, Davies, and O'Grady (1999) used the 1979 cohort of the National Longitudinal Survey of Youth to assess the effect of a long list of variables (SES, family structure, age, ethnicity, truancy, drug use, property crime, violent behavior, cognitive skills, educational expectations, highest grade completed, and contact with the criminal justice system) on occupational status and unemployment in young adulthood (ages 25–30). Involvement with the criminal justice system included being stopped by police, booked, charged, or convicted. Occupational status was defined using the Duncan Socioeconomic Indicator based on the respondents' job in 1992 and unemployment was a dichotomous indicator to differentiate between individuals who, in 1990, were employed during the entire year and those who were unemployed for at least one week during the year. In their models, contact with the justice system was not predictive of employment outcomes.

De Li (1999) assessed the impact of legal sanctions on status achievement in general (which included high school graduation, employment status, and job stability) using West and Farrington's London data on 411 working-class boys born in 1953. De Li's results indicated that legal sanctions (being convicted for a crime between the ages of 14 and 16) had a direct and negative effect on status achievement at age 18 and 19 after adjusting for pertinent covariates.

In their examination of the effects of arrest and juvenile justice interventions within the Rochester Youth Development Study, Bernburg and Krohn (2003) found that both arrest/contact and juvenile justice system intervention reduced the chance of high school graduation and employment in young adulthood. They further observed that although educational attainment substantially mediated the effect on adult unemployment, a significant effect of arrest and of juvenile justice intervention on adult employment remained, even when educational attainment was controlled. They further noted that the effect of arrest and intervention was more pronounced for impoverished families and African Americans.

Effects of sanctioning on employment are also reported in studies using offender based samples. Waldfoegel (1994) assessed the employment hypothesis using over 2,200 criminals charged with fraud or larceny from the Federal Probation and Parole Sentencing Supervision database. He reported a significant negative effect (at the .05 level) of a federal fraud conviction on the likelihood of employment and a negative but non-significant effect of a larceny conviction. Controlling for race, marital status and age, those who were convicted of fraud and put on probation had a reduced probability of employment of 3–5 percentage points, while those who were convicted and sent to prison had a reduced probability of employment of 5–9 percentage points.

Taking an approach that differed from many of the other papers in this area, Needels (1996) assessed the effect of incarceration on employment and earning patterns over a period of nine years among 1,176 men released from Georgia prisons. She was particularly interested in examining differences among the ex-convicts that would predict their subsequent employment. She reported that time spent incarcerated was not a significant predictor of employment outcomes. However, ex-prisoners who were never rearrested for another crime after release went on to earn significantly more money than those who continued to be criminally active (and were caught).

More recently, Kerley and Copes (2004) assessed the effect of contact with the criminal justice system on employment stability for white-collar as compared to street-level offenders. They used data on federal offenders collected by Forst and Rhodes (1992), a data set of offenders in eight U.S. states who were sentenced between 1973 and 1978. They reported that type of offender (white-collar vs. street level) was the most important predictor of stable employment post release. White-collar offenders were more likely to regain stable employment after being released than street-level offenders. However, several other predictors were important, including age of first offense, age of first arrest, age of first incarceration, number of arrests and total time sentenced, poverty, neglect/abuse, criminal family, race, educational attainment, and age.

### **Effects on Educational Achievement and Opportunities**

Hirshfield (2002) offers a theory to explain the effect of involvement in the juvenile justice system on reduced educational achievement and opportunities. Extending correspondence theories, he explains that “juvenile justice involvement, whether initiated in school or outside of school, socializes students into the role of criminal. . . youth who are identified by the juvenile justice system may be differentially subject to the criminal justice tools inside schools including the guards and zero tolerance disciplinary policies. These practices and policies, whether implemented by teachers or guards, not only may inadvertently socialize criminals within the school context but they also may exclude them from school, thereby relegating them to the surplus labor pool from which prisoners are disproportionately drawn” (p. 62).

Recently, Hirshfield (2002) examined the effect of juvenile justice system involvement on school achievement and enrollment utilizing a sample of 778 urban Black and Latino youth drawn from the Corner School Development Program evaluation. After carefully controlling for pertinent covariates, arrest increased the probability of repeating the eighth grade and dropping out of school, and also had an adverse effect on both academic performance and school attendance. He also reported that these negative effects were more robust among youth who had been arrested multiple times.

Very few studies have assessed the impact of juvenile justice system sanctions on subsequent educational factors. Tanner et al. (1999) reported no effect of juvenile justice system involvement (stopped by police, booked, charged, or convicted) on the highest grade completed or the likelihood of graduation after adjusting for a list of covariates, including delinquency. Their sample was taken from the 1979 National Longitudinal Survey of Youth. On the other hand, achieving a more thorough assessment of the hypothesis, Janosz, LeBlanc, Boulerice, and Tremblay (1997) used cross-sectional data from a sample of 791 French-Canadians ranging in age from 12 to 16. In their analysis, number of arrests was a significant predictor of high school dropout after adjusting for a list of covariates (including academic achievement, commitment to school, family relationships, parental monitoring, family structure, delinquent behavior, delinquent norms, and respect for authorities). Also, as noted above, Bernburg and Krohn (2003) found that both arrest/contact and

juvenile justice system intervention reduced the chance of staying in school and the chance of high school graduation.

### **Effects on Precocious Transitions to Young Adulthood**

Lizotte et al. (2004) reported on the effect of police contact and arrest on precocious transitions to young adulthood (becoming pregnant or causing a pregnancy, teen parenthood, school drop-out, and leaving home early) using data from the Denver Youth Survey, the Pittsburgh Youth Study, and the Rochester Youth Development Study. Their results suggest that, after controlling for pertinent covariates (including family structure, race, social class, attachment to parents, and commitment to school) early police contact or arrest before the age of 16 had very little effect on precocious transitions. One important exception to this conclusion emerged; arrest did increase the likelihood of school dropout among boys in Rochester, NY. Lizotte and colleagues also considered the effect of police contact and arrest in later adolescence (at age 16 and older) and found these variables to be important predictors of precocious transitions. Boys who reported police contact or who had been arrested in later adolescence were more likely to cause a teenage pregnancy, become a teen parent, drop out of school, and leave home early. Girls who reported police contact or who had been arrested in later adolescence were more likely to become pregnant as a teenager, drop out of school, and leave home early. Lizotte and colleagues caution the interpretation of these findings because both arrest in late adolescence and precocious transitions were measured contemporaneously so it is not possible to draw conclusions about the causal ordering of the variables.

### **Effects on Attitudes and Beliefs**

The literature relating the effect of legal sanctioning on change in attitudes and beliefs provides mixed results. Examining a longitudinal sample of California adolescents as they progressed from 9th to 12th grade, Ageton and Elliott (1974) reported a modest correlation ( $r=.10$ ) between legal processing and delinquent orientation. They conclude that their "initial findings suggest that the impact of interaction with the juvenile justice system on the development of a delinquent orientation is not as comprehensive as many labeling theorists would have us believe" (p. 97). They also noted that Anglo males were more susceptible than other adolescents to experience negative outcomes following involvement with the juvenile justice system. Ageton and Elliott posit that Anglo males may "feel more constrained by a system dominated by White secular authorities than minority youth who may perceive the label as created and applied by outsiders" (p. 97).

Jensen (1969) reported that offenders who were punished early in their career were more likely to develop a deviant identity as compared to experienced offenders who were apprehended later in their criminal career. However, contradictory to Jensen's findings, Hepburn (1977) reported that punishment had no differential effect on self-identification as a delinquent among novice as opposed to experienced

offenders. His analysis was performed on two randomly selected samples of 14–17 year old White boys; one sample had no record of police contact and the other had at least one formal contact with the police.

In examination of a different type of belief, perceived severity of legal sanctions, Apospori and Alpert (1993) reported that adults who received legal sanctions increased their perceptions of the severity of legal sanctions. Furthermore, individuals who received more severe sanctions were more likely to increase their perception of severity as compared to individuals who received lighter sanctions. Also, using a sample of 2,147 high school students, Thomas and Bishop (1984) reported that the relationship between punishment and perceived threat was not different for novice as compared to experienced offenders.

Given the findings of no substantial effect of arrest on subsequent offending in the Denver Youth Survey described earlier, Huizinga and Elliott (2003) examined the influence of arrest on several potential intervening factors following arrest that might explain these findings. These included the perceived chance of apprehension and severity of sanctions, parental supervision, perceived labeling by teachers, change in self-concept and belief, and changes in involvement with delinquent peers. They found that after arrest, roughly 1/3 of both genders reported no change, slightly under half (44%) an increase, and 22% a decrease in their perceptions of the chance of apprehension. They also found that about 80% of arrestees found imposed sanctions to be harsher or tougher than anticipated or of sufficient strength that they would try to stay out of trouble in the future. The respondents also reported that there was no substantial change following arrest in parental monitoring, teacher labeling, or in their perceived chances for future education, jobs, or general life opportunities. However, there was some evidence of a decreased belief in the wrongness of delinquent behavior, increases in neutralization (willingness to make excuses for delinquent acts), and decreases in feelings of guilt for involvement in delinquent acts. Also, although most arrestees indicated that their friends had not changed, they also indicated some increase in involvement with delinquent peers and some decrease in involvement with conventional peers following arrest. Huizinga and Elliott note that these findings suggest that in the high-risk neighborhoods of this study the juvenile justice system is not perceived as a “paper tiger,” and that arrest appears to have generated a more delinquent belief system and increased delinquent and decreased conventional peer involvement.

## Discussion

Empirical research on deterrence (the avoidance of a proscribed act because of an anticipation of a punitive sanction) over the past 40 years or so, began with community, state, or national studies, using aggregate level data, to examine the relationship between the proportion of crimes known to the police that resulted in arrest or other sanctions and official crime rates (e.g., Chiricos & Waldo, 1970; Gibbs, 1968; Kobrin et al., 1972; Tittle & Rowe, 1974). As indicated in the above review, although the findings from these studies were contradictory, some of these studies indicated,

for a variety of different crimes, a negative relationship between clearance rates and crime rates and a few studies indicated a negative relationship between increased sanctions and crime rates. It is interesting that the findings were often dependent, sometimes even within the same study, on the population size of the communities under study (Brown, 1978; Kobrin et al., 1972; Tittle & Rowe, 1974).

Shortly thereafter, a number of deterrence researchers began emphasizing that deterrence was more likely to depend on the perceived probabilities of apprehension and sanctioning by potential offenders than on objective probabilities (see Paternoster, 1987, for a list of such studies). In general, these studies demonstrated a negative relationship between perceived certainty of arrest or sanctions and subsequent delinquent behavior. However, most of these studies were cross-sectional, and Paternoster (1987) demonstrated that when longitudinal studies were used for this perceptual research, the relationships were much diminished. Further, when additional variables were added to avoid model misspecification, the effect of perceived certainty on subsequent delinquency was even further reduced and often became non-significant. Paternoster concluded this review by noting that perceptual deterrence had advanced as far as possible to the point of complex multi-wave models. However, he notes, that what we really know is that perceptions of the certainty and severity of sanctions do not seem to deter the trivial behaviors of high school and college students. With very few exceptions, there were almost no studies of high criminality samples at that time. Although Paternoster restricts this sampling admonition to adult samples, it is equally germane for high-risk potentially delinquent samples as well.

Among the additional variables added to these perceptual models are informal social sanctions, such as strained relationships with parents and friends, that may result from arrest or other formal sanctions. Often these informal sanctions further erode the significance of a direct effect of formal sanctions and are found to be of equal or greater importance than formal sanctions (Anderson, Chiricos, & Waldo, 1977; Bishop, 1984; Erickson & Gibbs, 1978; Nagin, 1978; Tittle, 1980).

Support for a deterrence or rational choice perspective using a more current multi-year longitudinal study was also provided by Matsueda et al. (2006). They found that, controlling for a range of other variables, as experiential risk of apprehension (indicated by the number of arrests divided by the number of self-report offenses) increased so did the perceived chance of apprehension, and that increased perceived chance of apprehension was accompanied by a decrease in future offending.

In addition to the examination of specific deterrence from a deterrence or punishment point of view, other studies, often beginning with a labeling perspective, were also examining the effect of arrest and formal sanctions on subsequent delinquent behavior. What is evident in the review provided earlier, is that, over several decades, the vast bulk of these studies found that arrest (to include studies of police contact resulting in monitoring by the police or others and studies employing conviction) results in equal or higher rates of subsequent offending. One of the strongest findings, being based on an experimental design, comes from the study by Klein (1974). Being counseled and released by police resulted in lower re-arrest rates than other more stringent alternatives, although police disposition had no effect on future

self-reported delinquency. It should be noted, however, that this experimental study is not complete, since it does not include a “no-treatment” control group. That is, the sample is limited to youth who were “picked up” by the police.

Studies from longitudinal surveys of adolescents and young adults (see Table 1) also found that arrestees were as likely or more likely to re-offend than non-arrestees. Thus, in over some 17 studies employing 14 different samples the results are quite consistent. Arrest (broadly construed) results in similar or higher rates of subsequent offending. In addition to the general consistency of these studies, it should be noted that the U.S. studies occurred during historical periods when sanctions were lenient and when sanctions were more severe. Thus, there is some indication that the findings are not entirely dependent on level of sanctions. Also, the findings occur in studies conducted in different countries and with substantively different juvenile justice systems. Thus, this finding appears quite robust.

There are some contradictory findings, however. Data provided by Smith and Gartin (1989) indicates that among a sample of youth that had a police contact and were followed to age 25, arrests for the first and second contacts significantly increased desistance from future police contact. However, for the third and fourth contacts, the effect of arrest was reduced, and by the fifth and sixth contact, arrest increased the probability of additional contact. In addition, Cameron (1964) reports that arresting novice shoplifters tended to stop their involvement in these offenses.

Findings from studies that examined the effects of sanctions quite uniformly found that that sanctions either had no effect on or increased the level of subsequent offending, and that as the severity of sanctions applied increased, the level of subsequent offending also either showed no change or actually increased. This generalization also appears quite robust being replicated in 14 studies, with only two studies providing contradictory findings and one of these observing that although there was a difference between no sanction and some sanction, there was no difference between severity of sanctions from fines through incarceration.

In addition, several studies have reported that variables anticipated to be affected by a labeling perspective of the effects of arrest and sanctioning are in fact being affected; for example, employment, education, and the development of a delinquent belief structure. Taking these various findings together, it appears that the majority of studies examining the effect of arrest or sanctions on employment find that arrest or sanctions have a negative effect on subsequent employment, including unemployment and time spent employed, although there are studies that find no effect. However, the assumption of a uniform motivation for employment across individuals needs verification, especially when illegal work or continuation in criminal activities may provide adequate or greater income.

That arrest and sanctions during adolescence lead to higher chances of high school dropout and to lower chances of high school graduation is also found in most of the studies that examined this issue. There is also some evidence that arrest and sanctions lead to a delinquent orientation, delinquent identity, or delinquent belief structure, although there is conflicting evidence whether such effects vary by stage of delinquent career (novice vs. experienced offender). There is also evidence that sanctioning increases the perceived level of the severity of sanctions, but findings about this are not consistent.

Overall, the various studies examining intervening factors related to a labeling perspective provide support for, and suggest some change in, mediating variables anticipated by this perspective. Although the number of studies using more general samples to examine the effect of arrest and sanctions through deterrence principles are relatively few, findings also support this perspective. Among those individuals apprehended and further sanctioned, the level of perceived sanctions for delinquency and crime appears to increase. Also, as indicated by Matsueda et al. (2006) as the actual risk of apprehension increases, so does the perceived chance of apprehension, which leads to a decrease in future offending.

Given the above, a question arises. Can there be empirical support for both deterrence/rational choice and labeling perspectives? The answer appears to be yes. Conceivably, both major theoretical orientations could be correct when applied to different subsets of individuals or, perhaps, when applied to certain types of offending behavior. For example, the effects may be different for novice vs. experienced offenders, younger vs. older individuals, for different socio-economic groups, for those living in different kinds of neighborhoods, for college students vs. their non-college age mates, and so on. And, the effects may be different when applied to those who misuse alcohol or other drugs vs. those involved only in larceny offenses or those involved only in violent offenses.

However, given that the weight of evidence suggests that arrest and sanctions either do not have much effect or increase subsequent delinquent behavior, it seems reasonable to ask why this should be the case. This is particularly significant, since the public at large and some criminologists seem to carry the opinion that of course arrest must have the effect of reducing future delinquency and crime. In addition, politicians often use a philosophy of arrest and more severe sanctions to indicate that they are doing something to reduce crime and protect citizens. And, juvenile and adult corrections, schools of criminology and criminal justice, and developers of intervention services are big businesses for which the volume of arrests and sanctions are important considerations.

Excluding long term incarceration, there are several reasons to expect that arrest followed by either no or some sanction would not have much effect on apprehended offenders.

1. Following arrest and many other sanctions, most offenders return to the same environment from which they came. Thus, the same factors that led to delinquent/criminal involvement and thus arrest are active and have not changed and would be expected to influence continued illegal behavior. Many arrested youth are returning to families that are not conventional, to limited opportunities for prosocial involvement, to delinquent peer groups or gangs, and to neighborhood environments where delinquent and criminal opportunities exist and arrest and more severe sanctions are not uncommon, perhaps even expected. Thus, both in the individual's social environment and perhaps to the individual themselves, there may be an indifference or even expectation of arrest for misdeeds that is considered a normal part of life. Returning to these risk and environmental conditions, involvement in future delinquency, even under the possibility of a future arrest and sanctions, seems likely.

2. The actual probability of arrest per offense (and hence sanctioning) is actually quite low. And, this holds for serious as well as minor offenses. Thus, certainty of apprehension, which has been generally found to be the strongest deterrent factor, is low. Also, it is likely that the effect of arrest dissipates over time, especially if it is not regularly reinforced. Since probability of arrest per offense is low, it would be assumed that the effect of arrest would not be high.
3. Most arrests, including the first, occur long after the initiation of offending and even serious offending (Elliott, 1994; Elliott, Huizinga, & Morse, 1986; Huizinga et al., 1995; Huizinga et al., 2003b). Thus, by the time of arrest, offending behavior may be more entrenched and offenders have already learned that the probability of arrest and sanction per offense is very low, so that the balance of "risk versus reward" for future offenses shifts toward the reward.
4. The severity of the sanctions beyond arrest may not match the severity of offenses of the apprehended individual. Because serious offenders often commit minor offenses with a greater frequency than minor or less serious offenders, both serious and minor offenders are most likely to be arrested for a minor offense. As a result, for serious offenders, the sanctions following arrest are unlikely to match the seriousness of their underlying offending behavior. This is an appropriate justice system response given the legal system in a free society, but limits the effect of the specific deterrence of arrest.
5. There are likely to be perceptual distortions in an individual's consideration of the likelihood of arrest, especially at the time of committing another offense when other factors such as opportunities, peer influence, the sense of fun or excitement are in play. People see and weigh things selectively and the consideration of the probability of an arrest or sanctions may have only a very small influence on participation in delinquent behavior. Similarly, individuals who are impulsive, or prone to risk taking, or have other psychological characteristics that diminish their capacity to incorporate risk into decisions influencing their behavior are unlikely to be particularly influenced by a previous arrest.
6. It is sometimes noted that general deterrence works only with individuals who are normally law abiding and who have internalized social norms from experiences within their family, school, religious groups, community, and peers. Those who have not internalized such norms are not likely to be deterred by threats of arrest and possible further punishment.
7. The treatment of apprehended individuals by the police also may affect the outcome of the arrest. If the individual is treated "fairly" and does not believe the arrest was the result of various status indicators such as race, social class, neighborhood of residence, particular dress, and so on, the arrest may have a greater effect. If on the other hand the treatment seems unfair or related to factors other than the offending behavior, then the arrest may be perceived as having little to do with the offense but rather with other characteristics of the offender or the offender's social group. In these cases, especially when the arrest results in increased sanctions against the individual, not only may the arrest and sanction be seen as irrelevant to the offending behavior, but may also engender anger toward the social system for what has been done to them as well as engender feelings that the basic rules of society do not apply or are irrelevant (cf. Vigil, 1995).

It is likely that there are other reasons for the lack of “positive” influence of arrest and sanctions on subsequent illegal behavior. However, based on the comments above and the observation that for many arrestees several of these factors are at work in combination, there is ample explanation for the observation that arrest has little or a harmful effect. In fact, under current social and justice system practices, it would seem rather unusual for arrest and ensuing sanctions, in general, to have the anticipated effect of decreasing subsequent delinquent and criminal behavior.

### ***Current and Future Contribution of the Longitudinal Studies***

The contribution of the longitudinal studies to the understanding of the influence of arrest and subsequent sanctions on subsequent behavior is slowly growing. However, among recent major longitudinal studies, extensive examination of the effects of arrests and sanctions is relatively rare. Although it is possible that we missed some particularly informative pieces of research, as part of the effort in preparing this chapter an extensive search was conducted for relevant published and unpublished reports, as well as communications with the principal investigators of some of the major longitudinal studies. Some of these investigators commented — “Well we certainly could have looked at the impact of arrest, but we just never have.” As noted in the following section about suggestions for the future, existing and future longitudinal studies have much to offer to the study of specific deterrence, and they should be encouraged to conduct such investigations.

### **Suggestions for the Future**

With the exception of true experimental studies, the longitudinal studies provide one of the best, if not the best, source for further exploration of both general and specific deterrence. Their ability to examine inter-individual differences in intra-individual change over long periods of time and over different phases of the life course makes them ideally suited for this purpose. Yet, few of the existing major longitudinal studies have conducted such examinations and none have taken full advantage of the potential for such explorations. As noted in the introduction, what is needed for major investigations of the impact of arrest are prospective longitudinal studies employing samples from general populations (not samples of those contacted by police), that include good measures of self-reported delinquency/crime, measures of police contact and arrest, and measures of subsequent processing and sanctioning within the justice system. In addition, it would be helpful to have measures of perceived risk of apprehension and perceived risk of different sanctions, as well as measures of attitudes about illegal behavior, neutralization, involvement with delinquent peers, neighborhoods, and so on. Although some current longitudinal studies of crime and delinquency do not have all these measures, all have data meeting the basic data requirements and some have extensive additional data such as measures of informal social control.

Contrary to the note of Sherman et al. (1997), that “these (longitudinal) studies cannot control for the rival hypothesis that the same factors that led to the youth being arrested also caused a higher level of repeat offending,” many and perhaps most of the factors that led a youth to be arrested exist in these longitudinal data sets and can be controlled through the use of precision or propensity matched arrest and control groups or, if sample size will not permit other methods, through the use of a “poor man’s” control in statistical models. In addition, the longitudinal studies have data about minor and serious offenders before their first arrest and include offenders who have never been apprehended and thus have the capacity to include a true “no treatment group” that is lacking in some examinations of the effect of an arrest so that a more complete look at the effect of arrest can be made.

In addition, several studies have indicated that the effect of arrest and sanctions may be different for different subsets of individuals and the longitudinal studies have the capacity to examine the effect of arrest on different kinds of offenders (e.g., offenders classified by age, gender, social class, stage of the life cycle, stage of delinquent/criminal career, frequency and seriousness of involvement in delinquent/criminal behavior, and so on). It should be further noted, that several prior research efforts in general deterrence have found substantial differences between urban and rural areas. Whether this occurs for specific deterrence is not as well known. Most existing longitudinal studies are concentrated in urban areas, so that findings from these studies should not be generalized to suburban or rural areas. If the opportunity occurs to include deterrence measures in new suburban or rural longitudinal studies, this opportunity should not be missed.

In fairness, all of the factors (e.g., demeanor, clothing, defiance, etc.) involved in the decision to contact and the decision to arrest are not present in these data sets and not all the factors involved in such decisions are necessarily even known. In fact, a good place for studies to begin might be considerations of the question: who gets arrested? That is, what distinguishes those who are arrested from those who are not, and are the characteristics of those arrested related to subsequent offending? Variables such as stupidity, impulsiveness, prior arrests of family members, what the police officer had for lunch, as well as offending behavior may be involved. This is a critical issue, since conceivably those who are arrested or arrested and given more severe sanctions are on a different life trajectory—a trajectory that leads to arrest and future offending. Thus, the advantage of a true experimental design is well known and obviously granted. However, many and perhaps most of the salient variables associated with arrest and subsequent offending have been identified in prior studies and are included in one or more of the longitudinal studies, so that a good deal of knowledge about deterrence can be distilled from these studies, and with the opportunity for replication across studies the strength and generalizability of the findings makes such efforts even more profitable and useful.

It should also be noted that the longitudinal studies have the capacity to add to the knowledge about levels of sanctions. Although looking at the efficacy of specific sanctions is most likely beyond the capabilities of current longitudinal studies, examining the influence of general levels of sanction severity would seem possible. That this should be feasible, is demonstrated by findings from the Bremen-Denver

cross-national project (Huizinga et al., 2003a) and the Edinburgh Study of Youth Transitions and Crime (McAra & McVie, 2007).

In this chapter we did not provide a critical review of the various studies cited and there are criticisms of many of the studies reviewed. First, findings from most studies about the effects of arrest and sanctions, even long-term multi-year studies, are often cross-sectional in nature or cover only relatively short periods of time (one or two years) and have not examined effects over more than one major developmental period. Thus, longer term outcomes are unknown. (An exception is Bernburg & Krohn, 2003.) Second, some studies include only individuals who have experienced an intervention such as arrest or a particular sanction. As noted earlier, for a truly adequate examination of the impact of arrest, a no-arrest group is required and to examine the effect of sanctioning a non-sanctioned group is needed. Third, few studies examine the intervening or mediating variables proposed by the deterrence, labeling, or other perspectives. Fourth, very few studies examine whether the effects of arrest and sanctions are the same or different among various subgroups. As noted earlier, there is some evidence that the effects of arrest and sanctions may vary by characteristics such as stage of delinquent or criminal career, social-economic standing, and age. Fifth, some studies do not have the necessary variables to adequately identify appropriate control groups or to employ such variables in statistical models.

Recent long-term multi-year longitudinal studies have the capacity to address most of these concerns. However, although the number of such studies examining arrest and sanctioning is increasing (see Table 1), it is still small, and very few have been used to full advantage to more completely examine the effect of arrest and sanctions.

Given these comments, and the important policy relevant and practical findings that should result, it would be helpful if future longitudinal studies were encouraged to include a focus on specific deterrence. It would also seem that existing longitudinal studies should be encouraged to conduct deterrence analyses, especially analyses focusing on the effect of arrest and sanctions. Further, given the value of replicated findings, at least some of the studies should be encouraged to work collaboratively, beginning with development of specific research questions, development of common measures, specification of appropriate analyses, and so on. Such a structure helps insure not only a broader view of the issue but makes the end products substantially more valuable. It seems the opportunity is there—the challenge of taking it awaits.

### ***Policy Implications and Suggestions***

Given that there are not replicated findings from experimental studies nor replicated findings from more extensive longitudinal studies from which stronger and more detailed findings are possible, policy implications based on the current knowledge of the effect of arrest on subsequent behavior must be tentative. In addition, there is the caution from Nagin (1998) about policy considerations or actions stemming from deterrence research that have shown no effect. Also, the potential effect of arrest

and sanctions on offenders is not the only reason for arrest. Issues of public safety, general deterrence, public concern for retribution, and victims' rights are also of concern (c.f., Smith, 2005).

Nevertheless, given the robustness of the finding that arrest either has little effect or results in an increase in subsequent delinquency across multiple studies, time periods, and different juvenile justice systems, coupled with the observation that increased sanctions also have little effect or result in increased subsequent delinquency, the following suggestions seem reasonable. First, police contact or arrest (*or perhaps apprehension by other social actors*) is needed for individuals violating legally proscribed norms, and such contact or arrest should be more consistent. This follows from the findings about certainty in both general and specific deterrence. Second, such contact or arrest and any subsequent sanction should be as lenient as possible within the limits of public safety.

This latter suggestion requires some additional explanation. If the impact of arrest (and by inference subsequent sanctions) has little effect on subsequent behavior, then the choice of options is up to the orientation and preference of the current social and political system. Whether to employ no sanctions, lenient sanctions, or harsh sanctions cannot be determined by the effect on the offender; the outcome is the same. Rather it must be determined on the basis of cost, humanitarian principles, need for victim concerns for retribution, and so on. However, the choice of lenient intervention seems justified on the basis of cost and the available evidence that indicates that more severe sanctions may result in increased subsequent levels of delinquency and crime and prolong delinquent and criminal careers and therefore result in reduced public safety.

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# **Part IV**

## **Future Directions**

# Advancing Knowledge About Causes in Longitudinal Studies: Experimental and Quasi-Experimental Methods

Rolf Loeber and David P. Farrington

## Introduction

What are the causes of delinquency is a very important topic in criminology. Causation is central to all theories that attempt to explain why some individuals and not others commit delinquent acts, why individuals commit crimes at some ages rather than others, and why some interventions to prevent or reduce delinquency are more effective than others. Since drawing conclusions about causes from longitudinal surveys is fraught with difficulties and possible artifacts, experimental studies constitute an important and indispensable tool to narrow down putative causes to those that are most likely to be “active” causes. The goal of this chapter is first to review key issues regarding the study of causes in criminology, and then to examine the use of experimental interventions in longitudinal studies to advance knowledge about causes. The second goal of the chapter is to illustrate several quasi-experimental approaches in longitudinal studies to establish which risk (and promotive) factors are likely to be causes. Risk factors predict a high probability of antisocial behavior, whereas promotive factors predict a low probability.

The following text builds upon key work in the social sciences that has focused on the identification and verification of causes (e.g., Blalock, 1964; Cook & Campbell, 1979; Kraemer, Lowe, & Kupfer, 2005; Singer & Willett, 2003; Wohlwill, 1973) and the need to address causes in longitudinal studies (Baltes & Nesselroade, 1979; Farrington, Ohlin, & Wilson, 1986a). We distinguish between two principal types of causes that can be studied in quasi-experimental research: (a) causes that explain *differences between individuals* in delinquency (an example is whether youth who do academically poorly at school are more likely to become delinquent than those who do well in school); (b) causes that explain *within-individual differences* in delinquency (an example is whether a boy’s worsening school performance is followed by his onset or intensification of delinquency). Causality can also be studied through experimental manipulation in which a putative causal factor is systematically introduced in an experimental group and its impact on individuals is assessed and compared to outcomes in a control group who were not exposed to the experimental manipulation (e.g., Farrington, 1983; Farrington & Welsh, 2006).

The contributions to the study of causes by Campbell and Stanley (1966) and Cook and Campbell (1979) have been outstanding. They stressed alternative causal

explanations (also called threats to the internal validity) that plague experimental and quasi-experimental studies (see also Farrington, 2003c; Shadish, Cook & Campbell, 2002). Among these alternative explanations are: (a) history: the observed effect is caused by other explanatory variables changing in the same time period; (b) maturation: the observed effect reflects a pre-existing trend; (c) testing: the observed effect is caused by previous testing of the participants; (d) instrumentation: the observed effect is caused by changes in measurement techniques; (e) regression: the observed effect is caused by statistical regression of extreme scorers to the mean; (f) selection: the observed effect is caused by pre-existing differences between the groups being compared; (g) mortality: the observed effect is caused by differential attrition from experimental and control groups; (h) instability: the observed effect reflects random variation; and (i) causal order: the true causal order is opposite to that hypothesized. When we discuss inferences about causality in the following text, each of these possible threats can serve as a backdrop to the identification of causes. Putative causal effects that can be explained by one of the nine alternatives need to be excluded from further consideration.

Amdur (1989) stressed that in criminological research there is often a confusion between measurement models and causal models. The measurement model is “an attempt to create meaningful variables out of a large number of interrelated items. The causal model is a statement about causal processes that produce correlations between these variables.” In Amdur’s (1989) review of studies, several authors “thought they were testing a causal model when in fact they were testing a measurement model” (p. 59).

The identification of causes is more difficult in some cases than in others. Of the known risk factors for delinquency, some concern discrete events, such as getting divorced, leaving home, or joining a gang. Establishing the causal effects of these life events tends to be more straightforward than establishing the causal effects of processes that often take place over months or years, such as poor communication between parents and children or poor child rearing practices. It should be noted, however, that even discrete causes may correlate with and sometimes operate through long-term processes. An example is becoming a single parent, which may be preceded by prolonged periods of conflict between partners and disagreements about parenting practices.

Farrington (1988), in reviewing causal issues, stressed that it is useful to distinguish between long-term and immediate influences on offending. This is important because it distinguishes explanations of the development of criminal people from explanations of the occurrence of criminal events, each of which has different causal foundations.

In summary, discrete events, long-term processes, and immediate antecedents to offending all require different causal probes. Kraemer et al. (2005) proposed the following sequence of probes (adapted to delinquency as an outcome):

- a. Is a factor correlated with delinquency? This serves to distinguish between non-correlated factors and correlated factors.
- b. For the correlated factors, does the factor precede delinquency? The answer to this question leads to the distinction between correlates and risk factors.

- c. For the risk factors, can the factor change or be changed? This serves to distinguish between those factors that are fixed markers and those that are variable risk factors.
- d. For the variable risk factors, does changing the risk factor alter the risk of delinquency? This step helps to distinguish variable markers from causal risk factors.

For ethical or practical reasons, only some of the causal processes can be manipulated in experiments to further buttress their causal status.

### *Prospective Longitudinal Surveys*

The main focus of this chapter is on investigating causes in longitudinal studies. Prospective longitudinal surveys involve repeated measures of the same people. Therefore, they involve at least two data collection points. The word “prospective” implies that risk and promotive factors are measured before outcomes. The most important surveys focus on community samples of hundreds of people, with repeated personal interviews spanning a period of at least five years (Farrington, 1979b). We focus on community surveys (as opposed to surveys of offenders) because they are needed to study the natural history of offending and the effects of risk/promotive factors and life events. In order to avoid retrospective bias, it is important to measure risk and promotive factors before the development of offending and to calculate prospective probabilities. We set a minimum of a five-year time period because we think that such a period is needed to provide adequate information about the natural history of the development of offending. We require interview data because we believe that official record data cannot provide adequate information on offending, risk and promotive factors, and life events.

In criminology, the main advantage of these longitudinal surveys is that they provide information about the development of offending over time, including data on ages of onset and desistance, the frequency and seriousness of offending, the duration of criminal careers, continuity or discontinuity in offending, and specialization and escalation. They also provide information about developmental sequences, within-individual change, effects of life events and effects of risk and promotive factors at different ages on offending at different ages (Farrington, 2003a; Loeber & Farrington, 1994).

While prospective longitudinal surveys have many advantages, they also have problems. The main challenge in these surveys is to draw convincing conclusions about causal effects. Because of their focus on naturalistic observation, longitudinal surveys find it difficult to disentangle the impact of any particular variable from the effects of numerous others. It is particularly difficult to rule out selection effects; for example, child abuse may predict delinquency because antisocial parents tend to abuse their children and tend to have delinquent children, without there being any causal effect of child abuse on delinquency. Also, the infrequency of data collection

often makes it difficult to pinpoint causal order. The best method of establishing causal effects is to carry out a randomized experiment (Robins, 1992).

Other problems can be overcome more easily. Attrition is a problem in some longitudinal surveys, but others have very high response rates (Farrington, 2003b; Farrington et al., 2006). Testing effects can also be problematic, but they can often be estimated. The length of time before key results are available is sometimes a problem, as is the confounding of aging, period and cohort effects, but these difficulties can be overcome by following up multiple cohorts in an accelerated longitudinal design (see Tonry, Ohlin, & Farrington, 1991, chapter 3).

## **Longitudinal-Experimental Studies**

More than two decades ago, Farrington et al. (1986a) in their book *Understanding and Controlling Crime: Toward a New Research Strategy* argued that:

1. The most important information about the development, explanation, prevention, and treatment of offending has been obtained in longitudinal and experimental studies.
2. New studies are needed in which these two important methods are combined, by embedding experimental interventions in longitudinal studies.

At the time, this book was quite influential; for example, it won the prize for distinguished scholarship of the American Sociological Association Criminology Section.

While a great deal of effort and money have been expended on both longitudinal and experimental research in the ensuing two decades, no longitudinal study of offending has yet been conducted containing several years of developmental data collection, an experimental intervention, and then several more years of developmental data collection. Why not?

There have been a number of longitudinal-experimental studies in criminology in which persons who did or did not receive an experimental intervention were followed up for several years (for reviews, see later). It is not controversial to argue for the desirability of adding a long-term follow-up to a randomized experiment. What is much more controversial is the desirability of embedding an experiment within an ongoing longitudinal survey, essentially because of concerns that the experiment might interfere with aims of the longitudinal survey such as documenting the natural history of development. This is the key type of research that is discussed here.

## ***Advantages of Experiments***

An experiment is a systematic attempt to investigate the effect of variations in one factor (the independent or explanatory variable) on another (the dependent or outcome variable). In criminology, the independent variable is often some kind

of intervention and the dependent variable is some measure of offending. Most criminological experiments are pragmatic trials designed to test the effectiveness of an intervention rather than explanatory trials designed to test causal hypotheses (Schwartz, Flamant, & Lelouch, 1980). The independent variable is under the control of the experimenter; in other words, the experimenter decides which people receive which treatment (using the word “treatment” very widely to include all kinds of interventions).

The focus here is on randomized experiments, where people are randomly assigned to different treatments. Providing that a large enough number of people are assigned (e.g., at least 50 per condition), randomization ensures that the average person receiving one treatment is equivalent (on all possible measured and unmeasured extraneous variables) to the average person receiving another treatment, within the limits of small statistical fluctuations. Hence, it is possible to isolate and disentangle the effect of the independent variable (the intervention) from the effects of all other extraneous variables (Farrington, 1983; Farrington & Welsh, 2005, 2006). However, it is also desirable to investigate intervening mechanisms (mediators). The main strength of randomized experiments is in excluding selection effects as a possible explanation.

### *Problems of Experiments*

Many problems arise in randomized experiments on offending. For example, it is difficult to ensure that all those in an experimental group actually receive the treatment while all those in a control group do not. Also, differential attrition from experimental and control groups can produce non-comparable groups and lead to low internal validity (Farrington & Welsh, 2005). There is often some blurring of the distinction between experimental and control groups (treatment cross-overs), leading to an under-estimation of the effect of the treatment. Angrist (2006) has described a method of correcting for this. Another difficulty is that participants and treatment professionals can rarely be kept blind to the experiment, and knowledge about participation in the experiment may bias outcomes or outcome measurement.

Typically, it is only possible in an experiment to study the effect of one or two independent variables at two or three different levels (different experimental conditions). Few of the possible causes of offending could in practice be studied experimentally, because few of the important variables could be experimentally manipulated (but see Farrington, 1979a, for experiments on causes of offending). Experiments are usually designed to investigate only immediate or short-term causal effects. However, some interventions may have long-term rather than short-term effects, and in some cases the long-term effects may differ from the short-term ones. More fundamentally, researchers rarely know the likely time delay between cause and effect, suggesting that measurements at several different time intervals are desirable. A longitudinal-experimental study deals with many of these problems.

Many ethical, legal, and practical issues arise in randomized experiments. For example, Farrington and Jolliffe (2002) carried out a study of the feasibility of

evaluating the treatment of dangerous, severely personality-disordered offenders using a randomized controlled trial. They found that all the clinicians were opposed to such a trial because they thought that everyone should be treated and that no-one should be denied treatment. However, where the number of persons who need or want treatment exceeds the number who can be treated (in light of available resources), random assignment may be the fairest way to select people for treatment. Cook and Payne (2002) have set out and answered objections to randomized experiments, and Boruch (1997) has provided detailed practical advice about how to mount such experiments successfully.

### *Advantages of Longitudinal-Experimental Research*

Strictly speaking, every experiment is prospective and longitudinal in nature, since it involves a minimum of two contacts or data collections with the participants: one consisting of the experimental intervention (the independent variable) and one consisting of the outcome measurement (the dependent variable). However, the time interval covered by the typical experiment is relatively short. Farrington et al. (1986a) argued that longitudinal-experimental studies were needed with three elements: first, several data collections, covering several years; second, the experimental intervention; and third, several more data collections, covering several years, afterwards. No study of this kind has ever been carried out on offending using interview data. A few experiments collected official record data for three or four years before and after an intervention (e.g., Empey & Erickson, 1972) but did not assess the effect of the intervention on criminal career trajectories.

An important advantage of a combined longitudinal-experimental study in comparison with separate longitudinal and experimental projects is economy. It is cheaper to carry out both studies with the same individuals than with different individuals. For example, the effect of interventions and the effect of risk factors can be compared on the same people. The number of individuals and separate data collections (e.g., interviews) is greater in two studies than in one (other things being equal).

More fundamentally, the two types of studies have complementary strengths and weaknesses, and a combined longitudinal-experimental study can hopefully build on the strengths of both. For example, the longitudinal survey can provide information about the natural history of development, while the experiment yields knowledge about the impact of interventions on development. Even if the experimental part could not be carried through successfully, the longitudinal-experimental study would yield valuable knowledge about the natural history of development, and quasi-experimental research on the impact of risk factors and life events would still be possible. Therefore, longitudinal-experimental research is arguably less risky than experimental research.

Experiments are for testing hypotheses. However, in the combined project, causal hypotheses could be generated in the longitudinal study from observed risk and protective factors and then tested on individuals in the experimental study. Experiments

are the best method of testing the effects of variations (between individuals) in an independent variable on a dependent one, whereas the longitudinal study can investigate the effect of changes (within individuals) in an independent variable on a dependent one. Hence, the combined project can compare the impact of variation with the impact of change, to see if the same results are obtained with the same individuals. This is an important issue, because most findings on risk factors for offending essentially concern variations between individuals, whereas most theories and interventions refer to changes within individuals (Farrington, 1988; Farrington, Loeber, Yin, & Anderson, 2002). The longitudinal and experimental elements are also complementary in that the experiment can demonstrate (with high internal validity) the effect of only one or two independent variables, whereas the longitudinal study can demonstrate (with somewhat lower internal validity, in quasi-experimental analyses), the relative effects of many independent variables.

It might be thought that an experimental study with a single pretest measure and a single posttest measure of offending would have many of the advantages of a longitudinal-experimental study, for example, in permitting the comparison of changes within individuals and variation between individuals. However, the simple pretest-posttest design could not distinguish between several different effects, such as an immediate lasting effect of an intervention, an immediate short-lived effect, no effect of an intervention on a pre-existing trend, or no effect of an intervention because of fluctuations in the outcome measure (see Farrington, 2006).

Some of the advantages of longitudinal-experimental research have been summarized by Blumstein, Cohen, and Farrington (1988). The impact of interventions can be better understood in the context of pre-existing trends or developmental sequences, which would help in assessing maturation, instability, and regression effects in before and after comparisons. The prior information about participants would help to verify that comparison groups were equivalent, to set baseline measures, to investigate interactions between types of persons (and their risk and protective factors and prior histories) and types of treatments, to establish eligibility for inclusion in the experiment, and to estimate the impact of differential attrition from experimental conditions. The long-term follow-up information would show effects of the intervention that were not immediately apparent, facilitate the study of different age-appropriate outcomes over time, make it possible to compare short-term and long-term effects and to investigate the developmental sequences linking them. The experimental intervention could help to distinguish causal or developmental sequences from different age-appropriate behavioral manifestations of the same underlying construct.

### ***Problems of Longitudinal-Experimental Research***

A major problem centers on the extent to which the experiment might interfere with the goals of the longitudinal study. In a simple experiment, some of the sample will be ineligible, some will be in the experimental group, and the remainder will be

in the control group. After the experimental intervention, it might be inadvisable to draw conclusions about the natural history of offending from the experimental group, since this would have been treated in an unusual way. The experiment may increase or decrease attrition (or cause differential attrition) from the longitudinal study. Hence, in drawing conclusions about the whole sample, results obtained with the ineligible, experimental and controls might have to be treated differently.

It is less obvious that experimental persons would have to be eliminated in investigations of impact questions using quasi-experimental analyses. If the experimental intervention could be viewed as just another independent variable impinging on them, investigations of the effect of non-manipulated independent variables could be based on the whole sample. Of course, it might be interesting to investigate whether the impact of an independent variable differed at different levels of another independent variable (e.g., in experimental and control groups).

It could be argued that each person should receive only one experimental treatment, because of the likely effect of the treatment in making the person different from a control or an ineligible. However, there may be good reasons to investigate the interactive effect of two consecutive treatments. The analysis of the data needs to mirror the factorial nature of the design. If the controls received a special treatment (e.g., being denied something that was usually available in the community), then it might even be argued that they also should not be included in a subsequent experiment.

The passage of time will inevitably cause problems. An experiment that was desirable and feasible at one time (e.g., at the start of a longitudinal study) may be less desirable and feasible some years later, because of changes in theory or policy concerns, in methodology, or in practical constraints (e.g., a change in a "gate-keeper" such as a police chief). Also, the participants in a longitudinal study will move around, and it may be that an experiment can only be conducted in a specific location. Possibly, only those who are residentially stable (at least in staying in the same metropolitan area) may be eligible to participate in the experiment, which might cause differential attrition. For a number of reasons, the eligibility of participants could change over time, as their personal circumstances changed.

Since it is likely that attrition will increase with the length of the follow-up, differential attrition could prove to be one of the greatest problems that need to be overcome in a longitudinal-experimental study (Farrington & Welsh, 2005). It is important to use methods that minimize attrition and to carry out research on this topic. For example, Farrington, Gallagher, Morley, Ledger, and West (1990) described the methods of tracing and securing cooperation used in the Cambridge Study in Delinquent Development, while Loeber and colleagues have done the same in the Developmental Trends Study and the Pittsburgh Youth Study (Cotter, Burke, Loeber, & Navratil, 2002; Cotter, Burke, Loeber, & Mutchka, 2005a; Cotter, Burke, Stouthamer-Loeber, & Loeber, 2005b; Navratil, Green, Loeber, & Lahey, 1994; Stouthamer-Loeber, 1993). Famous longitudinal researchers such as Robins (1966) and McCord (1979) were able to locate and interview high percentages of their samples over follow-up periods of 30 years or more.

## ***Key Longitudinal-Experimental Studies in Criminology***

Undoubtedly the best-known and most famous longitudinal-experimental studies in criminology are those by McCord (1978), Tremblay, Mâsse, Pagani, and Vitaro, (1996), Schweinhart et al. (2005), and Olds et al. (1998). All of these studies essentially added a long-term follow-up to a randomized experiment. The first two of these have provided a great deal of information about both the effects of the intervention and the development of offending, while the second two have focused more on the effects of the intervention.

McCord (1978) carried out the most important pioneering longitudinal-experimental study. In the Cambridge-Somerville study, the experimental boys received special counseling help between the average ages of 10 and 15, and over 500 boys in both experimental and control groups were then followed up for over 30 years afterwards, in records and through questionnaires and interviews (McCord, 1990). The treatment was ineffective in preventing offending, since about a quarter of both groups were known to have committed crimes as juveniles, while about two-thirds of both groups had been convicted as adults. Significantly more of the experimental boys had two or more convictions.

The Montreal longitudinal-experimental study (Tremblay, Pagani-Kurtz, Mâsse, Vitaro, & Pihl, 1995) is also very well-known. Initially, over 1,000 Montreal boys in 53 schools were rated by teachers on their disruptive behavior at age 6, and 319 scoring above the 70th percentile were randomly assigned to experimental or control groups. The experimental boys received skills training and parent training between ages 7 and 9. The results of the intervention showed that the experimental boys committed less delinquency (according to self-reports) between ages 10 and 15. The experimental boys were less likely to be gang members, to get drunk or take drugs, but they were not significantly different from the controls in having sexual intercourse by age 15 (Tremblay et al., 1996).

Another extremely influential experiment with a long-term follow-up is the Perry Preschool Project (Schweinhart et al., 2005). This was essentially a “Head Start” program targeted on disadvantaged African-American children. The experimental children attended a daily preschool program, backed up by weekly home visits, usually lasting two years, covering ages 3 and 4. The aim of the program was to provide intellectual stimulation, to increase cognitive (thinking and reasoning) abilities, and to increase later school achievement.

An important feature of this project is that its true significance only became apparent after long-term follow-ups to ages 15 (Schweinhart & Weikart, 1980) 19 (Berrueta-Clement, Schweinhart, Barnett, Epstein, & Weikart, 1984), 27 (Schweinhart, Barnes, & Weikart, 1993) and 40 (Schweinhart et al., 2005). As demonstrated in several other Head Start projects, the experimental group initially showed higher intelligence at age 4–5 but was no different from the control group by age 8–9 (Schweinhart & Weikart, 1980). This led to the argument that compensatory education was ineffective. However, by age 27, the experimental group had accumulated only half as many arrests as the controls—an average of 2.3 compared with 4.6 arrests. Also, they had significantly higher earnings and were more likely to be home

owners. A cost-benefit analysis showed that, for every \$1 spent on the program, \$7 were saved on the long term (Barnett, 1996). At age 40, 91% of the participants were interviewed (112 out of the original 123).

Another famous experiment was conducted by Olds et al. (1998) on the effects of home visiting. In Elmira (NY), 400 pregnant women were randomly assigned to receive home visits from nurses in pregnancy and for the first two years of their child's life, or to receive visits only in pregnancy, or to receive no visits. The nurses visited every two weeks and gave advice about child-rearing, infant nutrition, infant development, avoiding substance use, and maternal life-course development (family planning, educational achievement, and participation in the workforce). Fifteen years later, it was found that the women who had received visits in pregnancy and infancy had fewer substantiated reports of child abuse and neglect, and their children had fewer arrests and convictions, compared with the control group (Olds et al., 1997, 1998).

Three other experiments have been conducted with long follow-up periods (at least five years) but with few developmental analyses. In the Carolina Abecedarian Project, children aged 3 were randomly assigned either to receive full-time preschool child care (focusing on the development of cognitive and language skills) or not. At age 21, fewer of the experimental participants (but not significantly so) reported being convicted or incarcerated (Campbell, Ramey, Pungello, Sparling, & Miller-Johnson, 2002). In another preschool experiment, Mills, Cole, Jenkins, and Dale (2002) in Washington State randomly assigned children (average age 5) to either a cognitively-oriented or direct instruction preschool program. At age 15, the researchers found that the experimental and control participants did not differ significantly in their delinquency.

Kling, Ludwig and Katz (2005) evaluated the impact of the Moving to Opportunity program in five cities in the United States, in which vouchers were randomly assigned to low socioeconomic status (often minority) families to enable them to move to better areas. The effects of this move (and, hence, of different neighborhoods) on the offending of their children aged 15–20 were investigated. At least five years after the move, 1,807 youth were surveyed and 3,079 were searched in arrest records. The researchers found that there was little effect of the move on the prevalence of arrests overall, but there were desirable effects on the number of arrests of females for violent and property crimes and undesirable effects on the number of arrests of males for property crimes.

Several experimental studies now under way may eventually publish long-term longitudinal-experimental data on offending. One of the most famous of these is the Metropolitan Area Child Study (Metropolitan Area Child Study Research Group, 2002), in which 16 schools in two sites (Chicago and Aurora, Illinois) were randomly assigned to receive either a classroom intervention, or this intervention plus small group peer skills training, or these two interventions plus a family intervention, or no treatment. A two-year follow-up of high-risk children showed that early intervention (at ages 7–9) led to lower aggression (according to teachers and peers) in Aurora but to higher aggression in Chicago.

The Fast Track prevention trial, implemented by the Conduct Problems Prevention Research Group (2002) is also ambitious, large scale, and complex. At four

sites, children were identified as high-risk and randomly assigned to experimental or control groups. The experimental children received a “cutting-edge” program including home visiting, parent training, social skills and anger control training. However, up to the present time, no outcome data on delinquency has been published, only on child conduct problems (Conduct Problems Prevention Research Group, 2004).

Other ambitious experimental studies that may eventually have long-term follow-up data on offending include Raising Healthy Children (Catalano, Mazza, Harachi, Abbott, Haggerty, & Fleming, 2003), Focus on Families (Catalano, Haggerty, Fleming, Brewer, & Gainey, 2002) and Preparing For the Drug Free Years (Mason, Kosterman, Hawkins, Haggerty, & Spoth, 2003). Ambitious longitudinal-experimental studies have recently begun in Germany (Lösel & Beelman, 2003) and Switzerland (Eisner & Ribeaud, 2005). Hence, there have been a few longitudinal-experimental studies, but surveys with several years of interviews before and after an intervention have not been conducted in criminology.

### *Conclusions about Longitudinal-Experimental Studies*

Is it better to keep longitudinal and experimental studies separate, or should new longitudinal-experimental research, with survey data before and after an intervention, be implemented to advance knowledge about the development, causes, prevention, and treatment of offending? Such research has many advantages (specified above). However, the fact that such a project has never been carried out shows that it is difficult. Indeed, it is difficult enough to conduct separate longitudinal and experimental studies; only about 50 large-scale longitudinal studies and about 100 large-scale randomized experiments have been carried out in criminology (Farrington & Welsh, 2006, 2007).

And yet, it is not clear that adding an experiment would interfere with the goals of a longitudinal survey, or that those who receive an intervention should be deleted from developmental analyses. For example, most of McCord's (1979, 1982) analyses of the childhood antecedents of adult criminal behavior were carried out with the experimental group, because more extensive information was collected about these boys than about the control group. As mentioned, an experimental intervention could be viewed as one of many interventions that impinge on all persons over time.

There is, however, one finding in the literature that suggests that relationships between risk factors and offending might differ in experimental and control groups. In analyses of the effect of a nurse home-visiting program, Eckenrode et al. (2001) found that child maltreatment predicted early onset problem behaviors by the child only in the control group. They concluded that the home visits had reduced the impact of this risk factor in the experimental group.

Existing longitudinal-experimental studies (in which persons were followed up after an experiment) should be reanalyzed to investigate whether development is different in experimental and control groups and whether risk and protective factors

and life events have different effects in experimental and control groups. It would also be important to investigate whether long-term attrition is different in experimental and control conditions. In addition, it would be valuable in randomized experiments to analyze official offending data for several years before and after the intervention, to investigate the effects of the intervention on trajectories of offending and the amount of money saved.

Very little is known about the advantages and problems of longitudinal-experimental studies with several years of survey data before and after an intervention. In light of its advantages, a great deal could be learned by mounting such a study. Two decades ago, Farrington et al. (1986a) recommended following up four cohorts from birth to age 6, age 6 to age 12, age 12 to age 18, and age 18 to age 24, with interventions at ages 3, 9, 15, and 21. They suggested preschool and parent training interventions in infancy and childhood, peer and school programs at age 15, and employment and drug programs at age 21.

In our opinion, it would be highly desirable to mount new longitudinal-experimental studies that have at least three years of personal contacts with the subjects before and after an intervention, and that also have repeated, frequent data collection from a variety of sources. Large samples would be needed to have sufficient statistical power to investigate risk and promotive factors, criminal career parameters, and the effects of interventions on offending. These kinds of studies would not be cheap, although one way of minimizing the cost might be to add an experimental intervention to an existing longitudinal study. However, they could lead to significant advances in knowledge about the explanation, prevention, and treatment of offending and antisocial behavior.

## **Quasi-Experimental Approaches**

In practice, it is easier to carry out quasi-experimental analyses within prospective longitudinal surveys than to implement experimental interventions in such surveys. A quasi-experimental analysis tries to isolate the impact of a naturally occurring presumed causal factor (e.g., joining a gang) by treating it as though it was experimentally manipulated and then trying to eliminate plausible alternative explanations of observed effects discussed above. Prospective longitudinal data constitute the foundation for quasi-experimental approaches in studying the causes of offending.

Sometimes a catastrophe or a major positive event occurring in a population may trigger vast changes in criminality. For example, Costello, Compton, Keeler, and Angold (2003) examined the impact of the opening of a casino on an American Indian reservation, which took place in the course of a longitudinal study by the authors (the Great Smoky Mountains Study of Youth). The revenue from the casino was shared by every adult and child tribe member. This led to a reduction in Indian families with income below the federal poverty line, while non-Indian families did not benefit from the casino revenue. Children in Indian families showed a significant improvement in behavioral symptoms of oppositional/defiant and conduct disorder.

This study is exemplary in that it benefits from a natural event that affected some families compared to control families, established temporal order, and shed light on mediating factors.

Almost all studies of the causes of offending have carried out analyses between individuals showing, for example, that unemployed people commit more crimes than employed people and that this relationship holds after controlling for measured extraneous variables. As Farrington (1988) stated, "Causes are often inferred from variations between individuals rather than from changes within individuals" (p. 158). All studies on causation share a basic principle, but differ in the way that this principle applies to causal investigations. The principle is that investigations are best accomplished when there is variance in both the predictor and the outcome. Thus, it is impossible to investigate the putative causal status of a factor on offending if that factor cannot be scaled (either as present or absent, or on a more continuous scale). However, there usually is a range of options of variance in either the predictor or the outcome which can provide unusual power for the causal investigation.

Inherently, all inferences about causes are probabilistic (Farrington, 1988). As mentioned earlier, a succession of probes is needed to narrow down factors correlated with delinquency to those risk factors that have a causal status (Kraemer et al., 2005). Although the sequences of probes in quasi-experimental studies is more limited than in experimental studies, there are additional options. For example, Loeber and Stouthamer-Loeber (1986) undertook a meta-analysis of family factors predictive of delinquency. Inspired by Rutter's approach (1981) to risk and causal factors, Loeber and Stouthamer-Loeber (1986) argued that:

- a. Causally related variables are always intercorrelated with each other, and may have reciprocal effects.
- b. Causality requires that the putative causal factor precedes delinquency and a demonstration that the factor predicts delinquency.
- c. When family factors have a causal status then it is likely that more than one child in the family will be affected.
- d. If several family conditions are known to predict delinquency, then multiple handicaps within families are likely to result in an increased probability of delinquency in the offspring.
- e. There is a dose-response relationship, in that the higher the family handicaps the higher the probability of delinquency in the offspring.
- f. Changes in family functioning are followed by changes in the behavior of the offspring.

In summary, the determination of which risk factors operate as causal factors can be achieved through series of systematic tests. Each successful test will narrow down the causal status of hypothesized factors.

## *Causes Within Individuals*

For every study on causes within individuals, there are hundreds of studies of causes explaining between-individual variation in offending. This anomalous situation occurs despite the fact that within-individual causes are more important for interventions than between-individual causes. The key element in studying causes is investigating the behavior of the same individuals under different conditions (e.g., changing from employment to unemployment) and studying whether offending changes as a result of life transitions or exposure to risk factors (Farrington, 1988). Quasi-experimental analyses within individuals control for individual factors that do not change over time (e.g., gender and race). Since such factors cannot vary within individuals, it is arguable whether gender and race should ever be viewed as causal factors. However, the causal status of factors that are correlated with gender, such as changes in hormones, can be studied at the within-individual level.

Where a presumed cause of offending can be systematically manipulated in a randomized experiment, convincing conclusions can be drawn about causation. However, few presumed causes can be systematically manipulated, except for situational influences in field experiments on dishonesty (see Farrington, 1979a, 2007). In nonexperimental research on the causes of offending, more convincing conclusions can be drawn from quasi-experimental within-individual analyses than from between-individual analyses with statistical control of extraneous variables or structural modeling.

Quasi-experimental analyses within individuals have been carried out in the Cambridge Study in Delinquent Development. For example, getting convicted led to an increase in self-reported offending, and a plausible intervening mechanism was increased hostility to the police (Farrington, 1977). Males committed more offenses during periods of unemployment than during periods of employment, but only crimes of financial gain, such as theft, burglary, robbery, or fraud (Farrington, Gallagher, Morley, Ledger, & West, 1986b). Getting married was followed by a decrease in offending, while separating from a wife was followed by an increase (Farrington & West, 1995). Other examples are changes in individuals' offending as a result of entering and subsequent exiting of a gang (Gordon et al., 2004), and starting and terminating drug dealing (van Kammen & Loeber, 1994). The Gordon et al. (2004) study controlled for selection effects, in that boys who joined gangs were more delinquent before entering the gang than those who did not join.

We are aware of only one paper in criminology that compared whether causes identified by means of within-individual analyses were similar to or different from causes identified by means of between-individual analyses (Farrington et al., 2002; see Verthein & Köhler, 1997 for an example in medicine). Farrington et al. (2002) examined the course of offending of 506 boys in the oldest sample of the Pittsburgh Youth Study over seven data waves between ages 14 and 18 on average. Putative causes were only examined if they were available at each data wave. These putative causes were HIA problems (hyperactivity-impulsivity-attention problems), low achievement, depressed mood, poor supervision, low reinforcement, poor communication, low involvement, low SES, poor housing, and peer delinquency. The between-individual correlations were computed for each wave and then averaged across the 7 waves. In contrast, the within-individual correlations were calculated

for each boy (based on 7 waves) resulting in 370-380 correlations for boys who had admitted at least one delinquent act.

All ten variables were significantly correlated with delinquency in the between-individual analyses. The within-individual correlations with delinquency were on average lower, and statistically significant for only four variables: peer delinquency, poor supervision, low involvement in family activities, and poor parent-boy communication. To test whether the associations held prospectively, subsequent analyses investigated whether variables in one wave predicted delinquency in the next wave. Only poor supervision, low reinforcement, and low involvement predicted within individuals. We concluded that, although peer delinquency is correlated with offending between individuals, it is not a within-individual cause of offending. However, poor supervision, low involvement in family activities, and low parental reinforcement appeared to be causes in that, as they rose or fell over time, the delinquency of most participants would subsequently rise and fall as well. Thus, temporal covariation between putative risk factors and offending is one of the strictest criteria for causal status.

The results also showed that individuals varied considerably in their within-individual correlations between predictors and delinquency. For some, the correlation was negative, for others it was zero, and for others it was positive. Averaged across individuals, the within-individual correlation tended to be in the direction of the between-individual correlation. The point, however, is that causal factors that explain between-individual differences in offending are not necessarily operating in the same direction for all individuals. Thus, the causal status of a particular variable may vary from individual to individual. For example, poor housing was positively related to delinquency for boys living in bad neighborhoods but not for boys living in good neighborhoods.

It should be mentioned that modern statistical techniques (e.g., HLM and MLWIN) make it possible simultaneously to study between-individual and within-individual causes of delinquency. However, we are not aware that these techniques have yet led to a body of knowledge allowing comparisons between within-individual and between-individual causes of delinquency.

### *Underlying Causes*

Probably one of the most controversial issue in the causation of delinquency and antisocial behavior is which underlying (and often unobserved) causes are responsible for between-individual differences in offending. Numerous underlying causes have been proposed, such as low intelligence, poor executive functioning, hyperactivity, sensation seeking, impulsivity, poor self-control, poor emotional regulation, and so on (Amdur, 1989; Loeber, 2003). Most of the underlying factors are thought to be stable over time, but how early they emerge as stable phenomena is often not substantiated. Researchers and theorists agree in broad outline about the manifestations of antisocial and delinquent behavior, but they disagree about the nature of underlying factors or predispositions to antisocial behavior and delinquency. However, almost always researchers assume that underlying factors are essential elements of theories of antisocial behavior and delinquency.

Whereas the operationalization of the underlying factors is consistent in some areas (e.g., intelligence), different approaches to measurement and operationalization are very common (e.g., for executive functioning and impulsivity). Impulsivity, for example, is notoriously difficult to operationalize, partly because different measures of impulsivity often correlate very poorly (White et al., 1994). Investigators often differ in their operationalization of deficits of executive functioning (Giancola & Mezzich, 2000; Moffitt, 1993).

Ideally the magnitude of the association between an underlying factor and delinquency should be similar among studies. However, systematic reviews often show that the strength of association varies considerably from study to study (this applies, for example, to lack of empathy; see Jolliffe & Farrington, 2004). The fact that different underlying factors have different strengths of association with delinquency teaches us that not all underlying factors are equal. At this point, we also know very little about developmental aspects of different underlying factors (probably some emerge early, while others become more distinct later). It is also clear that the prevalence of some of the factors may decrease with development. This applies to hyperactivity (Hart, Lahey, Loeber, Applegate, & Frick, 1995), but it is also true for sensation seeking and impulsivity (Loeber, 2003).

A key question is whether the upslope and downslope of the age-crime curve can be explained by changes in underlying factors. We think that plausible candidates for explaining the upslope are a combination of the lowering of fearfulness from childhood to adolescence and an increase in sensation seeking and recklessness. Although it is likely that there are population variations in these and other underlying factors, we still know very little about which causes account for between-individual differences in changes in the underlying factors. For example, why is it that some individuals outgrow age-normative forms of delinquency earlier than others?

Several of the hypothesized underlying factors are thought to be biological. Increasingly, research findings have documented differences between delinquents and nondelinquents on low autonomic arousal, including low heart rate and low skin conductance (Raine, 1993). Neurotransmitters have been implicated, such as cortisol (McBurnett, Lahey, Capasso, & Loeber, 1996), revealing that neurotransmitters tend to be more strongly linked to violence than to property crime. Genetic investigations, often through twin or adoption studies (e.g., DiLalla, 2002) have postulated that delinquents and nondelinquents differ in terms of heredity. More recently, the research in this area has moved to investigating the molecular genetic bases of delinquency, but the focus is more on the genetic foundations of underlying factors than on delinquency itself (Rowe, 2002).

### *Causes of Nondelinquency and Causes of Desistance*

Most theories of delinquency focus on the explanation of deviance rather than its absence. In those cases where criminologists have been interested in the explanation of nondelinquency (e.g., Hirschi, 1969), the tools of explanation have been

the inverse of those used in theories that aimed to explain delinquency. For example, good bonding with parents was seen as a factor fostering nondelinquency, whereas poor bonding with parents was considered a cause of delinquency. Despite these good intentions, much more is known about the causes of delinquency than the causes of nondelinquency. Particularly important are causes that explain why some individuals do not commit serious delinquent acts such as robbery, aggravated assault, rape or homicide. Also crucial is the study of causes that can explain why individuals desist from serious delinquency.

It can be argued that understanding the causes of delinquency automatically leads to understanding the causes of nondelinquency. However, this is only the case for putative causal factors that are dichotomous (e.g., one or two parent families), because there is no new information in the opposite end of the spectrum. In the case of continuous factors, it is often the case that there are linear associations with delinquency, in that a high level of a factor is predictive of high delinquency, while a low level is predictive of low or no delinquency (called mixed promotive and risk effects). However, research shows nonlinear relationships as well (Loeber, Farrington, Stouthamer-Loeber, & White, *in press*; Stouthamer-Loeber, Loeber, Wei, Farrington, & Wikström, 2002). One type of nonlinear effect (called a promotive effect) is when some factor at one end predicts low or no delinquency (or the presence of prosocial behavior) but does not predict high delinquency at the other end, while in another nonlinear case (called a risk effect), a factor predicts high delinquency at one end but does not predict low or no delinquency at the other end.

Currently, theories about delinquency and antisocial behavior largely focus on the explanation of deviancy rather than its absence (see overviews in Farrington, 2005; Thornberry & Krohn, 2003). Yet, increasingly, explanations of delinquency include discussions of interventions that advance positive actions. In addition, explanations of why some individuals desist while others persist often rely on promotive and protective factors. These are major reasons why the array of plausible causes of delinquency also needs to include positive causal influences as well as negative ones.

It is also important to investigate protective factors, which are defined as variables that tend to nullify the effect of a risk factor (in an interaction effect; see Rutter, 1985). For example, if poor parental supervision predicted a high risk of offending only for males from low-income families, and not for males from high-income families, then high income might be regarded as a protective factor counteracting the effects of the risk factor of poor supervision. More research is needed to identify protective factors, and this should lead to interventions targeting protective factors (e.g., strengthening resilience).

## Conclusions

In reviewing causality problems arising in longitudinal studies more than ten years ago, we stressed (Loeber & Farrington, 1994) that the major issues were attrition, testing effects, the distinction between aging, period, and cohort effects, and

establishing causes with high internal validity. All these issues remain crucial. We also stressed, as we do here, the need to combine longitudinal and experimental studies, or at a minimum turn experimental studies into longitudinal studies so that the long-term impact of change agents can be ascertained. In addition, it has become clearer to us that we need to re-invigorate the search for causes in at least the following ways:

- We need a web-based inventory of what is known about the causal status of the sixty some known putative causes of delinquency. Such an inventory should not only summarize effect sizes but should also show which of the several crucial tests have been accomplished in narrowing down risk factors into causes. Ideally, such a website should also contain information about promotive causes that foster nondelinquency and can aid in explaining desistance. Systematic reviews and meta-analyses should be useful here (e.g., Farrington & Petrosino, 2001).
- A systematic survey is needed of the moderators and mediators of causes of delinquency. This will be of immense help in documenting processes that unfold over time.
- Causes that operate within individuals should be more vigorously investigated so that eventually meta-analyses across different studies can be undertaken, leading to better generalizations about what is known about within-individual causes and their implications for interventions.
- The investigation of causes always has purposes, of which theory and interventions are the ones most often cited. Another more rarely quoted purpose is the calculation of which causes are the most costly for society, and what the cost-benefit ratios are for reducing some rather than other causes. Here comes into play another set of analytic tools that model putative causes in relation to outcomes. For example, Ebel et al. (2007) examined the degree to which U.S. homicide rates might be lowered when the best preventive and remedial interventions were applied to populations of youth. Finding that one-third of homicides could be prevented, the authors then estimated that the incarceration cost could be reduced by \$5 billion. Another example is the calculation of to what extent changes in the age-crime curve brought about by efficacious interventions could result in reductions in crime rates, arrest rates, and incarceration rates (Loeber, 2006). Simulation studies, however, can never be better than our understanding of basic causal processes.

We also recommend an increased effort to study within-individual changes in variables followed by changes in delinquency because causes can be established more convincingly in this type of research. We need to establish which of the between-individual risk and promotive factors also apply in within-individual analyses. We think that this type of investigation would have clearer implications for future intervention studies. While a great deal has been learned from previous longitudinal studies, great advances could be made in longitudinal studies that include experimental and quasi-experimental investigations.

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# Appendix: Longitudinal Studies Reported on in this Volume

Akiva M. Liberman

The recent explosion in longitudinal research reports is well exemplified by the reviews in this volume. I have identified 64 longitudinal data sets of delinquency or aggression which these reviews draw upon. Yet, for most substantive questions, only a few of the relevant longitudinal data sets have been examined. Individual reviews in this volume report on findings from 8 to 29 of these longitudinal studies. Many of the studies not drawn upon represent analytic opportunities that have not yet been undertaken.

This Appendix consists of two tables. Table 1 shows which of the 64 longitudinal data sets were used in which review. Table 2 contains some basic features of each study, including study subjects' birth years, age at initiation of the study, the timing of follow-up data collection, approximate sample size, and sample inclusion by race and gender. A representative citation is provided for each study, and a web-site address is provided, if known.

The Appendix is restricted to longitudinal data sets which contain measures of offending, aggression, or anti-social behavior. While some other longitudinal studies are also referenced in some chapters, they are not listed here.

Of the 64 such longitudinal data sets identified, 9 are national U.S. studies, 35 are local U.S. studies, and 20 are of studies outside the U.S. Most of these studies involve subjects born since the 1970s. The findings from these studies have been reported in about 200 empirical papers cited in this volume; that majority of these have been published since 2000.

**Table 1** Longitudinal studies cited by review chapters, sorted by study location

Birth years (approx)	Study	Ch. 2. Trajectories	Ch 3. Early Childhood	Ch 4. Gangs	Ch 5. Adult Roles	Ch 6. Employment	Ch 7. Arrests
<b>U.S. NATIONAL STUDIES</b>							
1. 1957–1961	Monitoring the Future					X	
2. 1959–1965	National Collaborative Perinatal Project		X				
3. 1977–1982	National Longitudinal Study of Adolescent Health					X	
4. 1957–1964	National Longitudinal Survey of Youth 1979	X					X
5. 1980–1984	National Longitudinal Survey of Youth 1997					X	
6. 1961	National Supported Work Demonstration					X	
7. 1965–1969	National Survey of Children		X				
8. 1959–1965	National Youth Survey	X			X	X	X
9. 1991	NICHD Study of Early Child Care and Youth Development	X					

**U.S. CITY/STATE STUDIES, SORTED  
BY LOCATION**

10.	1977	Bloomington (IN) Longitudinal Study	X			
11.	1975-1982	Bloomington, IN, Knoxville, TN, & Nashville, TN: Child Development Project	X			
12.	1924-1935	Boston: Glueck Delinquents Sample	X	X	X	X
13.	1973-1976	Buffalo (NY) Longitudinal Survey of Young Men		X		
14.	1946-1955	California Youth Authority parolees released 1966-1972	X		X	
15.	1956-1978	California Youth Authority parolees released 1981-1992	X			
16.	1964-1974	Chicago (public housing sample)			X	
17.	1976	Cleveland Hospital study	X			

(continued)

Table 1 (continued)

	Birth years (approx)	Study	Ch. 2. Trajectories	Ch 3. Early Childhood	Ch 4. Gangs	Ch 5. Adult Roles	Ch 6. Employment	Ch 7. Arrests
18.	1972–1981	Denver Youth Study						
19.	1978–1980	Elmira (NY): Olds Elmira study		X	X		X	X
20.	1975	Iowa (small town sample)				X		
21.	1945–1974	Iowa: University of Iowa Adoption study		X				
22.	1990–1992	Memphis, TN: Olds Memphis Treatment study		X				
23.	1975–1977	Minnesota Parent-Child Project		X				
24.	1972 mean	Nebraska inmates				X		
25.	1961–1967	New Jersey: Rutgers Health and Human Development Project	X					
26.	1978–1979	New York City Hospital study		X				
27.	1973	New York State: Middle Adolescent Vulnerabil- ity Study (a.k.a. Lives Across Time)	X					
28.	1979–1983	North Carolina: Great Smoky Mountain Study	X					
29.	1961–1966	Ohio institutional sample				X		

30.	1973-1975	Oregon Youth Study	X					
31.	1975-1980	Pennsylvania and Georgia: Developmental Trends Study		X				
32.	1945	Philadelphia 1945 Birth Cohort	X				X	X
33.	1958	Philadelphia 1958 Birth Cohort	X					
34.	1983-1987	Philadelphia and Phoenix: Pathways to Desistance Study	X					
35.	1998-2000	Pittsburgh: Early Steps Pilot Study		X				
36.	1989-1992	Pittsburgh Mother and Child Project	X	X				
37.	1974-1981	Pittsburgh Youth Study	X		X			X
38.	1942, 1949, 1955	Racine, WI 1942, 1949, 1955 birth cohorts	X					X
39.	1975	Rochester Youth Development Study	X		X		X	X
40.	1975	Seattle Social Development Project	X		X			X
41.	1970-1974	St. Paul, MN: Youth Development Study					X	

(continued)

Table 1 (continued)

Birth years (approx)	Study	Ch. 2. Trajectories	Ch. 3. Early Childhood	Ch. 4. Gangs	Ch. 5. Adult Roles	Ch. 6. Employment	Ch. 7. Arrests
42. 1966–1989	Washington State: University of Washington Fetal Alcohol Follow-up Study		X				
43. 1988–1992	Washington State, U.S. & Victoria, Australia (grade 5–7)						X
44. 1958–1964	Ypsilanti, Michigan: High/Scope Perry Preschool Project		X				
<b>INTERNATIONAL STUDIES, BY COUNTRY</b>							
45. 1984	Australia: South Australia Study	X					
46. 1978	Canada: Montreal Longitudinal and Experimental Study	X	X	X			
47. 1996	Canada: Quebec (infants)	X					
48. 1980	Canada: Quebec Longitudinal Study	X					X
49. 1959–1961	Denmark (born 1959–1961)		X				
50. 1944–1947	Denmark: 1944–1947 Birth Cohort						X
51. 1953	England: Home Office Offenders Index Birth Cohort 1953	X					
52. 1969–1970	England, South London: Waltham Forest Borough		X				

53.	1952-1954	England, London: Cambridge Study in Delinquent Development	X			X	X
54.	1966	Finland: Northern Finland		X			X
55.	1974-1975	Germany: Bremen School to Work Study				X	
56.	1980-1983	Germany: adolescents	X				
57.	1967-1979	Holland: Zuid-Holland Longitudinal Study	X				
58.	1912-1965 (mean = 1948)	Netherlands: Criminal Career and Life-Course Study	X			X	
59.	1924-1981	Netherlands: Dutch National Crime Survey				X	X
60.	1977	New Zealand: Christchurch Health and Development Study					X
61.	1972-1973	New Zealand: Dunedin Multidisciplinary Health and Human Development Study	X		X		X
62.	1969-1972	Norway, Bergen: Olweus				X	
63.	1953	Sweden, Stockholm (1953 birth cohort)		X			
64.	1955-1958	Sweden, Stockholm: Clinic for the Study of Children's Development and Health		X			

**Table 2** Longitudinal study features, sorted by study location

Birth years (approx)	Study	Sample	Approx sample size (n); Sex (M, F); Race/ethnicity – if limited	Age, yr at Initiation	Follow-up Data Collection	Sample Citation	Website
<b>U.S. NATIONAL STUDIES</b>							
1. 1957–1961	Monitoring the Future	8th, 10th & 12th grade high school students, sampled since 1975, (annual n about 50,000)	longitudinal n = 3,000; M & F	14–18 in 1975	since 1976, follow up data collected biannually on subsample	Osgood, Wilson, O’Malley, Bachman, and Johnston (1996)	<a href="http://monitoringthefuture.org">http://monitoringthefuture.org</a>
2. 1959–1965	National Collaborative Perinatal Project	Children of maternity care patients at public clinic at Pennsylvania Hospital	school and CJ records for sample n = 3,000; M & F; African-American	at birth	follow-up exams at 4, 8, & 12 mos., and 3, 4, 7, 8 yrs.	Piquero, Gibson, Tibbetts, Turner, and Katz (2002)	<a href="http://permanent.access.gpo.gov/lps6793/www.nara.gov/nara/par/electronic/ncpp.html">http://permanent.access.gpo.gov/lps6793/www.nara.gov/nara/par/electronic/ncpp.html</a>
3. 1977–1982	National Longitudinal Study of Adolescent Health	nationally representative of students in grades 7–12	n = 21,000; M & F	grades 7–12 in 1994	follow-up interviews at 1, 2, 6 yrs.	Johnson (2004)	<a href="http://www.epc.unc.edu/addhealth">http://www.epc.unc.edu/addhealth</a>
4. 1957–1964	National Longitudinal Survey of Youth 1979	nationally representative (n=6,100); black, Hispanic, economically disadvantaged oversample (n = 5,300); military sample (n = 1,300)	n = 12,000; M & F	14–22 in 1979	interviewed annually through 1994, and biennially since	Hynes and Clarkberg (2005)	<a href="http://www.bls.gov/nls/">http://www.bls.gov/nls/</a>

5. 1980–1984	National Longitudinal Survey of Youth 1997	nationally representative; blacks and Hispanics oversampled	n = 9,000; M & F	12–17 in 1997	annual follow-up interviews	Paternoster, Bushway, Brame, and Apel (2003)	<a href="http://www.bls.gov/nls/">http://www.bls.gov/nls/</a>
6. 1961	National Supported Work Demonstration	criminal offenders, drug users, and youth dropouts with a history of unemployment, from 9 cities	n = 3,000; M & F	ex-offender group's mean age = 25 at initiation	follow-up interviews: total sample at 9 & 18 mos; subsample at 27 & 36 mos.	Uggen (2000)	<a href="http://www.indrc.org/publications/316/abstract.html">http://www.indrc.org/publications/316/abstract.html</a>
7. 1965–1969	National Survey of Children	nationally representative	n = 2,300; M & F	7–11 in 1976	follow up in 1981, 1987	Morash and Rucker (1999)	<a href="http://www.sscnet.ucla.edu/issr/da/index/technfo/M0681.HTM">http://www.sscnet.ucla.edu/issr/da/index/technfo/M0681.HTM</a>
8. 1959–1965	National Youth Survey	nationally representative	n = 1,700; M & F	11–17 in 1976	annual waves 1976–1980; 1983, 1987, 1990, 1993, 2002, 2003	Bushway (1998)	<a href="http://www.colorado.edu/ibs/NYSFS/">http://www.colorado.edu/ibs/NYSFS/</a>
9. 1991	NICHD Study of Early Child Care and Youth Development	from 10 U.S. Cities	n = 1,400; M & F	1 mo. in 1991	phase I: 0 to 3 yrs; phase II: through first grade; phase III: grades 2–6	NICHD (2004)	<a href="http://www.nichd.nih.gov/research/supported/seccyd.cfm">http://www.nichd.nih.gov/research/supported/seccyd.cfm</a>

(continued)

Table 2 (continued)

Birth years (approx)	Study	Sample	Approx sample size (n); Sex (M, F); Race/ethnicity – if limited	Age, yr at Initiation	Follow-up Data Collection	Sample Citation	Website
<b>U.S. CITY/STATE STUDIES, SORTED BY LOCATION</b>							
10. 1977	Bloomington (IN) Longitudinal Study	community sample	n = 168; M & F	6 mos. in 1977	follow up at 12, 24 mos.; varying ages 3–10; continuing to age 17	Bates, Pettit, Dodge, and Ridge (1998)	<a href="http://www.indiana.edu/~batescdl/bls.html">http://www.indiana.edu/~batescdl/bls.html</a>
11. 1975–1982	Bloomington, IN, Knoxville, TN, & Nashville, TN: Child Development Project	recruited at kindergarten enrollment; low SES schools oversampled	n = 600; M & F	5 yrs. in 1987–1988	annual follow up assessments through grade 11	Bates et al. (1998)	<a href="http://www.indiana.edu/~batescdl/cdp.html">http://www.indiana.edu/~batescdl/cdp.html</a>
12. 1924–1935	Boston: Glueck Delinquents Sample	500 delinquents from correctional schools, with matched sample of 500 nondelinquents from public schools	n = 1,000; M only; white only	10–17 at recruitment, study initiated in 1939	follow-up interviews at age 25 & 32; official data collected on delinquents to age 70; subsample reinterviewed around age 70 (n = 52)	Laub and Sampson (2003)	
13. 1973–1976	Buffalo (NY) Longitudinal Survey of Young Men	general population sample, with oversample at high risk for delinquency	n = 600; M only	16–19 in 1992	follow-up interviews at 18 mos., for a planned 5-yr panel study	Zhang, Welte, and Wieczorek (1999)	

14.	1946–1955	California Youth Authority parolees released 1966–1972	parolees released in late teens and early 20s	n = 500; M only	16–28 at release	retrospective; data for 7 consecutive yrs after release	Piquero, MacDonald, and Parker (2002)	<a href="http://webapp.icpsr.umich.edu/cocoon/NACJD-STUDY/03136.xml">http://webapp.icpsr.umich.edu/cocoon/NACJD-STUDY/03136.xml</a>
15.	1956–1978	California Youth Authority parolees released 1981–1992	3 samples of parolees: released in 1981–1982; 1986–1987; & 1991–1992	n = 1,400–2,000 per sample; M & F	18–25 at release	retrospective: arrest data to 2000	Ezell and Cohen (2005)	
16.	1964–1974	Chicago (public housing sample)	public housing sample	n = 118; M only; African-American only	16–26 about 1990	10 years of ethnographic data; follow-up interviews initiated in 2000; extensive retrospective data collection	Levitt and Venkatesh (2001)	
17.	1976	Cleveland Hospital study	children admitted to neo-natal ICU with very low birthweight	n = 500; M & F	born 1976	follow-up at 9 yrs	Breslau, Klein, and Allen (1988)	
18.	1972–1981	Denver Youth Study	sampled from high-risk neighborhoods	n = 1,500; M & F	7–15 in 1987	annual follow-up interviews 1989–1992 & 1995–1999; subsample in 2002	Esbensen and Huizinga (1993)	<a href="http://ojjdp.ncjrs.org/ccd/denver.html">http://ojjdp.ncjrs.org/ccd/denver.html</a>

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Table 2 (continued)

Birth years (approx)	Study	Sample	Approx sample size (n); Sex (M, F); Race/ethnicity – if limited	Age, yr at Initiation	Follow-up Data Collection	Sample Citation	Website
19. 1978–1980	Elmira (NY): Olds Elmira study	children of high-risk first pregnancy to low-income, unmarried women in semi-rural community	n = 400; M & F; White only	born 1978–1980	multiple follow-ups during first 2 yrs; at age 4, 15, & 19 (underway in 2007)	Olds, Hill, and Runsey (1998)	<a href="http://www.nursefamilypartnership.org/content/index.cfm?fuseaction=showContent&amp;contentID=113">http://www.nursefamilypartnership.org/content/index.cfm?fuseaction=showContent&amp;contentID=113</a> & navID = 101
20. 1975	Iowa (small town sample)	children of two-parent families in small towns	n = 236; M & F; white	7th graders in 1989	reinterviewed in 1991, 1997	Simons, Stewart, Gordon, and Elder (2002)	
21. 1945–1974	Iowa: University of Iowa Adoption study	adoptees from adoption agencies in Iowa; half with a biological parent with anti-social personality and/or substance use problems	n = 200; M & F	18–47	retrospective	Cadoret, Yates, Troughton, Woodworth, and Stewart (1995)	
22. 1990–1992	Memphis, TN: Olds Memphis treatment study	children of high-risk first pregnancy to low-income, unmarried women	n = 1,100; M & F; African-American only	born 1990–1992	follow-up at age 4 & 5; post-kindergarten follow-up underway (as of 1-23-07)	Olds et al. (2004)	<a href="http://www.nursefamilypartnership.org/content/index.cfm?fuseaction=showContent&amp;contentID=113">http://www.nursefamilypartnership.org/content/index.cfm?fuseaction=showContent&amp;contentID=113</a> & navID = 101

23.	1975–1977	Minnesota Parent-Child Project	high-risk population from Minneapolis public health clinic	n = 267; M & F; white	born 1975–1977	follow-up at 3, 6, 12, 18, 24, & 30 mos.; yearly through grade 3; 3 times ages 9–13; ages 16, 17.5, 19, 23, 26, & 28	Appleyard, Egeland, Manfred, and Sroufe (2005)	<a href="http://education.umn.edu/icd/Parent-Child/PCPOverview.html">http://education.umn.edu/icd/Parent-Child/PCPOverview.html</a>
24.	1972 mean	Nebraska inmates	incarcerated inmates	n = 650; M only	mean age = 28 in 1989–1990	retrospective month-to-month account, covering 25 to 36 mos., of criminal offenses and local life circumstances	Horney, Osgood, and Marshall (1995)	
25.	1961–1967	New Jersey: Rutgers Health and Human Development Project	random telephone survey of New Jersey	n = 1,400; M & F	12, 15, 18 in 1979–1981	follow up in 1982–1984; 1985–1987; 1992–1994	White, Bates, and Buyske (2001)	<a href="http://sociology.rutgers.edu/graduate/Core%20areas/health.html">http://sociology.rutgers.edu/graduate/Core%20areas/health.html</a>
26.	1978–1979	New York City Hospital study	born prematurely and admitted to the neonatal intensive care unit	n = 87; M & F	born 1978–1979	age 7–8	Ross, Lipper, and Auld (1990)	
27.	1973	New York State: Middle Adolescent Vulnerability Study (a.k.a. Lives Across Time)	grades 10–11 in Western NY	n = 1,200; M & F	15–16 in 1988–1989	follow up at 6, 12, 18 mos. (1989–1991); in 1996 & 2001	Wiesner and Windle (2004)	<a href="http://www.uab.edu/youthhealth/latdetails.html">http://www.uab.edu/youthhealth/latdetails.html</a>

(continued)

Table 2 (continued)

Birth years (approx)	Study	Sample	Approx sample size (n); Sex (M, F); Race/ethnicity – if limited	Age, yr at Initiation	Follow-up Data Collection	Sample Citation	Website
28. 1979–1983	North Carolina: Great Smoky Mountain Study	representative sample of rural youth, with oversample of Native Americans	n = 1,500; M & F	9–13 in 1992	annual follow up to age 16, and every 2 to 3 years thereafter	Mustillo et al. (2003)	
29. 1961–1966	Ohio institutional sample	correctional population sample	n = 250; M & F	16–21 in 1982	follow-up in 1995 & 2003	Schroeder, Giordano, Cermkovich (2007)	
30. 1973–1975	Oregon Youth Study	grade 4 classes in higher-crime areas of a medium-sized metropolitan region in the Pacific Northwest	n = 200; M only	9–10 in 1983–1985	annual follow-up, for at least 17 years	Stoolmiller, Kim, and Capaldi (2005)	
31. 1975–1980	Pennsylvania and Georgia: Developmental Trends Study	boys referred to university mental health clinics (mostly for Disruptive Behavior Disorders) in Pittsburgh, PA, Athens, GA, and rural GA	n = 177; M only	7–12 in 1987	annual follow-up, for at least 13 years	Wakschlag et al. (1997)	<a href="http://www.wpic.pitt.edu/research/famhist/DTS.htm">http://www.wpic.pitt.edu/research/famhist/DTS.htm</a>

32.	1945	Philadelphia 1945 Birth Cohort	boys who lived in Philadelphia from age 10 to 18	n = 10,000; M only	born 1945	police data through age 30; at age 25, interviews conducted with 6.2% subsample	Brame, Mulvey, and Piquero (2001)	<a href="http://www.icpsr.umich.edu/cocoon/NACJD/STUDY/07729.xml">http://www.icpsr.umich.edu/cocoon/NACJD/STUDY/07729.xml</a>
33.	1958	Philadelphia 1958 Birth Cohort	children born in Philadelphia in 1945	n = 27,000; M & F	born 1958	follow-up survey in 1988 (n = 782)	Brame, Bushway, and Paternoster (2003)	<a href="http://www.icpsr.umich.edu/cocoon/NACJD/STUDY/09293.xml">http://www.icpsr.umich.edu/cocoon/NACJD/STUDY/09293.xml</a>
34.	1983–1987	Philadelphia and Phoenix: Pathways to Desistance Study	adolescents adjudicated on serious felonies	n = 1,300; M & F	14–18 in 2001	follow-up interviews every 6 mos. for 3 years; annually thereafter	Piquero, Fagan, Mulvey, Steinberg, and Odgers (2005)	<a href="http://www.adjj.org/content/page.php?cat_id=2&amp;content_id=13">http://www.adjj.org/content/page.php?cat_id=2&amp;content_id=13</a>
35.	1989–2000	Pittsburgh: Early Steps Pilot Study	high risk boys (screened) from Women, Infants, & Children clinics	n = 120; M only	Age 2	age 3, 4, 5.5–6	Shaw, Dishion, Supplee, Gardner, and Arms (2006)	<a href="http://www.pitt.edu/~ppcl/projects.htm">http://www.pitt.edu/~ppcl/projects.htm</a>
36.	1989–1992	Pittsburgh Mother and Child Project	recruited from Pittsburgh Women, Infants, & Children clinics	n = 300; M only	6–17 mos. in 1991	follow up at age 2, 3.5, 5, 6, 8, 10, 15; age 17 planned	Shaw, Lacourse, and Nagin (2005)	<a href="http://www.pitt.edu/~momchild/">http://www.pitt.edu/~momchild/</a>

(continued)

Table 2 (continued)

Birth years (approx)	Study	Sample	Approx sample size (n); Sex (M, F); Race/ethnicity – if limited	Age, yr at Initiation	Follow-up Data Collection	Sample Citation	Website
37. 1974–1981	Pittsburgh Youth Study	sampled from 1st, 4th, & 7th grade; oversample of highly disruptive boys	n = 1,500; M only	7–13 in 1987	follow up every 6-mos. for 5 years; thereafter youngest & oldest samples (1st & 7th grade) interviewed annually	Stouthamer-Loeber, Wei, Loeber, and Masten (2004)	<a href="http://www.wpic.pitt.edu/research/famhist/PYS.htm">http://www.wpic.pitt.edu/research/famhist/PYS.htm</a>
38. 1942, 1949, 1955	Racine, WI 1942, 1949, 1955 birth cohorts	birth cohorts	n = 6,000; M only	born 1942, 1949, 1955	official records	D'Unger, Land, McCall, and Nagin (1998)	<a href="http://www.icpsr.umich.edu/cocoon/NACJD/STUDY/08530.xml">http://www.icpsr.umich.edu/cocoon/NACJD/STUDY/08530.xml</a>
39. 1975	Rochester Youth Development Study	Grades 7–8 in public school; males & high-arrest neighborhoods oversampled	n = 1,000; M & F	13 in 1988	follow-up every 6 mos. to 1992; annually 1994 to 1997; (wave 12 in 1997)	Thornberry et al. (2003)	<a href="http://www.albany.edu/hindelang/youth_study.html">http://www.albany.edu/hindelang/youth_study.html</a>
40. 1975	Seattle Social Development Project	from 18 elementary schools serving high crime neighborhoods	n = 800; M & F	5th grade in 1985 (incl. subsample from 1st grade in 1981)	follow-up in spring 1986; annually 1987–1991; 1993, 1996, & 1999	Hill, Howell, Hawkins, and Battin-Pearson (1999)	<a href="http://depts.washington.edu/ssdp/">http://depts.washington.edu/ssdp/</a>

41.	1970–1974	St. Paul, MN: Youth Development Study	high school students	n = 1,000; M & F	High school students in 1988	annual follow-up surveys for 12 years	Staff and Ugeen (2003)	<a href="http://www.extension.umn.edu/distribution/familydevelopment/components/7565_07.html">http://www.extension.umn.edu/distribution/familydevelopment/components/7565_07.html</a>
42.	1966–1989	Washington State: University of Washington Fetal Alcohol Follow-up Study	children diagnosed with fetal alcohol syndrome	n = 400; M & F	at least 6 yrs old	retrospective; life history interviews	Streissguth et al. (2004)	<a href="http://depts.washington.edu/fadu/FADU.projects.html#FASfollow">http://depts.washington.edu/fadu/FADU.projects.html#FASfollow</a>
43.	1988–1992	Washington State, U.S. & Victoria, Australia (grade 5–7)	5th, 7th, 9th graders	n = 2,900 in each site; M & F	10–15 in 2002	follow-up one year later	Hemphill, Toumbourou, Herrenkohl, McMorris, and Catalano (2006)	
44.	1958–1964	Ypsilanti, Michigan: High/Scope Perry Preschool Project	poor and at high risk of failing in school	n = 123; M & F; African-American only	3–4 in 1962–1967	annual follow up to age 8; at 10, 15, 23, 27, & 40	Weikart (1998)	<a href="http://www.highscope.org/Research/PerryProject/perrymain.htm">http://www.highscope.org/Research/PerryProject/perrymain.htm</a>

(continued)

Table 2 (continued)

Birth years (approx)	Study	Sample	Approx sample size (n); Sex (M, F); Race/ethnicity – if limited	Age, yr at Initiation	Follow-up Data Collection	Sample Citation	Website
<b>INTERNATIONAL STUDIES, BY COUNTRY</b>							
45. 1984	Australia: South Australia Study	apprehended by police at least once at ages 10–17	n = 3,300; M & F	born in 1984	criminal records only, through age 20	Marshall (2005)	
46. 1978	Canada: Montreal Longitudinal and Experimental Study	kindergarteners from low income families	n = 1,000; M; white	6 in 1984	annual follow-up through 1995; 1998–2000	Tremblay, Vitaro, Nagin, Pagani, and Seguin (2003)	<a href="http://www.pubinfo.vcu.edu/vabp/program_details.asp?id=124">http://www.pubinfo.vcu.edu/vabp/program_details.asp?id=124</a>
47. 1996	Canada: Quebec (infants)	representative sample of families with a 5-mo. old	n = 500; M & F; white	5 mos. in 1996	17, 30 42 mos.	Tremblay et al. (2004)	
48. 1980	Canada: Quebec Longitudinal Study	representative of kindergarteners; oversample of highly disruptive children	N = 2,000; M & F; white	6 in 1986–1987	annual follow-up to age 12; reinterviewed at 15	Cote, Tremblay, Nagin, Zoccolillo, and Vitaro (2002)	
49. 1959–1961	Denmark (born 1959–1961)	males from birth cohort	n = 4,300; M; white	born 1959–1961	follow-up at ages 1, 2; police and court records at 17–19 & 33–35 yrs.	Raine, Brennan, and Mednick (1997)	

50.	1944–1947	Denmark: 1944–1947 Birth Cohort	males from birth cohort	n = 29,000; M; white	born 1944–1947	police and court records through 1973	Brennan and Mednick (1994)
51.	1953	England: Home Office Offenders Index Birth Cohort 1953	birth cohort	n = 11,000; M & F; white	born 1953	official records to 1993	Francis, Soothill, and Fligelstone (2004)
52.	1969–1970	England, South London: Waltham Forest Borough	general population sample	N = 800; M & F; white	age 3	conviction records through 1993	Stevenson and Goodman (2001)
53.	1952–1954	England, London: Cambridge Study in Delinquent Development	general population sample from South London	n = 411; M	8–9 in 1961–1962	follow-up at ages 10, 14, 16, 18, 21, 25, 32, 48	Piquero, Farrington, and Blumstein (2007)
54.	1966	Finland: Northern Finland	birth cohort	n = 11,000; M & F; white	born 1966	assessment at age 1; official records through age 28	Rasanen et al. (1999)

(continued)

Table 2 (continued)

Birth years (approx)	Study	Sample	Approx sample size (n); Sex (M, F); Race/ethnicity – if limited	Age, yr at Initiation	Follow-up Data Collection	Sample Citation	Website
55. 1974–1975	Germany: Bremen School to Work Study	9th & 10th grade students	n = 700; M & F; white	13–14 in 1988	follow-up in 1993, 1995, 1997	Huizinga, Schumann, Ehret, and Elliott (2003)	
56. 1980–1983	Germany: adolescents	general population sample; E. Germans oversampled	n = 700; M & F; white	10–13 in 1993	three yearly follow-up assessments	Wiesner and Silbereisen (2003)	
57. 1967–1979	Holland: Zuid-Holland Longitudinal Study	general population sample	n = 2,000; M & F; white	4–16 in 1983	follow ups at 2-year intervals until 1991; 1997	Bongers, Koot, van der Ende, and Verhulst (2004)	
58. 1912–1965 (mean = 1948)	Netherlands: Criminal Career and Life-Course Study	4% sample of criminal cases tried in Netherlands in 1997	n = 4,600; M & F; white	12–65 in 1977 (mean = 29)	official criminal data combined with official life-circumstance data	Blokland and Nieuwbeerta (2005)	
59. 1924–1981	Netherlands: Dutch National Crime Survey	representative population sample over 15, oversample of 15–30	n = 2,200; M & F; white	15–72 in 1996	retrospective life history interviews	Blokland and Nieuwbeerta (2005)	

60.	1977	New Zealand: Christchurch Health and Development Study	birth cohort	n = 1,300; M & F; white	born 1977	follow-up at 4 mos. & 1 yr., annually to age 16; at 18, 21 & 25	Fergusson, Horwood, and Woodward (2001)	<a href="http://www.chmeds.ac.nz/research/chds/index.htm">http://www.chmeds.ac.nz/research/chds/index.htm</a>
61.	1972–1973	New Zealand: Dunedin Multidisciplinary Health and Human Development Study	birth cohort	n = 1,000; M & F; white	born 1972–73	follow-up at ages 3, 5, 7, 9, 11, 13, 15, 18, 21, 26	Moffitt and Caspi (2001)	<a href="http://www.iop.kcl.ac.uk/departments/?locator=351">http://www.iop.kcl.ac.uk/departments/?locator=351</a>
62.	1969–1972	Norway, Bergen: Olweus	school-based representative sample	n = 1,300; M; white	grades 5–8 in 1983	follow-ups in 1984 & 1985	Bendixen, Endresen, and Olweus (2006)	
63.	1953	Sweden, Stockholm (1953 birth cohort)	birth cohort	n = 15,000; M & F; white	born 1953	official records only, to ages 30	Hodgins, Kratzer, and McNeil (2001)	
64.	1955–1958	Sweden, Stockholm: Clinic for the Study of Children's Development and Health	representative sample	n = 212; M & F; white	born 1955–1958	five follow ups to age 2; 3, 4, 5, 8, 11, 14, 17	Stattin and Klackenborg-Larsson (1993)	

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