AN ASSESSMENT OF DETERRENT AND LABELING EFFECTS FOR VIOLENT OFFENDING SUBPOPULATIONS: A CONTEMPORARY METHODOLOGICAL APPROACH

By

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To my father, the late Edward David Ward, Sr.

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Traditional criminal justice responses to violations of the law remain paramount in efforts to control violence. Two competing theoretical propositions predict opposite effects of official intervention on crime. Specific deterrence theory suggests that official intervention results in the reduction of criminal behavior, whereas labeling theory suggests that it ultimately results in the amplification of criminal behavior. Using this theoretical backdrop, scholars have attempted to discern whether deterrence or labeling theory is correct which has resulted in mixed and inconclusive empirical findings. More recently, scholars have acknowledged that both labeling and deterrence theories may be relevant explanations of behavior since the effects of sanctions may be contingent on offender characteristics and may also differ in the short-term and long-term. However, key methodological problems and exacting data requirements have resulted in few empirical studies that have been able to generate much needed practical knowledge about how best to control violence with traditional criminal justice measures.

The current study places deterrence and labeling theories in a life-course and developmental context to study how official intervention may serve as a potential turning

point in a criminal trajectory. Informed by theoretical advances in life-course criminology, the study empirically determines for which violent offending subpopulation(s) official intervention is more likely to result in an increase in violent behavior and for which subpopulation(s) it is more likely to result in a decrease in violent behavior. Moreover, the current study assesses differences in sanction effects in the short-run and long-run across different offending subpopulations. Using data from the Rochester Youth Development Study, the current study employs both propensity score matching to account for possible selection artifacts and latent class growth analysis to bypass problems of subjective classification.

Findings indicate that three trajectory groups emerge from the data including high offenders, non-offenders, and low offenders. Propensity score matching was highly successful in removing bias in covariates for the total sample as well as for the non-offending and low offending trajectory groups. It was not successful at removing pre-matching covariate imbalances, however, for the high offending trajectory group. The treatment effect estimates provide partial support for short-run labeling effects for the sample as a whole and for the low offending trajectory group. Limitations of the study and directions for future research are discussed.

CHAPTER 1
INTRODUCTION

Youth Violence and Crime Control

Violent crime rates have begun to stabilize in recent years, following roughly a ten year period of steady declines (Rand, 2008). While national trends over the past two decades would not appear to be especially alarming, concerns over violent crime remain consistently high over time (Zimring, 1998) and recent years have been characterized by a relentless worry over the "*coming generation of 'super-predators'*" (McCollum, 1996, p. 3, *emphasis in original*). Interlaced within this pervasive disquietude over violent crime are specific points of emphasis that typically wax and wane over time. In the mid-1970s, youth violence took center stage during a brief three-year period (Zimring, 1998). A second coming of anxiety over the violent crime of adolescents has reemerged during the 1990s (Zimring, 1998) but, unlike its antecedent version, it has persisted as a topic of serious social concern today (e.g., see MacDonald et al., 2009, p. 1).

The problem of youth violence presents unique challenges for public policies, which have to balance concerns over youth development and crime control. Adolescence is viewed as a time period of extensive personal growth and development. It is during this time frame that individuals acquire the bulk of the social and human capital necessary to successfully navigate the adult social world. Recognizing adolescence as a critical period in the development of an individual, the legal system has considered youth to be less culpable for their actions and the juvenile justice system was formed to correct antisocial behavior and foster prosocial development (Kupchik, 2006). Some have maintained that this intermediary form of justice is

problematic, however, and that society should “leave the kids alone whenever possible” (Schur, 1973, p. 155). Drawing on the labeling perspective (Lemert, 1951), radical nonintervention suggests it is best to do nothing at all as intervening may not only fail to alleviate, but actually worsen, the crime problem by disrupting the acquisition of human and social capital and embedding youth in deviant social networks (Schur, 1973).

The rehabilitative ideals of juvenile justice, and most certainly the concept of radical nonintervention, have been largely set aside in favor of a legal system that “gets tough” on juvenile crime. These changes are, in part, due to a perceived inevitability of a major violent crime wave spearheaded by a new breed of violent youth (Zimring, 1998). Unlike conceptions of developing youth as normal or individuals in need of support and guidance, notions of youth as “super-predators” suggest adolescents will almost assuredly be antisocial as adults. The implications for formal crime control endeavors are resoundingly clear. To the extent that juvenile violence is indeed the product of individuals who are largely unchangeable, policies focusing on reducing violent victimizations through “get tough” strategies may be appropriate. However, to the extent that engaging in juvenile violence is a conscious decision made by an individual, the goal of official intervention is not the control of behavior through incapacitation but rather the control of subsequent behavior through the calculated utilization of punishment experiences. Strategies that maximize deterrent and minimize labeling effects would appear to be the most appropriate formal intervention approach to controlling future acts of violent crime.

The discourse on youth violence and crime control brings two key issues into the limelight—stability and change in behavior. These ideas naturally implicate the

developmental and life-course perspective, which focuses on understanding the unfolding of behavior over time and seeks to discern the effects of life events such as criminal justice system contact on behavioral trajectories (Farrington, 2006). Despite the fact that there is much to be gained theoretically and practically by placing sanction effects in a developmental context (e.g., see Moffitt, 1993; Sampson & Laub, 1993; 2003), surprisingly little research has addressed this critical area of inquiry (Huizinga & Henry, 2008). To evince the importance of a developmentally informed investigation of sanctions effects for the study of violent crime, offending trajectories and the role criminal sanctions may play in altering them are now discussed.

Criminal Trajectories

The presence of stability in criminal behavior is undeniable. "Adult antisocial behavior virtually requires childhood antisocial behavior" (Robins, 1978, p. 611). While there has been a plethora of theories that have advanced our ability to explain individual-level criminal behavior (e.g., see Agnew, 1992; Burgess & Akers, 1966; Hirschi, 1969; Gottfredson & Hirschi, 1990), the best predictor of future behavior remains past behavior (White, Moffitt, Earls, Robins, & Silva, 1990). The notion of stability in offending behavior does not imply, however, that individuals do not change their level of involvement in crime altogether; rather, stability in offending refers to the fact that there is considerable between-individual continuity in behavior over time (Farrington, 1989; 1992; Tracy & Kempf-Leonard, 1996). Criminal trajectories chart the course of offending over the life-course. Analogous to specifying the future location and speed of a projectile by knowing its origin and path, an individual's prospective behavior can be projected, at least in theory, from their retrospective criminal trajectory.

The importance of criminal trajectories to the modern era of criminology can be traced back to Hirschi and Gottfredson's (1983) discussions of the most well-known offending trajectory in criminology—the aggregate age-crime curve. They claimed that the age distribution of crime could not be accounted for by existing criminological theories, which helped to spark a number of explicit attempts to develop explanations of offending over the life-course (see Lahey & Waldman, 2005; Moffitt, 1993; Catalano & Hawkins, 2005; Thornberry & Krohn, 2005; Farrington, 2005; Sampson & Laub, 1993; 2003; LeBlanc, 2005; Wikstrom, 2005; for a review, see Farrington, 2006). Several of these theories essentially claim there are offenders who cluster into trajectory groups but whose behavior, when viewed in conjunction, creates the illusion of a single age-crime curve (Moffitt, 1993; Thornberry & Krohn, 2005; LeBlanc, 2005; Lahey & Waldman; 2005).

The existence of distinct groups of offenders is one of the most contentious issues in developmental and life-course criminology (Farrington, 2006). The bulk of the controversy centers on the interpretation of whether criminal trajectories are deflectable. "Developmental criminology, in practice if not in theory, tends to emphasize the notion that people get 'locked' into certain trajectories" (Sampson & Laub, 2005a, p. 14). Some view this as a critical mistake which characterizes humans as robotic and inappropriately deemphasizes the importance of change in offending behavior over time (Sampson & Laub, 1993; 2003; 2005a; Laub & Sampson, 2003; Laub, Sampson, & Sweeten, 2006). Sampson and Laub's criticisms are well received; scientists should indeed be hesitant to explicitly or implicitly deny the relevance of human agency in offending a priori. However, characterizing criminal trajectories does not automatically

preclude one from assessing the relevance of important life events such as justice system contact. In fact, as will become clear, there appear to be important benefits to assessing intra-individual change in offending behavior using the group-based trajectory framework (Nagin & Tremblay, 2005; Haviland & Nagin, 2005; Haviland, Nagin, & Rosenbaum, 2007: Haviland, Nagin, Rosenbaum, & Tremblay, 2008).[1]

Official Intervention as a Turning Point

While most antisocial adults were antisocial as children, it is also the case that many antisocial children do not become antisocial as adults (Robins, 1978). Put differently, evidence suggests there is substantial between-individual stability in behavior over time (Farrington, 1989; 1992; Tracy & Kempf-Leonard, 1996) but within-individual change in criminal behavior can coexist with this between-individual continuity (Farrington, 1990; Sampson & Laub, 1992; Verhulst et al., 1990). Embedded within long-term behavioral trajectories are life events (e.g., marriage, employment, justice system contact) that unfold over relatively shorter periods of time (Elder, 1985). Collectively known as transitions, these key life events that are nestled within trajectories of behavior may generate a turning point in one's path (Elder, 1985). Like a sudden force that changes the speed or direction of a projectile, key life events experienced by individuals can serve to deflect one's criminal trajectory from its likely course. A number of social factors have been shown to be relevant to altering criminal trajectories including marriage (Laub, Nagin, & Sampson, 1998; Sampson & Laub, 1993; Sampson, Laub, & Weimer, 2006), employment/work (Uggen, 2000; Uggen &

[1] Behavioral trajectories have become increasingly popular in recent years and they have provided for new and exciting research questions across a wide variety of disciplines (for a review, see Piquero, 2008).

Staff, 2001; Sampson & Laub, 1993; 2003; cf. Wright & Cullen, 2004) and military involvement (Elder, 1986; Sampson & Laub, 1993; 1996; 2003). Being subject to formal sanctions is also thought to be a critical life event that can serve as a turning point in an offending trajectory (Sampson & Laub, 1997); one which warrants further attention.

The importance of studying how official intervention may shape an offending trajectory is amplified when considering the stark reality that reactive criminal justice responses such as arrest and incarceration remain politically popular and are very difficult to supplant. While there is widespread recognition that the causes of violent crime are highly complex (see Farrington, 1998; Reiss & Roth, 1993) resulting in the need for comprehensive strategies to control violent crime, there is a pervasive disconnect between what research shows and what policy makers do (Austin, 2003; Tonry, 2006). In recent decades, there has been unremitting growth in expenditures across all three components of the criminal justice system. Police expenditures continue to lead the way topping 100 billion dollars, constituting a 420% increase over roughly a twenty-five year period (Perry, 2008). While judicial and correctional expenditures are each approximately half that of police expenditures, growth in spending on courts and corrections represent even larger proportional increases (Perry, 2008).

Given that funding for justice system functions is at an all time high and traditional criminal justice responses continue to play a salient role in efforts to control violent crime, it becomes increasingly important to more fully understand the role official intervention plays as a turning point in violent crime trajectories. Indeed, “the extent to which criminal justice sanctions…foster recidivism or help lead to the termination of

criminal activity is a central one [question] in criminology, and takes on even more importance given the recent incarceration increases in the United States" (Bhati & Piquero, 2008, p. 218).

Bettering or Worsening Violent Crime Trajectories?

While official intervention may play no appreciable role in influencing subsequent offending behavior one way or the other, two competing theoretical perspectives predict opposite effects of official intervention on subsequent offending behavior. Forming the backbone of criminal justice system operations, specific deterrence theory suggests that formal sanctioning results in the reduction of crime. Individuals who experience certain, swift, and adequately severe sanctions are thought to align their future behavior with societal expectations due to the threat of future punishments (Beccaria, 1963; Gibbs, 1975; Zimring & Hawkins, 1973). Conversely, labeling theory suggests a dismal outcome; official intervention is hypothesized to set in motion a chain of events that actually leads to the worsening of criminal behavior (Becker, 1963; Lemert, 1951; 1967). Specifically, those who are labeled by formal sanctions are likely to be blocked from conventional opportunities, form negative self-identities, and acquire supportive deviant others which embeds the individual into a career of crime (Paternoster & Iovanni, 1989). Then, "controlling" crime may be best accomplished by either applying formal sanctions to engage criminal deterrence or, perhaps somewhat ironically, by withholding official intervention to prevent triggering the labeling process.

On their face, deterrence and labeling theories would appear to be irreconcilable. The central question underlying sanction effects research in this case is a relatively simple one: does official intervention result in either a deterrent or labeling effect (or neither)? Figure 1-1 provides a concrete example of official intervention as a potential

turning point in a violent crime trajectory. Illustrated here is an individual who is tracking along a specific criminal trajectory and, at some point in time, experiences an official intervention. When formal sanctions are initiated, one of three possibilities emerges: the individual may stay the course (null effect), the individual may worsen his or her behavior (labeling effect), or the individual may decrease his or her offending (deterrent effect). From a deterrence and public policy standpoint, null and labeling effects are both undesirable to a lesser or greater degree, respectively. Official intervention is only useful as a violent crime control strategy when experience with justice system contact alters one's offending trajectory toward conforming behavior.

Bettering and Worsening Violent Crime Trajectories?

The seemingly incongruent propositions of labeling and deterrence theories can compel individuals to make an artificial choice between the two theories (Wellford & Triplett, 1993). Most studies assessing the effectiveness of controlling crime through official intervention have taken for granted that there is a single homogeneous population that is equally sensitive to sanction effects over time. The assumption of uniform susceptibility to sanctions appears untenable. "We should not expect labeling [nor deterrent] effects to be invariant across subgroups" (Paternoster & Iovanni, 1989, p. 381). In other words, both labeling and deterrence theories may simultaneously be relevant explanations of offending behavior since the effects of official intervention may be contingent on the characteristics of offenders (Huizinga & Henry, 2008; Thorsell & Klemke, 1972). Despite the acknowledgement by academics of the need to explore contingent labeling and deterrent effects since the 1970s (e.g., see Thorsell & Klemke, 1972), there have been only a handful of empirical studies that have heeded this advice (Huizinga & Henry, 2008, p. 249). This is an unfortunate empirical omission as

developmental and life-course theories anticipate that the size and direction of treatment effects are contingent upon one's behavioral trajectory (e.g., see Moffitt, 1993).

Figure 1-2 is a schematic demonstrating differential sanction effects for individuals following two hypothetical offending trajectories. While the outcome possibilities for the individuals following along each trajectory remain the same (i.e., null, deterrent or labeling effects), there may be fundamental differences in the actual outcomes for different groups of offenders. For example, Moffitt (1993) puts forth a dual taxonomy theory of criminal behavior where she suggests that there are two fundamentally different types of offenders. Adolescence-limited offenders engage in acts of crime and delinquency for a brief period of time as an expression of independence brought on by a maturity gap between their biological and social ages. Life-course-persistent offenders commit acts of crime and delinquency relatively consistently over time as a result of neuropsychological deficits and exposure to criminogenic environments. Moffitt (1993) predicts that adolescence-limited offenders would be subject to labeling effects since official intervention would interfere with a normal desistance process. However, life-course persistent offenders, whose etiology of offending is rooted early in life, would be unlikely to experience official intervention effects altogether (Moffitt, 1993).

In addition to different types of offenders having differential susceptibility to sanction effects, Sampson and Laub (1997) argue that it is important to not only consider the immediate crime enhancing or reducing effects of official intervention but also examine longer term outcomes. While there may be immediate deterrent effects of official intervention, being subject to formal sanctions may nevertheless set in motion

the labeling process ultimately resulting in deviance amplification (Sampson & Laub, 1997). Figure 1-3 adds this possibility to the diagram for the two hypothetical individuals following along distinct offending trajectories (cf. Figure 1-2). To complicate matters, then, both deterrent and labeling effects may be possible for the same individual. Findings supporting initial deterrent effects of arrest followed by subsequent labeling effects have been observed (e.g., see Sherman et al., 1991). In sum, labeling and deterrent effects may differ for different types of offenders and/or both theoretical processes may be relevant for the same type of offender leading to initial deterrent effects and subsequent labeling effects. These possibilities emerge when official intervention effects are considered within a developmental and life-course framework.

Present Study

Though there is much to be gained from life-course informed investigations of deterrent and labeling effects, "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" (Huizinga & Henry, 2008, p. 223). Taking a practical approach to the control of violent crime, the present study draws on theories of deterrence, labeling, and life-course criminology to examine how, if at all, official intervention may be most effectively employed to reduce subsequent offending behavior. More specifically, this research seeks to determine for which violent offending subpopulation(s) official intervention is more likely to result in an immediate increase in violent behavior and for which subpopulation(s) it is more likely to result in an immediate decrease in violent behavior. As a related point of investigation, the proposed research aims to uncover the duration of any initial labeling and deterrent effects and determine if the length of the effects

differ by violent offending subpopulations. The present study tackles two key problems—classification of offenders and selection artifacts—that stand in the way of addressing these critical empirical questions.

To assess whether official intervention serves as a turning point in the criminal trajectories of offenders, a classification procedure for different types of offenders is necessary. While individuals' prior records have been used to classify persons into offending subpopulations in previous research, there is a critical problem with this strategy. For example, two individuals who have been arrested once would be classified similarly despite the fact that one may be a chronic offender and the other an exceptionally infrequent offender. Then, classifying individuals based on their prior records as opposed to their offending behavior is somewhat imprecise if the goal is to assess contingent effects based on behavior. As an alternative to classification based on prior record, Huizinga and colleagues (1986) classified individuals according to a delinquency typology (e.g., non-delinquent, exploratory delinquent, non-serious delinquent, serious delinquent). However, there are critical problems with subjective classification procedures as well. First, a subpopulation can be subjectively classified as distinct but only represent random variation. Second, unusual subpopulations can fail to be classified at all. Third, statistical tests examining differences across subpopulations may be undermined by uncertainties about the reliability of subpopulation assignment (see Nagin, 2005). Finally, subjective classification schemes may fail to consider trajectories of offending behavior over time, which can limit their ability to assess whether official intervention serves as a turning point in certain criminal trajectories.

In addition to the problems associated with subjective classification, methodological limitations of previous studies have resulted in the inability to discern between labeling and deterrent effects for offending subpopulations with any definitiveness. One key problem with prior empirical work is the use of imprecise methodological designs that result in an inability to disentangle "selection artifacts" from real official intervention effects (Smith & Paternoster, 1990). With some exceptions (e.g., Chiricos, Barrick, Bales, & Bontranger, 2007; McAra & McVie, 2007; Smith & Paternoster, 1990), research designs have failed to employ strong methodology that addresses the problems of making causal inferences with non-experimental data. However, the classification and selection artifact problems can be resolved given the availability of high quality longitudinal data for official intervention effects research (Huizinga & Henry, 2008, p. 248) and counterfactual methodological techniques designed to approximate experimental designs with observation data (Haviland et al., 2007; 2008). In sum, the current work advances research on the utility of controlling violent offenders through traditional criminal justice measures by clarifying the theory underlying differential sanction effects using a developmental and life-course framework and by conducting an advanced and rigorous empirical analysis using data from a major longitudinal panel study. In doing so, the study provides answers to two urgent academic inquiries (i.e., what the effects of official interventions are for different violent offending subpopulations and how long these effects last) that have sweeping practical implications for the control of violence.

The current research begins by reviewing the relevant history, development and present state of thinking regarding the key theories of sanctions effects (Chapter 2).

Next, the widespread influence of the developmental and life-course paradigm is noted along with a detailed explanation of what is meant by key life-course ideas such as criminal trajectories, turning points and stability and change in offending (Chapter 3). Sanction effects are then placed in the developmental and life-course perspective to illuminate differential sanction effects hypotheses between offending trajectories and also, within offending trajectories, across the short- and long-runs (Chapter 4). Subsequently, literature on the effects of official intervention is reviewed with emphasis placed on what is known from studies examining contingent deterrent and labeling effects as well as what remains unknown or unclear due to data or methodological limitations (Chapter 5). Next, the data section details the Rochester Youth Development Study (RYDS), details the analysis sample and the measures used in the study, and provides descriptive statistics (Chapter 6). This is followed by the methodology section which includes an explanation of the conceptual underpinnings and methodological details of propensity score matching, latent class growth analysis and their methodological integration (Chapter 7). The next chapter focuses on the LCGA model selection and provides a detailed description of the number, shapes and proportion of individuals belonging to trajectory groups. In addition, similarities and differences between the violent offending subpopulations with respect to official intervention experiences, covariates, and violent crime outcomes are reported (Chapter 8). Constituting the main analyses, results of the integrated LCGA-PSM methodological procedures are detailed in the next chapter. First, bivariate relationships between official intervention and subsequent violent offending behavior are reported for both types of official intervention in both the short- and long-runs. Next, the successfulness

of the propensity score matching procedure is documented for the treatment of arrest and police contact. Following the matching procedure, treatment effect estimates of the official interventions are examined in both the short-term and long-term (Chapter 9). Finally, a summary of the principal empirical findings is offered with a detailed discussion of the implications of the current research for the effectiveness of violent crime control with traditional criminal justice approaches. Both the limitations of the current study and directions for future research are also considered (Chapter 10).

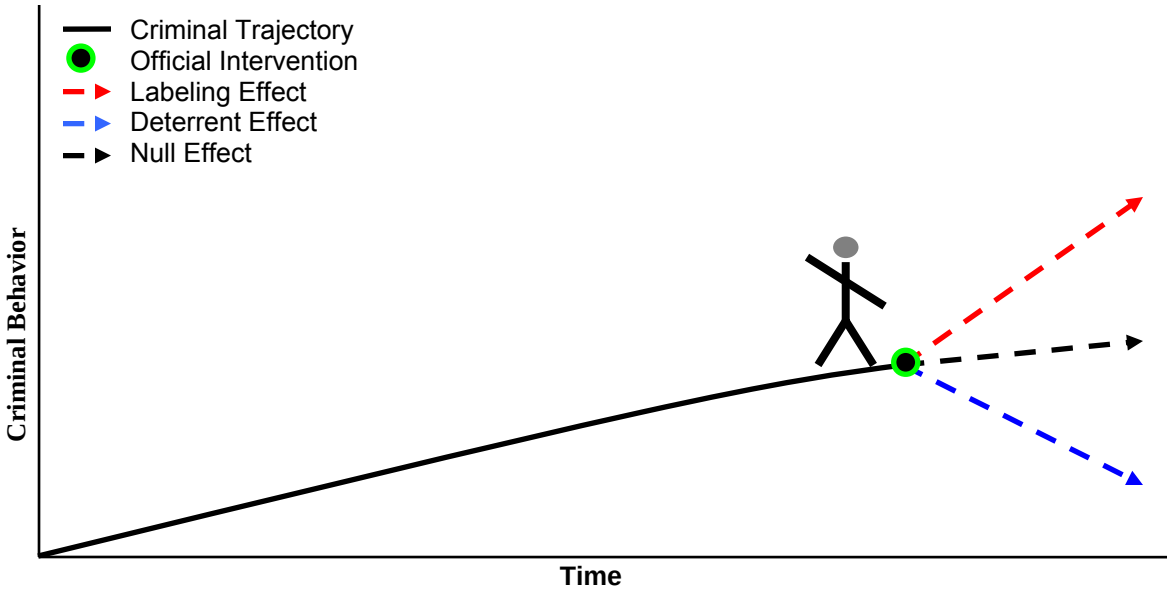

Figure 1-1. Illustrative model of official intervention as a potential turning point in a criminal trajectory.

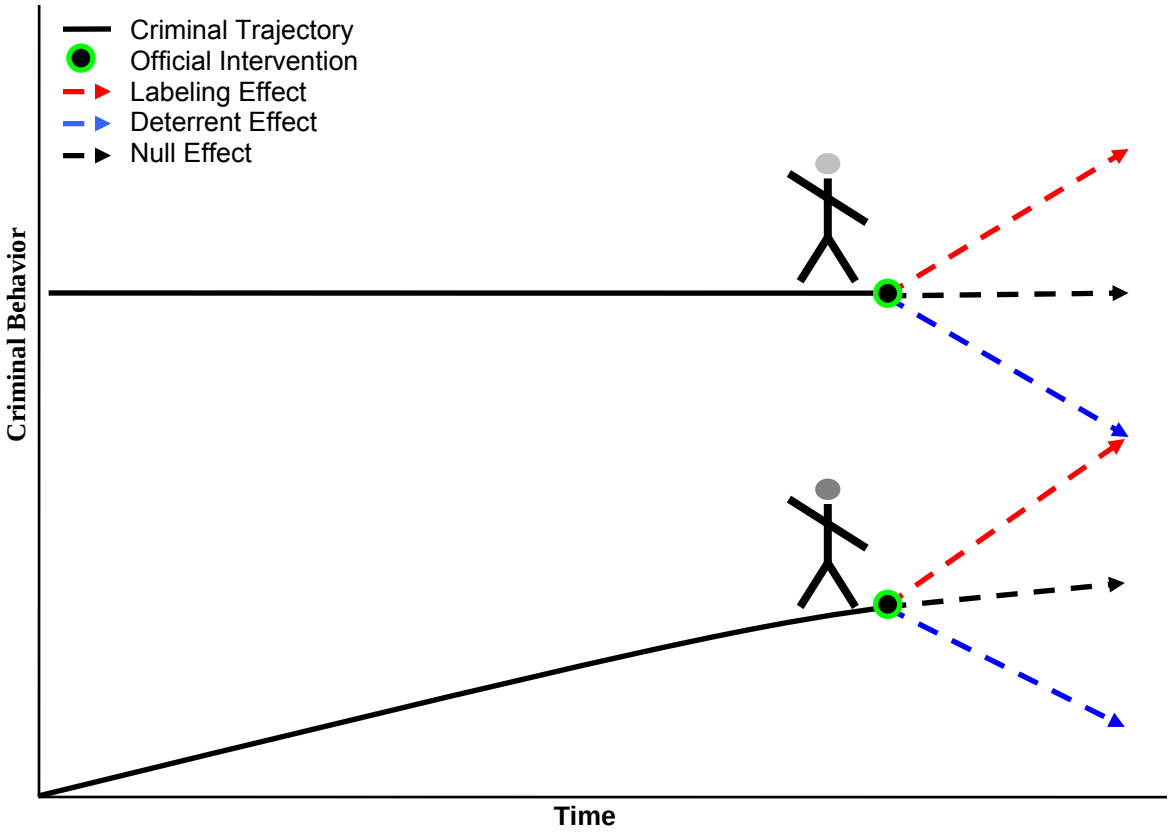

Figure 1-2. Illustrative model of official intervention as a potentially different turning point for divergent criminal trajectories.

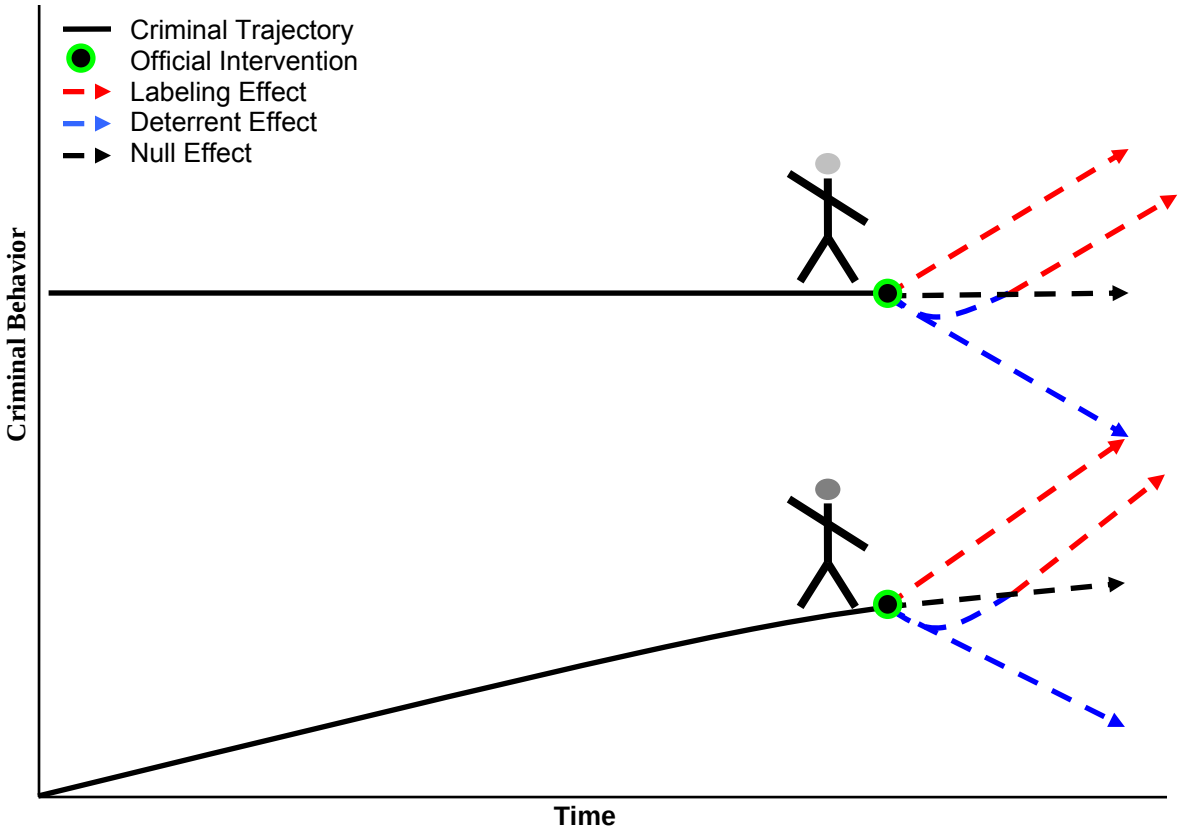

Figure 1-3. Illustrative model of official intervention as a potentially different turning point for divergent criminal trajectories with short-term and long-term differences.

CHAPTER 2
THEORIES ON SANCTION EFFECTS

Chapter 2 provides a comprehensive review of the two major theoretical frameworks from which the original competing sanction effects hypotheses are derived. First, the classical school of criminology is traced back to 18th century writings to articulate the foundation of deterrence theory, on which the modern day criminal justice system still rests. Focusing primarily on specific deterrence, key advances in the theoretical thinking about deterrence are explored which have served to clarify the mechanisms through which official intervention is hypothesized to reduce subsequent offending. Second, the emergence of the labeling perspective from its roots in symbolic interactionism is reviewed. Expanding on labeling theory's general notion that sanctions may ultimately increase subsequent offending, theoretical work that has explicated the causal pathways of the deviance amplification hypothesis is thoroughly explored.

Deterrence Theory

This section details the very long and decidedly influential history of deterrence theory in criminology and criminal justice. First, the origins of deterrence theory are elucidated by tracing historical beliefs about crime causation and the corresponding brutal nature of criminal punishments. Key changes in thinking about human behavior and the important legal and penal reform efforts that unintentionally led to the formulation of deterrence theory are discussed. Second, the theoretical assumptions regarding free will and human behavior reveal that choice, governed by the principle of utility, is the chief cause of crime. Third, the crux of deterrence theory is outlined, highlighting how certainty, severity and celerity of punishment are hypothesized to control one's choice to engage in crime and violence. Fourth, attention is paid to the

important distinctions drawn between specific and general deterrence and, fifth, consideration is given to the differences between absolute and restrictive deterrence. These subsections underscore how deterrence may operate at two interrelated levels and address the issue of what exactly constitutes evidence for a deterrent effect of criminal sanctions. Sixth, the notion of tipping points is explained and their applicability to specific deterrence is noted. Seventh, recent advancements in the theorizing about deterrence are described and the necessary restriction of deterrence theory to the study of formal punishments is noted. Finally, the significance of perceptions to the deterrence process is emphasized, which raises some new theoretical dialogue regarding possible outcomes of criminal sanctions.

The Origins of Deterrence Theory

Throughout history leading up to the 17th century, religious views maintained a stranglehold on the meaning of both the physical and social world. For more primitive societies, spiritualistic beliefs afforded humans a simple cause-and-effect explanation of events that they otherwise did not have the means or capacity to understand. The phenomenon of crime was no different and violations of the law were conceived of as being the product of demonic possession. Spiritualism was characterized by an ongoing battle between two forces—good and evil (Tannenbaum, 1938). Individuals were largely denied human agency, being reduced to pawns in a larger-than-life strategic game between opposing spiritual forces. Ironically, the flesh of human bodies was held accountable for the actions of intangible, possessive demons. By today's standards, the methods employed to both determine guilt and punish criminal offenders were heinous and nonsensical. Considered within historical context, however, they

were not especially surprising given prevailing thoughts on the transcendental nature of criminal conduct.

Criminal trials were known as ordeals which involved placing offenders in situations where there was the expectation of harm to the accused under the belief that god would intervene to protect those who were innocent. While there was regional variation in the particulars of ordeals, a common version of a “trial by fire” involved an offender walking a number of paces grasping a red-hot iron bar weighing one or three pounds, depending on the severity of the offense (Perkins, 1926). Those who emerged unscathed from an ordeal were found innocent. Unless an offender was sufficiently wealthy and able to successfully bribe the officiator to stage the outcome of the ordeal, there was always evidence of guilt. What’s more, for many offenders the determination of guilt and the punishment of death were inextricably linked. For example, some offenders were tied up and tossed into rivers to be deemed proven guilty if they actually drowned (Newman, 1978). In short, a true trial by ordeal essentially guaranteed that an offender was going to be found guilty and sometimes meant that one would pay immediately with their life.

Beginning in the mid 1600s, a marked shift in thinking about human behavior occurred and crime began to be viewed in a secular context. As opposed to being the work of spirits, criminal behavior was believed to be the result of ordinary and natural human agency. This revolutionary viewpoint saw men and women as persons who pursued their own interests without concerning themselves with the ramifications of their actions for others (Hobbes, 1967[1651]; Locke, 1988 [1689]; Rousseau, 1968 [1762]). Given people’s preoccupation with self-interests, Hobbes (1967) argued that a “social

contract" was essential to living in an environment that was free from anarchy (see also Locke, 1988; Rousseau, 1968). A social contract involves the relinquishment of some personal freedoms in exchange for society's protection from the harms and wrongdoings of other members of society. Importantly, individuals should willingly give up only the smallest portion of liberty absolutely necessary for adequate protection from other citizens (Beccaria, 1963 [1764]). The success of a social contract rests upon the development of a state capable of upholding its responsibilities to citizens without infringing upon their rights.[2] Thus, the biased, capricious and arbitrary natures of the legal and penal systems, which characterized trials and punishments during the reign of spiritualism and bled over into the 17th and 18th centuries, became a focal point of concern.

Primarily interested in reforming European penal and legal systems, two 18th century utilitarian social philosophers set out to provide a philosophical rationale for

[2] The relevance of the civilizing process (Elias, 1939a [1978]; 1939b [1982]) for the theoretical applicability of deterrence to violent crime should not be overlooked. Since the safeguarding of individual rights and liberties requires an entity capable of upholding its end of the social contract, it is not surprising that the process of state formation was set in motion during this time period. Detailing the state formation process in Europe, Elias (1939a; 1939b) offered one of the most important social-psychological explanations of violence to date. Brought on by inter-group tensions and rivalries, the process of civilization emerged in order to differentiate the behavior of courtly social groups from other segments of society. With increasing human interdependencies that resulted from a modernizing world, manners and civilized behavior spread from the courtly social groups to other members of society. More important here, however, are the ramifications the civilizing process had for the character and quality of violence. The civilizing process results in a rise in repugnance thresholds for violence, increases in drive-controls and guilt, and a removal of violence from everyday life. The process also entails the formation of a state "monopoly on violence" with respect to both the means and use of physical force (Elias, 1939a; 1939b). The monopoly of violence reserves violence for specific individuals such as the police and/or for specific time periods as in wartime (Elias, 1939a; 1939b). Resulting from these social changes in violence is the taming of expressive forms of violence. However, while use of violence for emotional satisfaction is lessened, the use of violence as a means to attain a specific goal is augmented. The deterrence doctrine maintains that the threat of legal sanctions is most effective in curbing instrumental crime (see Chambliss, 1967; Parker & Smith, 1979; Thomas & Williams, 1977). While it would be a gross misstatement to say that all violence in America is instrumental, the civilization process ensures that deterrence based criminal justice system responses have, at least in theory, relevance not only for crimes against property and society but also for violent crimes against persons. Violations of the law and violence can both be understood in instrumental terms and concepts of rationality are critical to understanding these behaviors (see Felson, 2009).

punishment and eliminate the arbitrariness and irrationality that plagued the administration of justice. Presented in the form of a collection of concise essays, *On Crimes and Punishment* remains one of the most influential scholarly works in the field of criminal justice (see Beccaria, 1963). In his book, Beccaria (1963) called for a number of reforms including easily accessible and interpretable codes of conduct, abolishment of the death penalty, impartial application of the law, and sufficient resources to mount a defense against a criminal accusation, to name a few. The rationales provided for legal and penal system modifications were well grounded; for example, Beccaria (1963, p. 32) argued that torturous methods of guilt determination should be abolished because its application ensures that "the very means employed to distinguish the innocent from the guilty will most effectually destroy all difference between them." In fact, the reform efforts of Cesare Beccaria and Jeremy Bentham were so well formulated and commonsensical that the two utilitarian social philosophers articulated a theory of criminal deterrence which remains the driving force behind the practice of modern day criminal justice in America as well as many nations across the globe. Indeed, unlike many theories of criminal behavior, deterrence theory provides both a straightforward explanation of crime and an unequivocal solution to the problem (Pratt, Cullen, Blevins, Daigle, & Madensen, 2006). To understand the elegance and explanatory appeal of deterrence theory, it is necessary to explicate key assumptions about human choice and its prescript as well as the phenomenon of crime.

Free Will, Pleasure and Pain

Most theories of crime are born out of the positivism tradition of science and attribute crime to causes that are internal and/or external to the individual but, importantly, largely out his or her direct control (e.g., see Agnew, 1992; Shaw & McKay,

1942). Deterrence theory takes a different approach and rests on the assumption that individuals have the ability to exercise free will and directly control their behavior (Beccaria, 1963). While there may be certain limits to the ability of humans to reason as well as individual differences in this capacity (Cook, 1980), deterrence theory assumes that individuals are rational actors and freely have the ability to act on their own accord (Beccaria, 1963; Bentham, (1948) [1789]; Gibbs, 1975). Thus, the cause of violent crime is as simple as a mere choice by the offender to engage in the antisocial behavior.

Individuals do not aimlessly make choices; rather, the theory of deterrence suggests that an individual's course of action can always be known. "Nature has placed mankind under the governance of two sovereign masters, *pain* and *pleasure*. It is for them alone to point out what we ought to do, as well as to determine what we shall do" (Bentham, 1948, p. 14, *emphasis in original*). As the concepts readily infer, pleasure refers to the experience of happiness, benefit, or advantage whereas pain refers to the experience of mischief or unhappiness (Bentham, 1948). All individuals inherently seek to experience pleasure and to avoid pain; this is known as the principle of utility and is assumed to be relevant at all times and dictate every decision making process (Bentham, 1948). "Systems which attempt to question it [the principle of utility], deal in sound instead of sense, in caprice instead of reason, in darkness instead of light" (Bentham, 1948). While Bentham makes a dauntless claim regarding the relevance of the principle of utility for human behavior, it is undeniably logical and enticingly parsimonious.

In addition to human free will and the principle of utility, deterrence theory assumes that crimes have certain pain and pleasure values which are inherent in the acts. Specifically, Bentham (1948) argues that the pains and pleasures of single acts vary according to their intensity, duration, certainty, and propinquity. Intensity refers to the strength of the pains and pleasures that result from engaging in a criminal act and duration concerns the length of time in which these two forces are experienced. Certainty has to do with how likely pains and pleasures are to occur if one decides to commit a crime. Propinquity pertains to the time that elapses before pain and pleasure is experienced once the criminal act is completed. In addition to these four elements that characterize the quality of pains and pleasures, two other factors help guide the maintenance of behavior overtime including fecundity and purity. The former refers to the chance that an act will be followed by sensations of the same kind, whereas the latter refers to the chance that an act will not be followed by sensations of the opposite kind (Bentham, 1948). Individuals who commit crimes receive pleasure from doing so and the pleasure associated with a given crime is thought to be similar for all individuals (Beccaria, 1963). Consequently, a scale of crimes can be formed that orders criminal acts based upon their gravity or, in other words, their pleasure invoking quality (Beccaria, 1963). Given the interlocking assumptions of free will and human governance by the principle of utility, the tactics necessary to formally control crime and violence emerge as relatively straightforward.

Certainty, Celerity and Severity of Punishment

Stemming from conceptions of men and women as rational and self-interested actors, the crux of deterrence theory rests on the proposition that punishment that is certain, swift and adequately severe will decrease the occurrence of crime (Beccaria,

1963; Gibbs, 1975). The certainty of punishment refers to the likelihood of one getting caught and receiving punishment for committing a particular offense (Gibbs, 1975). The theory maintains that if the certainty of punishment is greater, individuals should be less likely to commit crime. An especially high degree of certainty of punishment is thought to create a feeling of omnipresence such that the offender believes he or she could be under surveillance at any given time and, thus, caught for his or her offense. In the theory of deterrence, certainty takes a state of primacy over celerity and severity of punishment and a large body of empirical research supports this theoretical assertion (Nagin, 1998).

Celerity, or swiftness, refers to the duration it takes to experience the punishment following the commission of a crime (Gibbs, 1975). Punishments that are applied swiftly are thought to be more effective in reducing crime because they yield a clearer and stronger association between the crime and punishment (Beccaria, 1963; Gibbs, 1975). Despite this logical assertion, celerity has not been found to be especially relevant to deterring crime and violence (Clark; 1988; Nagin & Pogarsky, 2001).

The severity of punishment refers to the amount and type of punishment experienced by the offender for committing a crime (Gibbs, 1975). Recall, deterrence theory is a byproduct of the direct response by reformers to the biased and brutal nature of the administration of justice. While deterrence is best achieved by maximizing certainty and celerity of punishment, severity of punishment should not be maximized but be commensurate with the crime (Beccaria, 1963; Gibbs, 1975). There is also an interesting interaction between the certainty and severity of punishment. More specifically, punishments that are more severe tend to be applied with less certainty,

whereas punishments that are applied with a low degree of certainty must be more severe to invoke deterrence (see Beccaria, 1963).

While the principles of deterrence are largely responsible for the administration of justice today, the theorized relative importance of certainty, severity and celerity to the deterrence process did not take hold immediately. For instance, the conceptualization of the human as self-serving coupled with the emergence of new forms of wealth resulted in a dramatic surge in the number and types of offenses that were punishable by death in England (McLynn, 1989). During the reign of the Bloody Code in England (1688-1815), "the same crime could be prosecuted under totally different statutes and penalties [and] often a multiplicity of laws bore on the same crime. The theory and practice of criminal law were light-years apart" (McLynn, 1989, p. xiii).

To state that the gap between theory and policy has been closed entirely or even that policies are no longer enacted solely to influence the severity of criminal sanctions would be incorrect. Many statutes currently on the books in America lead to punishments that do not appear to be commensurate with the criminal act. For instance, Zimring, Hawkins and Kamin (2001) detail California's "three strikes" law in which the focus on controlling habitual offenders leads to instances of individuals receiving twenty-five years to life for larceny. Not surprisingly, three strikes legislation has resulted in a swelling of the prison population (Zimring et al., 2001) and some court districts such as those in the San Francisco region are reluctant to apply the penalty, resulting in a reduction in certainty of punishment (Males & Macallair, 1999). These examples underscore the point that while deterrence theory suggests a greater emphasis on certainty of punishment, often increases in the severity of sanctions are

initiated to engage criminal deterrence. It must be understood, however, that this approach may have negative feedbacks on the certainty of punishment (e.g., selective enforcement and application of the law) as well as the celerity of punishment (e.g., lengthy appeals processes for death sentences).[3] Despite potential problems with the practical application of the principles of deterrence, the theory logically suggests that manipulating certainty, severity and celerity of punishment will reduce criminal behavior and it is consistent with prevailing notions of individual culpability. The proposed solution to the crime problem that is derived from deterrence theory is thought to both prevent criminal behavior from occurring in the first place and correct it if it has already transpired.

Specific and General Deterrence

The sanctioning of offenders engages a deterrence process at two levels simultaneously (Zimring & Hawkins, 1973). When an offender is caught and punished in a swift fashion and with adequate severity, he or she is thought to reduce their future offending behavior due to the fear of future sanctions. This process is known as specific deterrence and it is so named because it's referring to the effects of punishment for the specific offender who committed the crime. Relatedly, attempts to deter the future behavior of a specific individual through formal intervention also serve as examples to the general public of the ramifications of committing a crime. The apprehension and formal sanctioning of criminals are widely covered by news and

[3] Most research on deterrence theory focuses on the additive effects of certainty, severity and celerity of punishment. While there is a general rank-ordering of the importance of the various factors of punishment as well as some important interrelationships among them such as those discussed above, one should not lose sight of the fact that deterrence theory does suggest that all three elements must be present to some extent for deterrence to be successful in reducing criminal offending (Stafford et al., 1986). Hence, if there was a high degree of certainty and celerity, for example, but essentially no severity of punishment, the utility of deterrence to control crime and violence would be miniscule at best.

media organizations (Jewkes, 2004). This process of reducing crime is known as general deterrence because the application of formal sanctions to individuals can also influence perceptions of certainty, celerity and severity of the general public as well.

Differences between general and specific deterrence are often noted in research but subsequently disregarded (Zimring & Hawkins, 1973). Stafford and Warr (1993) note the distinctions drawn between general and specific deterrence have muddied the waters and maintain that individuals' experiences with punishment and punishment avoidance are multifaceted. To Stafford and Warr (1993), specific deterrence is conceptualized to not only consist of direct experiences with punishment but also direct experiences of punishment avoidance. Recognizing the potential importance of knowledge of the punishment of others, they redefine general deterrence to be indirect experiences with punishment and punishment avoidance. "The point to be emphasized is that in most populations—whether members of the general public or punished offenders—people are likely to have a *mixture* of indirect and direct experience with punishment and punishment avoidance" (Stafford & Warr, 1993, p. 126, *emphasis in original*). This reconceptualization of deterrence theory is helpful here because it guides the expected effects of direct experiences of punishment across different types of offending populations. Specifically, Stafford and Warr (1993) argue that direct experiences of punishment and punishment avoidance should be most relevant to the behavior of habitual offenders and less relevant to novice offenders with little or no criminal justice contact (i.e., the latter group is driven more by their notion of general deterrence). While it is known that deterrence theory predicts an inverse relationship

with direct and indirect punishment experiences, the question as to what exactly would constitute evidence in support of a deterrent effect has not yet been broached.

Absolute and Restrictive Deterrence

A more unconventional distinction, but one of great importance to assessing the efficacy of deterrence theory, has been drawn between absolute and restrictive deterrence (Gibbs, 1986).[4] Absolute deterrence refers to whether the fear of punishment leads individuals to refrain from committing offenses altogether (Gibbs, 1986). In other words, an offender is deterred from crime only if they enter into a state of complete offending inactivity. As a more temperate interpretation of the success of official intervention, restrictive deterrence refers to the extent to which fear of legal sanctions reduces the overall number of crimes (Gibbs, 1986). Individuals need not cease their criminal offending but it should decrease following punishment experiences. While the main goal of formally sanctioning an offender is undoubtedly to achieve absolute deterrence, it may be impractical to attain this objective. Indeed, it would be unsagacious to view a substantial reduction in violent crime among chronic offenders attributable to official intervention as a total failure. Moreover, the idea of restrictive deterrence is more in line with views of criminal desistance as a dynamic or developmental process that unfolds over time (see Bushway, Thornberry, & Krohn, 2003). Restrictive deterrence acknowledges that punishment experiences may have

[4] An important distinction has also been drawn between "absolute" and "marginal" deterrence (Gibbs, 1986). In this case, absolute deterrence refers to the criminal deterrence that occurs by having a system of legal punishments in place as compared to a system in which no legal punishments are in place. Most concur that the existence of sanctions in general plays an instrumental role in reducing crime but many scholars contend this is a relatively uninteresting phenomenon. Marginal deterrence refers to the extent to which changes in legal punishments increase or decrease participation in crime. Scholars maintain that understanding marginal deterrent effects, not absolute deterrent effects, should be the principal aim of deterrence research.

greatly increased the awareness of the pains of crime making it considerably less likely in many situations, yet not sufficiently enough such that the pains outweigh the pleasures in all situations.

Tipping Points

Every change in the certainty, severity or celerity of punishment may not be accompanied by a change in offending behavior. It is conceivable that there is an optimal range in which marginal deterrence (i.e., changes in behavior associated with changes in legal punishments) occurs, but outside of which there is no relationship between punishment quality and criminal behavior. Indeed, penalties that are inappreciable to offenders may fail to have any deterrent value (Tittle & Rowe, 1974; Brown, 1978; Yu & Liska, 1993) and punishments that approach certain levels may have diminishing returns (Yu & Liska, 1993). Deterrence theory explicitly predicts some of these floor and ceiling effects. For example, deterrence theory argues that it is necessary that punishment reaches a certain degree of severity to engage criminal deterrence but need not be harrowing to do so (Beccaria, 1963). In theory, making punishments more severe than necessary fails to have any added deterrent benefit and could actually reduce the certainty of punishment (see Beccaria, 1963). Moreover, punishments such as the death penalty may trigger a brutalization effect, where there is an increase in offending among the general public following the execution of an offender (Bowers & Pierce, 1980). Then, there may be a lower threshold of punishment severity that must be reached in order for deterrent effects to occur at all. Similarly, there may be an upper threshold of punishment that, if exceeded, does not result in any additional gains in criminal deterrence and possibly could make matters worse.

Much of the empirical work on tipping points has focused on certainty of arrest within the context of general deterrence. This research has revealed that the effects of tipping points may differ across divergent demographic groups. For instance, Yu and Liska (1993) note that Blacks are subject to higher arrest certainty; they find that tipping points for certainty of arrest, especially ceiling effects, are especially pronounced for this demographic group. This idea can easily be applied within the context of specific deterrence. Because of differences in motivation, perceived rewards or the social costs of committing a crime, individuals may require different punishment experiences to reach their tipping point. Issues of equity in the administration of justice aside, uncovering information regarding the penalties necessary to trigger specific deterrence for certain types of offenders could be highly useful information for reducing crime and violence.

Modern Deterrence Theory

While the essential idea of deterrence theory remains that increases in certainty, severity and celerity of punishment lead to decreases in criminal behavior, the theory is considerably more nuanced and detailed than the original statements of the theory lead one to believe (Gibbs, 1986). Gibbs (1986) maintains that deterrence theory can be broken down into three premises and two corollaries. The premises state there is a direct relationship between objective properties of punishment and deterrence, there is a direct relationship between perceptual properties of punishment and deterrence, and there is an indirect relationship between deterrence and crime (Gibbs, 1986). The two corollaries state there is an inverse relationship between the perceptual properties of punishment and crime and, secondly, an inverse relationship between the objective properties of punishment and crime (Gibbs, 1986). While this conceptualization of

deterrence incorporates both perceptions and objective experiences, it also expands deterrence beyond certainty, celerity and severity of punishment into the realms of non-legal sanctions

There has been a push to extend deterrence theory beyond legal sanctions. On the heels of strong academic interest in societal reaction theories of crime, a renewed enthusiasm over the deterrence perspective occurred in the 1970s and 1980s when economists began applying rational choice perspectives to explain criminal behavior (e.g., see Becker, 1968; Heineke, 1978). Some contend that the reach of deterrence theory extends beyond formal sanctions (Zimring & Hawkins, 1973). In one view, formal sanctions are most likely to have deterrent effects through informal social sanctions (Andenaes, 1974; Gibbs, 1975; Blumstein & Nagin, 1976; Williams & Hawkins, 1989; Zimring & Hawkins, 1973). In other words, formal sanctions trigger informal, negative reactions from parents and community members which are ultimately responsible for any observed reductions in offending. Alternatively, rational choice theory subsumes formal sanctions, adding them to a conglomeration of costs and rewards of crime that are included in offender decision making (Cornish & Clarke, 1986; Paternoster, 1989a; 1989b).

While the inclusion of considerations of perceptions in deterrence theory is most appropriate, the inclusions of non-legal sanctions and other elements of punishment have been argued to be logically inconsistent with the theory of deterrence as its explicit focus is on the costs of formal sanctions (Akers, 1990). Akers (1990) also notes that extending the theory beyond formal sanctions encroaches on other theories such as social learning theory (i.e., differential reinforcement) (see also Akers & Sellers, 2009).

Thus, one view holds that to the extent that informal sanctions are incorporated as part of the deterrence process, the theory of deterrence upon which the criminal justice system is based is, strictly speaking, no longer being evaluated (Akers, 1990). However, some formidable scholars have praised efforts at connecting formal and informal sanctions. For instance, Nagin (1998) states "In my judgment the most important contribution of the perceptual deterrence literature…is the attention it has focused on the linkage between formal and informal sources of social control" (p. 19). The basic idea is that the deterrence process is invoked when an individual refrains from committing a crime because they fear arrest may bring about adverse social consequences (Williams & Hawkins, 1986). These ties between formal and informal sanctions may be important for policy considerations.

Regardless of whether one agrees that informal sanctions belong in the realm of deterrence theory, the consideration of them provides a mechanism which links justice system contact to decreased future crime. Significantly, this is a unique mechanism providing a different rationale for why arrests should be associated with decreased crime and violence. The other major mechanism, which is without controversy, is that of perceptions and the importance of them to modern deterrence theory is now discussed.

Perceptual Deterrence

While objective levels of certainty, severity and celerity of punishment are important to deterrence, perceptual deterrence theory holds that individuals make decisions based upon their knowledge or perceptions of these objective realities (Gibbs, 1986; Nagin, 1998). In other words, objective properties of punishment should influence sanction perceptions which should, in turn, influence decisions to engage in criminal behavior. This point would be moot if individuals were perfect perceivers and human

calculators of objective information. This, however, appears not to be the case; individuals do not accurately perceive their objective risk of punishment (Cook, 1980; Kleck, Sever, Li, & Gertz, 2005). Moreover, calculations of objective risk of punishment and the acceptability of perceived risks are not systematically the same across persons (Clarke & Cornish, 1985; Cornish & Clarke, 1986; Cook, 1980; Gibbs, 1986; Nagin & Paternoster, 1993; Nagin & Pogarsky, 2001; 2003). There are also individual differences in exposure to information about risks of punishment from a variety of sources including media, visibility of law enforcement, direct and indirect experiences with punishment, and daily conversations (Cook, 1980). "The validity of rational decision-making theories does not require a perfect correspondence between contingencies and perceptions of them, but does require some correspondence if the theories are to have any explanatory or predictive power" (Kleck et al., 2005, p. 627).

The realization that perceptions hold the key to whether objective properties of punishment influence criminal behavior shifted the focus of deterrence research from macro-level research and interrupted time-series impact studies to micro-level studies focusing on the effects of individual perceptions on crime (Nagin, 1998). In general, this line of research has revealed that certainty perceptions do influence criminal behavior as predicted by deterrence theory, but the effects appear to be rather modest (Pratt et al., 2006). More specifically, the relevance of perceptions for deterrence has been studied with cross-sectional, scenario-based, and panel survey approaches (Nagin, 1998). Both cross-sectional and scenario-based studies lend regular support for the relationship between risk perceptions and self-reported criminal behavior or criminal intentions (Nagin, 1998); however, panel studies and those with stronger

methodological designs typically find much weaker support for deterrence theory (Nagin, 1998; Paternoster, 1987), thereby highlighting the importance of using strong methodology.

Panel studies attempt to address the important temporal ordering question of whether risk perceptions cause crime or whether crime causes risk perceptions (Greenberg, 1981). Non-offenders have been thought to overestimate their sanction risks, which are lowered to more reasonable levels once the individual begins engaging in crime without being subject to punishment (Paternoster, Saltzman, Chiricos, & Waldo 1982; Paternoster, Saltzman, Waldo, & Chiricos, 1983a; 1983b; Saltzman, Paternoster, Waldo, & Chiricos, 1982). Some scholars maintain that the observed negative relationship between risk perceptions and sanctions is actually evidence for an "experiential effect," not a deterrent one (Paternoster, 1982; 1983a; 1983b; Saltzman et al., 1982). However, evidence for an experiential effect does not necessarily contradict deterrence theory. Recall, Stafford and Warr (1993) redefine specific deterrence as direct experiences with both punishment and punishment avoidance. Those individuals who elude detection would be more likely to continue to offend, or have lower risk perceptions, precisely because they have dodged authorities in the past (Stafford & Warr, 1993).

In testing the effects of direct and indirect experiences with punishment, Piquero and Pogarsky (2002) unexpectedly found an "emboldening effect," in which the behavior of those with punishment experiences appeared to actually worsen following official intervention. Offering alternative explanations for this finding, Pogarsky and Piquero (2003) propose that a positive relationship between punishment experiences and risk

perceptions could simply mean that punishment experiences identify those most committed to offending or that humans are illogical and subject to the gambler's fallacy (see also Piquero & Pogarsky, 2002). Termed the "resetting effect," they suggest that offenders who have a recent experience with punishment may think they are unlikely to get caught again in the near future (i.e., their risk perceptions actually decrease). Interestingly, the hypothesis of the augmentation of deviant behavior following a criminal sanction ordinarily falls under the purview of labeling theory.

Labeling Theory

This section provides an in-depth treatment of the one-hundred year history and development of labeling theory, which is an alternative theory of criminal sanctions. In doing so, it touches on the cyclical nature of labeling theory in academia which has involved states of development, prominence, irrelevance and revival. First, labeling theory's roots in the symbolic interactionism perspective are discussed, which provides the foundational ideas which were necessary for the emergence of a theory of criminal sanctions that anticipates problematic behavioral outcomes. Second, the basic idea of deviance amplification is traced back to the initial formulations that specify the effects of "tagging" one as a deviant. Third, the depth and breadth of labeling theory is considered by formalizing and discussing each of the theory's nine tenants. This discussion makes clear that labeling is a theory of criminal behavior which is independent from labeling as either a theory of law making or law enforcing. Honing in on the deviance amplification hypothesis (i.e., labeling as a theory of criminal behavior), fourth, important clarifications about the character and quality of deviance both before and after a labeling event are reviewed; the developmental nature and the critical role of formal labels in the transition from primary to secondary deviance are noted. Finally,

given the emphasis here on the criminal sanction as an independent variable, important developments roughly twenty years ago that have specified the causal pathways to secondary deviance and have sparked renewed interest in labeling theory are thoroughly examined.

The Roots of Labeling Theory

The roots of labeling theory are deeply entrenched in the sociological perspective of symbolic interactionism, which holds that social interaction and the communication of ideas, through language and symbols, is the lifeblood of human meaning (see Blumer, 1969). An individual's values, attitudes and self-identity all can be understood only in the context of social interactions with other members of society. Symbolic interactionism hinges on three premises: "human beings act toward things on the basis of the meanings that the things have for them…the meaning of such things is derived from, or arises out of, the social interaction that one has with one's fellows… [and] these meanings are handled in, and modified through, an interpretative process used by the person in dealing with the things he encounters" (Blumer, 1969).

While the first principal idea of symbolic interactionism describing human action is quite commonsensical, it is often neglected in modern sociological theories of human behavior (Blumer, 1969). Symbolic interaction theorists place considerable emphasis on understanding meaning as a key determinant of human actions and consider it misguided to focus research efforts on uncovering specific factors that influence behavior without considering meaning. According to the perspective, regarding meaning as a trivial intervening process or having causative factors envelop meaning unduly demotes its importance to behavior (Blumer, 1969).

In discussing the second key idea of symbolic interactionism, Blumer (1969) notes that meaning is neither physically inherent nor psychologically determined through human perceptions; rather, the meaning of activities, situations and persons is constructed through a process of social interaction. In addition to the importance of social interaction in creating meaning, the third chief notion of the perspective holds that a process of interpretation is critical. Occurring in two stages, an individual first communicates with his or herself and considers the things that have meaning and then handles meaning through interpretation. "The actors selects, checks, suspends, regroups, and transforms the meanings in the light of the situation in which he is placed and the direction of his action" (Blumer, 1969). In short, meanings have their influence on behavior through processes of social as well as self-interactions.

The process of self-reflecting over social interactions can be clearly seen in Charles Cooley's (1902) "looking-glass self," which is a prominent concept within the symbolic interaction tradition that is highly relevant to the labeling perspective. According to Cooley (1902), our idea of ourselves unfolds in a three-part process involving "the imagination of our appearance to the other person; the imagination of his judgment of that appearance, and some sort of self-feeling, such as pride or mortification" (p. 152). Importantly, "The comparison with a looking-glass hardly suggests the second element, the imagined judgment, which is quite essential" (Cooley, 1902, p. 152). Hence, the consideration of others' judgments of our appearance, not simply our appearance to others, is ultimately what drives self-feelings. The looking glass self draws attention to the importance of an individual's interpretations and expectations of social interaction as the key factor in eliciting a self-feeling that is

ultimately tied to one's behavioral response. In sum, the theoretical ideas developed within the symbolic interaction tradition of sociological thought set the stage for the emergence of a theory of labeling, which links criminal behavior with societal reactions or, more precisely, with perceptions and interpretations of social interactions.

Emergence of the Labeling Perspective

Drawing on the idea that self-conceptions stem from individual interpretations of one's beliefs regarding the way other individuals view him or her (Cooley, 1902), Frank Tannenbaum (1922; 1938) articulated the earliest versions of the labeling perspective and advanced the idea that labels play a key role in the development of anti-social behavior. Interestingly, his theoretical insights that have been firmly imprinted on the field of criminology grew, in part, out of his own intimate experiences on the wrong side of the law. Less than a decade after serving a year-long imprisonment sentence, Tannenbaum (1922) writes of the ramifications of incarceration maintaining that it is when an individual is "tagged" as a criminal and imprisoned that one's self- and social-worlds crumble beneath him or her. More specifically, individuals lose their own ambitions and interests and have conventional opportunities such as work slip away, impelling a change from a conformist to a deviant identity (Tannenbaum, 1922).[5]

These initial formulations of the labeling perspective drawn out of Tannenbaum's early writings on the American prison system (Tannenbaum, 1922) were more fully advanced later and applied to understand the influence of justice system contact on adolescents (Tannenbaum, 1938). In the midst of emerging perspectives of criminal

[5] Other scholars also noted the potential corrupting role of corrections endeavors in America (e.g., see Bonger, 1916; Shaw, 1930; Lombroso-Ferrero, 1911) but it was Tannenbaum's (1922) work that placed strong emphasis on these forces helping to birth the labeling perspective.

behavior that challenged the reign of biological positivism in the early to mid-1900s (Sellin, 1937; Sutherland, 1937; Shaw & McKay, 1942), Tannenbaum's (1938) labeling perspective judged criminal behavior not to be chiefly born at birth; rather, criminals were thought to be largely born out of a process of societal reaction to their behavior. "The first dramatization of the 'evil' which separates the child out of his group for specialized treatment plays a greater role in making the criminal than perhaps any other experience…The process of making the criminal, therefore, is a process of tagging, defining, identifying, segregating, describing, emphasizing, making conscious and self-conscious; it becomes a way of stimulating, suggesting, emphasizing, and evoking the very traits that are complained of" (Tannenbaum, 1938). Put plainly, an adolescent who has been "tagged" as a deviant assumes the menacing role (Tannenbaum, 1938). Labels possess their power because they are stigmatizing; deviant labels remain highly influential and consistently denouncing because negative images of criminals encompass society through various sources of media on a daily basis (Becker, 1963; Goffman, 1963).

Tannenbaum's (1922; 1938) work is especially important because it laid the foundation for a theory of human behavior—implicating formal intervention efforts in the production of anti-social conduct—and highlighted the importance of the labeling process for the lives of adolescents. The labeling perspective as a whole, however, extends well beyond a theory of criminal behavior. Before honing in on the major works that have expanded on the basic idea of deviance amplification, which is most relevant to the current study, each of the nine core ideas of labeling theory is outlined. Understanding these tenants helps to demonstrate that labeling as a theory of criminal

behavior (i.e., deviance amplification) is entirely separable from labeling as a theory of law making and law enforcing. In part, it is this very distinction that is responsible for the acceptance of deviance amplification as a mainstream criminological theory today.

Nine Tenants of Labeling Theory

Formulating nine key statements that summarize the key ideas of labeling theory and capture the breadth and depth of the perspective, Clarence Schrag (1971) writes:

> (1) no act is intrinsically criminal, (2) criminal definitions are enforced in the interest of the powerful, (3) a person does not become a criminal by violation of the law but only by the designation of criminality by authorities, (4) due to the fact that everyone conforms and deviates, people should not become dichotomized into criminal and non-criminal categories, (5) the act of 'getting caught' begins the labeling process, (6) 'getting caught' and the decision-making in the criminal justice system are a function of offender as opposed to offense characteristics, (7) age, socioeconomic class, and race are the major offender characteristics that establish patterns of differential criminal justice decision-making, (8) the criminal justice system is established on a freewill perspective that allows for the condemnation and rejection of the identified offender, (9) labeling is a process that produces, eventually, identification with a deviant image and subculture, and a resulting 'rejection of the rejectors' (Schrag 1971, pp. 89-91; see also Wellford, 1975, p. 333).

Wellford (1975) rightly notes the heart of labeling theory lies in the first, sixth, and ninth assertions; taken together, these assertions formulate a theory of law making, a theory of law enforcing, and a theory of criminal behavior, the latter of which is especially relevant to the current investigation which seeks to examine the effects of criminal sanctions on violent offending behavior. The nine propositions can be usefully divided into those most relevant to law making and enforcing and those more material to explaining criminal and deviant behavior.

Several theoretical statements directly pertain to labeling as a theory of law making and law enforcing. The first and second statements of labeling theory remind us that crime is a socially defined phenomenon and that the enactment and enforcement of

laws is guided by power relations. The sixth claim of labeling theory forms the basis of the "status characteristics hypothesis," which states that certain types of individuals will be more likely to experience deviant labels regardless of their offending behavior (see Paternoster & Iovanni, 1989). Tied to conflict and critical criminological thought (Bonger, 1916 [1905]; Marx, (1964) [1844]; Quinney, 1980), this proposition holds that who gets arrested, prosecuted and punished is driven largely by irrelevant, non-behavioral characteristics. An extension of the sixth assertion, the seventh tenant specifies which characteristics enter into the decision-making processes of criminal justice professionals which include an individual's gender, race, and socioeconomic status.

Any in-depth discussion of labeling theory necessitates consideration of the aforementioned propositions which are directly pertinent to labeling as a theory of law making and law enforcing; it is these more critical assertions that are partially responsible for the academic dismissal of labeling theory in general during the 1970s and 1980s (Wellford & Triplett, 1993). Critical scholars charged labeling theorists with focusing on non-powerful, easy targets such as "nuts, sluts, and preverts" (Liazos, 1972). According to critical scholars, the work of labeling theorists invoked issues of power and conflict incompletely (Best, 2004). Despite only borrowing key ideas from critical perspectives and refraining from becoming engulfed in ideology, labeling theory as a whole nevertheless became seen as wedded to conflict and critical thought greatly contributing to its rejection among mainstream scholars during the 1970s and 1980s (Wellford & Triplett, 1993).

Several tenants point to the importance of official labeling for characterizing one as a criminal and highlight the ramifications of the labeling process. Collectively, the third and fourth tenants suggest that since crime and deviance is fairly common in society, it is the act of officially stamping one as a deviant which is relevant to offender categorization. That is, classifications based on rule-violating behavior are argued to lack utility. It is seen as somewhat problematic that a person who has violated the law in which no social reaction occurs is not conceived to be a deviant, but a person that is falsely accused of violating the law in which a social reaction occurs is thought to be a deviant (Gibbs, 1966). This criticism is certainly warranted and it makes little sense to contend that studying rule-breaking behavior is unimportant.[6] At the same time, these two propositions do make clear the emphasis that labeling theory places on "tagging" and the importance of this event for designating one as an outsider. Gibbs (1966) claims that labeling theorists "have never specified exactly what kind of reaction identifies deviant acts" (p. 13). While there is some degree of imprecision, the fifth proposition does state that "getting caught" is the key event that sets in motion the process of labeling. Given the emphasis that is placed on official acts and labeling by individuals in positions of authority (Becker, 1963; Garfinkel, 1956), getting caught by the police serves as a very logical point of origination of the labeling process.

Once an individual has been caught (i.e., an official label has been applied), the ninth proposition maintains that the end result is the worsening of delinquency. This statement is most important and forms the basis of what is known as the "deviance

[6] Indeed, the very idea of deviance amplification requires that rule-breaking behavior is an important area of study in criminology. Once an individual is labeled, it is their rule-breaking behavior that is ultimately thought to increase (Paternoster & Iovanni, 1989). In other words, it seems decidedly less fruitful to study the effects of labeling on labeling (as opposed to the effect of labeling on rule-breaking behavior).

amplification hypothesis," which specifies that the application of a deviant label ultimately leads to secondary deviance (Schrag, 1971; Paternoster & Iovanni, 1989). The adverse ramifications that official labels may have for offending behavior are made possible by the eighth tenant. This proposition is simply an acknowledgment that operations of the criminal justice system are predicated on human free will and, consequently, rejection of the individual does not conflict with prevailing thoughts on law enforcement. Recall, the ideas of free will and rationality are the same notions upon which deterrence theory is based (Bentham, 1948).

While criminal justice system operations may be based on theories of criminal deterrence, deviance amplification remains a possible outcome of official intervention because criminal justice system professionals do not contend that the experience of getting caught (i.e. being labeled a criminal) is a problem in and of itself—it only becomes problematic if it leads to adverse outcomes consistent with the deviance amplification hypothesis. Making explicit the ideas implicit in the ninth tenant of labeling theory, the theoretical literature that is responsible for the development and eventual formulation of testable hypotheses of deviance amplification is now reviewed.

Primary and Secondary Deviance

The labeling perspective became considerably clearer with the advent of Lemert's (1951) distinction between primary and secondary deviance coupled with his explanation of the transition between these two types of deviance (i.e., a process of individual action and societal reaction unfolding overtime). Primary deviance is characterized by anti-social behavior brought on by a wide variety of circumstances including those that may be sociological, cultural, psychological, biological, or situational in nature. While the initial sources of anti-social behavior are many and varied, primary

deviance is “rationalized or otherwise dealt with as functions of a socially acceptable role” (Lemert, 1951, p. 75). Put differently, an individual perceives his or her deviant behavior to be justifiable or not necessarily inconsistent with society’s behavioral expectations. Thus, participation in primary deviance does not have a sizeable influence on an individual’s sense of self or his or her perceived role in conventional society (Lemert, 1967).

While changes in attitudes, behaviors and roles are not evident with primary deviance, these features form the hallmark of secondary deviance. According to Lemert (1951), participation in secondary deviance takes place when “a person begins to employ his deviant behavior or a role based upon it as a means of defense, attack, or adjustment to the overt and covert problems created by the consequent societal reaction to him” (p. 76). At this stage, the multitude of possible causes that could have driven patchy acts of primary deviance become largely inconsequential and societal reaction replaces these factors as the preeminent cause of more intense and structured nonconforming behavior (Lemert, 1951). Sharing the common cause of formal societal reaction, it is secondary deviance that proves to be most problematic; thus, the very behavior society was attempting to regulate conspicuously takes a turn for the worse.

Understanding the character of primary and secondary deviance is important but it is of equal significance to understand how individual actions and societal reactions bring about this marked shift in the quality and character of deviant behavior. Outlining the transition from primary to secondary deviance, Lemert (1951) sketches an interaction sequence that progresses in roughly eight steps:

> (1) primary deviation, (2) social penalties, (3) further primary deviation, (4) stronger penalties and rejection, (5) further deviation, perhaps with

> hostilities and resentment beginning to focus upon those doing the penalizing, (6) crisis reached in tolerance quotient, expressed in formal action by the community stigmatizing the deviant, (7) strengthening of the deviant conduct as a reaction to the stigmatization and penalties, (8) ultimate acceptance of deviant and social status and efforts at adjustment on the basis of the associated role (p. 76).

This description of the typical labeling process brings two ideas to light, which are slightly at odds with one another. First, the passage from primary to secondary deviance usually does not take place overnight. There is an ongoing interaction between the individual and society that occurs over a period of time, implying that labeling is a developmental process. However, following cycles of acts of primary deviance and societal reactions to them is the momentous event of being officially labeled a deviant. The problematic behavior reaches a point where it is no longer tolerated and a formal intervention occurs that officially stamps an individual as a deviant. While the transition to secondary deviance tends to unfold over time (Lemert, 1951; 1967), transformations in identity are most likely during "status degradation ceremonies" especially those that involve an official act (Garfinkel, 1956). Second, then, key stigmatizing events by formal institutions play a decisive role in an individual's behavioral conversion (Becker, 1963).[7] Importantly, it is formal interventions such as arrests or convictions that serve as official labeling events; indeed, the criminal justice system maintains exclusive rights over whom is to be considered criminal (Garfinkel, 1956). These pivotal formal labeling events are of primary interest to labeling scholars studying how sanctions may influence the transition from primary to secondary deviance.

[7] It is important to note that formal stigmatization is most likely to have a problematic effect on behavior when there is no attempt made to reintegrate the nonconforming individual back into conventional society, which is typical of formal intervention efforts in the United States (Braithwaite, 1989).

One of the chief criticisms levied against labeling theory is that it provides no explanation for criminal or deviant behavior in the first place (Akers, 1968) and labels may be neither necessary nor sufficient conditions for the maintenance or amplification of criminal behavior (Mankoff, 1971). Davis (1961, p. 461, *emphasis in original*) notes that "labeling theorists who claim that reactive processes of society provide *the* causal factor in deviance are thereby providing only a partial view of a highly complex problem." At the same time, some maintain that there has been a plethora of theories that have been advanced to explain specific aspects of anti-social behavior, with few attempts to explain criminal behavior in any general sense (Lemert, 1951). "This has been occasioned in no small way by the preoccupation with the origins of pathological behavior and by the fallacy of confusing *original* causes with *effective* causes. All such theories have elements of truth, and the divergent viewpoints they contain can be reconciled with the general theory here if it is granted that original causes or antecedents of deviant behaviors are many and diversified" (Lemert, 1951, p. 75 *emphasis in original*). In other words, while there are numerous divergent primary sources of violent and anti-social behavior, the labeling process is a general explanation of behavior as it results in the intensification and/or long-term stability of the problematic behavior irrespective of the initial origins. As will become clear, this is a critical theoretical notion which directly implicates formal intervention as a potential turning point, albeit in a socially undesirable direction, in a violent behavior trajectory.

The involvement of formal institutions in the labeling process is more nuanced than simply stamping rule-breaking criminals as deviants; formal institutions first define what is right and wrong and then utilize their authority to select what codes to enforce

and which individuals to label as deviant (Becker, 1963). In one of the most recognizable assertions of the labeling perspective, Becker (1963, p. 9) states that "deviance is not a quality of the act the person commits, but rather a consequence of the application by others of rules and sanctions to an 'offender.' The deviant is one to whom that label has successfully been applied; deviant behavior is behavior that people so label." In other words, specific behaviors and certain individuals are not necessarily inherently deviant. Instead, acts and actors become deviant when society defines them as such. Rule-breaking behavior involves violating a code of conduct but being a deviant requires the acquisition of a label.

Becker's (1963) delineation between rule-breaking behavior and the affixation of deviant labels creates an important two-by-two labeling typology. He refers to individuals who do not engage in rule-breaking behavior and do not have deviant labels as "conformists." This represents the majority of people in society. Individuals who become involved in rule-breaking behavior and are appropriately labeled for doing so are referred to as "pure deviants." For some individuals, however, their rule-breaking behavior and their assigned label or lack thereof does not neatly align. Specifically, those individuals who violate codes of conduct but are not labeled deviant are known as "secret deviants" and those who do not violate rules but are nevertheless labeled are known as "falsely accused" (Becker, 1963). Labeling theory suggests a worsening of behavior is likely for those labeled who were originally involved in primary deviance (pure deviants) and the onset of real problematic behavior for those labeled who were initially engaged in conventional activities (falsely accused). Absent the attachment of a formal label, the theory predicts deviance amplification processes will not likely be

triggered; and, this is thought to hold irrespective of whether the individual actually engages in rule-breaking behavior (i.e., whether an individual is a secret deviant or conformist).

While the experience of being formally labeled is paramount to the transition to secondary deviance, some individuals may accept and others may reject their newly assigned statuses (Becker, 1963; Davis, 1961; Turner, 1972; see also Paternoster & Iovanni, 1989). Becker (1963) refers to individuals who are demarcated as deviant and view themselves as different from conventional society as “outsiders.” Importantly, outsiders are individuals whose deviant identity becomes their “master status” meaning that its effect is all-encompassing and overbearing (Becker, 1963). Outsiders reshape themselves in light of their new trenchant identity and simultaneously have their social experiences and opportunities remolded around them. More specifically, the acceptance of a deviant label causes a breakdown in the social resources necessary to successfully navigate the conventional social world and pushes the individual toward deviant groups that further solidifies the transition to a deviant career (Becker, 1963). It is the acquisition of deviant peer groups, following both the experience and acceptance of a deviant label that marks the final step in the development of a career criminal (Becker, 1963; see also Paternoster & Iovanni, 1989). While the works of Becker (1963) and Lemert (1951; 1967) are among the most important contributions to the labeling perspective, their efforts fall short of fully explicating a testable theory of labeling. The causal pathways to secondary deviance became formal hypotheses some twenty years after these important contributions were first introduced to the field.

The Causal Pathways to Secondary Deviance

Following a period of intense skepticism about labeling theory that centered on its definitional weaknesses (Gibbs, 1966; 1972), its perceived strong ties to conflict theory (Wellford & Tripplett, 1993), and its key theoretical problems (Akers, 1968), new life was given to the theory with the publication of two seminal articles in 1989 (see Paternoster & Iovanni, 1989; Link, Cullen, Struening, Shrout, & Dohrenwend, 1989). Together, these two studies helped to clarify the specific causal pathways that lead to secondary deviance and to discharge those criticisms levied against labeling theory which were unsubstantiated. Paternoster and Iovanni (1989) maintained that the prototypical causal pathway to secondary deviance begins with a public labeling event which leads to, in order, the exclusion from conventional activities, the alteration of self-identity, the acquisition of supportive deviant others and, finally, secondary deviance. If a label is hidden from public view, however, they claim that it is unlikely to set in motion the sequence of events that lead to secondary deviance. Just as if an individual was not labeled at all, a private label will typically have no influence on the individual and the result of the labeling event will be uninterrupted access to conventional activities as well as maintenance of one's identity and supportive conventional others. In general, labeling theory suggests that much depends upon whether a label is made public or kept private.

The roots of labeling theory in symbolic interactionism remind us that there may be more to the labeling process than the objective labeling event. One thing that is resoundingly clear from prior empirical work on labeling theory is that the specific causal pathways thought to occur when labels are either public or private do not always occur (Paternoster & Iovanni, 1989). In other words, an individual can disregard or, ultimately,

accept a deviant label in cases that each of these main pathways does not anticipate. In instances where a label is made public, two possibilities arise in which an individual may not become involved in secondary deviance. First, if societal reactions to a public labeling event are such that they are ultimately inclusive, this can result in the reintegration of the individual back into society (Braithwaite, 1989). Reintegrative responses seek to prevent the labeled individual from being excluded from normal routines and conventional activities, which can thwart secondary deviance (Paternoster & Iovanni, 1989). Unfortunately, the United States generally does not engage in the reintegrative shaming of violent and other criminal offenders (Braithwaite, 1989). Second, if an individual is excluded from normal routines and conventional activities, the individual can potentially reject their ascribed status in a process known as "deviance disavowal" (Davis, 1961). Despite being excluded from society, the individual maintains their conventional self-identity and, therefore, does not progress to secondary deviance.

Just as a public label may not always lead to the problem of secondary deviance, a private label may not always prevent it. If a label is kept private, an individual may nevertheless reorient his or her identity in accordance with it in a process known as "deviance avowal" (Turner, 1972). Paternoster and Iovanni (1989) argue that even if a private label helps an individual escape social exclusion from conventional life, the individual alters his or her identity anyway through a process akin to "self-labeling" (see Thoits, 1985). The private labeling event influences the person's identity directly, guiding the individual toward deviant peers and secondary deviance. Thus, the deviance amplification hypothesis is a probabilistic statement of causation which holds that a labeling occurrence may block one from conventional activities and normal

routines, may alter one's of self-identity, and may lead to the acquisition of supportive deviant others, which could ultimately result in secondary deviance (Paternoster & Iovanni, 1989). While additional empirical work on the labeling process is needed, some evidence suggests that labels influence subsequent behavior and that deviant peers and blocked access to conventional opportunities are especially important intervening mechanisms (Bernburg, 2002; Bernburg & Krohn, 2003; Bernburg, Krohn, & Rivera, 2006).

During the peak of labeling theory in the 1960s, the scholarly idea that mental patient labels were directly responsible for underlying chronic mental illnesses was advanced (Scheff, 1966). This bold assertion on the etiology of mental illness was argued to be inaccurate since many individuals suffered severe mental illnesses irrespective of any secondary deviance effects (Gove, 1980). Individuals undergo systematic behavioral and/or mental health testing prior to being labeled suggesting that serious mental illness is a preexisting condition (Gove, 1980). Link and colleagues (1989; see also Link, 1982; 1987; Link et al., 1987) saw merit in both claims about the contributions of labels to behavioral problems and critiques regarding the direct effects of labels. Noting that most prior research focused on the direct effects of labels or on the indirect effects through self-identity only, Link (1982) first drew attention to other potential intervening mechanisms such as education and employment. To the extent that labels influence the ability to secure employment, earn sufficient income, and achieve educational goals, additional stressors may be placed on the individual resulting in increased mental health problems (Link, 1982). Link (1982) found that being labeled a mental patient had adverse affects on work status and income, controlling for

psychiatric problems among other variables. Five years later, Link (1987) discovered that, in addition to problematic income and work outcomes, labels led to self-devaluation.

Drawing on his prior research on mental patients, Link along with his colleagues (1989) developed a “modified labeling theory” of mental patients, which has important relevance for investigations into the influence of sanctions on subsequent criminal behavior (Krohn, Lopes, & Ward, in press). Their theory suggests that individuals first consider what it means to be a mental patient and think through the anticipated discrimination and devaluation that accompanies such a label. If an individual is labeled, then these societal conceptions become directly relevant to the mental patient but if the individual is not labeled then these impressions are immaterial. Their model suggests that individuals who experience an official label will respond to it by keeping it a secret, withdrawing from social life, or educating members of society. These tactics, especially withdrawing from social interactions, can lead to adverse consequences for one’s social network ties, earning power, and self-esteem.

Regardless of one’s coping response to being labeled, this experience can also directly result in problematic self-esteem, occupation and social network outcomes. That is, an individual’s perception of the discrimination and devaluation experienced by mental patients leads to an “expectation of rejection” which is responsible for the social and occupational difficulties (Link, 1987; Link et al., 1989). Importantly, self-devaluation, loss of earning power and problems with prosocial network ties are thought to ultimately exacerbate the underlying mental illness (Link et al., 1989). Thus, secondary deviance arises indirectly and self-esteem, earning power, and social

network ties are the important mediating variables which transmit the effects of deviant labels, irrespective of whether coping strategies such as withdrawal are invoked in the causal chain (Link et al., 1989; see also Paternoster & Iovanni, 1989).

In sum, Paternoster and Iovanni's (1989) theoretical clarifications of the labeling process for criminals and Link and colleagues' (1989) discussions of the effects of mental patient labels suggest that official labels ultimately lead to secondary deviance and point to similar intervening variables as the ones that are integral to the labeling process. However, there is one major point of contention; Link and colleagues (1989) do not specify a specific causal chain for the mediating variables and, instead, suggest each one of the intervening variables may be relevant in its own right. While empirical research is needed to test these slightly different assertions, the deviance amplification hypothesis is clear in its implication of access to conventional activities, sense of self-image, and social network ties as the causal factors that lead to secondary deviance. Nearly one-hundred years of refinement has resulted in testable hypotheses and a theory of criminal behavior that, not only stands apart from labeling as a theory of law making and law enforcing, but is gaining traction among mainstream scholars as a serious explanation for criminal behavior. This is due in large part to its ability to neatly fold into existing theories of crime and deviance—especially those theories aimed at explaining crime over the life-course (Sampson & Laub, 1997).

CHAPTER 3
THE DEVELOPMENTAL AND LIFE-COURSE PERSPECTIVE

Chapter 3 provides an overview of the origins and major principles of the developmental and life-course perspective. First, the emergence of the developmental and life-course criminological perspective is traced back to the intriguing relationship between age and crime. Next, the formal birth of life-course criminology in the early 1990s is explored focusing on the timely arrival of both state dependence and dual-taxonomy approaches to explaining the meaning of the aggregate age-crime curve. While both theoretical explanations of offending focus on stability and change, they do so in decidedly different ways which has important implications for the current research. The major building blocks of long-views of crime—trajectories, transitions and turning points—are then presented. Finally, the important and highly controversial topic of whether distinct criminal trajectory groups exist is discussed. In short, Chapter 3 provides the necessary background of the major ideas and theories of the developmental and life-course perspective for the consideration of the effects of sanctions over time across different offending subpopulations—the focus of Chapter 4.

The Age-Crime Curve

One of the closest things in the field of criminology to an empirical fact is the existence of the aggregate age-crime curve, which appears as a bell-shaped distribution that is positively skewed (Hirschi & Gottfredson, 1983). Crime increases swiftly during the early teen years, reaches a peak during late adolescence, undergoes a sharp decline in early adulthood and then continues to gradually fall off through adulthood (Hirschi & Gottfredson, 1983). Given these characteristics, it comes as no surprise that most theories of crime, and the empirical efforts to test them, have focused almost

exclusively on the offending behavior of adolescents and young adults (Sampson & Laub, 1993). It is these individuals who commit the bulk of property and violent offenses, therefore providing the largest concerns for criminologists and policy makers alike. Nevertheless, focusing on this group neglects the significance of childhood and adulthood as periods relevant to explaining criminal behavior (Sampson & Laub, 1993).

The relationship between age and crime has been argued to transcend crime types, cultures, places and times (Hirschi & Gottfredson, 1983). In a noteworthy assessment of long-term historical trends in violent crime, Eisner (2003) finds there is strong similarity in the age patterns of violent crime across six different European areas spanning four centuries. Still, not all empirical evidence points to the position that the age-crime curve is invariable. For instance, Monnkonen (2001) examined violent crime patterns over two centuries and concluded that the especially large number of offenders in late adolescence/early adulthood appears to be a relatively new phenomenon. Moreover, other research has found that the age-crime curve appears to differ for divergent crime types. For example, violent criminal offending peaks later than non-violent crime and decays less rapidly (Blumstein & Cohen, 1987; Blumstein, Cohen, & Farrington, 1988; Farrington, 1986). This research has cast some doubt on the uniformity of the age-crime curve throughout history and the extent to which age is similarly related to participation in various forms of crime. Nevertheless, the relationship between age and crime in the aggregate remains one of the most consistent empirical findings in criminology.

Most early attention given to the robust relationship between age and crime turned out to be more practical and descriptive in nature than theoretical (see, however,

Greenberg, 1977; Farrington, 1986). For instance, two National Academy of Science panels in the late 1970s and 1980s focused on the practical issues of deterrence and incapacitation (Blumstein, Cohen, & Nagin, 1978) and on describing and defining the importance of criminal careers (Blumstein, Cohen, Roth, & Visher, 1986). In speaking on the use of the "career" concept in criminology, Gottfredson and Hirschi (1988, p. 38) state that "Blumstein et al. do not bring to the career concept meanings derived from a conception of crime or a theory of criminality. Rather, they simply announce a decision to apply it to crime." Ten years following Hirschi and Gottfredson's (1983) seminal article, the formal birth of life-course criminology occurred as the most notable attempts to make theoretical sense of the aggregate age-crime curve and explain criminal behavior at various stages of human development were advanced (see Moffitt, 1993; Sampson & Laub, 1993).[8] It is these initial efforts, which remain leading explanations nearly twenty years later, which paved the way for a number of additional developmentally sensitive explanations of criminal behavior (see Lahey & Waldman, 2005; Catalano & Hawkins, 2005; Thornberry & Krohn, 2005; Farrington, 2005; LeBlanc, 2005; Wikstrom, 2005).

The Birth of Developmental and Life-course Criminology

It has been found that "adult antisocial behavior virtually requires childhood antisocial behavior" (Robins, 1978, p. 611) but it has also been found that "most antisocial children do not become antisocial as adults" (Gove, 1985, p. 123). These

[8] Moffitt's (1993) and Sampson and Laub's (1993) theories were not the first to highlight the developmental nature of criminal offending. For example, Thornberry (1987) proposed an interactional theory of crime and delinquency, which suggested that learning and control factors affect crime and delinquency all in an interacting fashion over the life-course. While this theory is important and has been applied to explain stability and change in offending over time (see Thornberry & Krohn, 2005), the formal arrival of the life-course criminology paradigm is probably best traced back to 1993.

classical statements highlight the two fundamental issues the developmental and life-course perspective attempts to address—stability and change in behavior. There is remarkable stability in offending over time; indeed, the best predictor of future behavior remains past behavior (White et al., 1990). However, stability in crime and violence is not perfect and is more nuanced than at first glance. More specifically, stability refers to between-individual continuity in behavior as opposed to absolute stability in behavior (Farrington, 1989; 1992; Tracy & Kempf-Leonard, 1996). In other words, there is considerable consistency in the "rank ordering" of individuals' level of criminal involvement but behavioral changes within-individuals are real and likely possibilities (Sampson & Laub, 1992).

Taking decidedly different approaches in their attempts to explain the age-crime curve and to address issues of stability and change in criminal behavior over time, the publication of two original works (Moffitt, 1993; Sampson & Laub, 1993; see also Sampson & Laub, 1992) thrust the life-course perspective into the criminological spotlight in the early 1990s.[9] On the one hand, Moffitt (1993) maintained that two

[9] There are three major types of explanation for the aggregate age-crime curve, which include population heterogeneity, state dependence and taxonomy approaches (Ezell & Cohen, 2005). While there are several specific theories that fall within these three categories, Gottfredson and Hirschi's (1990) self-control theory, Sampson and Laub's (1993; 2003) age-graded social bonding theory, and Moffitt's (1993) dual-taxonomy theory are the most notable and probably also the most representative of the types of accounts. The latter two theories represent life-course approaches to the study of crime and are discussed in this chapter at-length. While not a life-course theory, Gottfredson and Hirschi's (1990) self-control theory warrants mention as a competing explanation, or more precisely, an alternative idea. Gottfredson and Hirschi (1990) draw an important distinction between criminality and crime and propose that criminality (i.e., one's propensity to engage in crime) remains relatively stable throughout the life-course whereas participation in crime changes over time in accordance with the aggregate age-crime curve. Gottfredson and Hirschi (1990) claim that the relationship between age and crime cannot be explained by current criminological theories and, instead, suggest that age has a direct effect on crime (see also Hirschi & Gottfredson, 1983). However, explanations centered on maturation are not really explanations at all as they simply claim that things just unfold naturally over time (Dannefer, 1984). Thus, the population heterogeneity account highlights potentially important and persistent differences between individuals who may help to explain stability in crime but fails to explicitly address the meaning of the aggregate age-crime curve and change in offending behavior.

fundamentally different types of individuals—life-course-persistent and adolescence-limited offenders—were characterized by stability and change in offending, respectively. On the other hand, Sampson and Laub (1993) argued that all individuals experience both stability and change in behavior over time. Opposing the idea of different types of offenders, they have maintained over the years that the "field of life-course criminology would benefit from a more process-oriented, generalized account of within-individual behavioral stability and change over time" (Laub, Sampson, & Sweeten, 2006, p. 329).

The concurrent arrival of these developmental theories with different emphases helped to create a synergistic effect that propelled life-course criminology to the forefront of criminological thought; despite some noteworthy theoretical advances in the field since then (e.g., see Akers, 1998), the perspective has remained the prevailing organizing principle of modern criminology. It is worth noting that some claim the attention given to developmental and life-course explanations is largely unfounded. For instance, it has been argued that developmental and life-course theories merely borrow concepts from other criminological theories failing to offer any truly unique explanations of criminal offending (Akers & Sellers, 2009, pp. 319-320). While life-course criminological theories do use some of the same variables as existing theories of crime, they apply them in different ways resulting in novel explanations of offending over the life-course. For instance, some theories suggest that the causes of crime and delinquency are fundamentally different for certain types of individuals. Additionally, some theories anticipate that the causes of crime for the same individual may vary over time. The idea of studying crime and violence over different developmental stages across the life-course has firmly taken hold, serving to both invigorate numerous

substantive areas of criminological research as well as spark heated theoretical debates on the meaning of criminal involvement over time.

Dual Taxonomy Theory

Foreshadowing taxonomic theoretical efforts, Hirschi and Gottfredson (1983) write: "Taken at face value, the career criminal notion suggests division of the criminal population into discrete categories: those who offend occasionally or sporadically for a typically brief period of time and those who offend regularly over an extended period of time. If the division proves valid, two general age-crime distributions could be extracted from the data" (p. 574). Moffitt (1993) was the first to run with this basic idea, claiming that there are two fundamentally distinct types of criminals—life-course-persistent and adolescence-limited offenders.

Life-course-persistent offenders constitute only approximately ten percent of all offenders but exhibit the most problematic behavior. These individuals are troubled with neuropsychological deficits stemming from a host of sources that include, but are certainly not limited to, maternal drug abuse, child abuse/neglect and exposure to toxic agents in the womb. Moffitt (1993) maintains there is an important overlap between individuals who have neuropsychological deficits and those who have high exposure to criminogenic environments. It follows that children who are most at risk for neuropsychological deficits are often also situated within environments that make them least likely to overcome these problems. They tend to have parents that lack resources and cognitive abilities to cope with their difficulties. Moreover, these individuals may reside in dysfunctional homes that also lay in neighborhoods with insufficient social and community resources. Given the experience of both biological and sociological setbacks, evocative interactions are seen as an important contributing factor in the

development of life-course-persistent offending. An evocative interaction occurs when a child's initial problematic behaviors, which may stem from neuropsychological deficits, elicit destructive responses from parents or other individuals who serve to further draw out the child's problematic behavior. In short, possessing neuropsychological deficits and being exposed to problematic environments often co-occur; the compounding effect of these two factors during the formative years provides the recipe for the emergence of the life-course-persistent offender.

Once the path of a life-course-persistent criminal trajectory has been charted, these offenders maintain their status through both reactive and proactive interactions (Moffitt, 1993). The former refers to differences in the way these individuals experience and interpret interactions with others whereas the latter refers to self-selection into environments that support one's criminal lifestyle. Their responses to social situations combined with their choices for social interaction keep these individuals locked into their chronic offending trajectory.

Evocative, reactive and proactive interactions all lead to adverse ramifications for the individual across the life-course. First, life-course-persistent offenders experience contemporaneous consequences by carrying with them into specific situations the very traits that have caused trouble in the past. Second, these individuals experience cumulative consequences by having prior differences and problems snowball over time. Importantly, both contemporaneous and cumulative consequences are thought to narrow options for change. Specifically, Moffitt (1993) suggests "that opportunities for change will often be actively transformed by life-course-persistents into opportunities for continuity." Hence, life-course-persistent offending is an ongoing developmental

process in which offenders actively participate, even if unwittingly, in the maintenance of their criminal identity.

A second group of offenders, who constitute the overwhelming majority of all individuals, are known as adolescence-limited offenders (Moffitt, 1993). The crime committed by this group has a distinct etiology and their offending is characterized as being a normal part of human development. Indeed, a small, third group of individuals, who abstain from crime altogether, are the men and women who are thought to be abnormal (Moffitt, 1993). The adolescence-limited group neither engages in crime during childhood nor offends during adulthood. By definition, this group fills out the bell shape characteristic of the age-crime curve (their life-course-persistent counterparts are responsible for the long tail that gradually decreases through late adulthood). During the teenage years, adolescence-limited offenders mimic the behaviors of life-course-persistent offenders because they experience a maturity gap between their biological and social ages. Youths' decisions to commit minor crimes and status offenses as well as use drugs are reinforced by the fact that these behaviors provide adolescents with a way to assert their independence and demonstrate their readiness for adult activities. Unlike life-course-persistent offenders, adolescence-limited offenders are able to desist from crime because they are responsive to changing reinforcement contingencies. While offending during the late teenage years serves a function during this time period (i.e., the assertion of independence), they abandon it for conventional pursuits once they reach adult status when the rewards and benefits associated with fulfilling conventional adult roles become apparent.

In sum, Moffitt's (1993) theory as originally formulated predicts two qualitatively distinct types of offenders whose distributions, when superimposed, form the aggregate age-crime curve. Her theory explains both the large number of offenses committed by adolescents and the sudden drop and apparent gradual decline in offending throughout adulthood, which are characteristic of the relationship between age and crime in the aggregate. The dual taxonomy suggests that adolescence-limited individuals undergo change in criminal behavior over time whereas life-course-persistent individuals experience stability in offending across the life-course. As will be shown, categorization of offenders yields curious hypotheses about the influence of sanctions for different offending subpopulations.

Age-Graded Social Bonding Theory

Noting that prior research has suggested the existence of both continuity and discontinuity in offending, Sampson and Laub (1993) develop a theory that seeks to explain both of these empirical facts. Unlike Moffitt's (1993) theory which is characterized by stability in one group and change in another, Sampson and Laub's (1993; 2003; see also Laub & Sampson, 2003) age-graded social bonding theory stresses stability and change within every individual. Borrowing ideas from Hirschi's (1969) social-control theory and Shaw and McKay's (1942) social disorganization theory, they maintain that structural context (e.g., low family SES, residential mobility, mother's employment) influences crime and delinquency through its effects on informal, family and school social controls. Given there is considerable stability in structural context over time (Shaw & McKay, 1942) and thus constancy in informal social controls (see Bursik, 1988; Sampson & Groves, 1989), they argue that there is continuity in behavior throughout the life-course (Sampson & Laub, 1993; 2003). While anti-social

behavior in adolescence tends to carry over into adulthood, acquiring prosocial social bonds in adulthood results in the termination of criminal offending.

Sampson and Laub (1993; 2003) emphasize the quality and character of the social bonds, principally in the family and employment arenas, as opposed to the specific timing of the social bonds.[10] Hence, it is more important to marry someone that has the qualities of a good spouse at any time in adulthood than it is to marry someone without these qualities at a particular age. They maintain that the "pathways to crime *and* conformity are mediated by social bonds to key institutions of social control" (Sampson & Laub, 1993, *emphasis in original*). Then, their age-graded social bonding theory acknowledges that change in behavior is a two-way street; just as the acquisition of strong social bonds to conventional society during adulthood is hypothesized to be the driving force behind criminal desistence, the erosion of ties to these social institutions is thought to increase the likelihood of participation in criminal behavior. One unique aspect of Sampson and Laub's (1993; 2003) theory is this ability to not only explain criminal desistence but also explain the onset of offending in adulthood. In 2003, Sampson and Laub extended their age-graded social control theory to include notions of criminal opportunity and incorporate individual choice as important factors in influencing crime (Laub & Sampson, 2003). They now note that social control, routine activities and human agency, directly and interactively, influence offending across the life-course (Laub & Sampson, 2003). This inclusion of human agency is particularly noteworthy and relevant for the current study that seeks to place sanction effects within a developmental and life-course perspective.

[10] Some developmental theories, however, place heavy emphasis on timing (see Thornberry, 1997).

Sampson and Laub's (1993; 2003) theoretical approach to explaining the age-crime curve does not see it as the merging of two distinct curves (cf. Moffitt, 1993); rather, the aggregate age-crime curve is viewed as a conglomeration of many unique, individual criminal trajectories over time. All offenders terminate their criminal activity at one time or another (Sampson & Laub, 2005a; 2005b). In this view, the tail of the aggregate age-crime curve is a reflection of differences in the timing of essentially inevitable commitments to conventional ways in adulthood. Thus, most individuals are strongly bonded to social institutions in early adulthood and, by late adulthood, almost everyone has claimed a stake in conformity. Central to Moffitt's (1993) dual taxonomy and Sampson and Laub's (1993; 2003) age-graded social control theories as well as other developmental and life-course theories are the ideas of trajectories, transitions and turning points. In preparation for placing sanction effects in a developmental perspective, it is these important building blocks of the perspective which are now discussed.

Trajectories, Transitions, and Turning Points

"A trajectory is a pathway or line of development over the life span such as work life, marriage, parenthood, self-esteem, and criminal behavior" (Sampson & Laub, 1992, p. 64). In a full life-course view, a complete offending trajectory would include one's criminal involvement over childhood, adolescence, early and even late adulthood. Thornberry (1997) maintains that trajectories have several important dimensions, two of which include entrance and success. Entrance refers to the fact that some individuals embark on a trajectory whereas other individuals do not. With respect to criminal behavior, a small minority of individuals refrain from engaging in criminal behavior at any age (Moffitt, 1993). These individuals do not offend and therefore do not enter into

a criminal trajectory. Everyone else, however, enters into a criminal trajectory which has some shape or another over the life-course (Sampson & Laub, 2005a).

While entrance distinguishes who participates in a given trajectory, the dimension of success acknowledges there are important differences in the quality and character of a trajectory for different people. For example, most individuals enter into a work trajectory but only some have highly fulfilling careers across the life-course. With respect to crime, most individuals will enter into a criminal trajectory at some point and there will be varying degrees of "success" or involvement in crime across the life-course (Thornberry, 1997). Recalling the idea of between-individual stability, this differential success between individuals will be largely preserved, though not perfectly, across the life-course. Irrespective of this between-individual continuity in behavior, however, it is expected that there will be changes in success experiences within-individuals over time. How these changes are thought to occur is now explored.

The trajectory concept is often taken to indicate one's behavioral fate is essentially sealed (Sampson & Laub, 2005a). However, an individual's criminal trajectory is not etched in stone and can potentially be altered in meaningful ways. Termed transitions, important life events that occur over the short-term such as marriage and justice system contact are embedded within trajectories (Elder, 1985). In some cases, these transitions can serve as turning points in one's trajectory (Elder, 1985). Several different life transitions have been considered as capable of potentially turning a criminal trajectory for better or worse; these include marriage (Laub, Nagin, & Sampson, 1998; Sampson & Laub, 1993; 2003; Laub & Sampson, 2003; Sampson, Laub, & Weimer, 2006), employment/work (Uggen, 2000; Uggen & Staff, 2001; Sampson &

Laub, 1993; 2003; Laub & Sampson, 2003; cf. Wright & Cullen, 2004) military involvement (Elder, 1986; Sampson & Laub, 1993; 1996; Laub & Sampson, 2003) and, importantly, justice system contact (Moffitt, 1993; 2006; Sampson & Laub, 1997).

Aside from the type of specific transition that is experienced (e.g., justice system contact, marriage), there is some debate as to what makes a life transition a turning point in a criminal trajectory. Thornberry (1997) discusses the importance of timing, which refers to the idea that transitions which occur at specific times in the developmental cycle are likely to alter one's trajectory. For instance, the experience of off-time or precocious transitions to adulthood, which are increased by gang membership in adolescence, can have lasting adverse effects on criminal behavior in adulthood (Thornberry, Krohn, Lizotte, Smith, & Tobin, 2003; Krohn, Ward, Thornberry, Lizotte, & Chu, 2011). The timing dimension of a trajectory emphasizes the fact that the exact same event such as employment may have different outcomes on one's trajectory at different points in time. Recall that some place stronger emphasis on the quality or character of the event (e.g., see Sampson & Laub, 1993).[11] Hence, if one were formally arrested, as opposed to merely being questioned by the police, the event would hold more salience. As a consequence, the experienced transition might be more likely to serve as a turning point in one's offending trajectory. Then, both the timing and the quality of the transition are important factors which may influence the likelihood of a life transition becoming a turning point.

[11] Interestingly, though, their work sometimes neglects this argument. For instance, they explore the causal effect of marriage in general—not wedlock to a prosocial or resourceful spouse (e.g., see Sampson, Laub, & Wimer, 2006).

There is also differing opinions as to what constitutes evidence of a turning point. In one view, turning points have been conceptualized as being abrupt shifts in one's trajectory (Elder et al., 1991). That is, the experience of a key life event causes a sharp turn in one's behavior, knifing off the past and providing the individual with a fresh outlook. In another view, turning points are more akin to initiation points that set in motion a gradual change in behavior over time (Pickles & Rutters, 1991; Sampson & Laub, 1997). In this case, evidence of behavioral change may not be immediately apparent but would reach detectable levels as time passes. In either case, the individual's likely course has, for better or worse, been altered. A turning point, of some variety, has occurred if a detectable change in behavior can be traced back to the experience of a specific transition and non-spuriousness can be established.

As a final point of consideration, the concept of a trajectory need not refer to the pathway over the entire life-course. There are many instances of scholars usefully employing the idea of trajectories to study some behavior over time, often just a few years or more (Ward, Stogner, Gibson, & Akers, 2010), over a single developmental period (Broidy et al., 2003; Tremblay et al., 2004; Shaw, Gilliom, Ingoldsby, & Nagin, 2003; Shaw, Lacourse, & Nagin, 2005), or over just a couple of developmental periods such as childhood and adolescence (Nagin et al., 2003; Broidy et al., 2003). This is not entirely surprising given the lack of data sets that are readily available with measures across the entire life-course. Typically in these cases when the trajectory concept is employed, the objective of looking at behavior over the long-run remains the same (see, however, Horney, Osgood, & Marshall, 1995). Also similar, then, is the causal goal of discovering whether certain life transitions serve as turning points in a behavioral

trajectory and whether these transitions may serve as turning points for one type of trajectory but not for another as some criminological theories would claim (e.g., Moffitt, 1993). This latter possibility necessitates a discussion on the existence of distinct criminal trajectory groups.

Distinct Criminal Trajectory Groups?

The possibility that qualitatively distinct groups of offenders exist is among the most disputed issues in all of developmental and life-course criminology (Farrington, 2006). There are several theories that maintain there are one or more distinct subpopulations of offenders which are responsible for the shape of the age-crime curve (Moffitt, 1993; Thornberry & Krohn, 2005; LeBlanc, 2005; Lahey & Waldman; 2005). The most notable of them is Moffitt's (1993) dual-taxonomy theory, which explicitly predicts two qualitatively distinct types of criminals, namely adolescence-limited and life-course-persistent offenders. Her hypotheses have sparked numerous empirical investigations aimed at uncovering the validity of the dual taxonomy and the existence of distinct groups of offenders in general. These efforts have employed the latent class growth analysis methodology (LCGA), which is also known as semi-parametric group-based modeling (SPGM) (Nagin, 2005). This technique, which will be discussed in considerable detail in Chapter 7, empirically determines the number of trajectory groups, their shapes, and the proportion of individuals belonging to each group (Nagin, 2005).

In the first application of the latent class growth analysis technique, Nagin and Land (1993) used conviction records from a cohort of over 400 males (see West & Farrington, 1973; 1977) and found four trajectory groups: non-offenders, adolescence-limited offenders, and two variations of chronic-offenders. Employing data on a sample

of males from the National Youth Survey, McDermott and Nagin (1998) found a three group model to be supported by the data. As anticipated by theory (see Moffitt, 1993), the three groups included non-offenders, adolescence-peaked, and chronic offenders (McDermott & Nagin, 1998). Using data from multiple sources including several Racine Birth Cohorts, the Second Philadelphia Birth Cohort, and the Cambridge Study in Delinquent Development, D'Unger and colleagues (1998) uncovered evidence of four to five offending groups in each analysis. While adolescence-limited offenders and chronic-offenders were consistently found across studies, the proportion of individuals belonging to these groups was unanticipated by theory such that there were more chronic offenders. Additionally, they found that a group of low-level chronic offenders emerged from the data on a fairly consistent basis.

Moffitt's (1993) theory anticipates only a single group of chronic offenders. However, evidence using LCGA suggests that two groups may be inadequate to characterize all offenders (Moffitt, 2006). Moffitt and her colleagues (1996) found evidence for a "child-limited aggressive children" group, who exhibit some similarities to the life-course-persistent offender group during childhood but undergo a recovery of sorts leading to the avoidance of life-long problems, which are characteristic of the life-course-persistent offenders. Other scholars have found evidence for a "low level chronic" group which has been found to consistently offend from childhood through adolescence (Fergusson et al., 2000) or from adolescence through adulthood (D'Unger, Land, McCall, & Nagin, 1998; Nagin et al., 1995). These individuals persist in offending as do life-course-persistent offenders but do so at considerably lower offending rates.

There is also some theoretical discussion of the possibility of an “adult-onset” group which begins its offending in adulthood (Patterson, 1997).

Uncovering the number and character of trajectory groups has been a very difficult empirical endeavor. The trajectory groups that are uncovered may depend on the type and quality of the data being employed (Moffitt, 2006). Eggleston, Laub and Sampson (2004) found that individuals may be classified in different trajectory groups when using longer periods of data, raising questions about group classifications. Some contend that LCGA is not suited to test whether there are a specific number of trajectory groups (Skardhamar, 2010). While simulation studies show that LCGA can identify real groups and correctly classify individuals into these groups (Brame, Nagin, & Wasserman, 2006; Nagin, 2005), this accuracy is dependent upon the existence of true subpopulations (Skardhamar, 2010). If the method cannot “test” for the existence of these subpopulations and their existence is not known a priori, then the usefulness of the technique may be lessened (see Skardhamar, 2010).

In focusing on the time periods of childhood and adolescence, Piquero (2008, p. 43) states that “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.” While LCGA will likely find a small number of groups whether they truly exist or not (Skardhamer, 2010), it is indeed remarkable that different studies find important similarities in offending behavior over time when these groups empirically emerge from

different samples. But, the jury is still out on the dual taxonomy as well as whether other qualitatively distinct subpopulations of offenders indeed exist.

In sum, consideration of life-course theories and notions has flooded scholarly thinking in many disciplines, influencing an assortment of substantive research topics across the behavioral and social sciences (Featherman, 1983). Criminology has undoubtedly been strongly influenced by the popular trend to take long-views of social phenomenon. What is clear is that the idea of distinct criminal trajectories and the LCGA methodology provide exciting hypotheses and ways to assess whether the experience of a certain life transition, such as justice system contact, serves as a turning point in a criminal trajectory. If hypotheses regarding risk factors that distinguish group membership and those related to differential effects across trajectory groups pan out as expected, evidence for the existence of distinct criminal trajectory groups may be bolstered. For now, the consideration of trajectories and trajectory groups may provide important insights into how the effects of official intervention on subsequent offending behavior operate. This may impart some understanding for the mixed bag of empirical evidence on the effects of sanctions—see Chapter 5—as well as provide policy-relevant findings for the control of crime and violence.

CHAPTER 4
SANCTION EFFECTS IN A DEVELOPMENTAL AND LIFE-COURSE PERSPECTIVE

With a fundamental understanding of the contradictory predictions of the deterrence and labeling perspectives as well as an appreciation of the theories and issues raised by the life-course criminological paradigm, Chapter 4 places theories of sanction effects within the developmental and life-course perspective. First, a discussion of whether official intervention is a life transition substantial enough to cause a turning point in a criminal trajectory in general is offered. Focusing on taxonomic ideas and theories, the possibility that official intervention may be just an experienced life transition for some but may be more than that for others—marking a critical turning point in their offending trajectories—is illuminated. Next, the possibility that the immediate effects of official intervention may differ from the longer term effects is thoroughly explored. Finally, potential differences in the role that specific types of official intervention play in bringing about deterrent or labeling effects are discussed.

Official Intervention: Life Transition or Turning Point?

The interlocking nature of trajectories and life transitions such as justice system contact can, in some cases, create turning points in one's criminal trajectory (Elder, 1985). "Theories limited to time-stable factors are thus incapable of unpacking the zigzagging and temporally variable patterns of offending" (Sampson, 2000, p. 712). From the standpoint of both deterrence and labeling theories, the experience of official intervention is hypothesized to be a life transition worthy of turning point status in a criminal trajectory. With respect to altering one's trajectory toward unconformity, state sanctions are viewed as an important institution of social control which is linked to cumulative disadvantage (Sampson & Laub, 1997). "[Labeling] theory specifically

suggests a ‘snowball’ effect—that adolescent delinquency and its negative consequences (e.g., arrest, official labeling, incarceration) increasingly ‘mortgage’ one’s future, especially later life chances molded by schooling and employment” (Sampson & Laub, 1997, p. 15).

The application of a deviant label has been likened to a “status degradation ceremony” in which an individual’s total identity is transformed and his or her social rank is sent plummeting (Garfinkel, 1956). The criminal label can become an individual’s “master status” that trumps one’s former identity and ties to conventionality (Becker, 1963). Experiencing an official label often knifes off conventional opportunities leaving individuals with few choices but to offend (Sampson & Laub, 1997). Then, the life transition of experiencing a sanction is much more than an event that simply comes and goes. Should the label stick to the individual, it seems possible it may have lasting effects on behavior across the life-course.

Among the major theories of crime, labeling theory has been argued to be the only true developmental theory given its focus on the intervening mechanisms that unfold over time that cause primary deviance to develop into secondary deviance (Loeber & LeBlanc, 1990; Sampson & Laub, 1997). The integration of labeling theory within the developmental and life-course perspective is not forced but rather quite natural. Unfortunately, however, “despite its obvious affinity to a life course, developmental framework, labeling theory has rarely been viewed from this perspective” (Sampson & Laub, 1997, p. 9). This omission has resulted in the failure of many prior research efforts to consider a labeling event as a potential turning point in a criminal trajectory.

Deterrence has served as the backbone of the criminal justice system for hundreds of years and specific deterrence theory suggests that direct sanction experiences play an important role in curbing one's offending behavior (Beccaria, 1963; Gibbs, 1975). However, the blending of deterrence theory with the developmental and life-course perspective is not quite as seamless. Unlike labeling theory, deterrence theory does not focus on a particular event (e.g., the "stamping" of one as criminal) marking the definitive moment which will determine whether one will continue their problematic behavior over the long-term. For instance, Stafford and Warr (1993) note that direct and indirect experiences with punishment and punishment avoidance, all actively play a role in influencing behavior. When an active offender is not experiencing official intervention, they are experiencing punishment avoidance by definition. This immediate source of information factors into one's decision to continue offending.

It is true that individuals who do not have much experience with crime tend to rely more heavily on indirect experiences with punishment and punishment avoidance by necessity (Stafford & Warr, 1993). For most, justice system contact is no everyday event and what happens to oneself is much more important than what happens to one's peers or neighbors. When one does experience official intervention, this personal and relatively infrequent experience may play an important role in altering the likely course of a criminal trajectory. Moreover, a meaningful deflection in a criminal trajectory toward conformity need not lead to absolute deterrence. When coupling these ideas with the possibility of differential sanction effects, it becomes clear that official intervention is an important life transition that may constitute a turning point, for better or worse, in criminal trajectories.

Turning Points within Trajectory Groups

Gottfredson (2005) notes that current typological research hinges on the assumption that adding age or time to aid in offender classification efforts leads to better predictive ability and a greater understanding of treatment effects. While some disagree with this line of research (e.g., see Gottfredson & Hirschi, 1990; Gottfredson, 2005), most scholars recognize that the effects of sanctions will likely differ across divergent types of offenders. This argument has been advanced in theoretical discussions concerning labeling effects as well as those concerning deterrent effects. With respect to the former, Braithwaite (1989) states "The first step to productive theorizing about crime is to think about the contention that labeling offenders makes things worse. The contention is both right and wrong."[12] Similarly, Paternoster and Iovanni (1989, p. 381) note that "We should not expect labeling effects to be invariant across subgroups." With respect to the latter, Pogarsky (2002, p. 432) claims that "punishment threats are consequential only for a subgroup of the general population." At the extremes are "acute conformists" who comply with the law for reasons related to morality and "incorrigibles" who are committed to violating the law and are uninfluenced by sanctions. Finally, the rest of society is "deterrable" meaning they are subject to the effects of sanction experiences (Pogarsky, 2002). Countless other statements on the contingent nature of labeling and deterrent effects can be gathered from studies and theoretical discussions over the years.

[12] Braithwaite (1989) is specifically referring to the different outcomes of labels that are tied to differences in how officials respond to criminals. The current study, however, deals with differences in outcomes that are the result of offenders' differential responses to criminal sanctions.

Perhaps, Sherman (1993, p. 445) puts it best: "Does punishment control crime? This question provokes fierce debates in criminology and public policy. Yet there is ample evidence that it is the wrong question. Widely varying results across a range of sanction studies suggest a far more useful question: under what conditions does each type of criminal sanction reduce, increase, or have no effect on future crimes? Answering that question is central to the future of research in crime and delinquency".[13] The salience of this question for criminology and public policy still rings true today (Huizinga & Henry, 2008; Bhati & Piquero, 2008).

While there is generally an agreement that the effects of sanctions will likely differ across conditions and types of individuals, specific hypotheses specifying how labeling and deterrent effects are conditional upon offending subpopulations are not well developed (Chiricos et al., 2007). Compared to more experienced offenders, novice offenders have been argued to be more likely to experience deterrent effects (Thorsel & Klemke, 1972; Packer, 1968; Matsueda, Kreager, & Huizinga, 2006), but also more likely to experience labeling effects (Horwitz & Wasserman, 1979; Paternoster & Iovanni, 1989). In one view, Paternoster and Iovanni (1989) contend that labeling effects may level off as an individual becomes committed to deviance, which indicates that novice offenders would be more likely to experience them. For individuals offending at high levels over time, the labeling process may have already run its course. In another view, experienced offenders have committed more crime before an arrest than novice offenders have, which may make novice offenders more susceptible to specific deterrence due to their greater perceived certainty of punishment following

[13] Sherman's (1993) theory of defiance draws on notions of legitimacy and fairness to explain why there may be differences in the effects of sanction experiences on subsequent behavior.

arrest (Matsueda et al., 2006). Given there are hypotheses suggesting that official intervention may serve as a turning point in either direction for certain criminal trajectories, how the effects of official intervention vary across offending subpopulations is truly an empirical question. Still, taxonomic approaches do provide strong theoretical rationale for anticipating how the effects of sanctions will differ across divergent offending subpopulations.

Among the major approaches that explicitly address stability and change in behavior over time, the dual taxonomy approach unambiguously suggests that different types of individuals may react differently to certain life transitions (cf., state-dependence approach, Sampson & Laub, 1993; 2003). Specifically, Moffitt (1993; 2006) contends that adolescence-limited offenders will naturally desist from criminal behavior once the rewards of conventionality in early adulthood become apparent. Individuals who are caught for their violations of the law may experience problems with this transition to conformity. That is, they become “snared” by the criminal justice system causing them to forgo the natural desistence process. Another group, life-course-persistent offenders, should be largely unaffected by punishment experiences since their proactive and reactive interactions will steer them toward adverse consequences and persistent offending throughout the life-course. Thus, Moffitt (1993) clearly anticipates the experience of life transitions may serve as a turning point for one type of individual but will have little influence on another type of individual.

Moffitt (1993) is not alone in her categorization of offenders; half of the major developmental theories employ the idea of trajectory groups in one form or another (Moffitt, 1993; Thornberry & Krohn, 2005; Lahey & Waldman, 2005; LeBlanc, 2005).

LeBlanc (1997; 2005) proposes a three group typology which involves persistent, transitory, and common offenders. Persistent offenders are primarily influenced by their lack of control, common offenders are largely influenced by criminal opportunities, and transitory offenders are influenced by mixture of lack of control and opportunities. LeBlanc (2005) argues that "individuals are unique at birth and they remain unique throughout the life span. As a consequence, the level of control is different for each individual and they do not experience the same set of situations…In addition, there are short-term (over time and place) within-individual differences in offending because of adaptation, the way an organism adapts through the integration of new external elements or through structural change to fit with the environment" (pp.156-157). Blending these ideas, it follows that the experience of official intervention may differ amongst individuals who "remain unique throughout the life span."

Thornberry and Krohn (2005) do not explicitly hypothesize distinct trajectory groups per se, but they do suggest the causes of offending may vary across time and there are differences in those who tend to start early versus those who tend to start late. For example, later starters (ages 18 to 25) will display more continuity than earlier starters (ages 12 to 18) because they have more cognitive deficits. Importantly, they note that justice system sanctioning, among such things as family bonds and school success, can influence behavior trajectories (Thornberry & Krohn, 2001; 2005). Their theory tends to focus on the consequences of antisocial behavior in leading to additional problematic behavior (i.e., labeling effects) rather than on deterrent effects. Still, given their emphasis on offending trajectories and key differences between individuals who

start offending at separate phases of the life-course, there may be some dissimilarities in the ways in which criminal sanctions influence the behavior of early and late starters.

Finally, Lahey and Waldman (2005) think it is useful to categorize individuals into subpopulations but they reject the idea that there are only two groups. Rather, they suggest something akin to a continuum of offenders and highlight four main trajectory groups. "We posit that the same set of child characteristics and social factors influence conduct problems in children who follow all developmental trajectories, but differences in developmental trajectories result from different combinations of the same set of causal influences" (Lahey & Waldman, 2005, p. 27). While the authors do not explicitly address labeling or deterrent effects, these subpopulations may have differential responsiveness to key life transitions such as justice system contact even though the etiology of offending is similar. Following logic similar to Moffitt (1993), it would stand to reason that chronic offenders, who have very high levels of antisocial propensity, would be highly unlikely to experience deterrent effects.

There are some noteworthy variations in the ways in which these developmental and life-course theories categorize offenders. Some suggest there are qualitatively distinct groups with different etiologies, another suggests some differences in those who start offending at different times, and another suggests different offending groups reflect varying degrees of the same underlying construct such as antisocial propensity. Putting these differences aside for the moment, these theories have an important commonality. They all suggest there is utility in thinking that types of offenders may differ in one or more key respects. By acknowledging that trajectory groups may be indicative of some real differences between offenders, it follows that those following along distinct

offending trajectories may be differentially responsive to life transitions such as justice system contact experiences.

A number of interesting ideas and possibilities emerge when considering sanction effects across different criminal trajectories. To name a few, the “falsely accused” (Becker, 1963) cannot, by definition, experience a deterrent effect but they can be subject to labeling effects. “Incorrigibles” (see Pogarsky, 2002) or those on a chronic offending trajectory such as “life-course-persistent offenders” (see Moffitt, 1993) would be likely unresponsive to deterrent effects. And, the labeling process may also be irrelevant for these individuals or have already run its course. Individuals who have decreasing levels of offending over time, may have their desistance process hastened by justice system contact (i.e., the formal costs of crime may give the individual one more reason to stop offending) or reversed (i.e., the labeling process may be invoked, “snaring” individuals as Moffitt (1993; 2006) might anticipate).

The point here is that placing sanction effects within a developmental and life-course perspective and focusing on trajectory groups reveals exciting possibilities. The life transition of official intervention may serve as a turning point (deterrent or labeling) for individuals following along one particular trajectory but may have no influence on offending behavior for individuals following along another trajectory. Then, both deterrence and labeling theories may be relevant explanations of crime but for different types of individuals. Interestingly, the consideration of sanction effects within a developmental and life-course perspective also adds the possibility that deterrent and labeling effects may both be relevant for explaining the behavior of the same individual,

both in the short- and long-runs (see Sampson & Laub, 1997). It is this possibility that is now explored.

Turning Points in the Short- and Long-runs

Representing a fundamental departure from the predictions of taxonomic theorizing, Sampson and Laub's (1993; 2003) age-graded social bonding theory rejects the idea that there are fundamentally distinct groups of offenders who may respond to sanctions in different ways. While their theory does not categorize offenders into distinct types, it is certainly not devoid of important developmental implications. Most relevant here is that their theory leads one to consider the long-run consequences of official intervention, which may very well differ from the more immediate ones (Sampson & Laub, 1997).

"For the most part, research on labeling has consisted of cross-sectional studies or panel studies entailing modest follow-up periods *within* rather than *across* developmental phases" (Sampson & Laub, 1997, p. 9, *emphasis in original*) and they claim "that looking only at the direct effects of official sanctions is misleading" (Sampson & Laub, 1997, p. 18). Recall, labeling theory has been argued to be a true developmental theory because it focuses on deviance amplification as a process that unfolds over time (Loeber & LeBlanc, 1990; Sampson & Laub, 1997). A run-in with the law may constitute a turning point in the life of an individual (Sampson & Laub, 1997) but the labeling process does not happen overnight. Labeling involves a sequence of stages including blocked access to conventional opportunities, alteration of self-identity and the acquisition of delinquent peers (Paternoster & Iovanni, 1989). Then, despite what occurs as an immediate outcome of justice system contact such as a deterrent or

null effect, the consequences of official intervention may lead to subsequent involvement in crime in the long-run.

The importance of Sampson and Laub's revision to their age-graded social bonding theory is critical for the theoretical possibility that both deterrent and labeling effects may be experienced by the same individuals. Sampson and Laub's (2003) revised theory suggests that criminal behavior is a function of one's experiences with key life events that can foster or erode bonds to conventional society as well as a product of one's routines and rationally-calculated decisions. Their explicit incorporation of rationality suggests that individuals weigh the costs of committing crime and are thereby subject to the laws of deterrence. In the short-run an individual may lower their offending behavior if they experience justice system contact since their perceptions of certainty were boosted. Sampson and Laub's (1997) acknowledgement that official intervention can also have longer-term adverse consequences leads to the possibility that people may be deterred in the short-run but pulled toward crime in the long-run.

One might wonder why official intervention is not anticipated to result in a short-term labeling effect and a long-term deterrent effect (see Figure 1-3). Why this is certainly an empirical possibility, the theoretical rationale is lacking which would predict this sequencing of events. On the one hand, Nagin (1998) draws attention to differences in deterrent effects across the short- and long-runs; he claims that "while large amounts of evidence have been amassed on short-term deterrent effects, little is known about long-term effects. Evidence from perceptions-based deterrence studies on the interconnection of formal and informal sources of social control point to a possibly substantial divergence between long- and short-term effects" (p. 4). In this

case, however, Nagin (1998) is referring to the fact that a very effective policy in the short-term, which subjects a large proportion of the population to the rule of law, becomes less effective in the long-term. When a substantial number of individuals are subjected to punishment, this draws attention to the prevalence of the offending behavior. As a consequence, individuals disregard the law which is thought to be out-of-tune with typical behavior. Get tough policies centered on deterring marijuana smoking serve as a prime historical example of this process. While one should be aware that immediate deterrent effects may vanish if sanctions in the long-run become commonplace, this is unlikely for serious crimes especially those involving violence. Regardless, this process does not suggest deterrent effects to develop following labeling or null effects. If anything, it anticipates that any immediate deterrent effect would erode in the long-run.

Types of Official Interventions

Understanding how official intervention effects may differ in the short- and long-runs and determining the extent to which sanction effects may vary across offending subpopulations are important steps toward more fully evaluating the efficacy of deterrence and labeling theories. However, examining contingent labeling and deterrent effects of a single official intervention may overly simplify a complex problem. Kobrin and colleagues (1972) were among the first to introduce the idea of a “sanction pattern” in which there are different levels of sanctions within the various stages of the criminal justice system. This idea, which was originated in the general deterrence framework, can be applied within the specific deterrence framework (Huizinga & Henry, 2008). It is reasonable to expect that official intervention effects may vary based upon the severity of the sanction (Huizinga & Henry, 2008) and some evidence finds that this

is indeed the case (e.g., see McAra & McVie, 2007). Thus, a developmentally informed investigation into the effects of sanctions would be better off if it involved some consideration of the type of official intervention being experienced by an offender.

According to specific deterrence theory, punishments must be applied with certainty and celerity and should have a certain degree of severity in order to deter criminal behavior effectively; these three factors are thought to interact to produce criminal deterrence (see Stafford et al., 1986). Research, however, clearly shows that it is certainty of punishment that is most important in the deterrence process (Nagin, 1998), a fact which may have important implications for determining whether official intervention serves as a turning point in a criminal trajectory. If certainty of getting caught really is the bulk of how deterrence operates, it could be the case that a minor form of official intervention like a police contact may be sufficient to influence sanction perceptions and thereby initiate criminal deterrence. Recall, however, the idea of tipping points in which sanction effects must reach a certain level to trigger deterrence. From this logic, more severe types of official intervention may be needed to invoke criminal deterrence. To the extent that severity of punishment is relatively unimportant, more severe sanctions including arrest and incarceration may serve to increase the likelihood of a labeling effect.

The type of sanction could also hold differential importance across divergent types of offending subpopulations. As an example, let us consider offenders belonging to a chronic offending trajectory. These individuals may already have had the adverse consequence of official intervention happen to them. With little chance of making things worse for this group, having a more serious official intervention such as incarceration

may serve to increase the likelihood that the offender will be deterred. Now consider a novice offender, a minor form of official intervention such as a police contact may activate deterrence without risk of setting in motion the labeling process. A more serious form of official intervention such as an incarceration may activate the deterrence process but also expose one to deviant peers, cause an alteration of one's self-identity, and limit life chances in the employment and educational realms. Thus, a stern sanction for individuals following along a very low offending trajectory may do more harm than good in the long-run.

Determining how the effects of official intervention operate is no easy task and requires the consideration and blending of multiple ideas, theories and perspectives. From this discussion, it appears that empirical studies should seek to examine the effects of different gradations of official intervention (e.g., police contact, arrest) across offending subpopulations and across time. While this is an ambitious undertaking, it may be necessary for one to fully grasp the efficacy of deterrence and labeling theories as well as to yield policy-relevant information concerning the control of crime and violence with traditional law enforcement mechanisms, which will remain a staple of crime reduction efforts for the foreseeable future.

CHAPTER 5
REVIEW OF SANCTION EFFECTS LITERATURE

Chapter 5 summarizes what is known empirically about the effects of sanctions on subsequent criminal offending. Studies that have examined the link between official interventions and later offending in aggregated samples are first reviewed. This literature serves to highlight differences in empirical findings across studies, which may stem from the fact that most empirical tests ignore the possibility that sanction effects may vary depending on characteristics of the offender. Next, recent research that has begun to explore contingent labeling and deterrent effects across disaggregated samples (i.e., different types of offenders) is reviewed. Particular emphasis is given to empirical research focused on examining differential sanction effects for fundamentally different types of offenders such as novice and chronic criminals. Finally, the strengths and weaknesses of this limited body of empirical research are fully discussed setting the stage for the current study's investigation into the effects of official intervention across divergent violent offending subpopulations.

Sanction Effects in the Aggregate

Research that is cast as either deterrence or labeling studies are equally relevant to the analysis of the effects of official intervention on behavior, which can usefully be categorized into studies that examine the effects of arrest and studies that examine the effects of sanctions that follow an arrest (Huizinga & Henry, 2008). With respect to the former, some of the most relevant research on the labeling and deterrent effects of arrest on violent behavior stems from the Minneapolis Domestic Violence Experiment (MDVE) (Sherman & Berk, 1984a) and the six replication studies known collectively as the Spouse Assault Replication Program (SARP) (see Maxwell, Garner, & Fagan,

2002). With funding from the National Institute of Justice and the Police foundation, Sherman and Berk (1984a) conducted a randomized experiment to examine the effects of mandatory arrest policies on domestic violence recidivism. Consistent with the options police typically have in handling domestic violence cases, police were instructed to arrest the offender, separate the two parties, or advise the couple on how to handle its problems. Officers were given a color coded pad that instructed them to administer one of the three treatments to an eligible domestic violence offender at random. Delivered treatments were nearly perfect for arrests but only about 75% successful for the advise and separate treatment conditions. Unsuccessful delivered-designed matches typically comprised of individuals who were randomly assigned to receive advise or separate treatments but ended up receiving the arrest treatment. Two data sources were used for recidivism including victim interviews and official reports. With respect to victim interview data, individuals who were arrested were less likely than those who were advised to recidivate. With respect to official report data, individuals who were arrested were less likely than those who were separated to recidivate (see Sherman & Berk, 1984a). While this seminal work was used as evidence of a specific deterrent effect, it was not without conceptual and methodological flaws.

Binder and Meeker (1988) note that the MDVE study was more akin to a pilot study than a rigorous scientific study that was worthy of suggesting sweeping policy changes. They claimed that the Sherman and Berk (1984b) policy summary report that was released was essentially propaganda and note there were important problems with the randomization, sample attrition, operationalization of recidivism, methodological technique and interpretation of weak effects. For example, comparison groups made by

Sherman and Berk (1984a) were not a priori choices and they interpreted coefficients when the overall model was statistically insignificant. The problems surrounding this initial study led to six replications studies with some minor variations in their research protocols.

The SARP was funded to determine the generalizability of the empirical findings from the MDVE (see Maxwell et al., 2002). In the Charlotte replication, Hirschel and Hutchinson (1992) found no evidence for either a labeling or a deterrent effect for victim or offender data across multiple measures of recidivism, including prevalence, incidence and time to failure. The results from the Omaha replication revealed, for the most part, similar findings. However, a notable exception was that individuals who were issued an arrest warrant (i.e., they were absent at the scene) had substantially lower recidivism across all measures (Dunford, Huizinga, & Elliot, 1990). Perhaps, these individuals knew they could not violate the law or draw attention to themselves given their outstanding warrants. Unlike the Omaha and Charlotte studies that showed by-and-large null findings, the Dade County replication study (Pate & Hamilton, 1992) and the Colorado Springs replication study (Berk, Campbell, Klap, & Western, 1992) both found evidence for a specific deterrent effect with victim data but not with official report data. Intriguingly, the Milwaukee study revealed an initial deterrent effect followed by a longer term labeling effect (Sherman et al., 1991). Compared to those not arrested, those receiving a short arrest of approximately 2.8 hours showed an initial deterrent effect for the first 30 days following the official intervention. However, when following the recidivism of these individuals over a longer period of time, those arrested experienced a labeling effect that persisted for a year (Sherman et al., 1991).

In short, the initial MDVE revealed deterrent effects and the SARP studies yielded a mix of findings ranging from null effects to both deterrent and labeling effects. Moreover, many "efforts at synthesis vary almost as much as do the published reports from the individual sites" (Maxwell et al., 2002, p. 55) so, in many respects, it has been difficult to draw any firm conclusions about specific deterrent effects of mandatory arrest policies.

Conducting the most sophisticated meta-analyses to date, Maxwell and colleagues (2002) took on the task of synthesis using data from five of the MDVE/SARP experimental sites. They examined recidivism using both victim and official report measures. The researchers deal with complexities associated with multi-site analysis and also take into account the variability in the existence, number, and timing of victim interviews as well as differences in the collection methods of official data. The researchers employ 4,032 incidents which involve assaults by males of their female partners, comparing the arrest treatment group to a lumped, non-arrest treatment group. Official data revealed that approximately 23% of the sample had recidivated where victimization data registered that 43% of the sample had recidivated (Maxwell et al., 2002). Across five outcome measures and controlling for site and suspect's characteristics (age, use of intoxicants, race, married, prior arrest, and employment), there were consistently smaller rates of recidivism among individuals in the arrest treatment group compared to those grouped together in the non-arrest group. Overall, then, the best evidence that exists from studies that have analyzed aggregated samples of MDVE/SARP data is that there is a very small specific deterrent effect of arrest among the general population (Maxwell et al., 2002).

Aside from these classic field experiments, there is a growing body of literature employing various methodologies that has examined the effect of arrest on subsequent offending behavior. There are numerous studies that have found support, or partial support, for labeling theory (Bernburg & Krohn, 2003; Farrington, 1977; Gold & Williams, 1970; Huizinga, Elliott, & Dunford, 1996; Huizinga & Esbensen, 1992; Huizinga, Weiher, Espiritu, & Esbensen, 2003; Kaplan & Damphouse, 1997; Paternoster, 1978; Paternoster & Piquero, 1995; Smith, 2006). For example, Bernburg and Krohn (2003) found that having police contact or being arrested as a juvenile significantly increased serious crime and general delinquency in early adulthood and this effect was significant while controlling for adolescent delinquency, family poverty, educational capabilities, and race. In direct contrast to studies that fail to support traditional criminal justice approaches to curbing crime, a couple of studies have found support for deterrence theory (Cameron, 1964; Smith & Gartin, 1989). For example, Smith and Gartin (1989) reported that arrest led to a reduction in the number of future police contacts and that arresting an individual during a police contact also led to a reduction in future police contacts compared to not arresting an individual. However, this latter finding only held for the first few police contacts; in fact, after five contacts official intervention effects are more supportive of labeling.

Several studies report null findings, failing to yield support for either deterrence or labeling theory (Hemphill, Toumbourou, Herrenkohl, McMorris, & Catalano, 2006; Huizinga et al., 1986; Klein, 1986; McAra & McVie, 2007; Thomas, 1977). Unlike the majority of studies examining the effect of arrest on behavior, McAra and McVie (2007) notably employed propensity score matching which is a rigorous methodological

technique aimed at uncovering causal effects with observational data. They found that once individuals were adequately matched there were no significant official intervention effects on either the prevalence or frequency of serious delinquency.

When considering studies that examine other sanctions that may follow an arrest, a similar picture emerges. That is, a number of studies offer some support for labeling theory (Berg, Consterdine, Hullin, McGuire, & Tyrer, 1978; Bernburg, Krohn, & Rivera, 2006; Chiricos et al., 2007; Fagan, Kupchick, & Liberman, 2003; Hagan & Palloni, 1990; Huizinga et al., 2003; Klein, 1986; McAra & McVie, 2007; Thornberry, 1971; Schneider, 1982; Shannon, 1985; Wolfgang, Figlio, & Sellin, 1972) but only a few offer any support for deterrence theory (Murray & Cox, 1979; Thistlewaite, Wooldredge, & Gibbs, 1998; Ventura & Davis, 2005). Additionally, there are several studies that report essentially no effect of official intervention on subsequent behavior (Brennan & Mednick, 1994; Gottfredson, 1999; Huizinga & Espiritu, 1999; Rose & Hamilton, 1970; Smith & Akers, 1993; Spohn & Holleran, 2002; Taxman & Piquero, 1998).

Some studies have more explicitly examined the extent to which the type of official intervention matters in producing deterrent or labeling effects. For example, McAra and McVie (2007) found that appearing in juvenile court leads to increases in serious delinquent behavior whereas simply being arrested had no influence on subsequent behavior. For the sample as a whole, an arrest alone may not constitute a “status degradation ceremony” (see Garfinkel, 1956) thereby failing to trigger the labeling process. Thus, this study in particular draws attention to the potential importance of considering different types of official intervention as possible turning points in individuals’ criminal trajectories.

In a comprehensive review of the deterrence literature, Nagin (1998) finds that cross-sectional and scenario-based empirical research provides evidence that perceptions do indeed influence criminal offending and claims there is much stronger evidence for the existence of a substantial deterrent effect than what existed previously. However, the empirical studies that are of the highest methodological quality tend to show considerably weaker results compared to those which are conducted with cross-sectional data or those which employ few or no control variables (Paternoster, 1987). A recent meta-analysis finds that perceptions of certainty of arrest have a statistically significant but relatively weak effect on criminal behavior (Pratt et al., 2006). Taking all of the experimental and non-experimental studies into account, there is no clear support for labeling or deterrence theory in the aggregate. This is true for different types of sanctions such as arrest and those that follow arrest. The only thing that is fairly clear from prior research is that the effects of official intervention, if they exist, are considerably more nuanced requiring investigations into samples that are meaningfully disaggregated and follow individuals over the short- and long-terms.

The Contingencies of Sanction Effects

Of the studies that have examined contingent official intervention effects across different groups, subpopulations have typically been defined with respect to sex, race, prior record, and stakes in conformity (Chiricos et al., 2007). Several studies have examined contingent labeling and deterrent effects across sex (Baumer, 1997; Chiricos et al., 2007; Gendreau, Little, & Goggin, 1996) and race (Ageton & Elliott, 1974; Berk et al., 1992; Bernburg & Krohn, 2003; Chiricos et al., 2007; Harris, 1975). While it is of academic interest to understand how official intervention effects may vary across race or sex, it is of less practical import to isolate these contingent effects due to policy

considerations. That is, differential treatment based on demographic characteristics can result in charges of racial and gender bias in the administration of justice. To a lesser extent, subpopulations that are based on stakes in conformity are subject to the same problem. Sherman, Smith, Schmidt, and Rogan (1992) find that individuals with high stakes in conformity (e.g., employed, high school graduate, married) had lower recidivism rates (see also DeJong, 1997). However, it may also be unethical to differentially intervene based upon whether an offender, for example, has a job or is married.

Determining the effects of official intervention across offending subpopulations (i.e., those with a “prior record”) appears to be freer from the ethical problems of divergent treatment of offenders. In reality, selective intervention already occurs since police officers and prosecutors have considerable discretionary power. Moreover, selective intervention policies informed by differential effects for offending subpopulations would guide police officers and prosecutors in a more systematic fashion thereby helping to ensure that different treatment is the result of behavioral tendencies and not innate characteristics such as race or gender. Thus, for these reasons, the most pressing need for research may be on the extent to which the contingency of “prior record” moderates the effect of official intervention on subsequent offending behavior.

In one of the earliest studies examining deterrent and labeling effects across offending subpopulations, Cameron (1964) found that arresting novice shoplifters typically resulted in the cessation of their delinquent behavior. The results of this study echoed the notion that official intervention effects for novice offenders would be stronger

than for more experienced offenders (Packer, 1968; Thorsell & Klemke, 1972). In a study of drunk drivers, Taxman & Piquero (1998) used Cox proportion hazard models and found that punishment sentences were more likely to increase recidivism than rehabilitation sentences. Moreover, this effect was particularly pronounced for first-time offenders.

In a study conducted in New York City, DeJong (1997) found evidence consistent with labeling theory but only for novice offenders. More specifically, she found that first-time offenders who were incarcerated for misdemeanors were more likely to be rearrested than individuals who were not incarcerated for their offenses. Similar findings were reported by Horwitz and Wasserman (1979) who found that the positive relationship between severity of official intervention and the likelihood of recidivism held only for first-time juveniles.

Klein (1974) collected two years worth of recidivism data on 100 juveniles from each of 13 police agencies that he categorized as either high or low diversion agencies. He found that arrest and system processing decreased the rate of subsequent offending; however, this was found to be the case only for more experienced offenders and not for first-time offenders. In examining the effects of arrest within a criminal career paradigm, Smith and Gartin (1989) found that the experience of being arrested is more likely to terminate the criminal career of novice offenders whereas it may only reduce offending rates among more experienced offenders. Interestingly while another scholar found labeling effects for incarceration using the same data set (see Shannon, 1980; 1985), Smith and Gartin (1989) found deterrent effects for their outcomes. This

point draws attention to the need to consider different types of official intervention within the same study.

Matsueda and colleagues (2006) linked subsequent offending behavior to experiential risk of apprehension, which they defined as the number of arrests divided by the number of self-reported offenses. They found that as experiential risk of apprehension increased, the perceived risk of apprehension also increased causing a reduction in subsequent offending behavior. These results are consistent with specific deterrence theory. What is important is that experienced offenders have lower experiential risk of apprehension values than novice offenders, meaning that experienced offenders are likely less deterred by arrest.

A recent felony convictions study which aimed to redirect attention to the contingent nature of labeling effects found evidence for labeling theory in general but did not find the observed labeling effect to be conditioned by “prior record” (Chiricos et al., 2007). Specifically, those who had adjudication withheld were less likely to recidivate compared to those who had been adjudicated. In a rare but highly notable attempt to place sanction effects in a developmental framework, Bhati (2007; see also Bhati & Piquero, 2008) classified individuals based upon divergence between their post-release micro trajectories and counterfactual trajectories to explore whether labeling, deterrent, or null (incapacitation) effects occurred. Her analysis concluded that approximately 56% were unaffected by incarceration (i.e., only incapacitation effect), 40% experienced a deterrent effect and only 4% experience a labeling effect. Importantly, those with a greater number of arrests were less likely to experience

deterrent effects but those who were sanctioned closer to those prior arrest "clusters" were more likely to experience deterrent effects.

With a mixed bag of empirical evidence, Smith and Gartin (1989) have noted that "perhaps the only conclusion we can draw at this point is that the relationship between punishment and future offending among both novice and experienced offenders is problematic" (p. 97). Unfortunately, some twenty years later this observation still largely appears to be true. With recent studies continually providing mixed results, key limitations of prior research and only a few explicit attempts to place sanction effects in a developmental and life-course framework, much remains unknown about what types of official interventions result in turning points for what types of criminal trajectories and for how long. Before attempting to address this question empirically, it will be useful here to outline the key problems with prior research that are, in part, responsible for the inability to make any firm conclusions regarding deterrent and labeling effects.

Key Limitations of Prior Research

Prior research has revealed important insights into the relationship between criminal sanctions and subsequent offending in the aggregate, across different types of offenders, and in both the short-run and long-run. However, there are several important limitations of prior research that temper the theoretical conclusions about labeling and deterrent effects. The principal issues include: data, offender categorization, and selection effects.

Data

In a critique of Sampson and Laub's (2005a) empirical work, Robins (2005) reminds us that official arrest records may suggest termination of a criminal career because of data problems. For instance, individuals who committed crimes while in

prison, or serving in the military, or whose crimes were simply undetected do not get counted (Robins, 2005). This is a major problem when assessing whether official intervention serves as a turning point, for better or worse, in a criminal trajectory. Using official data to estimate offending trajectories can be problematic for another reason. Arrest records and true offending behavior may be correlated but they are certainly not the same thing. Hence, "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" (Huizinga & Henry, 2008, p. 223). Therefore, empirical efforts aimed at uncovering the effects of official intervention on subsequent offending are probably on better ground if they employ self-report data. Specifically, it is important to employ self-report data on actual offending behavior and not self-report data on arrests, since the latter suffers from the same problems as official records. Importantly, self-report data has been shown to be a reliable and valid source of information for many empirical investigations (Thornberry & Krohn, 2000).

Another key data problem is the use of longitudinal data sets with just a couple of waves. While two-wave panel data can help with the establishment of time-order, it does not provide enough time points to examine the effects of official intervention in a developmental perspective. Simply estimating offending trajectories requires, at minimum, three waves of data. Modeling quadratic or cubic changes in offending trajectories requires four or five waves, respectively. Factoring in the need to follow-up offenders both in the short- and long-runs, it becomes clear that a multi-wave data set designed to follow individuals over an extended period of time is necessary to

investigate whether official intervention serves as a turning point in a criminal trajectory. While there is certainly not an abundance of such data sets, they do indeed exist (Huizinga & Henry, 2008). However, these multi-wave panel studies have yet to be fully exploited to address the key theoretical questions, which have been illuminated herein by placing official intervention effects research in a developmental and life-course framework, in a methodologically rigorous fashion.

Offender Categorization

To assess whether official intervention serves as a turning point in the criminal trajectories of offenders, a classification procedure for different types of offenders is necessary. While individuals' prior records have been used to classify persons into offending subpopulations in previous research, there is a critical problem with this strategy. For example, two individuals who have been arrested once could be classified similarly despite the fact that one may be a chronic offender and the other an exceptionally infrequent offender. Then, classifying individuals based on their prior records as opposed to their offending behavior is somewhat imprecise if the goal is to assess contingent effects based on one's offending trajectory.

As an alternative to classification based on prior record, Huizinga and colleagues (1986) classified individuals according to a delinquency typology (e.g., non-delinquent, exploratory delinquent, non-serious delinquent, serious delinquent). However, there are problems with subjective classification procedures as well. First, a subpopulation can be subjectively classified as distinct but only represent random variation. Second, unusual subpopulations can fail to be classified at all. Third, statistical tests examining differences across subpopulations may be undermined by uncertainties about the reliability of subpopulation assignment (see Nagin, 2005). Finally, subjective

classification schemes may make it difficult to capture trajectories of offending behavior over time, which can limit their ability to assess whether official intervention serves as a turning point in a criminal trajectory. Nagin (2005; see also Nagin & Land, 1993) proposes a unique solution to this problem that empirically classifies individuals into distinct trajectory groups, which can be used to assess whether the effects of a particular event differ across these subpopulations.

Selection Effects

In addition to the problems associated with subjective classification, methodological limitations of previous studies have resulted in the inability to discern between labeling and deterrent effects for offending subpopulations with any definitiveness. A key problem with prior empirical work is the use of methodological designs that are not well-suited for disentangling "selection artifacts" from real official intervention effects (Smith & Paternoster, 1990).[14] With some exceptions (e.g., Chiricos et al., 2007; McAra & McVie, 2007; Smith & Paternoster, 1990), research designs have failed to employ strong methodology that addresses the problems of making causal inferences with non-experimental data.

All is not lost. The data, classification, and selection artifact problems can be addressed given the availability of high-quality longitudinal data for official intervention effects research (Huizinga & Henry, 2008, p. 248) and the advancement of integrated methodological techniques capable of estimating treatment effects across different

[14] Selection artifacts occur when the relationship between official intervention and subsequent behavior is biased due to a correlation of the official intervention variable and the error term in a regression model (Smith & Paternoster, 1990). It would be ideal for scientific purposes to randomly assign individuals to treatment groups; however, ethical and practical concerns frequently make experimental designs in social science impractical. For these reasons, the implementation of experimental designs in criminology is a rare occurrence (for some classic examples, see Klein, 1986; Sherman & Berk, 1984a).

trajectory groups with observational data (Haviland & Nagin, 2005; Haviland et al., 2007; 2008).

Given this synthesis of what is known, what is unknown, and why certain things remain unknown, in regards to the effects of official intervention on subsequent offending, I am now in a position to outline the data and the methodological approach used for the current analysis that addresses each of the aforementioned limitations of prior research.

CHAPTER 6
DATA

Chapter 6 describes the data and measures, explains the research methodology and outlines the analytic strategy. First, the particulars of the Rochester Youth Development Study (RYDS) are discussed, which serves to demonstrate that these data are particularly well-suited to address the empirical aims of the current study. Following a general discussion of the RYDS, the procedures for handling missing data and arriving at the final analysis sample are detailed. Next, the measurement of violent offending behavior, official intervention, and the forty covariates is discussed. Chapter 6 then concludes detailing descriptive statistics for the analysis sample on the whole.

Overview of Data

The current research employs data from the RYDS, which is an ongoing longitudinal study aimed at understanding the causes of crime and delinquency in a developmental and life-course perspective. The RYDS features a total of fourteen waves of data, collected in three phases. Phase one consists of nine waves of data collected at six month intervals beginning in 1988 and ending in 1992. Phase two consists of three waves of data gathered at annual intervals beginning in 1995 and

ending in 1997. Finally, phase three consists of two waves of data collected with a two year interval beginning in 2003 and ending in 2005. The principal respondents (Generation 2 or "G2") were approximately 13.5 years of age at wave one and 31 years of age at wave fourteen.[15] The RYDS is a disproportionate stratified sample of 1,000 individuals in which at-risk youth (i.e., males and individuals living in high crime areas) were over-sampled. The Generation 2 sample is comprised of 73% males and 27% females. With respect to race, the sample is 68% African American, 17% Hispanic, and 15% White (the male subsample is approximately 63% African American, 18% Hispanic, and 19% White). The overall retention rate is good. For instance, 78.5% of the 1,000 individuals in the original sample participated in the survey during wave thirteen, which is some fifteen years from the data collection time point.

Research subjects were interviewed in sessions lasting approximately an hour and were asked to answer questions on a wide variety of subjects. These topical areas include but are certainly not limited to such things as neighborhood characteristics, peer influences, gang involvement, social supports, educational aspirations and commitments, psychological well-being, family structure and relationships, sexual activity, and one's behaviors such as self-reported delinquency, violence and drug use. These self-report surveys are supplemented by official records from Rochester Public Schools, police records, social services data, and United States Census data. In addition, the RYDS also followed the primary caregivers (Generation 1 or "G1") of the main research subjects (G2) for the first eight waves in phase one and all three waves

[15] The RYDS distinguishes between the principal respondents, who were adolescents during phase one of the study, and their primary caregivers who were adults during this time and were also interviewed as part of the RYDS. The former are referred to as Generation 2 (G2) whereas the latter are referred to as Generation 1 (G1).

in phase two. This provides information about things such as the parents' behaviors and experiences, their perceptions of G2 and their peers, their relationships and living environments and, in many cases, similar information that was collected from G2 which can be used to validate and/or compare perceptions between the two generations.[16]

The RYDS is one of a few data sets that is ideal for official intervention effects research. Non-experimental studies necessitate the use of data from longitudinal studies with probability sampling designs and high-quality measures (Huizinga & Henry, 2008). One of the key features of the RYDS data is the breadth of data in terms of variables and constructs included in the data set, which is particularly important to the current research endeavor that attempts to isolate official intervention effects with counterfactual methods (i.e., propensity score matching). These methods create covariate balance only on measured constructs used for treatment model estimation. Thus, having measures of relevant covariates is essential. Another important feature of the RYDS data is the longitudinal, multi-phase component which ensures there are sufficient waves of data to estimate offending trajectories and still assess the effects of official intervention across the short- and long-runs. Moreover, the large number of waves also permits the estimation of trajectories that are not simply linear over time; directional changes can be modeled with, for example, quadratic terms (Nagin, 2005). Finally, the RYDS contains measures of self-reported offending across waves that will be used to estimate the violent offending subpopulations. This is a substantial

[16] While not the focus here, it is worth mentioning that the RYDS is now interlaced with the Rochester Intergeneration Study (RIS) which was funded to follow the main research subjects' children (Generation 3 or "G3"). Needless to say, this provides a unique opportunity to look into the transmission of crime and life circumstances across three generations.

improvement over studies with two-wave panel designs and those that estimate contingent labeling and deterrent effects with official data as the measure of behavior.

Analysis Sample

The current investigation into the consequences of official intervention for subsequent violent behavior across violent offending trajectory groups employs data from phases one and two (waves two through ten) from the males from the RYDS.[17] Individuals included in the analysis sample had to be interviewed during every wave between waves six and ten and had to have valid police intervention records during waves seven and eight. In addition, individuals must have been interviewed at least twice between waves two through five to be included in the analysis sample.

The analysis sample was selected with the rationale that the subject had to be interviewed such that confounding variables used to predict the probability of receiving official intervention were generally available (wave six), data on officially recorded arrests and self-reported police contacts were available (waves seven and eight), and the violent offending outcomes in the short-run (wave nine) and long-run (wave ten) were observed. Given the ability of LCGA to accommodate missing data in its estimation of behavioral trajectories, some missing data was permitted from waves two through five. Nevertheless, missing interviews were not a serious concern during this time period; 93.4% of the final analysis sample was interviewed during each of waves two through five and approximately 5.9% was surveyed during all but one of these waves. Just less than 0.7% of individuals were interviewed in only two out of four of

[17] Data from wave one is not used because one of the six items that is part of the violence measure is not assessed during this wave. Rape is also not asked in waves eleven and twelve in phase two. Therefore, wave ten is used to assess the long-term effects of official intervention.

these waves (waves two through five). Therefore, no individual in the analysis sample had his violent offending trajectory estimated from less than three out of five of the time points since individuals had to be interviewed in wave six to be included in the analysis sample. Moreover, very few trajectories were estimated with any missing data at all (only 6.6%). The final analysis sample that fit the above criteria consists of approximately 82% of the original RYDS males (N=595). Across several important demographic factors as well as violent criminal behavior, there were no significant differences between the analysis sample and those individuals who did not make the analysis sample but were interviewed during wave one (see Table 6-1).[18]

As with most data sets, not all of the 595 male respondents comprising the analysis sample contained valid data on every covariate of interest. Even under the ideal case of data from valid respondents being missing completely at random, using a listwise deletion procedure here would be especially problematic given the importance of maintaining a sufficient sample size to estimate the effects of official intervention on subsequent violent offending behavior across each of several trajectory groups. Several options for addressing missing data were considered.

Haviland and colleagues (2005; 2007; 2008) developed the LCGA-PSM methodology and recommended dealing with missing data by creating missing indicator dummy variables and replacing missing values with any score such as the covariate mean given its ease of implementation and appropriateness within the propensity score matching framework. The logic behind this approach is that even though an individual's

[18] Means are reported for interpretation purposes but the p-values for the significance testing are associated with the chi-square statistic for dummy variables (Black, Hispanic, family poverty status) and the Wilcoxon-Mann-Whitney statistic for the lone count variable (violent crime).

response to a particular item that he or she did not respond to is not observable, the fact that he or she did not respond to the item is observable and may reveal important information about the individual's propensity to receive some treatment (see Rosenbaum & Rubin, 1984; see also Haviland et al., 2007). Including missing indicator variables with arbitrary replacement of missing scores (e.g., mean replacement) to address the problem of missing data results in the potential for the propensity scores to balance both observed scores on the item or scale as well as the frequency of missing scores. In this way, the fact that the individual did not respond to an item is used as information helpful in predicting the propensity to receive treatment and, as a consequence, preserves the case in the analysis.

For the present study, however, this approach to dealing with missing data within a propensity score matching framework turned out not to be viable for two primary reasons. First, many of the covariates that have missing data are reported by G1 (i.e., parental reports). Moreover, these variables tended to be the ones with the highest level of missing information such as the neighborhood disorganization measure, the Achenbach aggression scale, and the PC's educational expectations for the child variable (see Table 6-2). While one can probably safely argue that G2 not responding to a question may reveal important information about G2's likelihood to experience official intervention, it is not quite as clear how non-response by G1 would be useful in predicting G2's treatment likelihood. Second, Haviland et al. (2007) argue it is best to estimate propensity scores and perform matching within trajectory groups. When using missing indicator variables in the models for predicting treatment within trajectory groups, the problem of complete/quasi-complete separation of data points was

exacerbated for certain latent classes. Ultimately, then, missing data was handled with multiple imputation (see Alison, 2001).

Multiple imputation uses information from variables in the model and others which are available that may predict missing values. Unlike imputation through regression, random error is added to each imputed value. Addressing the list of problems associated with single imputation approaches to dealing with missing data, multiple imputation is a process whereby the imputation procedure is repeated multiple times to produce a single stacked data set containing *n* copies of the original data set with different imputed values. That is, the *n* imputed values are different because a random component is added to the value that is the best prediction given the variables included in the imputation model (Alison, 2001). With relatively low levels of missing data, as is the case in the present study, five data sets are sufficient to achieve a high level of efficiency. The single stacked data set can then be separated into five individual data sets and analysis can be performed as usual on each of the imputed data sets. When data analysis is completed on each of the *n* imputed data sets, the results are then averaged and a special standard error is used yielding an overall estimate of the effect. SAS's Proc "MIANALYZE" is utilized for this purpose.

Measures

This section outlines the measures used in the current study. First, the observed variable used to track violent crime trajectories prior to the intervention and assess the violent crime outcomes is discussed. Second, the variables used to define the two types of official intervention experiences are examined. Finally, the covariates used to estimate the propensity scores that are employed in the matching procedure are detailed.

Behavioral Trajectory and Outcome: Violent Crime

Youth violence has been identified as a serious social problem and one that is likely to continue to be responded to with traditional approaches to justice. As a consequence, it has been suggested in this study that there is an important practical need to understand how official intervention experiences may influence offending trajectories for different types of violent offending subpopulations. In addition to these practical considerations, studying violence provides for a fairly conservative test of ideas underlying deterrence and labeling theories. That is, given the etiologies of crimes against persons and crimes against property may differ such that the former could be argued to be less influenced by rational choice, official intervention effects found for one or more violent offending subpopulations are noteworthy (especially if a strong methodological design is employed).

Violent crime. The outcome variable of interest is self-reported violent crime which is measured similarly as the variables employed to estimate the trajectories of violent crime prior to the experience of official intervention. Violent crime is a variety index measuring the total number of six different acts of violence an individual committed in a given wave. The six acts include: attacking someone with a weapon, participating in a gang fight, throwing things at people, committing robbery, raping someone, and engaging in other assault.[19] Possible scores on this measure range from zero to six. With respect to the trajectories, waves two through six are used to estimate the latent class growth analysis models. With respect to the outcome variable, waves

[19] Variety indexes are among the most reliable forms of self-reported measures of delinquency (Belson, 1986; Hindelang, Hirschi, & Weis, 1981).

nine and ten are used to examine the short-term and long-term effects of official intervention, respectively.

Treatment Conditions: Official Intervention

Earlier the idea was advanced that it may be important to consider different types of official intervention experiences when investigating how justice system contact may serve as a turning point in a criminal trajectory. For instance, deterrent or labeling effects may not fully be set in motion without a certain grade or type of sanction. There is a clear difference between an individual who has been arrested with no subsequent action taken by the criminal justice system and one who has been arrested and incarcerated for his offense. Similarly, a different experience occurs when an individual has been contacted by the police and when one has been arrested. Ideally, each of these three types of official intervention would be examined in the same study. In the current study, however, incarceration was too infrequent of an official intervention experience among this sample of juveniles to include in the analysis. I discuss the implications of this omission in Chapter 10 of the study.

Police contact and arrest. Using both self-report data from the RYDS and official records data from the Rochester Police Department and County Wide Registration, two types of official interventions are available and used as the treatment conditions. First, police contact is a self-report measure of whether an individual experienced one or more police contacts during waves seven or eight. Second, arrest is a dichotomous official records measure of whether an individual experienced one or more arrests during waves seven or eight.[20] Two waves are employed to ensure a sufficient number

[20] Official records data on police contacts was not used since there was little variance in this measure across waves seven and eight. Self-reported arrest was also considered but descriptive analysis

of individuals actually experienced an official intervention so that the effects of the treatment can be estimated across different violent crime trajectories.

Covariates

The treatment model consists of a large number of covariates from several different waves of data. Some time-stable measures were captured at wave one (e.g., race) whereas other measures that vary across time are available at various waves. In most cases, these measures are taken from wave six which is the wave immediately prior to the designated period for treatment. Wave six is selected as these measures are temporally close, yet precede in time, the official intervention experience. The following potentially relevant confounder variables are organized into the following subcategories: demographics, neighborhood characteristics, family dynamics, school factors, peer associations, values and mental states, prior delinquent behavior, and prior justice system contact. In describing these measures, the general theoretical significance of controlling for each subcategory of variables is offered. It is worth noting that all of the scales were summed and divided by the total number of items that comprised the scale. This returns the multi-item scales to their original metrics so that the response options to the individual items can be used to aid in the interpretation of basic descriptive statistics.

Demographics

Some of the more important control variables in sanction effects research include those identified from the "status characteristics hypothesis," which states that certain

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revealed there were fewer self-reported arrests as compared to official records. Nevertheless, results from self-reported arrests largely conform to those reported herein and, consequently, the focus will be placed on the presentation of results from official records data for arrest.

types of individuals such as minorities are more likely to experience official intervention irrespective of their criminal involvement. Put differently, "Given the occurrence of a deviant action (delinquency) the decision of organizational agents to sanction officially (to label) an actor is in part determined by the social attributes (race, sex, social class) of the offender…" (Paternoster & Iovanni, 1989, p. 364). Since minorities have been found to commit a greater proportion of personal crimes than their demographic counterparts (Hindelang, 1978), for example, controlling for basic demographics is essential when assessing the extent to which deviant labels influence subsequent violent crime. In addition, it is critical to control for the respondent's age given the established relationship between age and crime (see Hirschi & Gottfredson, 1983).

Race. Race is a nominal variable that has been recoded into a series of dummy variables. Specifically, Blacks and Hispanics are coded as 1 on their respective dichotomous variables. Whites are left out of the treatment model and serve as the reference group.

Age. This common demographic factor is a continuous measure of the respondent's age during wave six.

Family poverty. Family poverty is a dichotomous variable that serves to distinguish between respondents who come from families who are above the federally defined poverty level from those who come from families who are below it. To create this measure, the number of individuals in G1's house was determined and the before tax income was calculated. These two values were used and compared against the U.S. Government's federal poverty chart. Respondents with family incomes below the

federal poverty line were coded as 1 whereas those above the poverty line were coded as 0.

Neighborhood characteristics

Neighborhood characteristics may play a critical role in determining the rate of justice system contact and involvement in delinquency. Research suggests that certain segments of society are subject to higher levels of policing, making the risk of experiencing an official intervention greater in these areas. Importantly, the social disorganization perspective leads one to believe that certain neighborhoods such as those with concentrated disadvantage are more likely to have higher crime rates (Shaw & McKay, 1942). While social disorganization and collective efficacy are characteristics of the neighborhood and are conceptualized to operate as macro-level phenomenon, problematic neighborhood environments can be a breeding ground for criminal opportunities at the individual-level making controlling for neighborhood factors highly important here.

Proportion African American. This variable is a measure of the proportion of African Americans residing in the youth's neighborhood. The measure comes from U.S. Census data for Monroe County during the 1990 data collection period. The proportion of Blacks in the neighborhood is calculated by taking the number of Blacks (not of Hispanic origin) and dividing them by the total number of individuals residing in the corresponding census tract. Thus, higher scores reflect a greater proportion of African Americans in the neighborhood. Values on this measure can theoretically range from 0 to 1.

Proportion in poverty. Proportion in poverty is a measure of the proportion of families in poverty residing in the youth's neighborhood. As for the proportion African

American measure, the measure comes from 1990 U.S. Census data. The proportion of families living in poverty was calculated by taking the number of families in poverty and dividing this value by the number of families residing in the corresponding census tract. Thus, higher scores signify a greater proportion of families living in poverty. The possible scores on this measure range from a minimum of 0 to a maximum of 1.

Neighborhood disorganization. Tapping the level of conflict and violence, drug use, and general disorder in the adolescent's neighborhood as perceived by the primary caregiver (G1), this variable is a measured with a seventeen item scale. The adolescent's primary caregiver was asked to report the extent to which a number of things were a "big problem," "sort of a problem," or "not a problem." Specifically, they were asked about how much of a problem the following things were in their neighborhood: different racial or cultural groups who do not get along with each other; vandalism, building, and personal belongings broken and torn up; little respect for rules, law and authority; high unemployment; winos and junkies; prostitution; abandoned houses or buildings; sexual assaults or rapes; gambling; burglaries and thefts; run down and poorly kept buildings and yards; assaults and muggings; street gangs or delinquent gangs; syndicate, mafia or organized crime; buying or selling stolen goods; drug use or drug dealing in the open; and, homeless street people. Higher scores indicate greater neighborhood disorganization.

Neighborhood integration. Neighborhood integration is a seven item measure tapping the extent to which neighbors know one another and whether the people who live in the neighborhood can be relied upon. For example, on a four point Likert type scale, the adolescent's primary caregiver (G1) was asked: how many people live in your

neighborhood do you know by sight?; how many people live in your neighborhood do you talk to on a regular basis?; how often do you and other people who live in your neighborhood borrow things like tools or recipes from each other?; how often do you and other people who live in your neighborhood ask each other to drive or take your children somewhere? Response options included "never," "seldom," "sometimes," and "often." Higher scores on this measure are indicative of higher neighborhood integration.

Neighborhood satisfaction. This variable is a three item measure tapping into the primary caregiver's satisfaction with the neighborhood. On a four-point Likert type scale, respondents were asked to state how satisfied they were with: your relationship with people in this neighborhood; the way people in this neighborhood take care of where they live; the neighborhood as a good place to bring up children. Response options ranged from very dissatisfied to very satisfied and higher scores on this measure are indicative of higher neighborhood satisfaction.

Neighborhood arrest rate. Neighborhood arrest rate is a measure of the resident arrest rate for the respondent's census tract. Using official records, the total number of individuals arrested within the youth's census tract was divided by the population and standardized by 1,000. Higher values indicate a greater neighborhood arrest rate.

Family dynamics

The structure and dynamics of the family can play an important role in creating opportunities for crime and delinquency. Specifically, traditional family structure and primary caregivers that play an active role in supervising their children can reduce opportunities for offending. Individuals who are strongly bonded to their primary

caregivers may have more to lose from having justice system contact experiences and engaging in offending in general.

Family structure. Family structure is a dichotomous variable indicating whether a respondent lives at home with both parents. This measure is mostly reported from the respondent's primary caregiver (G1) but some adolescents (G2) did provide data on the family structure when G1 interviews were unavailable. Responses were coded such that 0=does not live with both parents and 1=lives with both parents.

Parental supervision. Parental supervision is a four item scale measuring the quality of supervision that the youth self-reports. On a four point Likert type scale, respondents were asked to rate the extent to which the primary caregiver knows where the child is and knows who they are with when they are away from home as well as how important these two supervisory tasks are to the primary caregiver. Higher values designate greater parental supervision of the respondent by the primary caregiver—whether this is one of the biological parents or some other male or female.

Parental attachment. This variable is an eleven item scale tapping into how attached the respondent is to the primary caregiver (PC) (e.g., mom, dad, other male, or other female). Respondents were asked a number of questions which include: you get along well with your PC; you feel that you can really trust your PC; your PC does not understand you (reverse coded); your PC is too demanding (reverse coded); you really enjoy your PC; your PC interferes with your activities (reverse coded); you think your PC is terrific; you feel very angry toward your PC (reverse coded); you have a lot of respect for your PC; you feel very proud of your PC; and, you feel violent toward your

PC (reverse coded). Higher scores indicate a greater level of parental attachment to the primary caregiver.

PC's educational expectations for child. This variable is a two-item measure tapping the primary caregiver's (G1) expectancies regarding whether the youth will succeed in educational pursuits. The primary caregiver was asked whether they believed G2 would graduate from high school and whether he would go on to college. Response options to these two questions were "no," "depends," and "yes." Higher scores indicate greater expectations for the child to further his or her education.

Attachment to child. Attachment to child is an eleven item scale measuring how attached the primary caregiver (G1) (e.g., mom, dad, other male, or other female) is to the adolescent (G2). This is a parallel scale to the parental attachment scale above meaning the items are similar but this measure instead asks about how attached the primary caregiver is to the child. Higher scores indicate a greater level of attachment to the adolescent.

Consistency in discipline. Consistency in discipline measures the youth's perception of their primary caregiver's (G1) patterns of discipline. Respondents were asked the following questions: how often do you get away with things; once PC decides a punishment, how often can you get out of it; how often do you get punished sometimes, but not other times, for doing the same thing; how often does PC have to ask you to do the same thing more than once; when you get punished, how much does the kind of punishment you get depend on PC's mood. Response options were "often," "sometimes," "seldom," and "never". Higher scores are coded such that they indicate lower consistency in discipline.

School factors

Individuals with greater attachments and commitments to school tend to be less involved in delinquent activity. Moreover, these individuals have more to lose if they were to get into trouble with the law and have less time to do so because of their participation in scholastic activities. Since school factors are related to both the participation in delinquency and contact with the justice system, these constructs are important control variables in sanction effects research.

Commitment to school. Commitment to school is a ten item scale measuring one's level of agreement on a four point Likert scale to statements pertaining to school commitment. For instances, respondents were asked: whether school is boring to you (reverse coded); you like school a lot; you don't really belong at school (reverse coded); you usually finish your homework; you try hard at school; and, getting good grades is very important to you. Response options ranged from "strongly agree" to "strongly disagree." Higher scores on this measure designate greater commitment to school.

Aspirations for college. Aspirations for college is a two item scale indicating how important it is for one to, first, graduate from high school and, second, go to college. On a four point Likert type scale, response options ranged from "not important at all" to "very important." Higher scores indicate greater aspirations for college.

Prosocial activities. This variable is a five item scale measuring one's involvement in prosocial activities many of which take place in the school environment. On a four point Likert type scale with response options of "never," "seldom," "sometimes," and "often," respondents were asked how often they: took part in school sports such as intramurals and varsity sports; took part in school activities like clubs or special events like a school play; took part in organized sports or teams outside of

school; took part in any organized musical or singing group, including in school; and, took part in other organized groups like the "Y", Boys and Girls Clubs, or scouts. Higher scores on this measure indicate greater involvement in prosocial activities.

Educational expectations. Educational expectations is a parallel measure to the parent's education expectations of child scale. While the items are the same, this measure taps the youth's (G2) own expectancies regarding whether he will succeed in his educational pursuits. As with the other measure, higher scores indicate greater expectations for the individual to further his education.

School clubs with friends. The last of the school factor variables taps the extent to which individuals engage in school activities such as clubs or special events like a school play with their friends. Response options on this measure include "never," "seldom," "sometimes," and "often." Higher scores on this measure indicate greater involvement in school clubs with friends.

Peer associations

One of the more robust predictors of crime and violence in the literature is peer delinquency. There is also a long line of evidence also suggesting that gang involvement influences violence above and beyond the contributions of peer delinquency (e.g., see Thornberry et al., 2003). In addition to the facilitating effects of peers and gangs on delinquency, being involved with delinquent peers or affiliated with gangs may also make one more likely to experience justice system contact (e.g., see Tapia, in press). Thus, controlling for the effects of peers is important in sanction effects research and six measures are used here to tap into several distinct aspects of peer influence.

Peer delinquency. Peer delinquency is an eight item scale measuring the proportion of one's peers that engage in delinquent acts. Respondents were asked to indicate whether "none," "a few," "some," or "most" of their friends have done delinquent things which, for example, include: stole something worth more than $100; attacked someone with a weapon with the idea of seriously hurting them; took a car or motorcycle for a ride or drive without the owner's permission; and, damaged or destroyed someone else's property on purpose. Higher scores indicate greater peer delinquency.

Peer drug use. Peer drug use is a four item scale measuring the proportion of one's peers that engage in drug use. Respondents were asked to indicate whether "none," "a few," "some," or "most" of their friends have used marijuana, crack, alcohol, or other drugs (e.g., LSD, heroin, acid). Higher scores indicate greater peer drug use.

Peer delinquent values. This variable is a six item scale that measures whether one's peer group would say it was "wrong," "not say anything," or "say it was okay" if the respondent engaged in certain delinquent acts including: using a weapon or force to get money or things from people; hitting somebody with the idea of hurting them; stealing something worth $50; damaging or destroying someone else's property on purpose, taking a car or motorcycle for a ride or drive without the owner's permission, and skipping classes without an excuse. Higher scores on this measure reflect higher peer delinquent values.

Dating. Dating is a single item dichotomous variable measuring whether an individual has a special "boyfriend" or "girlfriend." Responses were coded such that 0=no boyfriend/girlfriend or 1=boyfriend/girlfriend.

Sexual activity under fifteen. This variable is a dichotomous indicator measuring whether the respondent had engaged in sexual intercourse prior to the age of fifteen. Responses were coded such that 0=no sexual activity under fifteen and 1=sexual activity under fifteen.

Gang involvement. Gang involvement is a dichotomous measure of whether an individual was involved in a gang at any time between waves two through six. Beginning in wave two, respondents were asked during the time period since the last interview if they were a member of a street gang or "posse" (the term used by Rochester adolescents). Responses to the questions of gang participation during the five waves on interest were recoded such that 1=gang involvement (one or more waves) and 0=no gang involvement.

Values and mental states

Individuals with delinquent values have less respect for the law and/or do not personally subscribe to the norms of socially acceptable behavior. Those with less belief in conventional society may be more likely to engage in crime and violence (Hirschi, 1969). Moreover, individuals who have delinquent values may have more run-ins with the law as they fail to respect the authority of the legal system. In addition to one's values, aggressive behavior has been related to certain mental states such as depression. And, given their current mental state, depressed individuals may be less concerned than they otherwise would be with the ramifications of justice system contact. Given these potential theoretical ties to both the treatment and the outcome of interest, these measures also represent variables to include in the treatment model.

Delinquent values. Delinquent values is an eleven item scale measuring how wrong one thinks it is to engage in a variety of delinquent behaviors. For example,

respondents were asked about acts of delinquency such as: using hard drugs, drinking alcohol, stealing something worth $50, taking a car or motorcycle for a ride without the owner's permission, damaging or destroying someone's property on purpose, or hitting someone with the idea of hurting them. Response options included "not wrong at all," "a little bit wrong," "wrong," and "very wrong." Higher scores indicate greater approval of delinquent behaviors.

Depression. This variable is a fourteen item scale tapping how frequently individuals felt depression symptoms such as feeling hopeful about the future; feeling depressed or very sad; not feeling like eating because you felt upset about something; or, thinking seriously about suicide. On a four point scale, response options ranged from "never" to "often"; thus, higher scores indicate greater depression symptoms.

Self-esteem. Self-esteem is a nine item scale measuring the extent to which an individual agrees or disagrees, on a four point Likert scale, with statements about oneself. Examples of statements tapping self-esteem include: at times you think you are no good at all (reverse coded); you feel that you have a number of good qualities; you feel you do not have much to be proud of (reverse coded); or, you feel you are at least as good as other people. Higher scores indicate greater self-esteem.

Prior delinquent behavior

The importance of controlling for prior behavior in sanction effects research should be relatively obvious. The best predictor of future behavior remains past behavior (White et al., 1990) and deviant behavior is naturally correlated with official intervention experiences. While the LCGA assigns like individuals to the same latent class, prior delinquent behavior is explicitly included to assess the balance on these key covariates at waves two through six. In addition to controlling violent crime specifically, it is a good

idea to control for delinquency more generally. Drug use and general delinquency are naturally related with justice system contact and they are also related to violent behavior. Indeed, ingestion of certain substances such as alcohol or pcp can lead to violence and committing general delinquency would lead to more opportunities for violent encounters. Drug use and general delinquency are measured at wave six as described below.

Violent crime. Violence is a self-reported variety index tapping into the total number of six distinct acts of violence an individual committed in a given wave. The violent acts include: attacking someone with a weapon; engaging in a gang fight; throwing things at people; committing robbery; raping someone; and, engaging in other assault. Scores on this measure range from zero to six and higher scores indicate greater involvement in violent crime. This is the same measure that is used for the behavioral trajectories and outcomes. To be clear, a violence variable for each wave is included in the treatment model. In other words, five different violent crime measures (waves 2 though 6) are employed as covariates.

General delinquency. General delinquency is a self-reported variety index tapping into the total number of thirty-two distinct acts of general delinquency committed in wave six. The general delinquency acts cover a wide range of criminal and delinquent behavior and include such things as: truancy; carrying a weapon; property damage; public rowdiness; theft of several different amounts (e.g., less than $5, $5-50, $50-100, $100+); auto-theft; assault; robbery; obscene phone calls; fraud; marijuana sales; hard drug sales; etc. Scores on this measures range from zero to thirty-two and higher values reflect greater involvement in various acts of general delinquency.

Drug use. This variable is a self-reported variety index tapping into the total number of ten different drugs that were used in wave six. The drugs include: marijuana; inhalants; hallucinogens; cocaine; crack; heroin; pcp; tranquilizers; downers; and, uppers. Scores on this measure range from zero to ten with higher scores indicative of higher drug use variety during wave six.

Aggression. Aggression is measured with eleven items from the Child Behavior Checklist which was administered to the parents during wave six (Achenbach, 1991). This measure assesses general aggressive tendencies in youth but also includes some items that reflect hyperactivity and impulsiveness. Primary caregivers of subjects reported whether G2 had done any of the following: been unable to sit still or been restless or hyperactive; been cruel to animals; been cruel, bullying, or mean to others; demanded a lot of attention; felt others were out to get him; been impulsive or acted without thinking; screamed a lot; been stubborn, sullen or irritable; had sudden changes in mood or feelings; had temper tantrums or a hot temper; and, threatened people. Response options included “never”, “sometimes,” and “often.” Higher scores on this measure indicate greater aggression.

Prior official intervention

Individuals with a history of justice system contact will be more likely than those without it to experience it again. This is, in part, because justice system contact is an “official” measure of delinquency and reflects involvement in crime. Additionally, when an individual is arrested or an official police contact has been made, the individual may be integrated into offender databases either formally or informally and may be suspected of committing other crimes in which officers do not have immediate leads. For these reasons, it is imperative to control for prior official intervention experiences.

Previous contact or arrest. The variable is measured using official records data from the Rochester Police Department and County Wide Registration. The measure is a dichotomous indicator of whether an individual experienced one or more arrests or police contacts during waves one through six. That is, the variable was coded such that 0=no previous contact or arrest and 1=previous contact or arrest.

Descriptive Statistics

Table 6-2 contains descriptive statistics of the violent crime and official intervention measures as well as the covariates used to predict the probability of receiving an official intervention treatment. For each variable measured at the interval level of measurement, the mean, standard deviation, minimum and maximum are reported. For dichotomous variables, the standard deviation is not reported. In this case, of course, the mean can be interpreted as the percentage of individuals in the analysis sample who scored "1" on the dummy variable. In addition to the standard descriptive statistics that are reported, the last two columns show the number of valid cases for variables with missing information and the wave(s) in which the variable was measured, respectively. When a specific value of N is not given for a variable, it has no missing information in the analysis sample (N=595). The discussion below focuses on describing the "typical" or average male in the sample as well as highlights the prevalence of dichotomous measures. While measures of dispersion are not explicitly discussed in most cases, it should be kept in mind that there tended to be substantial variation in the sample across most of these measures as indicated by the standard deviation as well as the minimum and maximum scores (see Table 6-2).

With respect to basic demographics, the analysis sample is comprised of 63% Blacks, 18% Hispanics, and 19% Whites, which is very similar to the original RYDS

male subsample. Nearly one-third (30%) of respondents' families are below the federal poverty line. And, the typical youth is roughly 16 ½ years of age at the time of the wave six interview.

The average respondent resides in an area with heavy concentrations of minorities and relatively high levels of poverty. Specifically, the residents in a typical census tract are 53% African American and 33% of a typical tract's residents are in poverty. The neighborhood arrest rate ranges from approximately no arrests to about eight arrests with an average of 4.12 arrests. The average neighborhood is not especially well-integrated with an average neighborhood integration score equal to 2.19. On average, neighbors tend to interact and do favors for one another more or less "seldom." Nevertheless, the 2.88 average neighborhood satisfaction score reported by the primary caregiver is close to the scale anchor of "satisfied." The mean neighborhood disorganization score is 1.64. Thus, the various physical and social disorders in a run-of-the-mill neighborhood are closer to being somewhat of a problem than being not a problem at all. What is clear is that the typical neighborhood is not plagued with neighborhood disorganization as being a large problem.

Approximately 33% of youth respondents live with both parents. Despite the fact that many of these adolescents live in a fractured home environment, the typical young male experiences fairly high attachment to his primary caregiver and reports high levels of parental supervision. On average, the primary caregiver reports relatively similar levels of attachment to the youth as the average youth does. The individual most responsible for raising G2 tends to be somewhat consistent in their discipline of G2. On

average, he or she deviates from consistent punishment patterns closer to seldom than often.

The typical respondent has fairly high aspirations for college; a mean score of 3.34 is a fair amount above the scale anchor corresponding to “important.” For the sample as a whole, these high hopes for college are coupled with moderately-strong school commitment (mean=3.08). However, the average male in the sample engages in prosocial activities and school clubs with friends closer to seldom than often. Interestingly, G2 reports higher levels of educational expectations for themselves than G1 does of them.

Over one-quarter of the males in the sample (27%) were involved in a gang prior to the treatment period. Overall, 46% percent of the males are dating during wave six and 34% had engaged in sexual intercourse prior to the age of fifteen. On average, respondents have a small proportion of their friends engaging in delinquency and drug use. Likewise, the typical respondent’s peer group has relatively low delinquent values. Turning attention to values and mental states, the average male seldom experienced depressive symptoms and had a pretty good self-image overall. Similar to one’s peer group, male respondents had relatively low levels of delinquent values on average.

With respect to violent criminal behavior prior to wave seven, the average violent crime score was 0.56 during wave two and this value steadily decreased to a low of 0.36 by wave six. In wave six, the mean general delinquency score was 1.38 and the average drug use score was only 0.15. The most delinquent individual engaged in 19 distinct acts of general delinquency and the individual with the greatest drug use variety used five different substances. On average, primary caregivers reported that their youth

had relatively low levels of aggression. Justice system contact during waves one through six was not uncommon. Specifically, 34% of the males had at least one police contact or arrest during this time period as measured by official statistics.

Table 6-3 takes a closer look at the percentage of individuals committing zero through six acts of violence prior to and following the official intervention treatment period. In wave two, almost 65% of the sample committed zero acts of violence, approximately 21% committed one type of violent act, and approximately 14% of the sample committed two or more distinct types of violent acts. In wave six, about 75% of the sample failed to commit any acts of violence, less than 17% committed one type of violent act, and about 8% of the sample engaged in two or more types of violence. In general, the sample as a whole was becoming less violent between waves two and six. More specifically, the proportion of individuals in each violent category (i.e., one to five distinct acts of violence) declined fairly systematically during the pre-treatment period whereas the proportion of individuals in the non-violent category (i.e., zero distinct acts of violence) increased. With respect to the outcomes, at wave nine approximately 85% of the sample did not commit any acts of violence and at wave ten about 81% of the sample refrained from engaging in violence. Those committing violence committed between one and five types of violent acts in wave nine and between one and four types of violence in wave ten. Despite the better half of the sample being non-violent in a given wave both prior to the official intervention period and during the short-run and long-run follow up periods, a sizeable proportion of the sample engaged in one type of violence and, across all waves, some individuals committed several distinct acts of violence. As a final point of consideration regarding the sample's violent offending

behavior, the maximum score was at least four across all waves indicating that at least one male committed four or more distinct types of violence at each point of observation.

Regarding the violent behavioral outcomes, the mean values for violent crime in the short- and long-runs are 0.21 and 0.26, respectively (see Table 6-2). With respect to the official intervention treatments, approximately 22% of the sample experienced one or more arrests as measured by official records whereas about 32% experienced one or more police contacts as measured by self-reports. That is, of the 595 males in the analysis sample, 131 persons had been arrested between waves seven and eight and 188 individuals reported having had a police contact during this same time. On the whole, justice system contact is not a rare event which, of course, is a necessary precondition of any data used to estimate treatment effects of official intervention across the entire sample (Chapter 8 explores whether there also is a nontrivial proportion of individuals who have experienced justice system contact in each violent offending trajectory group specifically).

Supplementing Table 6-2, Table 6-4 reports the descriptive statistics for variables that had missing data following the multiple imputation procedure. The means reported in the table are the average of the five multiple imputed data sets. It should be noted that the means and standard deviations here closely mirror those reported for the valid cases (cf. Tables 6-2 and 6-3). Solving the problem of missing data, the multiple imputation procedure has had little to no effect on the measures of central tendency and dispersion for the various covariates. With an understanding of the RYDS data, the analysis sample used in the current study, the measurement of the key constructs as well as a sense of the descriptive statistics, the research methodology used to assess

the effects of official intervention experiences on subsequent violent offending across different trajectory groups can now be discussed in detail.

Table 6-1. Comparison between analysis sample and individuals excluded from the analysis sample across valid cases for key demographic factors and violent behavior at wave one.

	Mean Analysis Sample	Mean Excluded Group	p-value
Race			
Black*	0.625	0.686	0.182
Hispanic*	0.180	0.179	0.984
Family poverty level*	0.290	0.295	0.933
Proportion African American	0.531	0.544	0.587
Proportion in poverty	0.333	0.326	0.614
Violent crime#	0.443	0.577	0.240

Notes: * Chi-square significance test. # Wilcoxon-Mann-Whitney significance test.

Table 6-2. Descriptive statistics for valid cases in the analysis sample (N=595).

	Mean	SD	Min	Max	N	Wave
Violent crime outcomes						
Short-run	0.21	0.59	0.00	1.00	---	9
Long-run	0.26	0.63	0.00	1.00	---	10
Official Intervention						
Officially recorded arrest	0.22	---	0.00	1.00	---	7-8
Self-reported police contact	0.32	---	0.00	1.00	---	7-8
Covariates						
Demographics						
Race						
Black	0.63	---	0.00	1.00	---	1
Hispanic	0.18	---	0.00	1.00	---	1
Age	16.47	0.81	13.90	18.20	---	6
Family poverty	0.30	---	0.00	1.00	550	4
Neighborhood characteristics						
Proportion African Amer.	0.53	0.27	0.00	0.99	---	1990C
Proportion in poverty	0.33	0.14	0.00	0.56	---	1990C
Neighborhood disorganization	1.64	0.64	1.00	3.00	502	6
Neighborhood integration	2.19	0.68	1.00	4.00	557	5
Neighborhood satisfaction	2.88	0.66	1.00	4.00	558	5
Neighborhood arrest rate	4.01	1.98	0.12	7.87	---	A
Family						
Family structure	0.33	---	0.00	1.00	---	6
Parental supervision	3.52	0.46	1.25	4.00	576	6
Attachment to parent	3.39	0.43	1.36	4.00	572	6
PC's edu. expectation of child	2.33	0.83	1.00	3.00	515	6
Attachment to child	3.48	0.46	1.73	4.00	535	6
Consistency in discipline	2.36	0.52	1.00	4.00	562	6
School Factors						
Commitment to school	3.08	0.38	1.10	4.00	529	6
Aspiration for college	3.34	0.86	1.00	4.00	---	6
Prosocial activities	1.68	0.66	1.00	3.80	---	6
Educational expectations	2.60	0.73	1.00	3.00	591	6
School clubs with friends	1.23	0.57	1.00	4.00	---	6
Peer Associations						
Peer delinquency	1.32	0.51	1.00	4.00	558	6
Peer drug use	1.41	0.53	1.00	3.50	571	6
Peer delinquent values	1.31	0.42	1.00	3.00	591	6
Dating	0.46	---	0.00	1.00	---	6
Sexual activity under 15	0.34	---	0.00	1.00	---	---
Risky time with friends	2.16	0.71	1.00	4.67	559	6
Gang involvement	0.27	---	0.00	1.00	---	2-6
Values and Mental States						
Self-image	3.21	0.42	2.22	4.00	---	6
Depression	1.98	0.47	1.00	3.57	---	6

Table 6-2. Continued

	Mean	SD	Min	Max	N	Wave
Delinquent values	1.40	0.43	1.00	3.20	593	6
Prior criminal behavior						
Violent crime (Time 1)	0.56	0.91	0.00	4.00	579	2
Violent crime (Time 2)	0.48	0.87	0.00	5.00	583	3
Violent crime (Time 3)	0.47	0.85	0.00	4.00	588	4
Violent crime (Time 4)	0.37	0.72	0.00	4.00	587	5
Violent crime (Time 5)	0.36	0.73	0.00	5.00	---	6
General delinquency	1.38	2.32	0.00	19.00	---	6
Drug use	0.15	0.49	0.00	5.00	---	6
Aggression	0.38	0.35	0.00	1.83	533	6
Prior Justice System Contact						
Previous contact or arrest	0.34	---	0.00	1.00	---	1-6

Notes: N values only reported for variables that have missing cases. If no N value is reported N=595.
1990C=1990 United States Census for Monroe County.

Table 6-3. Number of males committing zero through six distinct acts of violent crime before and after the treatment period.

	Number of distinct acts of violent crime						
	0	1	2	3	4	5	6
Violent crime trajectory							
Time 1 (wave 2)	375	121	49	29	5	0	0
	(64.77)	(20.90)	(8.46)	(5.01)	(0.86)	(0.00)	(0.00)
Time 2 (wave 3)	407	107	41	22	5	1	0
	(69.81)	(18.35)	(7.03)	(3.77)	(0.86)	(0.17)	(0.00)
Time 3 (wave 4)	412	114	30	27	5	0	0
	(70.07)	(19.39)	(5.10)	(4.59)	(0.85)	(0.00)	(0.00)
Time 4 (wave 5)	435	106	32	11	3	0	0
	(74.11)	(18.06)	(5.45)	(1.87)	(0.51)	(0.00)	(0.00)
Time 5 (wave 6)	448	100	30	15	1	1	0
	(75.29)	(16.81)	(5.04)	(2.52)	(0.17)	(0.17)	(0.00)
Violent crime outcomes							
Short-run (wave 9)	506	66	16	3	3	1	0
	(85.04)	(11.09)	(2.69)	(0.50)	(0.50)	(0.17)	(0.00)
Long-run (wave 10)	483	81	20	9	2	0	0
	(81.18)	(13.61)	(3.36)	(1.51)	(0.34)	(0.00)	(0.00)

Notes: Percentages (%) reported in parentheses.

Table 6-4. Descriptive statistics for variables with imputed data (N=595).

	Mean	SD	Min	Max
Demographics				
Family poverty	0.30	0.46	0.00	1.00
Neighborhood characteristics				
Neighborhood disorganization	1.67	0.64	1.00	3.00
Neighborhood integration	2.20	0.67	1.00	4.00
Neighborhood satisfaction	2.88	0.66	1.00	4.00
Family				
Parental supervision	3.52	0.46	1.25	4.00
Attachment to parent	3.38	0.44	1.00	4.00
PC's edu. expect. of child	2.29	0.82	1.00	3.00
Attachment to child	3.47	0.46	1.73	4.00
Consistency in discipline	2.35	0.52	1.00	4.00
School Factors				
Commitment to school	3.05	0.39	1.10	4.00
Educational expectations	2.60	0.74	1.00	3.07
Peer Associations				
Peer delinquency	1.32	0.51	1.00	4.00
Peer drug use	1.41	0.52	1.00	3.50
Peer delinquent values	1.31	0.42	1.00	3.00
Risky time with friends	2.17	0.71	1.00	4.67
Values				
Delinquent values	1.40	0.43	0.82	3.20
Prior criminal behavior				
Violent crime (Time 1)	0.57	0.91	0.00	4.00
Violent crime (Time 2)	0.48	0.87	0.00	5.00
Violent crime (Time 3)	0.47	0.85	0.00	4.00
Violent crime (Time 4)	0.37	0.72	0.00	4.00
Aggression	0.38	0.34	0.00	1.83

Notes: Mean and SD are averages of the five multiple imputed data sets. Min and Max are minimums and maximums across all five of the data sets.

CHAPTER 7
METHODOLOGY

Chapter 7 lays out the integrated methodology employed to determine whether there are differential sanction effects for violent offending subpopulations. Given the advanced nature of the integrated methodology, a separate treatment of each of the two statistical methods is offered. The latent class growth analysis approach to classifying individuals into distinct latent offending subpopulations is first reviewed. Subsequently, the propensity score matching approach to dealing with the issue of selection artifacts is explained. The two methodologies are then discussed together to illustrate how this integration is a useful approach for assessing official intervention as a turning point in a criminal trajectory for divergent violent offending subpopulations. Finally, the analytic strategy followed in the current study is summarized.

Method

Explicitly addressing the issues of offender classification and selection effects that were identified as key problems with prior research, this section details the Latent Class Growth Analysis—Propensity Score Matching (LCGA-PSM) integrated methodology employed to test the research hypotheses. The latent class growth analysis and propensity score matching analysis sections are each subdivided into three topics: conceptual overview, technical details, and model estimation. In this way, pertinent information regarding the motivation for the technique and the basic ideas that underlie it are first articulated, which can be used to facilitate comprehension of the key equations and the model estimation procedures.

Latent Class Growth Analysis

Conceptual overview

There has been a growing interest in studying criminal behavior in a longitudinal framework that stretches well beyond using longitudinal data to establish one of the three classical requirements for causal inference—time-order. Longitudinal growth modeling permits an assessment of intra-individual change in behavior or the modeling of individual-level heterogeneity in behavioral trajectories over an extended period of time. Several methodological approaches to modeling behavior over time have been advanced, two of which include hierarchical linear modeling (Bryk & Raudenbush, 1987; Goldstein, 1995; Raudenbush & Bryk, 2002) and latent growth analysis (McArdle & Epstein, 1987; Meredith & Tisak, 1990; Muthén, 1989; Willett & Sayer, 1994).[21] Unlike wave to wave regression, which yields estimates of relative change in a sample over time (i.e., similarity of the rank ordering of subjects from one wave to another), these approaches capture absolute change in offending behavior within the individual. As a result, they have been extremely influential in helping to study the initial status and trends of criminal trajectories as well as the relationships between initial status and trends and factors that influence these parameters.

While informative, these approaches are not without specific limitations. Most notably, both make the assumption that a continuous distribution function underlies the data. In other words, hierarchical linear modeling and latent growth analysis assume everyone's trajectory follows the same general path through time, albeit there is variation around the population's average trajectory which is typically in the form of a

[21] One key difference between HLM and latent growth analysis is that the former assumes observed variables and the later assumes latent variables.

multivariate normal distribution (Nagin, 2005, p. 25). However, a variety of disciplines including criminology have theories which suggest that distinct developmental groups may exist and, some claim, that the effects of certain life transitions embedded within trajectories may result in different outcomes across divergent developmental groups (Caspi, 1998; Cloninger, 1986; Kandel, 1975; Loeber, 1991; Moffitt, 1993; Patterson, DeBaryshe & Ramsey, 1989). The assumption of a continuous distribution function made in hierarchical linear modeling and latent growth analysis is quite restrictive and shuts the door to empirical investigations into phenomena that are of important substantive interest to some developmental theories of crime.

In response to this limitation of traditional growth modeling approaches, a third methodological technique aimed at modeling individual-level heterogeneity in behavioral trajectories has been gaining traction in recent years. Known both as semi-parametric group-based modeling (Nagin & Land, 1993; Nagin, 2005) and latent class growth analysis (Muthén & Muthén 1998-2010), this technique assumes the presence of a latent categorical variable that differentiates subgroups of individuals, which may have distinct etiologies as specified by taxonomic theories of offending (e.g., see Moffitt, 1993).[22] When relaxing the assumption of a continuous distribution function, there may be natural groupings of like trajectories that readily emerge from the data. That is, the variation around the population's average trajectory that is apparent with traditional growth modeling methods may constitute meaningful clusters of individual trajectories.

Given the idea that groups of individuals may be following along distinct offending trajectories, a problem arises regarding how to classify individuals into developmental

[22] Other common terms used to refer to the technique include trajectory analysis, finite mixture modeling and group-based modeling.

groups. As previously discussed, subjective classification procedures may identify groups as distinct but represent only random variation, may fail to identify meaningful groups, and may yield unreliable statistical tests as a result of uncertainties of group assignments (Nagin, 2005). Additionally, with subjective classification, one cannot determine whether developmentally distinct trajectory groups may exist outside of their assumed existence (Nagin, 2005).[23] To complicate matters, latent trajectory groups are, by definition, not directly observable. If they were known, standard methods would be sufficient for estimating parameters across the different classes of offenders. To overcome these problems, Nagin and Land (1993) pioneered an innovative technique that empirically classifies individuals retrospectively based on existing longitudinal observations of behavior (see also Nagin, 2005).[24] In short, while the mathematics behind the methodology are fairly involved, the principal idea is that the number of trajectory groups as well as the proportion of individuals within each group and the growth characteristics of the distinct developmental trajectories can be empirically determined using longitudinal data (Nagin, 2005). The technique permits the best of both worlds; it provides a way to assess within-individual changes in offending over time

[23] As a counterpoint, some argue that latent class growth analysis or semi-parametric group-based modeling cannot test for the existence of groups (see Skardhamar, 2010).

[24] An alternative approach to latent class growth analysis is generalized growth mixture modeling. Some discussion of why this approach is not employed here is warranted. Unlike latent class growth analysis, generalized growth mixture modeling allows variation around the means of the trajectory groups (Muthén, 2001). Then, growth mixture modeling is a hybrid of the group-based approach and traditional growth modeling approaches and typically results in the extraction of fewer groups to adequately model the data. However, Nagin (2005) notes this approach has a number of important drawbacks that limit its utility. First, modeling both between-group and within-group variation provides for a highly complicated statistical model which limits its applicability to model count and other types of data. Second, modeling within-group variation leads to an important conceptual dilemma, since it may become unclear what exactly constitutes a group. This is perhaps the most important limitation. Several developmental theories suggest there are fundamentally different types of offenders and generalized growth mixture modeling may only muddy, not clear, the waters. Third, the layering of heterogeneity which is characteristic of the general growth mixture modeling approach creates problems with model identification. Fourth, generalized growth mixture modeling may create false groups due to these model identification issues (see also Bauer & Curran, 2003; 2004).

while simultaneously capturing important between-individual differences in the intercept and growth factors of different trajectories.

Technical details

While group membership is not directly observable, the number of trajectory groups can be ascertained from the data and distinct trajectories can be captured with unique polynomial functions of time (Nagin, 2005). I begin with an explanation of the general base model and focus on acts of violent crime as the behavioral trajectory of interest. The base model has the following formula:

$$P(Y_i) = \sum_{j}^{J} \pi_j P^j(Y_i),$$

where Y_i is one's sequence of violent crime over time, π_j is the probability of a random individual in the population belonging to trajectory group *j*, $P^j(Y_i)$ is the probability of Y_i given one's group membership *j*. The summation indicates that the *J* conditional likelihood functions, $P^j(Y_i)$, require aggregation to result in the unconditional probability of the data. Thus, on the left side of the equation, $P(Y_i)$ is the unconditional probability of observing individual *i*'s sequence of violent crime over time (Nagin, 2005, p. 25).[25] The general likelihood of the entire sample (N) can be found as the product of the individual likelihood functions as shown:

[25] For a given group, there is the assumption of conditional independence meaning that one's behavior at time *t* is independent of one's prior behavior. In other words, for individuals in the same group, their behaviors are assumed not to be serially correlated (i.e. individual level trends from the group trend are uncorrelated). However, there will be serial correlation elsewhere such that the prior behaviors will be correlated with future behaviors in the general population (Nagin, 2005, p. 26). The conditional independence assumption is not unique to LCGA. HLM makes the same assumption of conditional independence but does so at the individual level. LCGA makes this assumption at the group level (Nagin, 2005, pp. 26-27).

$$L = \prod^{N} P(Y_i)$$

The base model can be easily adapted for censored normal, binary, and count data. I skip ahead and specify the equations relevant to count data since the current study is measuring violent crime trajectories of the count variety. The formula for the adapted base model for count data (Poisson distribution) is shown below:

$$p^j(y_{it}) = \frac{\lambda_{jt}^{it} e^{-\lambda_{jit}}}{y_{it}!} \qquad (y_{it} = 0,\ 1,\ 2,...)\ ,$$

where, $p^j(y_{it})$ is the probability of one having committed any non-negative integer number of violent acts at a given time, λ_{jt} is the mean event rate of violent crime for all individuals belonging to group *j* at time *t*, λ_{ji} is the expected number of violent crimes for all individuals belonging to group *j* at time *t*. Thus, the model assigns a probability to each possible outcome such that there is a probability of committing zero acts, a probability of committing one act, a probability of committing two acts, so on and so forth. Importantly, the probability assigned to each specific outcome possibility ($p^j(y_{it})$) is dependent upon λ_{jt} (Nagin, 2005, p. 32).[26]

[26] For example, if λ is equal to one, then there will be a relatively high probability for the occurrences of zero or one acts of violence, lower probability of the occurrences of two acts, still lower probability of occurrence of three acts, so on and so forth. If λ is higher and equal to say five, there will be a relatively low probability of occurrence for zero acts of violence, an increasing probability up through five acts of violence (with the probability peaking at five), and then decreasing probabilities past five acts of violence. Thus, when λ for each group becomes sufficiently large, results from the Poisson model and the censored normal model will converge.

In the basic Poisson model, the link function connects the event rate for all individuals with time by assuming λ_{jt} varies with time as suggested by the following formula:

$$\ln(\lambda_{jt}) = \beta_0^j + \beta_1^j T_{it} + \beta_2^j T_{it}^2 + \beta_3^j T_{it}^3,$$

where T_{it}, T_{it}^2, and T_{it}^3 are time, squared time, and cubed time and β_0^j, β_1^j, β_2^j, and β_3^j are the four estimated parameters that indicate the level and trend in offending behavior for each group over time.[27] In other words, these parameters determine the shape of the trajectories. One can also estimate linear or quadratic forms of the model by not including and modeling squared and cubed time or just cubed time, respectively. The full model is discussed here for the sake of completeness but one should keep in mind that an all-quadratic model is used to identify the latent classes of violent offenders in this study. The quadratic model is more parsimonious than the general cubic model and is a logical choice to capture changes change in trajectories over time in this study; more specifically, the trajectories are estimated only over a few years during adolescence and the quadratic term is sufficient to model meaningful changes in violent crime among the offending subpopulations during this time period.

To those with experience modeling within-individual change in a hierarchical linear modeling framework, the above equation should share many similarities to the analysis of population average trajectories of count data. A key difference, however, is

[27] It is worth noting that the modeling of $\ln(\lambda_{jt})$, as opposed to λ_{jt}, is a matter of convenience as it ensures that the algorithm used to estimate maximum likelihood estimates cannot result in negative values of λ_{jt}. Specifically, taking the exponential of log of the event rate for individuals in group j at time t (i.e., $e^{\ln(\lambda_{jt})}$) will always yield a value greater than zero.

that there are *J* sets of the parameters estimated for the model. Thus, for each trajectory group there are four parameters that are estimated that correspond not to overall population averages but to group-specific averages. To understand the meaning of these parameters, it is helpful to consider the parameter estimates for a model with a two class solution (*J*=2). For simplicity, I assume these two classes are non-offenders and adolescence-limited offenders. First, a group of non-offenders would have four parameter estimates that all failed to be significantly different from zero (*j*=1). This is because non-offenders, by definition, have a violent crime trajectory which has an intercept equal to zero ($\beta_0^1 = 0$), a slope that is not increasing or decreasing ($\beta_1^1 = 0$), and no directional changes in behavior over time ($\beta_2^1 = 0$; $\beta_3^2 = 0$). Second, another group akin to Moffitt's (1993) adolescence-limited offenders would have several statistically significant parameter estimates (*j*=2). Assuming the first point of data collection was after onset of offending, these offenders would have a significant positive initial status estimate ($\beta_0^2 > 0$), a significant positive linear term ($\beta_1^2 > 0$), but a significant negative quadratic term ($\beta_2^2 < 0$).[28] Importantly, the different parameters that are estimated for each group permit assessments of differences across groups in both the amount of violent crime and the development of this behavior over time (Nagin, 2005). For example, the level difference between a group of non-offenders and a group of steady offenders would be important and so would the difference between a group of steady offenders and another group of desisting offenders.

[28] The cubic term may or may not be significant depending on how bell shaped the offending behavior was over time.

Model estimation

Longitudinal data is necessary in order to employ LCGA procedures and, at minimum, three time points are necessary to estimate linear trajectories of violent crime. Additional time points are necessary if a researcher seeks to measure changes in the direction of a trajectory (modeled by quadratic and cubic terms). Thus, a minimum of five waves of data are necessary to fully model levels and changes in offending trajectories. Given the current study is focusing on the effects of sanctions as turning points in the lives of individuals following along different trajectories (e.g., chronic offenders), modeling directional changes in offending trajectories is important. However, given there are only five waves of data and the trajectories span just a couple of years, intra-individual change in violent offending can be adequately captured using linear and quadratic terms. The model search described below begins with a quadratic model rather than the more complex cubic model.

Once the base model has been appropriately adapted for one's data type (e.g., Poisson) and the number of parameters per group to be estimated have been determined (e.g., quadratic model), one estimates several models each with a different number of latent classes specified (e.g., c=2, 3, 4). One must select among the different models estimated and various statistics are employed for this purpose. The most common statistic to determine the number of trajectory groups is the Bayesian Information Criteria (BIC), which can be used to determine the number of groups in both nested and unnested models (D'Unger, Land, McCall, & Nagin, 1998; Nagin, 1999). BIC scores are calculated with the following equation:

$$BIC = \log(L) + \log(n) * k.$$

where *L* denotes the maximum likelihood, *n* denotes the sample size, and *k* denotes the number of parameters. BICs exact a penalty for adding parameters thereby favoring parsimony (Jones, Nagin, & Roeder, 2001; Nagin, 2005).[29] Following the selection of the model with the appropriate number of groups, the chosen model can be further refined in an optional second stage that eliminates non-significant growth parameters. For instance, the violent offending patterns of abstainers, by definition, can be adequately modeled with only an intercept since they have neither growth nor change in growth over time. Since BIC scores favor parsimony, models that successfully capture the intra-individual changes in the sample with the fewest number of parameters will be preferred.

Upon settling on a final LCGA model, posterior probabilities of group assignment can be used as a way of assessing the relative precision of the best fitting model. When the mean posterior probabilities of group assignment are greater than the standard cutoff of 0.7, it indicates that the model is adequately precise in its group assignments (see Nagin, 2005). In addition, the odds of correct classification (OCC) can be used to assess model precision. Values greater than five for each trajectory group indicate adequate class assignment (Nagin, 2005). Additional details regarding assessing model precision are discussed during the presentation of the results (Chapter 8).

There are several important descriptive outputs immediately worthy of consideration once it has been determined that the best fitting model is adequately

[29] Mplus BIC scores have positive values and the choice of the best model is given by the smallest BIC score. Mplus BIC scores = log (*L*) + log (*n*) * *k*. Nagin's (2005) BIC scores differ by a factor of negative two; thus, BIC = -2 log (*L*) + log (n) * *k*.

precise in its group assignments. The number of distinct developmental trajectories that are extracted from the data is obviously important. And, the proportion of individuals belonging to each identified subpopulation is also very useful information. Finally, the statistical significance of the parameter estimates and the shapes of each trajectory group should be explored. Visual representations of the distinct trajectories can greatly facilitate group comparisons. These trajectory groups can then be used for various purposes, one of which is to estimate "treatment effects" across the different subpopulations. Obtaining treatment effects with observational data in general is the subject of the next section.

Propensity Score Matching Analysis

Conceptual overview

Centuries ago John Locke (1975 [1690]) stated: "that which produces any simple or complex idea, we denote by the general name cause, and that which is produced, effect" (p. 324). While the meaning of cause and effect is quite clear, establishing a causal relationship in social science has proven to be most challenging. At the heart of methodological approaches to uncover causal relationships is an attempt to observe the counterfactual (i.e., what would have occurred within an individual if he or she would have experienced a different treatment condition). Although the counterfactual framework is largely contributed to works of Neyman (1923) and Rubin (1974; 1978; 1980; 1986), the importance of knowing what would have happened in the counterfactual case has a long history which can be traced, at least, as far back as the time of Aristotle (Holland, 1986). In the counterfactual framework, each individual is thought to have both a possible outcome of receiving treatment and a possible outcome of not receiving treatment (Guo & Fraser, 2010). Naturally, the measured outcome is

always the possible outcome that corresponds to the treatment condition that was actually experienced. The counterfactual, or the possible outcome that corresponds to the treatment condition that was not experienced, can never truly be measured in actuality (Guo & Fraser, 2010). This constitutes the fundamental problem of causal inference (Holland, 1986). Then, the "central task for all cause-probing research is to create reasonable approximations to this physically impossible counterfactual" (Shadish, Cook, & Campbell, 2002, p. 5).

The scientific approach that is regarded as the "gold-standard" for causal investigations is the experimental design, which was first formally developed and implemented in the field of agriculture during the 1920s (see Fisher 1925; 1926). The experimental design involves randomly assigning individuals to either an experimental condition or a control condition. In theory, this reduces all differences between the groups with the exception of the treatment, which is left to the experimenter to manipulate. Hence, true randomization is the hallmark of experimental designs. While the counterfactual can never truly be observed and, consequently, a single individual's treatment effect will never be known, the counterfactual framework holds that the "average treatment effect" can be obtained by taking the difference in means between the experimental and control groups (Guo & Fraser, 2010). If the two groups are truly equal across all factors other than the treatment following randomization procedures, then the mean difference is the typical causal effect of the treatment on the outcome of interest.

While experimental designs drive research efforts in the natural sciences, ethical and practical considerations constrain opportunities for experimental designs in social

science. As a consequence, observational studies tend to dominate many social science fields including criminology. A number of methodological techniques (e.g., ordinary least squares regression, matching, stratification) have been advanced over the years to establish causal relationships when experimental data is unattainable, impractical or, simply, unavailable (Guo & Fraser, 2010). Matching in observational studies is a popular methodological technique because it closely mimics experimental designs; matching creates group equivalency on variables that are used explicitly in the matching procedure. For example, matched-pairs methodology employed to assess the effects of official intervention would involve taking one individual with certain characteristics (e.g., white, male, single-parent home) that was arrested and matching him or her to another individual who has the same characteristics but did not get arrested. In this way, group equivalency is achieved on key confounders leaving the researcher to estimate the average treatment effect of arrest. Of course, the researcher is forced to make the assumption that there are no additional variables that were not used in the matching procedure which would substantially bias the findings. The more relevant control variables one includes in the matching procedure, the closer it becomes to a true experimental design and the more likely that the empirical findings are not subject to substantial amounts of hidden bias.

The interaction of two key problems with traditional matching approaches limits their utility for establishing cause. First, the number of potential confounders that can be included in traditional matching procedures is limited. Taking the above example with only three binary covariates, there are eight (2^3) possible combinations of individuals; in this case, matching those arrested with those not arrested across these

variables would probably be easily accomplished. However, things get considerably more complex if say there are just ten potentially relevant control variables. The number of possible combinations of those variables jumps to 1,024 (2^{10}). Finding matches for individuals when only a handful of covariates are relevant can be difficult, if not impossible, and the complications grow exponentially with each added covariate under the matched-pairs method (see Rosenbaum, 2002). Second, when using observational data, the assumption of ignorable treatment assignment is likely to be violated. The assumption holds that, conditional on covariates, the assignment of an individual to a treatment or control group is independent of the outcome (Guo & Fraser, 2010). If this assumption is violated, it means that some covariate is significantly related to treatment assignment. Taken together, these two issues create a difficult dilemma. While it would appear wise for researchers to account for as many covariates as possible to meet the assumption of ignorable treatment assignment, it becomes increasingly difficult to find matched pairs as additional covariates are considered.[30]

Acknowledging the benefits of matched methods but noting the practical limitations of such approaches, Rosenbaum and Rubin (1983) developed a matching technique which seeks to create covariate balance across a large number of variables by matching individuals on only one variable—their propensity for treatment. They demonstrated that, in theory, individuals can be matched based on a single propensity score which can create covariate balance on all variables that are used to make up the

[30] An additional assumption of propensity score analysis is the stable unit treatment value assumption (SUTVA), which is the a priori supposition that an outcome value for an individual assigned to a specific treatment is similar regardless of the mechanism used to assign treatment and irrespective of the type of treatments received by other individuals (Guo & Fraser, 2010). There is no reason to suspect that these assumptions have been grossly violated. Specifically, an individual's treatment status should not influence outcomes of other individuals via changes in social interactions or environmental conditions.

propensity score (Rosenbaum & Rubin, 1983). True propensities for treatment cannot be directly known but they can be estimated using logistic regression methods. Interestingly, using propensity scores that are estimated has been shown to actually remove more bias than would be the case if true propensity scores could be used since the former removes both systematic and random biases whereas the latter only removes systematic imbalance (see Joffe & Rosenbaum, 1999).

Technical details

With an understanding of the logic behind the counterfactual framework and the problem propensity score matching attempts to solve, key equations and model estimation procedures are now discussed. Rosenbaum and Rubin (1983) defined the propensity score for individual *i* as the conditional probability of assignment to a treatment ($Wi = 1$) versus non-treatment ($Wi = 0$) given the vector of observed covariates (x_i), which is represented in mathematical form as follows:

$$e(x_i) = pr(Wi = 1 \mid X_i = x_i)$$

While the complex derivations and proofs are omitted here, $e(x_i)$ is a balancing measure that summarizes the information available from the observed covariates. Rosenbaum and Rubin (1983; see also Rosenbaum, 2002) demonstrated that the propensity score itself can balance the differences across the vector of observed covariates thereby creating equivalent treatment and control groups, across observed variables, with a single score. When individuals have the same propensity score, it does not automatically guarantee in practice that they will have the exact same set of observed covariates (e.g., male, White, single-parent). In fact, there will be instances in which two individuals with the same propensity scores do not have the same x_i. Importantly,

however, any differences in x_i between individuals with the same propensity scores are chance differences as opposed to systematic differences (Rosenbaum, 2002).

Guo and Fraser (2010) show that the assumption of ignorable treatment can be linked to the propensity score with the following equation:

$$x_i \perp w_i \mid e(x_i).$$

This equation indicates that a treatment assignment (w_i) is independent of the vector of observed covariates (x_i), conditional on the propensity score ($e(x_i)$).[31] There are two important implications of this equation. First, individuals with similar propensity scores should have a similar vector of observed covariates (less any chance differences), meaning that the distribution of the observed covariates will be similar across the treatment and control groups. Second, every individual has the same probability of treatment assignment conditional on the propensity, mimicking randomization of treatment and control groups as in true experiments (Guo & Fraser, 2010).

To assess the success of the propensity score matching procedure for creating covariate balance, standardized bias statistics are utilized. First, for the unmatched sample, the standardized bias statistic is defined as the absolute difference in covariate means (or proportions for dichotomous variables) divided by a special standard deviation. Its formula is given by:

$$d_X = 100 * | M_{Xt} - M_{Xp} | / s_x,$$

[31] Notation: independent ($\perp$); conditional ($|$)

where d_X denotes the standardized bias statistic for some covariate *X* before matching, M_{Xt} is the mean of the treatment group, and M_{Xp} is the mean of the group of potential controls before matching. s_X is the special standard deviation for continuous measures and is calculated as follows:

$$s_X = \sqrt{(s_{Xt}{}^2 + s_{Xp}{}^2)/2},$$

and, is calculated as follows for dichotomous measures that are coded in binary format:

$$s_X = \sqrt{(M_{Xt}(1 - M_{Xt}) + M_{Xp}(1 - M_{Xp}))/2}.$$

Thus, a standard bias statistic in this form that is equal to 30 signifies that the covariate means or proportions are out of balance by 30% of a standard deviation. The standard cutoff value for a covariate to be considered to be balanced is ≤ 20, which equates to a difference in covariate means or proportions less than or equal to 1/5 of a standard deviation (Haviland et al., 2007). In the ideal case, covariates would have values equal to 0 indicating perfect balance. In practice, however, excellent covariate balance is present when standardized bias statistics are below a value of 10.

With matching procedures other than 1:1 pair matching, the formula for the post-matching standardized bias statistic is modified slightly to account for the fact that a treated individual is matched to one or more controls and/or a control individual is matched to one or more treated individuals. Specifically, while the general formula for the standardized bias statistic remains similar, the means for the treated and/or control groups are not simple arithmetic means. Extending Haviland and colleagues (2007; 2008) formula for 1:K variable matching to full matching and eliminating the stratification

summation (as no stratification is conducted in this analysis) leads to the following formula:

$$d_{Xm} = 100 * | M_{Xtm} - M_{Xcm} | / s_x ,$$

where d_{Xm} denotes the standardized bias statistic for some covariate *X* after matching, M_{Xtm} is the unweighted mean of means for the treated matched to the controls, and M_{Xcm} is the unweighted mean of means of the controls matched to the treated. For the following notation, subscript *i* refers to treated participants and subscript *j* represents control participants. To ultimately obtain M_{Xcm}, M_{ci} is first calculated which represents is the average of X_{cij} for all *j* controls matched to participant *i*. The formula for M_{ci} is as follows:

$$M_{ci} = \frac{1}{m_i} \sum_{j=1}^{m_i} X_{cij}$$

where X_{cij} is the value of covariate *X* post-matching for the j_{th} control that matches to treated participant *i*. These mean values, M_{ci}, are then averaged across n_t total treated individuals as the following formula illustrates:

$$M_{Xcm} = \frac{1}{n_t} \sum_{i=1}^{n_t} M_{ci} ,$$

The value for M_{Xtm} is calculated similarly for matching procedures, such as full optimal matching, that also have the potential for a variable number of treated individuals to be matched to a single control subject (e.g., see Figure 7-1). Analogous to the previous calculations, the formula for M_{tj} is given by:

$$M_{tj} = \frac{1}{m_j} \sum_{i=1}^{m_i} X_{tji} \ ,$$

where subscript *i* still refers to treated participants and subscript *j* still represents the control participants. X_{tji} is the value of covariate *X* post-matching for the i_{th} treated that matches to control participant *j*. Thus, M_{tj} is the average of X_{tji} for all *i* treated individuals matched to each control participant *j*. M_{tj} is then averaged across n_c total control individuals as shown:

$$M_{Xtm} = \frac{1}{n_c} \sum_{j=1}^{n_c} M_{cj} \ .$$

In the case of full matching with variable treatment-control and control-treatment possibilities, the post-matching means M_{Xtm} and M_{Xcm} do not represent simple arithmetic means as do the pre-matching means M_{Xt} and M_{Xp}. Still, they are easily calculated and directly comparable as they use the same "special" standard deviation (s_X).

Percentage bias reduction statistics are often calculated in conjunction to assess the extent to which the matching procedure reduced the pre-existing covariate imbalances. With the standardized bias statistics calculated both pre- and post-matching, the percentage bias reduction (*%BR*) is simply calculated as follows:

$$\%BR = 1 - (d_{Xm} / d_X) \ .$$

Collectively, the covariate balance statistics permit one to assess whether covariate balance has been achieved through propensity score matching for each covariate as measured by a standard metric (i.e., $d_{Xm} \leq 0.20$) as well as to specify exactly how much bias has been reduced. These analyses are noteworthy for their transparency and that

they are conducted independent of, and prior to, the stage of estimating the treatment effect.

Recall that an individual's counterfactual can never truly be observed but that an average treatment effect can be obtained. When $x_i \perp w_i \mid e(x_i)$ is true and $e(x_i)$ balances the observed covariates as indicated by acceptable standardized bias statistics, the formula for the unbiased average treatment effect (ATE) at a given propensity score $e(x_i)$ is:

$$E[E(Y_1,\mid e(x_i),W_i = 1) - [E(Y_0,\mid e(x_i),W_i = 0)] = E[E(Y_1 - Y_0 \mid e(x_i)],$$

In other words, for all individuals with the same propensity score, the mean difference in outcome between the treatment and control groups serves as an unbiased estimate of ATE at that particular propensity score. And, in the case of propensity score matching, the mean of these propensity-specific matched differences is the unbiased estimate for the ATE as shown below:

$$\text{ATE} = E[E(Y_1 - Y_0 \mid e(x_i)] = E(Y_1 \mid W = 1) - E(Y_0 \mid W = 0).$$

With the technical details laid out, attention is now turned to model estimation procedures.

Model estimation

The first step in model estimation is the specification of the treatment model to be used to obtain the best estimates of the true propensity scores. Correct specification of a treatment model is necessary because effect estimates are dependent upon initial model specification (Rubin, 1997). Guo and Fraser (2010) note that the correct specification of a treatment model involves obtaining propensity scores that balance the two groups on the observed covariates. Including theoretically relevant covariates and

specifying the appropriate functional form of them, including polynomial and interaction terms as necessary, are the keys to success (Guo & Fraser, 2010). Some researchers have casted a wide net identifying as many covariates as possible in the data (see Bingenheimer, Brennan, & Earls, 2005). However, having more covariates in the model than necessary can actually worsen problems with common support (Smith & Todd, 2005) and result in wider ranges for estimated treatment effects (see Hirano & Imbens, 2001). Therefore, instead of including all possible variables in the data set, potentially relevant covariates with theoretical links to treatments and outcomes are included (see Chapter 6). Interactions terms are added to the treatment model if balance is not successfully achieved with a strictly additive treatment model.

With estimated propensity scores, the matching procedure is now executed. The current study employs optimal matching procedures (see Haviland et al., 2007). Unlike greedy matching approaches such as nearest neighbor matching, optimal matching is not subject to the same problems including common support region sensitivity to the specifications of the treatment model and large case loss due to insufficient common support overlap (Guo & Fraser, 2010). With greedy matching, propensity score matched pairs are built by selecting a treated subject and finding the best available control subject that has not yet been used, under matching without replacement. Once the treated subject has been matched to the control subject that pair is eternally wedded. This type of matching is “greedy” in the sense that a matched pair being formed is the only concern at the time. The procedure pays no mind that the ability to form other good matched pairs later may be greatly hampered by the current pairing.

While greedy matching approaches pick the best pair at any given time, optimal matching is a smart process whereby matched pairs are built, and sometimes rebuilt, with the end goal of obtaining the lowest total distance between the treatment and control propensity scores. Pairs that are initially wedded can later be divorced from one another should the treated and control subjects be better paired with other individuals to best solve the optimization problem. Thus, optimal matching approaches are not greedy since they are concerned with the overall group of matched pairs rather than the current pairing.

Within optimal matching, full matching is employed which matches each treated individual to one or more controls and each control individual to one or more treated. For the current study, full matching is critical as the small sample size across trajectory groups will need to be retained. An optimal full matching approach retains all individuals in the sample and matches treated and control subjects into matched sets which results in the best solution to the overall optimization problem. It is not uncommon to have a single treated matched to a large number of control subjects or vice versa. While this procedure is a departure from 1:1 matching, it tends to result in substantial improvement in overall bias reduction especially when compared to 1:K or variable matching approaches (e.g., see Guo & Fraser, 2010). The matching procedure is executed in 'R' using 'optmatch' (for a detailed review, see Hansen, 2004; see also Guo & Fraser, 2010).

Given full matching is a relatively new procedure, Figure 7-1 provides an illustration of how full matching differs from other more common approaches to matching (i.e., 1:1, 1:K). In 1:1 matching, each treated subject is matched to exactly

one control subject. While this approach under an optimal matching scheme tends to reduce substantial amounts of bias, it does not use all of the available information. In 1:K matching, each treated subject is matched to a specific number of control subjects. In this example, 1:K matching happens to use most of the available information given there are just slightly more than twice the number of controls to treated subjects but it does so at a huge cost. Several of the matched pairs do not have similar propensity scores and bias reduction in this case will be less than ideal. While not illustrated, an alternative to 1:K matching is achieved by permitting K to vary (with or without restrictions). This is known as "variable matching" and is a matching strategy that does result in better bias reduction as compared to 1:K matching where K is fixed at some value. Full matching is an extension of this 1:K variable matching scheme which also allows K:1 variable matching, typically resulting in the greatest level of bias reduction (see Guo & Fraser, 2010). As illustrated in Figure 7-1, full matching uses all available information and matches each treated subject to one or more control subjects and/or matches each control subject to one or more treated subjects. In short, the example demonstrates that full matching results in substantial bias reduction which was not the case with 1:K fixed matching and will use all available information which was a limitation of the 1:1 matching strategy in this example.

The next step involves a reassessment of the differences between the treated and control groups along each of the covariates used to estimate the propensity scores. In the context of propensity score matching, Haviland and colleagues (2008) note that "it is a mistake to see the *t* statistic as a formal test of balance when applied to estimated quantities, such as the propensity score, which were estimated from the data to

discriminate treated and control groups" (p. 431; see also Ho, Imai, King, & Stuart, 2007; Imai, King, & Stuart, 2006). A main reason *t* statistics do not constitute formal tests of covariate balance is because balance is a property not of the population but rather of the observed sample itself (Ho et al., 2007; Imai et al., 2006). T-statistics are not directly comparable pre- and post-matching since these two samples are often of different sizes (Haviland et al., 2007). More importantly, *t*-tests are functions of remaining observations, variance of treated and control groups, and the ratio of remaining treated and control subjects. As such, a *t*-statistic can actually lead to a conclusion that balance has been achieved even though balance has really worsened in reality (Ho et al., 2007; Imai et al., 2006). In addition to these key problems with using *t*-tests, the nature of hypothesis testing is such that it leaves researchers with the impression that balance has been achieved and that any remaining imbalance is simply ignorable; however, less bias is always better and this is especially so for any covariate that may be strongly related to the outcome of interest (Ho et al., 2007; Imai et al., 2006).

As opposed to using *t*-statistics, the absolute standardized difference in covariate means (d_x) is directly compared to their corresponding pre-matching values. In this way, one is able to determine how successful the optimal full matching procedure was in removing differences between the treated and control groups across the observed covariates. If this process is unsuccessful (i.e., there are substantively meaningful differences between the treated and control groups across one or more observed covariates after matching), polynomial and interaction terms are added to the treatment model and the process of optimal matching and reassessment will continue. Ultimately,

the goal is to obtain two groups that lack differences with respect to all observed covariates.

Once all differences have been adequately removed across all covariates, the final step in the model estimation procedure involves the estimation and interpretation of the ATE. For this purpose, the Hodges-Lehmann aligned rank test is employed to estimate the ATE of the optimally and fully matched sample. In addition to the statistical significance of the Hodges-Lehmann aligned rank test, the absolute standardized difference in means is used which yields an estimate of effect size (Haviland et al., 2007).

An LCGA-PSM Integrated Method

Latent class growth analysis provides researchers with an important tool to classify individuals into different developmental trajectories and model intra-individual change in behavior over time. Propensity score matching analysis permits researchers to create covariate balance on observed variables, to explicitly assess the relative success of the matching procedures, and to estimate average treatment effects. Recently, Haviland and colleagues (Haviland & Nagin, 2005; Haviland et al., 2007; 2008) noted the unique benefits of each of these two techniques and suggested that they could be integrated to address important questions that emerge from the life-course criminological literature. As has been thoroughly discussed in this paper, many life-course theories anticipate that the effects of treatments may depend on one's behavioral trajectory (e.g., Moffitt, 1993). The method is well-suited to answer whether there are trajectory group-specific effects of certain treatments such as official intervention. Moreover, the use of latent class growth analysis and propensity score matching in conjunction helps to create balance both on prior outcome history and on other measured confounders,

respectively, providing for a good estimation of the treatment effect with observational data (Haviland et al., 2007; 2008).

The LCGA-PSM integrated method is essentially the sequential employment of latent class growth analysis followed by propensity score matching within trajectory groups. Discussing the procedure, Haviland and colleagues (2007) write:

> The integration of group-based trajectory modeling and propensity scores is composed of a three-stage analysis. The first stage involves estimating a group-based trajectory model for the outcome and participants of interest…In the second stage, each treated individual is matched with one or more untreated individuals…We then check the degree of success of the matching strategy in achieving balance…In the third stage of the analysis, the treatment effect of the event of interest…is analyzed…within and across trajectory groups. (p. 249).

In short, two highly useful techniques in their own right are brought to bear on longitudinal data in a systematic and straightforward manner, which ultimately provides an ideal way to determine whether an official intervention experience serves as a turning point, in either direction, for one or more distinct violent offending trajectory groups.

Analytic Strategy

Group based trajectory models are estimated using a self-reported violent delinquency scale composed of counts of violent criminal acts. Specifically, a Poisson model is estimated and the number of violent offending trajectory groups is determined using the BIC score and other relevant selection criteria. Next, posterior probabilities of group assignment and other common post-estimation statistics are examined to determine the relative precision of the best fitting model. The number of trajectory groups, the proportion of individuals within each trajectory group, and the shapes of the offending trajectories are explored. To supplement this descriptive analysis, trajectory

group-specific descriptive statistics are reported and one-way ANOVA models are estimated. This serves to determine whether any covariates are significantly related to trajectory group membership.

With the best fitting LCGA model, individuals within each subpopulation are split into two groups: 1) those receiving official intervention and 2) those not receiving official intervention. Covariate balance is assessed prior to matching using the absolute standardized difference in covariate means. Next, logistic regression models are used to estimate propensity scores for each individual. Optimal full matching procedures are implemented and then covariate balance is reassessed for each subpopulation to test the successfulness of the matching procedures. Changes to the treatment model are done as necessary and this cyclical process is repeated until the best possible model has been estimated or no substantial bias remains. In addition to carrying out propensity score matching within each trajectory group, it is also done for the sample as a whole.

The integration of the two advanced quantitative techniques culminates with the estimation of the treatment effect. More specifically, with two statistically similar groups within each subpopulation, statistical significance of the effect of official intervention on violent outcome behavior is calculated for each of the violent offending subpopulations using the Hodges and Lehmann (1962) aligned rank test. Additionally, the absolute standardized difference in outcome means is reported as a measure of effect size. Similarly, the treatment effects are estimated for longer-term outcomes to determine whether there are differential effects over time. This trajectory group-specific analysis is

repeated for the other official intervention (e.g., arrest). For general comparative purposes, the analysis is carried out on the sample as a whole as well.

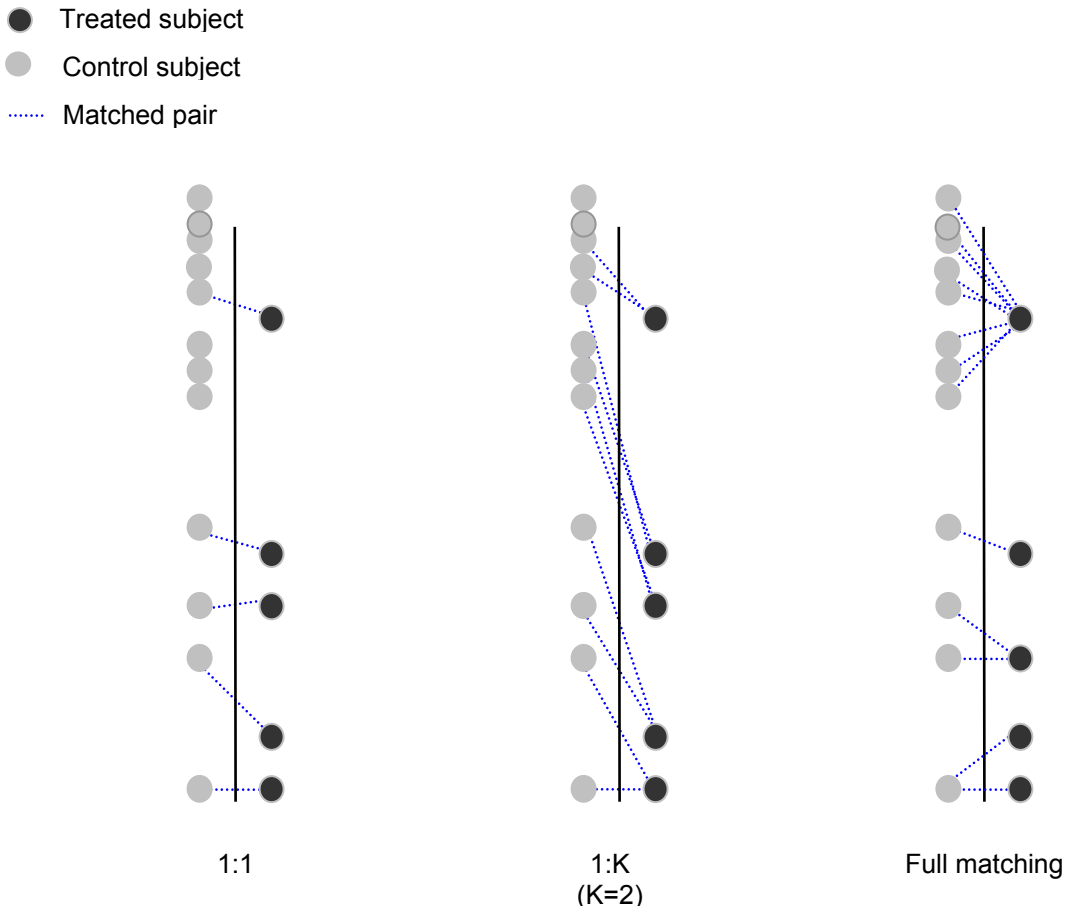

Figure 7-1. Illustration of three alternative matching strategies.

CHAPTER 8
RESULTS: VIOLENT CRIME TRAJECTORIES OF OFFENDING

Chapter 8 presents the results of the Poisson LCGA model search for the appropriate solution and describes in detail characteristics of the trajectory groups that emerge from the selected model. First, standard model selection criteria are used to select the model with the appropriate number of latent classes from five alternative Poisson quadratic LCGA models that have one through five latent classes, respectively. With the number of violent crime trajectory groups determined, the model is refined to determine the order of the polynomials necessary to capture the within-individual change for each violent offending trajectory group. Next, the precision of trajectory group assignments is assessed using standard criteria including mean posterior probabilities, odds of correct classification, and correspondence between the proportion of individuals in each latent class based upon the estimated model and the proportion based on maximum probability assignment. The parameter estimates of the selected model are then discussed and the shapes of the trajectories and the proportion of individuals belonging to each are reviewed. To supplement this descriptive analysis, trajectory group-specific descriptive statistics and results of one-way analyses of variance are reported. Finally, the trajectory group-specific treatment experiences and violent crime outcomes in the short- and long-runs are explored.

Model Selection

Table 8-1 contains information on key model selection criteria for five different quadratic latent class growth models with latent classes ranging from one to five. As evidenced by the high Bayesian Information Criteria (BIC), the one class model is by far the worst fitting model suggesting the population as a whole appears to be comprised of

some meaningful subpopulations. The three and four class models have BIC scores that are relatively similar to one another yet substantively lower than the one, two, and five class models. The three class model has the lowest BIC and the Schwarz (1978) and Kass & Asswerman (1995) metric for model comparison suggest there is nearly a 72% chance that the three class model is the correct model. There is approximately a 28% chance that the four class model is the correct model and essentially no chance that the one, two, or five class model accurately represents the data.

It is worth noting that in addition to using model selection criteria such as the BIC, Nagin (2005, p. 75) states that "selection must balance the sometimes competing objectives of model parsimony and capturing the distinctive features of the data." In the present study, parsimony is favored since the identification of a large number of latent classes is of no utility if the effects of official intervention cannot be ascertained within the trajectory groups. If this is the case, the analysis moves from one of causal discovery to one that is quite descriptive in nature. The four class Poisson LCGA quadratic model that was estimated essentially breaks the "low" class—that is in the three class model and will be discussed shortly—into two different latent classes. One of these classes is slightly trending upward and the other is slightly trending downward but, importantly, both are offending at levels that are between the other two trajectories identified. Estimates of the precision of class assignments of the four class model are not all above conventional cutoffs though they are nearly so. In sum, the three class model is selected as the best fitting model given the BIC provides the strongest support for it and most of the key features of the data are captured by it while keeping trajectory

groups as large as possible.[32] This latter point is important as studies have had difficulty matching within small trajectory groups (Haviland et al., 2007).

The selection of the number of latent classes is of primary concern for any latent class growth analysis but an optional second stage of model selection can further refine the model by adjusting the number of free parameters necessary to model changes in offending over time across each of the latent classes. For instance, some trajectory groups which are stable across time (e.g., non-offenders) may adequately be captured with an intercept only and, as a consequence, estimated growth parameters for this class would be statistically insignificant. Similarly, the trajectory of certain latent classes may be adequately captured with just a linear term. Recall the BIC favors parsimony so the lowest values on these metrics would be expected for models with the fewest growth parameters that adequately represent all relevant within-individual change in violent crime over time. Indeed, when higher order polynomials are estimated and lower order polynomials are sufficient to describe the level and trends in the trajectories, standard errors of the growth parameters and intercepts can be elevated potentially rendering certain parameter estimates insignificant (Nagin, 2005). Table 8-2 contains model selection criteria for a parsimonious three class Poisson LCGA model with the highest significant polynomials which are necessary to model the change exhibited in each specific violent crime trajectory (i.e., 2,0,2) and the all-quadratic trajectory model that was estimated for selection of the number of latent classes (i.e., 2,2,2). There is strong evidence in favor of the parsimonious three class model which restricts the order

[32] While model selection criteria as well as practical considerations lead to the selection of the three class model, the differences in the two aforementioned groups in the four class model may be substantively important from a theoretical standpoint, a point which is discussed in some detail in the final chapter of the study.

of polynomials to only those above the intercept that are significant at the highest order ($p<0.05$). Thus, the simpler model is selected to investigate the research hypotheses regarding differential sanction effects across violent offending subpopulations.[33]

Assessment of Model Adequacy

Before interpreting the parameters and analyzing the trajectories themselves, an important procedure in LCGA is assessing the extent to which the selected model has accurately assigned individuals to specific latent classes. The LCGA model estimates a probability of belonging to each of the latent classes for every individual. That is, every individual receives three different estimated probabilities of group membership in which the total sums to one and individuals are assigned to a particular violent crime trajectory based upon "maximum probability" assignment. For example, consider a hypothetical individual with probabilities of belonging to latent classes one through three equal to 0.8, 0.125, 0.075, respectively. This individual would be assigned to be a group member in latent class one based on maximum probability assignment since he or she had an 80% chance of belonging to that group. Group mean posterior probabilities can be used as a measure of model adequacy; Table 8-3 contains the mean posterior probabilities for group assignment to the three violent crime trajectories (column) by the assigned group based on maximum probability assignment (row). For individuals who were actually assigned to class one, their mean posterior probabilities of belonging to classes one through three are 0.891, 0.000, and 0.109, respectively (see Table 8-3). It is to be expected that the highest average posterior probabilities for group assignment are for the groups to which individuals were actually assigned. This is the case here. In an

[33] While this optional model refinement stage was undertaken, it is important to note that it did not have any influence on the proportions of individuals falling within each latent class.

ideal world, values of one would grace the diagonal of the table signifying that individuals could be perfectly classified into distinct trajectories. In practice, however, the ideal will not be achieved; the conventional cutoff for average posterior probabilities for group assignment across all groups has been set to 0.7 (Nagin, 2005).

Focusing on the diagonal of Table 8-3, all of the three violent crime trajectory groups have values that exceed 0.7. In fact, each of these values is at least above 0.8 indicating good model precision. Other statistics also suggest that individuals have indeed been assigned to violent crime trajectory groups with adequate precision. Table 8-4 contains values of the quantity π_j minus P_j, which provide information on the concordance of the proportion of individuals belonging to the latent classes based on estimated model (π_j) and the proportion of individuals belonging to them based on maximum posterior probability assignment rule (P_j). This is a commonsensical comparison of the adequacy of a LCGA model. If individuals were assigned to groups with absolute certainty, the difference between these values would be zero. Importantly, this number will be small when there is low assignment error. Overall, there appears to be acceptable degrees of concordance between π_j and P_j for each of the three latent classes. Specifically, the proportion assigned to classes one and two is slightly smaller whereas the proportion assigned to latent class three is somewhat larger.

Table 8-4 also reports the odds of correct classification (OCC_j), which offers another way of assessing the adequacy of trajectory group assignments. OCC_j values that are larger signify better accuracy in class assignment. As a conventional cutoff, values exceeding five have been suggested to indicate high assignment accuracy (see

Nagin, 2005, p. 89). For all violent crime trajectory groups, the OCC_j exceeds this conventional cutoff also indicating good model adequacy.[34] With the number of latent classes determined, the order of polynomials necessary to capture within-individual change in violence across the trajectory groups specified, and the demonstration that the parsimonious three class Poisson LCGA model is adequately precise in its classification of individuals into subpopulations, attention is now turned to describing the relevant features of the estimated violent crime trajectories for the three distinct groups.

Trajectory Group Characteristics

Figure 8-1 plots the model estimated expected counts of distinct violent acts for the total sample. The total sample average trajectory is slightly declining in a linear fashion from waves two through six. While this graphic of a simple growth curve model informs the average trend of the aggregated sample, it was noted earlier that several model selection criteria suggested meaningful subpopulations exist in the data; specifically, the strongest evidence supported three distinct violent crime trajectory groups.

Figure 8-2 plots the model estimated expected counts of distinct violent acts for the trajectory groups across waves two through six, permitting a concise graphical overview of how violent crime unfolds over time for the different trajectory groups and aiding in the assignment of meaningful group names. From this point forward, latent classes one through three will be referred to as high offenders, non-offenders, and low offenders, respectively. An initial observation is that there is evidence of between-individual stability in offending behavior over time. Across all of the waves, non-

[34] The OCC_j is calculated as follows: $[\mu_{pp} / (1 - \mu_{pp})] / [\pi_j / (1 - \pi_j)]$.

offenders have the lowest expected counts and high-chronic offenders have the highest expected counts. The violent crime trajectory of the low offenders is also sandwiched between high offenders and non-offenders across all waves of data.

With respect to within-individual stability in violent crime over time, the trajectory groups are marked with a mixture of within-individual stability and within-individual change. One class has strong stability in the lack of violent criminal behavior. Non-offenders consistently have essentially zero expected counts of offending across all waves. The low trajectory group has an expected count of approximately 0.8 at wave two but by wave six the expected count drops to about 0.4. This decline does not occur in a linear fashion. Specifically, the expected counts for the low offending group drop during the first couple of time points and then stabilize. The high offending group exhibits the highest expected counts across all waves but its offending trajectory follows an inverted parabolic shape. That is, the level of violence of this group rises slightly from wave two to wave three, peaks at approximately 1.8 acts of violence during wave three, and then decreases to approximately 1.2 acts by wave six.

Table 8-5 summarizes the total number of individuals and proportion of individuals in each violent crime trajectory group. Just under half of the sample is classified as low offenders and nearly forty percent are classified as non-offenders. The high trajectory group that emerged from the data is smaller, comprising approximately eleven percent of the sample. Importantly, each subpopulation identified by the LCGA procedure contains a fairly sizeable proportion of the sample which is a necessary prerequisite to investigating treatment effects of official intervention across distinct violent crime trajectories. At the same time, the high trajectory group only has 68 males in it leading

to some concern over the size of this group. Recall, that matching within trajectory groups becomes more difficult for smaller subpopulations (Haviland et al., 2007).

Supplementing the graphical display of the violent crime trajectories (see Figure 8-2), Table 8-6 contains the parameter estimates, standard errors, t-statistics, and p-values for the intercept and growth factors for the three latent classes. Non-offenders (class two) have their trajectory characterized by an intercept only. High and low offenders (classes one and three) have their trajectories captured with an intercept as well as both linear and quadratic terms. For all groups, the intercepts are statistically significant. This parameter estimate is not especially important as it only indicates whether the expected counts of violent acts during wave two is significantly different from one. More meaningful are the size and direction of statistically significant parameter estimates for the linear and quadratic terms which were needed to describe within-individual change in violent offending behavior for the high and low offending groups. High offenders have a statistically significant linear term that is positive which indicates that the log of the expected counts of violent crime increases by 0.175 ($p<0.01$). Of course, this growth is not linear given the significant quadratic term which is negative ($p<0.01$). The low group has a statistically significant linear term that is negative indicating the log of the expected counts of violent crime decreases 0.415 ($p<0.01$). This decline is also not linear, however, as indicated by the positive quadratic term for low offenders in this case ($p<0.01$). In short, the sizes and directions of the linear and quadratic terms for the high and low offending trajectories are responsible for their observed inverse parabolic and j-shaped curves (see Figure 8-2).

In sum, a parsimonious three class Poisson LCGA model adequately captures the data. The trajectories that emerged from the data provide some important differences with respect to between-individual and within-individual stability and change in violence over time. And, these distinct patterns of offending that were identified through empirical means will prove useful in investigating the extent to which official sanctions make matters better or worse for individuals following along distinct offending trajectories. Before assessing official intervention effects on subsequent violent behavior, however, trajectory group-specific descriptive statistics are reported.

Trajectory Group-specific Descriptive Statistics

Chapter 6 reported the descriptive statistics for the aggregated sample. This section reports descriptive statistics for each of the three violent offending trajectories. In addition, one-way ANOVAs are estimated which assess whether there are significant differences between the means of the forty covariates across the distinct trajectory groups. It is important to point out here that many of the covariates are measured at wave six, which is during the final wave of the violent crime trajectory. Therefore, any significant differences in covariate means across the trajectory groups should not be construed as “risk factors” for trajectory group membership. This is especially the case with respect to time-varying variables, which comprise the overwhelming majority of the covariates. Moreover, it is probably best not to think of these covariates as outcomes of one’s violent crime trajectory. Rather than focusing on a causal connection, it might be preferable to view these associations as supplementing general descriptions of the trajectory groups.

Table 8-7 contains the means for the covariates measured at the interval level of measurement and the means for binary covariates across the three trajectory groups.

In addition, the table contains results of an overall F-test for mean differences and Tukey's statistical tests for specific group comparisons.

There are a couple of notable differences between the groups with respect to demographics. The high offenders (class one) have a greater proportion of Blacks and are older on average than the other two classes. High offenders are comprised of 75% Blacks are have an average age of 16.76. The three trajectory groups have similar levels of Hispanics and there are also no statistically significant differences in family poverty levels.

There is only one neighborhood characteristic that is significantly related to trajectory group membership. Those individuals in the low offending group have a significantly higher percentage of African Americans residing in their neighborhood (55%) than non-offenders (49%). However, the difference between high offenders (57%) and non-offenders does not reach statistical significance—even though high offenders have the highest mean percentage of African Americans residing in the neighborhood. The rest of the covariates are not significantly different across the trajectory groups but many are, at least, in a direction that would be consistent with theoretical expectations.

Unlike neighborhood characteristics, all six family factors are significantly related to trajectory group membership. Non-offenders (40%) are significantly more likely than both low (30%) and high (24%) offenders to live with both parents. Non-offenders also tend to experience greater levels of parental supervision and their primary caregivers have higher levels of attachment to them than both low and high offenders. Non-offenders are more likely to be attached to their primary caregiver and to receive

consistent discipline than low offenders only. Finally, primary caregivers' educational expectations for their high-offending child are significantly lower than those of both non-offending and low-offending juveniles.

Four out of five school factors have significant F-statistics indicating that these variables too are associated with trajectory group membership on the whole. However, only three of the five Tukey tests reveal significant mean differences between specific groups. Of these three factors, non-offenders exhibit significantly more commitment to school compared to low and high offenders and they tend to engage in more school clubs with friends than do low offenders. Non-offenders also have higher aspirations for college than do low and high offenders. Relatedly, low offenders also have significantly higher aspirations for college than do high offenders.

All of the peer influence covariates are significantly and strongly related to trajectory group membership. With the exception of the variables in the prior criminal behavior subcategory, upon which the trajectory groups are based, the peer influence variables tend to have the largest F-statistics and have means that are the most noticeably different across the trajectory groups. Six of the seven variables are such that all group means are significantly different from one another. High offenders have greater peer delinquency, higher peer drug use, greater peer delinquent values, more involvement with dating, have higher amounts of risky time with friends, and more gang involvement than low and non-offenders; and, low offenders have greater levels of these variables as compared to non-offenders too. Compared to only 25% of non-offenders, 43% of high offending males, and 40% and low offending males have

engaged in sexual intercourse before the age of fifteen. These differences between the two offending groups and the one non-offending group are statistically significant.

Consistent with literature on the relationship between depression and delinquent values and offending behavior, trajectory group membership is related to these variables. High offenders have greater delinquent values than both low and non-offenders. Relatedly, low offenders have higher delinquent values than non-offenders. Non-offenders exhibit fewer depressive symptoms than both low and high offenders. There were no significant differences in the self-image means across the three latent classes.

It goes without saying that there are large and significant differences in the means of violent offending behavior across the three distinct trajectory groups. More relevant to our discussion is the fact that general delinquency and drug use exhibit a similar relationship to trajectory group membership. That is, high violent offenders engage in more general delinquency and more drug use than low and non-offenders and, also, low violent offenders significantly commit more of these types of behaviors compared to non-offenders. On average, low and high violent offenders have similar levels of aggression and these two groups both have significantly higher levels of aggression than do non-offenders. Similarly, these two groups have experienced higher levels of prior justice system contact in the form of an arrest or police contact as compared to non-offenders, which is not entirely surprising.

Trajectory Group-specific Intervention Experiences and Outcomes

Recall, about 22% of the sample experienced official intervention in the form of an actual arrest during either or both waves six and seven as measured by official police records in Rochester (n=131). This is a fairly sizeable proportion of the sample

permitting a general assessment of the effects of official intervention on subsequent violent offending behavior for the male sample as a whole. What is more pertinent to the current discussion is whether there is a sufficient number of individuals who experienced official intervention across each of the three violent crime trajectories. If a trajectory group has only a few individuals who experienced an official intervention then it may be impossible to estimate a treatment effect of official intervention on subsequent violent offending for that group with the current data. Fortunately, this does not appear to be the case. Figure 8-3 displays the number of individuals arrested and not arrested across each of the violent offending trajectory groups. All trajectory groups had a number of individuals who experienced an arrest during the period of interest. Not surprisingly, the high offenders had the greatest proportion of individuals having experienced an official intervention (41.2%) and the non-offenders had the lowest (14.5%). The proportion of low offenders experiencing official intervention lies in the middle at 23.5%.

Whereas approximately 22% of the sample experienced an arrest as measured by official data, 32% of the sample experienced a police contact during either or both waves six and seven as measured by self-report data (n=188). Figure 8-4 displays the self-reported official intervention experiences of individuals across each of the three violent crime trajectories. Mirroring the pattern of results of arrest, the self-report data on police contact reveals that non-offenders have the lowest proportion of individual experiencing official intervention (19.2%) followed in order by low offenders (37.2%) and high offenders (50.0%) Not surprisingly, the number of individuals experiencing official intervention in the form of a police contact is higher compared to arrest across all

trajectory groups; hence, arrests follow police contacts but not all police contacts result in arrests. In sum, there appears to be a sufficient number of individuals who experienced the treatment whether that is an arrest as recorded by official records or a police contact as documented through self-report (see Figures 8-3 and 8-4).

The proportion of individuals experiencing official intervention across the trajectory groups makes logical sense; that is, the more problematic the trajectory group, the greater proportion of individuals experiencing official intervention. Focusing on the efficacy of policing, it is reassuring to note that individuals belonging to more problematic violent crime trajectory groups are experiencing official interventions at the highest rates. However, focusing on the ramifications of official intervention for subsequent behavior across distinct violent offending trajectory groups may or may not support the justice system contact patterns that are observed herein.

Table 8-8 contains the violent crime means for three different trajectory groups. A general examination of the wave nine and wave ten mean outcome violent crime scores shows that between-individual stability in offending is largely preserved over both the short- and long-runs. That is, high offenders still have the highest violent crime mean in both outcome waves, followed in order by low offenders and non-offenders. Results from one-way ANOVA and Tukey's B tests reveal that these mean differences are all statistically significant in the short-run (wave nine), however, high offenders and low offenders are not significantly different from one another in the long-run (wave ten). These two groups do have significantly higher violent crime means than non-offenders in the long-run. With a very thorough description of the three violent offending trajectory groups, I now turn to investigating whether self-reported police contacts and/or officially

recorded arrests alleviate, exacerbate, or have no affect on the violent criminal behavior in the short- and long-runs for the aggregated sample and, most importantly, for each of the distinct offending trajectories.

Table 8-1. Model selection criteria for five different Poisson LCGA models estimating violent crime trajectories.

Latent Class	Log Likelihood	BIC	Prob. model correct
1	-2730.044	5479.254	0.000
2	-2387.141	4819.002	0.000
*3	-2342.381	4755.037	0.720
4	-2330.550	4756.928	0.280
5	-2328.991	4779,364	0.000

* indicates selected model.

Table 8-2. Model selection criteria for two alternative three class Poisson LCGA models with different orders of polynomials across the trajectory groups.

Order of Polynomials	Loglikelihood	BIC	Probability model correct
(2,2,2)	-2328.991	4779.364	0.000
*(2,0,2)	-2342.480	4742.457	1.000

* indicates selected model.

Table 8-3. Mean posterior probabilities for trajectory group assignment for the selected three-group Poisson LCGA model.

	Group		
Assigned Group	1	2	3
1	0.891	0.000	0.109
2	0.000	0.899	0.101
3	0.058	0.126	0.816

Table 8-4. Additional diagnostics for the selected three-group Poisson LCGA model.

Assigned Group	π_j	P_j	$\pi_j - P_j$	OCC_j
1	0.131	0.114	0.017	54.383
2	0.415	0.393	0.022	12.535
3	0.454	0.492	-0.038	5.331

Table 8-5. Total number and proportion of individuals within each violent crime trajectory group (N=595).

Assigned Group	N_j	$\%_j$
High (C1)	68	11.4
Non-offenders (C2)	234	39.3
Low (C3)	293	49.2

Table 8-6. Parameter estimates for the selected three class Poisson LCGA model.

Assigned Group	Parameter	Estimate	S.E.	t	p-value
High (C1)	Intercept	0.507	0.119	4.255	0.000
	Linear	0.175	0.083	2.114	0.035
	Quadratic	-0.064	0.021	-3.086	0.002
Non-offenders (C2)	Intercept	-3.472	0.422	-8.129	0.000
Low (C3)	Intercept	-0.296	0.102	-2.900	0.004
	Linear	-0.415	0,114	-3.646	0.000
	Quadratic	0.065	0.028	2.335	0.020

Table 8-7. Trajectory group-specific descriptive statistics and ANOVA results.

	High (C1) Mean	Non-offenders (C2) Mean	Low (C3) Mean	F-test	Tukey's B
Demographics					
Race					
Black	0.75	0.55	0.66	6.11**	C1>C2; C3>C2
Hispanic	0.16	0.20	0.17	0.38	None
Age	16.76	16.35	16.50	7.13**	C1>C2; C1>C3
Family poverty	0.24	0.28	0.33	1.48	None
Neighborhood characteristics					
Proportion African Amer.	0.57	0.49	0.55	4.94**	C3>C2
Proportion in poverty	0.33	0.31	0.34	1.50	None
Neighborhood disorganization	1.68	1.60	1.66	0.51	None
Neighborhood integration	2.02	2.21	2.22	2.37	None
Neighborhood satisfaction	2.81	2.93	2.86	1.07	None
Neighborhood arrest rate	4.25	3.78	4.13	2.69	None
Family					
Family structure	0.24	0.40	0.30	4.40*	C2>C3; C2>C1
Parental supervision	3.41	3.60	3.48	6.11**	C2>C3; C2>C1
Attachment to parent	3.46	3.46	3.32	7.92**	C2>C3
PC's edu. expectations of child	1.93	2.47	2.30	9.86**	C2>C1; C3>C1
Attachment to child	3.36	3.61	3.41	14.05**	C2>C3; C2>C1
Consistency in discipline	2.32	2.44	2.29	5.67**	C2>C3
School Factors					
Commitment to school	2.98	3.16	3.03	9.13**	C2>C3; C2>C1
Aspiration for college	2.97	3.49	3.30	10.53**	C2>C3>C1
Prosocial activities	1.60	1.75	1.63	2.72	None
Educational expectations	2.47	2.69	2.56	3.28*	None
School clubs with friends	1.16	1.32	1.17	5.08**	C2>C3
Peer Associations					
Peer delinquency	1.82	1.11	1.37	61.30**	C1>C3>C2

Table 8-7. Continued.

	High (C1) Mean	Non-offenders (C2) Mean	Low (C3) Mean	F-test	Tukey's B
Peer drug use	1.96	1.19	1.45	69.64**	C1>C3>C2
Peer delinquent values	1.64	1.17	1.34	38.56**	C1>C3>C2
Dating	0.75	0.33	0.50	22.01**	C1>C3>C2
Sexual activity under 15	0.43	0.25	0.40	7.74**	C1>C2; C3>C2
Risky time with friends	2.71	1.87	2.26	47.82**	C1>C3>C2
Gang involvement	0.88	0.04	0.31	144.99**	C1>C3>C2
Values					
Self-image	3.14	3.23	3.21	1.16	None
Depression	2.10	1.90	2.02	6.73**	C1>C2; C3>C2
Delinquent values	1.71	1.25	1.45	38.85**	C1>C3>C2
Prior criminal behavior					
Violent crime (Time 1)	1.74	0.00	0.75	164.75**	C1>C3>C2
Violent crime (Time 2)	2.09	0.00	0.49	302.30**	C1>C3>C2
Violent crime (Time 3)	2.06	0.00	0.47	310.45**	C1>C3>C2
Violent crime (Time 4)	1.61	0.00	0.38	228.24**	C1>C3>C2
Violent crime (Time 5)	1.38	0.00	0.41	139.35**	C1>C3>C2
General delinquency	4.07	0.32	1.60	93.74**	C1>C3>C2
Drug use	0.50	0.03	0.17	26.33**	C1>C3>C2
Aggression	0.47	0.26	0.46	22.36**	C1>C2; C3>C2
Prior Justice System Contact					
Previous contact or arrest	0.53	0.20	0.41	20.88**	C1>C2; C3>C2

* $p \leq 0.05$; ** $p \leq 0.01$

Table 8-8. Trajectory group-specific mean outcomes in the short-run and long-run.

	High (C1) Mean	Non-offenders (C2) Mean	Low (C3) Mean	F-test	Tukey's B
Short-run (wave 9)	0.47	0.04	0.28	19.46**	C1>C3>C2
Long-run (wave 10)	0.40	0.16	0.31	5.50**	C1>C2; C3>C2

* p≤0.05; **p≤0.01

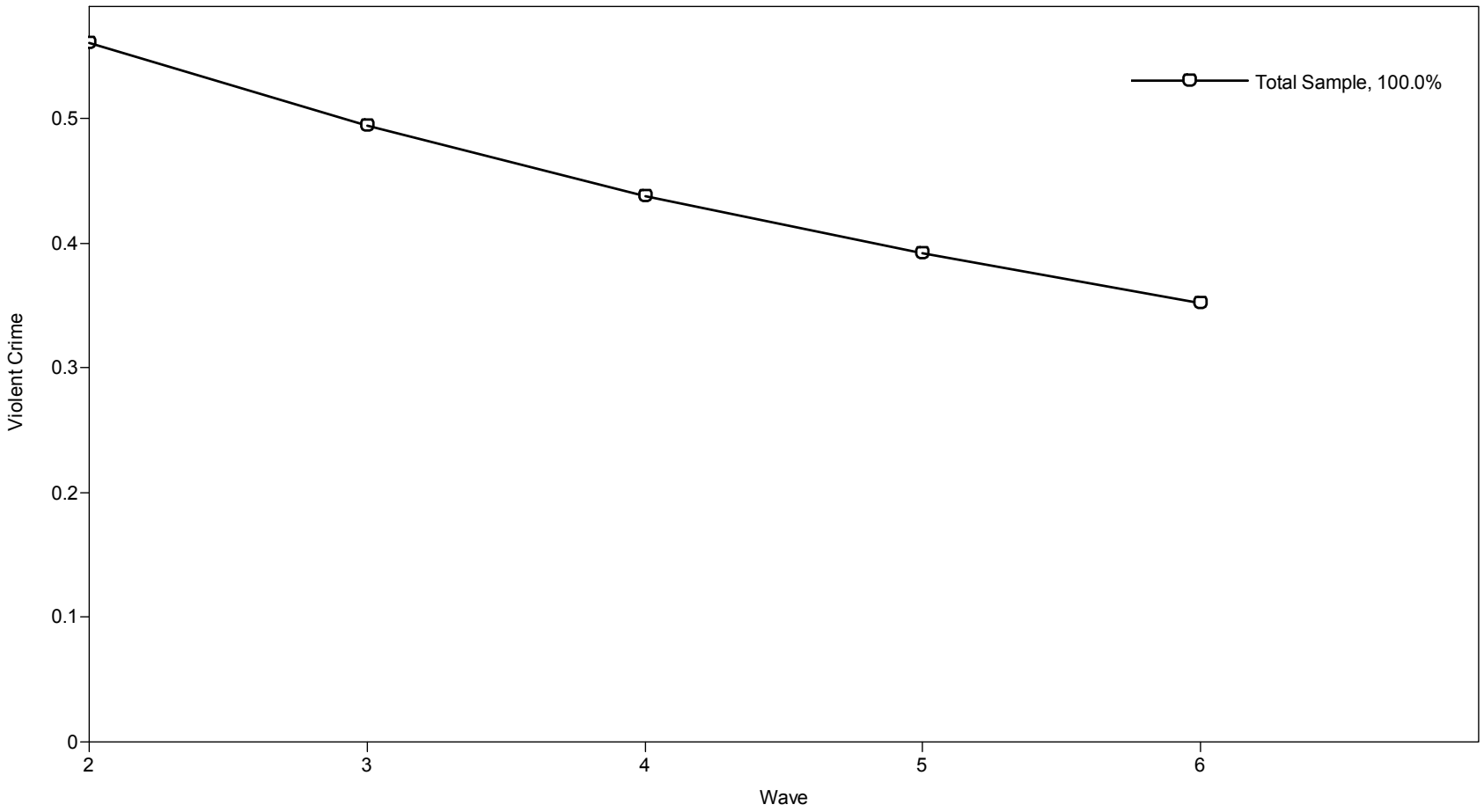

Figure 8-1. Estimated trajectory of violent offending during adolescence for the total sample (c=1).

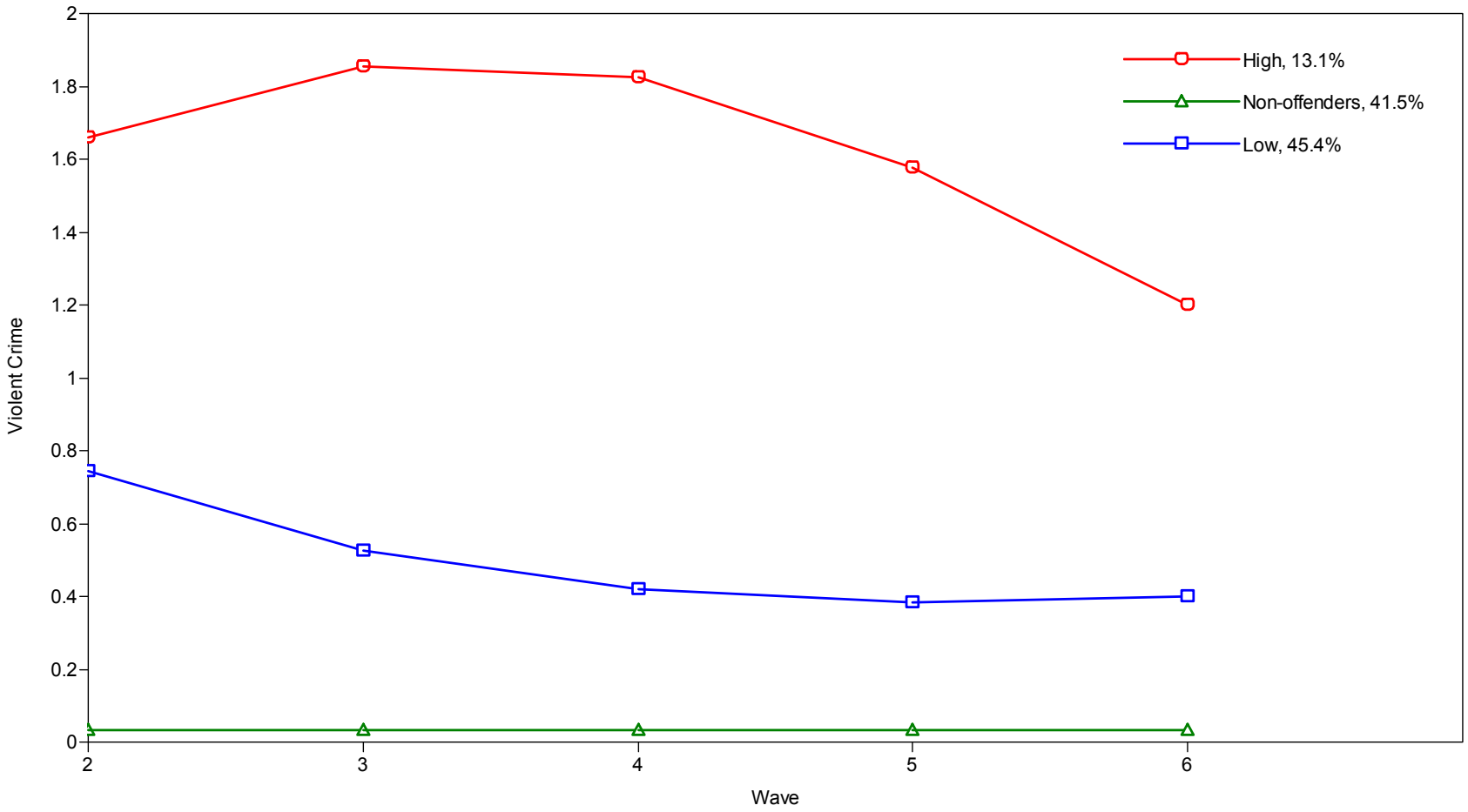

Figure 8-2. Estimated trajectories of violent offending during adolescence for the three latent classes (c=3).

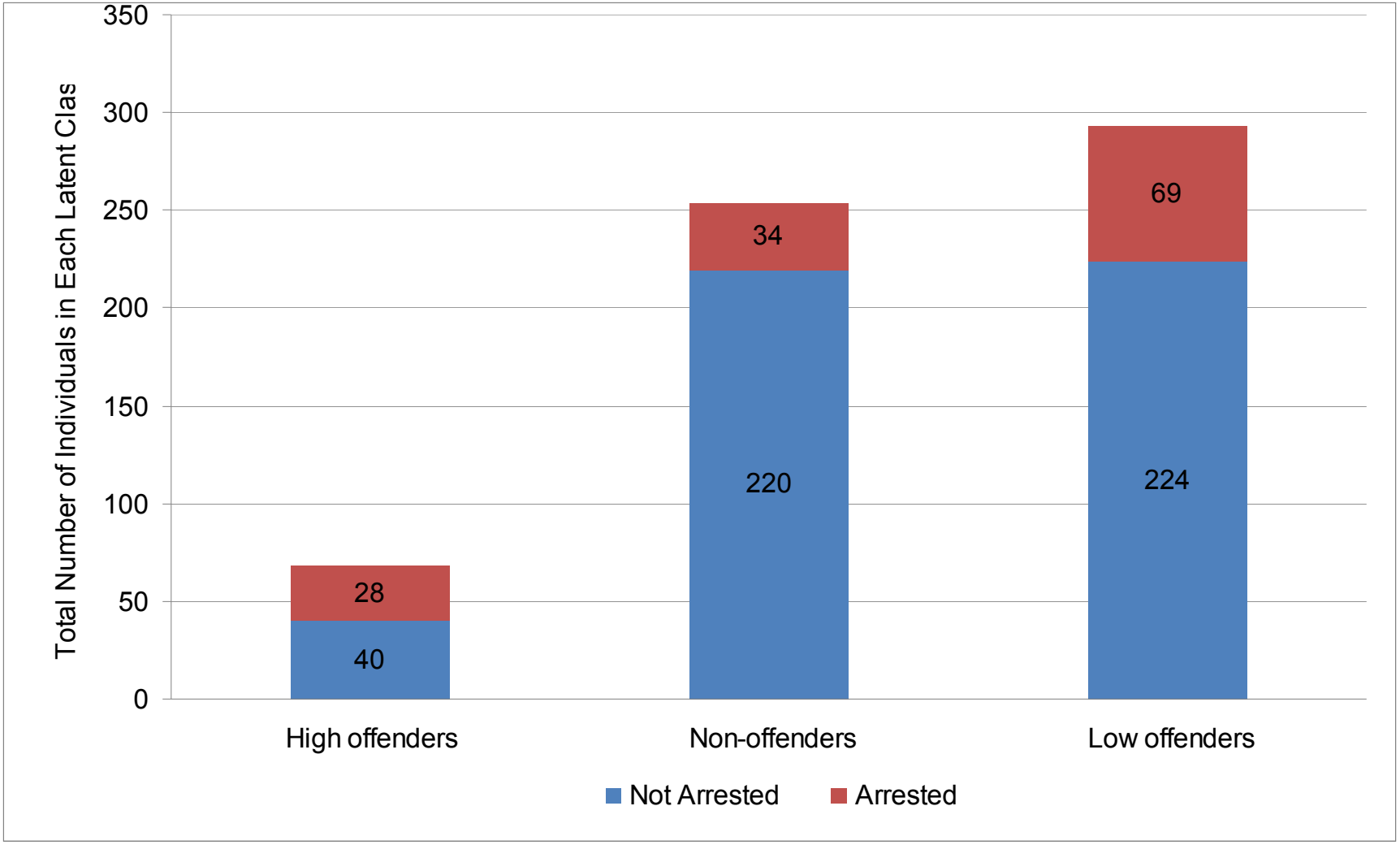

Figure 8-3. Number of individuals experiencing arrest in waves seven and/or eight across the three violent crime trajectory groups.

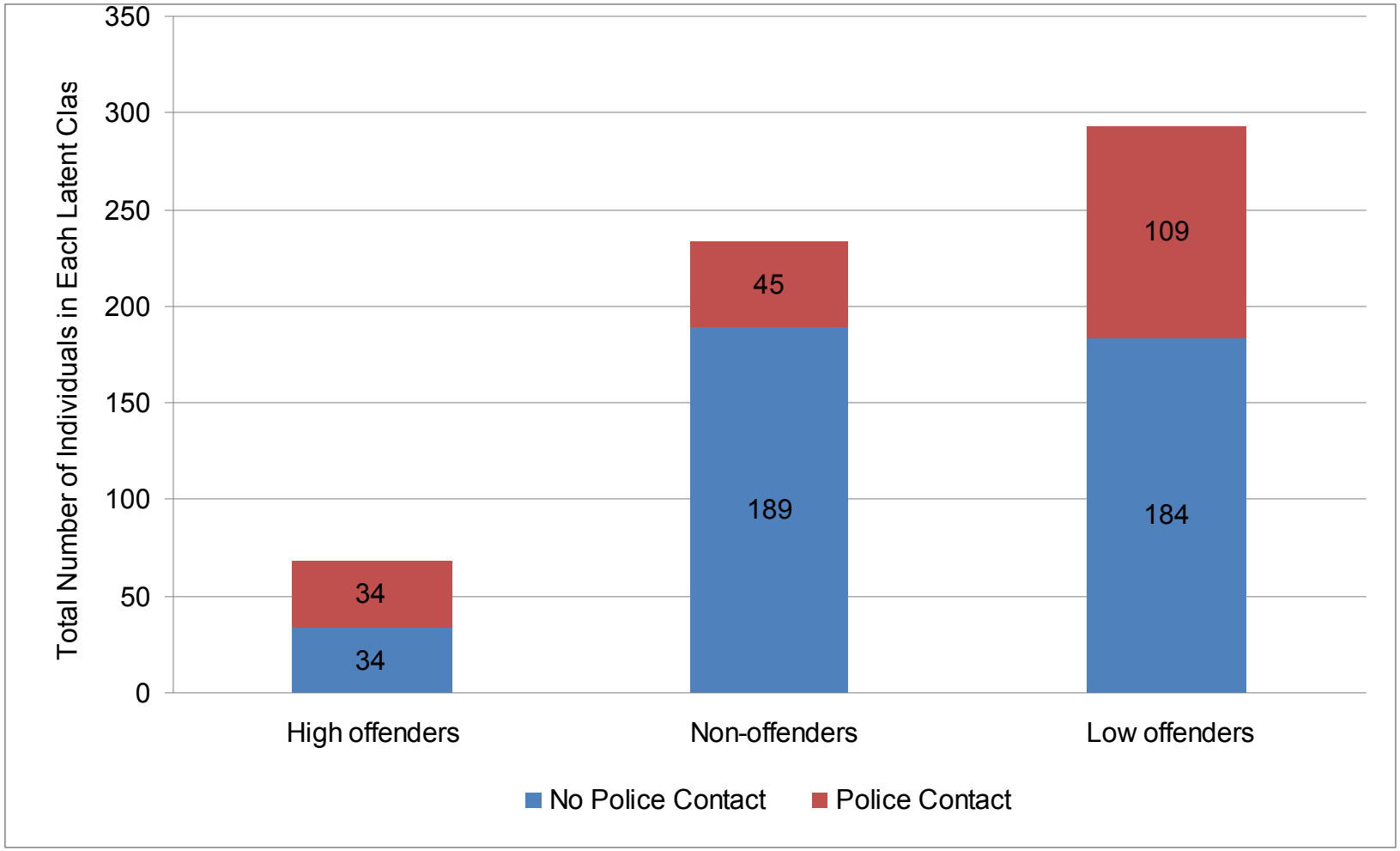

Figure 8-4. Number of individuals experiencing police contact in waves seven and/or eight across the three violent crime trajectory groups.

CHAPTER 9
RESULTS: SHORT-TERM AND LONG-TERM SANCTIONS EFFECTS

Chapter 9 reports the main findings of the study, which include the extent to which official intervention effects vary across distinct violent crime trajectories and whether any deterrent or labeling effects differ in the short- and long-runs. First, bivariate estimates of the relationship between official intervention and subsequent violent offending are reported for the aggregated sample and the three violent crime trajectory groups. Next, the results of the propensity score matching procedures are reported demonstrating the successfulness of the technique for creating covariate balance on potentially important confounder variables for the aggregated sample and, of course, for each of the trajectory groups. These results are reported for both official intervention treatments. Finally, the effects of the two treatments on subsequent violent offending are estimated with the propensity score matched samples to assess whether deterrent or labeling effects exist for the aggregated sample and for one or more violent offending trajectory groups specifically. These models are estimated with both short-run and long-run outcomes to ascertain whether any total sample effects and/or trajectory group-specific effects that are free from bias differ over time.

Bivariate Relationships between Official Intervention and Violence

This section examines the extent to which official intervention experiences are associated with violent behavior without controlling for any covariates. Should a statistical relationship between official intervention and violent behavior fail to exist at all, it might be unnecessary to employ propensity score matching to attempt to create covariate balance across forty different covariates with the ultimate goal of estimating

an unbiased treatment effect.[35] In other words, a first-order issue is whether the independent and dependent variables of interest are related to one another at the bivariate level.

Bivariate Relationships in the Short-run

The bivariate effects of official intervention on violent behavior are estimated using the Wilcoxon-Mann-Whitney test, which assesses differences between two groups when the dependent variable is not both measured at the interval level and normally distributed. Recall, the violent crime outcome variable is measured at the interval level but is a count of the variety of violent crime acts that an individual engages in during a specific time period. Common of count outcomes employed in criminological research, the variable is positively skewed and hence not normally distributed. With that said, the bivariate relationship between arrest and violent behavior in the short-run is first examined.

Figure 9-1 illustrates the mean differences between those experiencing an arrest and those not experiencing an arrest for the total sample and across each of the three trajectory groups. In addition, the figure contains the main conclusions (p-values) of the Wilcoxon-Mann-Whitney tests for statistical significance (see Table 9-1 for details). Focus first on the total sample which is situated at the left side of the table. The average violent crime score in the short-run for the males that were not arrested during the treatment period is 0.162 whereas the mean for those who were arrested is over twice as high, with an average value of 0.374. This total sample mean difference in violent crime during wave nine is 0.212 and is statistically significant ($p<0.001$).

[35] This, of course, is under the assumption that there is no reason to suspect a suppression effect.

Before reporting trajectory group-specific results, it is worth mentioning that while the statistical test reported herein is referred to as a “bivariate” statistical test it is not exactly a bivariate test. Recall, the latent class growth analysis disaggregated the sample into violent offending trajectory groups and the LCGA statistical model, by definition, does not permit within-class variation in the intercepts or growth factors. Therefore, the bivariate relationship between arrest and violent behavior within a trajectory group is better conceptualized as a partial relationship between these two variables with prior violent behavior as the lone control variable. Given prior violent offending behavior is likely among the most important control variables, if not the most important, it may not be surprising to find non-significance for some of these “bivariate” relationships—even without controlling for a host of other relevant variables.

With that said, focus now on the three trajectory group-specific bivariate findings. As an initial observation, the relationships between arrest and subsequent violent offending behavior in the short-run across the three trajectory groups are in a direction consistent with the labeling perspective. However, only the mean difference for the low offending trajectory group is statistically significant (see also Table 9-1). The wave nine violent crime mean difference between those arrested and those not arrested for the low offending trajectory group equals 0.165 ($p \leq 0.05$). While the mean difference between high offenders who experienced an arrest and those who did not is 0.293, which is the highest of any group, there is no evidence of a statistically significant bivariate relationship between arrest and violent crime in the short-run ($p > 0.10$). The mean difference for the non-offending trajectory group is non-significant and equals 0.053, which was the lowest of the three groups ($p > 0.10$). Interestingly, the average violent

crime score for those individuals who were on a low offending trajectory is actually higher than it is for those individuals who were following along a high offending trajectory but were not subject to the official intervention of arrest.

Figure 9-2 reports the short-run bivariate results for the self-reported police contact official intervention. Unlike the arrest results for wave nine, the mean differences are all noticeably larger and, importantly, statistically significant in this case (see also Table 9-2). For the sample on the whole, the mean for individuals experiencing a police contact during the treatment period is about 3 ½ times higher than for individuals who did not experience a police contact during this same time period. That is, the mean difference is 0.302 and is statistically significant ($p \leq 0.01$). Focusing on the trajectory group-specific analyses, police contacts are significantly related to violent crime in the short-run for all three of the latent classes. On average, high offenders who experience a police contact commit 0.412 times more types of violent crimes than their counterparts ($p \leq 0.05$). While the mean difference for non-offenders is again the smallest of the three groups, it is equal to 0.084 and is statistically significant ($p \leq 0.05$). For the low offending trajectory group, the average outcome difference for males who were contacted by the police and those who were not equals 0.285 ($p \leq 0.01$).

In sum, the total sample analysis indicates there are statistically significant mean differences in wave nine violent crime for both the arrest and police contact treatments. In the short-run, there is consistent empirical support for labeling theory across the different violent offending trajectories specifically for the police contact treatment. The arrest analyses showed trends that were in a direction consistent with labeling theory, but only the low offending trajectory group had a short-run outcome that was

significantly different from zero. A very interesting observation that occurred across the trajectory groups for both official intervention treatments that has perplexing implications is worth noting here. Specifically, the mean differences were consistently largest for high offenders, second largest for low offenders, and smallest for non-offenders. Putting aside for the moment that these findings are bivariate relationships, these patterns raise the possibility that labeling may be somewhat more pronounced precisely for those individuals who should, by definition, have the most justice contact experiences. These issues are further explored in the discussion. The focus now is on whether there are bivariate relationships between official intervention experiences and violent crime several years after the justice system contact experiences.

Bivariate Relationships in the Long-run

Sampson and Laub (1997) raise the possibility that short-term sanction effects may vary from longer-term ones. Somewhat unexpectedly, only low offenders who were arrested had worse outcomes compared to their counterparts but individuals who experienced a police contact were more likely to have higher violent criminal behavior than their counterparts at wave nine regardless of which behavioral trajectory they belonged to. Examination of outcomes at wave ten permit an assessment of whether those who experienced labeling effects at wave nine continue to experience these effects at wave ten or whether they are even further pronounced once labeling has had additional time to run its course. In addition, it permits one to assess whether delayed labeling effects may emerge for the arrest treatment and, while not hypothesized, whether evidence of deterrence exists in the long-run.

Figure 9-3 illustrates the mean differences between individuals experiencing an arrest and those not experiencing an arrest for the total sample as well as the distinct

violent offending trajectory groups. None of these bivariate relationships are statistically significant and all are quite small (see also Table 9-3). Mean differences for the total sample as well as for high offenders and non-offenders are in a direction consistent with labeling but are not significant ($p>0.10$). The mean difference for the low offending group is in a direction consistent with deterrence but is not significant ($p>0.10$). Thus, the significant bivariate relationships found at wave nine for the total sample and the low offending group disappear by wave ten.

Figure 9-4 contains a summary of the relationship between police contact during waves seven and eight and violent crime at wave ten. Overall, individuals experiencing a police contact commit more types of violence compared to those not experiencing a police contact ($p\leq0.01$). With respect to the trajectory group-specific analyses, only low offenders experiencing a police contact had significantly worse outcomes than their trajectory group counterparts; the significant mean difference for this group equals 0.192 ($p\leq0.01$). The mean differences for the high offending and non-offending groups are 0.147 and 0.047, respectively, but these are not statistically significant ($p>0.10$). The wave ten mean differences for the high offending, non-offending, and low offending trajectory groups correspond to approximately 47%, 56%, and 52% of their wave ten values, respectively. In short, while the short-run violence outcome patterns for the police contact treatment are similar, labeling effects are less strong in wave ten than they are in wave nine. Moreover, significance tests suggest that short-term labeling effects may persist over the long haul only for low offenders.

Covariate Balance Assessments of Treatment Models

While the previous analysis was informative, it did not account for a number of variables (other than, to a certain extent, prior behavior) that could potentially confound

the relationship between official intervention and violent behavior in the short- and long-runs. This section takes on the task of creating equivalent groups on measured covariates through propensity score matching for the sample as a whole and for each of the three violent offending trajectories across both treatments of official intervention. To begin, covariate means are estimated for those experiencing official intervention and those not experiencing it and standardized bias statistics are computed. This analysis is carried out across each of the five MI data sets for the aggregated sample as well as for the three violent offending subpopulations. MI averages of covariate means and standardized bias statistics are highlighted in the Tables 9-5 through 9-12.[36] This initial assessment of the unmatched samples shows which covariates are "out of balance" and demonstrates the need for a statistical remedy to the problem, which in this case is propensity score matching analysis.

Using a logistic regression model, conditional probabilities (i.e., propensity scores) of receiving official intervention were estimated using the forty covariates as predictors. This was repeated for each of the MI data sets for the total sample and for each trajectory group. As outlined in the methodology section, the propensity scores are then used in the optimal full matching with restrictions routine in 'R' (see Hansen, 2004). Mean differences and standardized bias statistics are recalculated after matching for each MI data set. The right side of Tables 9-5 through 9-12 contains the MI average

[36] Pre-matching covariate means and standardized bias statistics for variables with no missing information are similar across the imputed data sets. Only those variables that have imputed data have differences across the five MI data sets. Post-matching, all covariates may have differences across the MI data sets (even those with no missing information) since the estimated propensity scores will differ by some amount given they are estimated with slightly different data sets due to the random component that is added to imputed values during the multiple imputation procedure. Overall, any pre-matching and all post-matching differences are relatively small across the different imputed data sets and the averages of the MI data sets well represent the covariate means and standardized bias statistics. Details on covariate balance for each of the individual MI data sets for all analyses reported herein are available upon request.

standardized bias statistics after matching and percentage of bias reduction statistics. With this information, the ability of the estimated propensity scores to create quasi-equivalent treatment and control groups is discussed.

Collectively, these steps demonstrate the extent to which the matching procedure can produce unbiased treatment effect estimates, under both the strongly ignorable treatment assignment and stable unit treatment value assumptions. Unfortunately, there is no guarantee that propensity score methods will be able to create covariate balance on all variables in general or for every trajectory group specifically (Haviland & Nagin, 2005; Haviland et al., 2007; 2008). But, the benefit of knowing this information upfront is a boon as the researcher can state for which trajectory groups a particular data set can support causal inference and for which it cannot. Traditional statistical approaches such as regression analysis do not yield this information in such a transparent fashion.[37]

Treatment Model for Arrest

Total sample

Table 9-5 contains the covariate means and standardized bias statistics for the aggregated sample both pre- and post-matching for the arrest treatment condition. Before matching on propensity scores, 28 of the original covariates are significantly out of balance (i.e., d_X>0.20) and five other covariates have standardized bias statistics that are greater than ten. This degree of imbalance pre-matching should come as no

[37] Regression modeling requires due diligence in ensuring the appropriateness of employing certain control variables. As an example, consider a data set in which all of the individuals who experience an arrest cluster very high on a variable measuring risky time with friends whereas all those not experiencing an arrest cluster on the very low end of this measure. Regression analysis will blindly "control" for risky time spent with friends even though this variable in this case could not really be held constant in a meaningful sense.

surprise given our reliance upon observational data. Approximately 74% of the males that were arrested were Black compared to 59% of those individuals who were not arrested. However, the treatment and non-treatment groups were 19% and 18% Hispanic, respectively. Correspondingly, one notices that the standardized bias statistics indicate that Blacks are significantly out of balance (d_{Black}=31.74) while Hispanics have very good balance ($d_{Hispanic}$=3.65). Age is the covariate that has the second highest standardized bias statistic (d_{Age}=77.71) indicating that the covariate means are out of balance pre-matching by nearly 80% of a standard deviation. At the same time, age is a covariate that provides a good example of the need to consider practical differences in means in addition to balance statistics. For instance, even though this covariate is highly out of balance, the differences in the average ages are not especially large in a practical sense. Those experiencing an arrest tend to be just under 17 whereas those not experiencing an arrest are about 16 and 4 months on average.

Individuals who are arrested during waves seven and/or eight tend to reside in neighborhoods that have a greater portion of their residents in poverty and of African American racial background. Not surprisingly, the arrest group has a higher average neighborhood arrest rate. While these covariates are out of balance according to conventional criteria, the neighborhood integration, satisfaction, and disorganization measures have acceptable levels of pre-matching balance. Four of the six family factors are significantly out of balance pre-matching. There are non-trivial differences in the covariate means of the parental supervision, primary caregiver's educational

expectations of child, and attachment to child covariates as well as important differences in the proportion of individuals living with both parents.

Four out of five of the school factors are significantly out of balance and the commitment to school covariate is just shy of also exceeding the conventional cutoff. The means for aspirations for college, involvement in prosocial activities, educational expectations and involvement in school clubs with friends are higher by an amount that suggests these covariates are substantially out of balance, which could bias any estimate of the effect of arrest on subsequent violent crime. The peer association cluster of variables also has all but one of the covariates significantly out of balance. The mean differences in peer delinquency, peer drug use, peer delinquent values, and risky time spent with friends are such that these covariate means are larger for the arrest group than the group of potential controls. In addition, the proportion of individuals who are actively dating and have been involved in a gang is substantially greater for the arrest group.

With respect to values and mental states, only the delinquent values covariate is significantly imbalanced. The covariate means for self-image and depression have very good balance pre-matching. As is to be expected, prior delinquent behavior is substantially imbalanced across the two conditions such that those who were arrested have higher violent crime covariate means across all time points than those who were not arrested. In addition, individuals who were arrested tend to have higher levels of general delinquency and aggression. They also tend to have higher levels of drug use but this covariate just missed the conventional cutoff ($d_{DrugUse}$=19.54); still, balance could be substantially improved for this covariate too. Consistent with the idea that the

best predictor of future behavior is past behavior, the covariate tapping prior justice system contact is out of balance by over 93% of a standard deviation. This was the variable that was most out of balance pre-matching.

The average covariate is out of balance by nearly 30% of a standard deviation and the logit of the propensity score is imbalanced by nearly 1.5 standard deviations, which is quite large. There were covariates in each sub-area that were significantly out of balance suggesting it is important for sanction effects researchers to account for various things such as demographics, prior behavior, and school factors. In short, the treatment and potential control groups for the sample as a whole are different in key respects meaning that the "bivariate" estimates reported earlier are biased estimates of the influence of arrest on subsequent violent behavior.

In practice, it would be considered a great success if all of the standardized bias statistics were less than 10 post-matching. At the least, one hopes to see each of these values less than the standard cutoff for acceptable covariate balance, which is set to 20 by convention. Of course, the closer the statistic is to zero the better. After matching individuals on their propensity scores, all of the covariates have acceptable levels of balance. Moreover, the matching procedure has resulted in excellent balance for all but one of the covariates. Peer delinquency is the covariate that is most out of balance post-matching but this variable not only has an acceptable level of balance but fairly good balance ($d_{PeerDelinquency\text{-}m}$=10.65). In all but four covariates (family poverty, neighborhood disorganization, consistency in discipline, and depression), bias has been reduced and often by substantial amounts. For a typical covariate, 70% of the bias has been removed by matching and the median percentage bias reduction is equal to 87%.

Percentage bias reductions in the covariates topped out at approximately 98%. While bias actually worsened for four variables as noted by their negative %BR scores, the post-matching standardized bias statistics for these variables are still all below seven. The average covariate differs by less than 4.5% of a standard deviation post-matching. Moreover, the small mean differences that remain are far less systematically in a direction consistent with expectations. Given these facts, it is not surprising that the logit of the propensity score is essentially perfectly balanced following the optimal full matching procedure. Thus, the matching procedure for the arrest treatment for the aggregated sample was highly successful. I now assess whether this good fortune in post-matching covariate balance also occurs for the propensity score matching procedure within each trajectory group.

Trajectory groups

Tables 9-6 through 9-8 report the covariate means and standardized bias statistics for high offenders, non-offenders, and low offenders, respectively. Focusing first on high offenders, 22 covariates have standardized bias statistics greater than 20 and another 10 variables have values that exceed 10 (see Table 9-6). Family poverty is out of balance by nearly 33% of a standard deviation but the other demographic variables are not substantially out of balance. Five of the six neighborhood factors are imbalanced prior to matching. Those experiencing an arrest live in neighborhoods with a greater percentage of African Americans and families in poverty. These individuals also reside in neighborhoods that tend to have lower neighborhood satisfaction scores, higher arrest rates, and higher integration. With respect to family factors, there are non-trivial differences in the covariate means for the parental supervision, attachment to parent, primary caregiver's educational expectations for child, and constancy in

discipline covariates. Those individuals who experienced an arrest were involved in substantially lower prosocial activities and school clubs with friends. The differences between the two groups for the other school factors are not substantially out of balance but they could be improved. Two of the seven peer association factors are out of balance according to the conventional cutoff. Individuals who were arrested during the treatment period also had higher delinquent values than those not arrested by a difference of nearly 3/10 a standard deviation. Most of the prior criminal behavior covariates are imbalanced but not necessarily in directions that would be expected. The covariate with the greatest pre-matching mean difference is prior justice system contact. On average, the typical covariate is out of balance by approximately 25% of a standard deviation and the logit of the propensity score is imbalanced by a sizeable 3.15 standard deviations.

The right half of Table 9-6 demonstrates that a large number of covariates remain out of balance post-matching. Specifically, 15 covariate means differ by more than twenty percent of a standard deviation. While the average covariate is now out of balance by less 19% and the median covariate is now imbalanced by approximately 9%, the means of the logit of the propensity score are still different by more than three standard deviations. In short, the propensity score matching procedure has not produced two groups similar enough to support causal inference.[38] While this is

[38] Using a logistic regression model, conditional probabilities (i.e., propensity scores) of receiving official intervention were estimated using the forty covariates as predictors. It is important here to mention an unanticipated point of departure from the original analysis plan. The problem of complete separation of data points was detected. Complete separation of data points occurs when an outcome variable is perfectly predicted by a single variable or set of predictors. Quasi-complete separation of data points occurs when an outcome variable is nearly perfectly predicted by a variable or set of variables. The difference between the two is that in the latter case there remains at least one instance across a predictor variable or set of predictor variables for which both outcomes are observed. The practical implications of these problems for propensity score matching are substantial. When an individual's treatment

unfortunate, it is not entirely surprising given the prior research of Haviland and colleagues.

The non-offending trajectory group has a total of 17 covariates that are substantially imbalanced and another 9 that have less than optimal levels of bias before matching (see Table 9-7). The proportion of Blacks is substantially higher and the proportion of Hispanics is lower for low offenders who were arrested compared to those who were not arrested. Two of the six neighborhood factors including proportion African American and neighborhood arrest rate are imbalanced. Three of the seven family factors are imbalanced with the primary caregiver's educational expectations for child covariate being the most so. This variable is out of balance by slightly more than 0.5 standard deviations. The child's own educational expectations for himself is the only school factor that is not adequately balanced pre-matching. The covariate means for peer delinquency, peer delinquent values, and risky time spent with friends are larger by a non-trivial amount for low offenders who were arrested during the treatment period. Those not arrested exhibited higher levels of depressive symptoms and lower levels of delinquent values. Not surprisingly, the prior violent criminal behavior of non-offenders is not significantly different across the five time points and these means are essentially zero in all cases. The prior justice system contact variable has the greatest standardized bias statistic and is out of balance by nearly an entire standard deviation.

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assignment is perfectly predicted (i.e., they have a predicted probability of zero or one), a major assumption of the distribution of propensity score is violated. Without being able to obtain useable propensity scores, advanced quantitative matching methods are rendered useless.

The issue of complete or quasi-complete separation of data points is relatively uncommon in large samples; however, this problem can occur at a nontrivial rate when a large number of variables are used to predict a binary variable with a small sample size. In this study, forty covariates are being used to predict a binary official intervention treatment variable within trajectory groups of which some are relatively small. To address this problem, Firth's regression was used. Nevertheless, successful balance was unable to be obtained. Matching for this group using propensity scores estimated for the total sample also resulted in unsatisfactory balance achievement.

On average, covariate means and proportions are different by nearly 24% of a standard deviation and the logit of the propensity score is imbalanced by 1.83 standard deviations.

Following the optimal full matching procedure, all but one of the covariates has acceptable levels of bias. That is, only drug use has a standardized bias statistic that is greater than 20.[39] It is worth noting that the matching procedure actually worsened the bias of eleven of the forty covariates. However, this trade-off proved to be beneficial overall given a median bias reduction of 58% and a 96% bias reduction in the logit of the propensity score. A typical covariate is now imbalanced by less than 10% whereas it was imbalanced by nearly 24% prior to matching. While acceptable levels of balance were not achieved for the drug use variable, covariate balance was substantially improved for the non-offending group overall to a level that supports reasonably strong causal inference.

The low offending trajectory group is no exception to pre-matching covariate imbalances. Specifically, 20 of the covariates have standardized bias statistics that exceed the standard cutoff and another 10 variables have mean differences greater than 1/10 a standard deviation. The proportion of Hispanics and mean age differences between those arrested and not arrested are different by non-trivial amounts. With respect to out of balance neighborhood characteristics, the arrested group has a substantially higher percentage of its residents in poverty and a higher area arrest rate. Family structure, primary caregiver's educational expectations for child, and PC's

[39] Including drug use and the violent crime measures created quasi-complete separation of data points. To address this, these variables were excluded from the propensity score model. Firth's regression was attempted with these variables included but this resulted in other covariates being out of balance post-matching.

attachment to child are imbalanced by amounts that could confound the results linking violence and official intervention for this trajectory group. Three of the five school factors are out of balance with aspirations for college being the most problematic covariate in this subgroup. Five of the seven peer association covariates differ by more than 1/5 of a standard deviation. While the covariates of self-image, depression, and delinquent values are balanced pre-matching, most of the variables capturing prior delinquent behavior are out of balance. Finally, the proportion of low offenders experiencing prior justice system contact who were arrested is over twice the proportion of those not arrested, a value that separates the means by approximately 75% of a standard deviation. On average, covariates differ by more than one-quarter of a standard deviation and the logit of the propensity score is out of balance by over one and two-thirds of a standard deviation.

As the right part of Table 9-8 illustrates, the matching procedure has resulted in substantial reductions in bias. On average, covariates are out of balance by less than 6% of a standard deviation and no covariate is out of balance by more than 13% of a standard deviation. Only one covariate (consistency in discipline) experienced a bias increase. In addition to no variables exceeding the conventional cutoff, only two covariates (aspirations for college and educational expectations) exceeded a standardized bias statistic of 10. The median and mean bias reductions were 75% and 60%, respectively. The percentage bias reduction in the logit of the propensity score is 99%; these means now differ by only approximately 2% of a standard deviation. In short, the treatment and control groups have been matched with a high degree of success, which ultimately means that strong causal inference for the effects of arrest on

subsequent violence can be made from post-matching analysis conducted within the low offending trajectory group.

Across all trajectory groups, there are substantial differences in covariate means between those experiencing an arrest and those not experiencing it during waves seven and/or eight. This is not to say that all of the same variables are in balance or out of balance across the three trajectory groups. In fact, this is clearly not the case. Only neighborhood arrest rate, primary caregiver's educational expectations for child, drug use, and prior justice system contact are substantially imbalanced across all groups ($d_x \geq 20$). And, only neighborhood disorganization, commitment to school, and self-image are reasonably balanced pre-matching across the trajectory groups ($d_x < 20$). Of the remaining covariates, 17 are imbalanced across one violent offending trajectory group and 16 are imbalanced for two groups. These pre-matching differences in covariate balance are informative since they suggest potentially different selection mechanisms across divergent offending trajectories. Attention to pre-matching imbalances across latent classes can also identify variables that may be robustly related to official intervention across trajectory groups such as neighborhood arrest rate and drug use.

In sum, it has been shown that, prior to matching, some covariates have consistent imbalance across the trajectory groups and many others are imbalanced in some trajectory groups but not in others. Despite some important differences in which covariates are imbalanced across the latent classes, it can be safely concluded that each trajectory group has a large number of covariates that have substantial bias and a number of others that have less than optimal amounts of bias. Without removing these key differences in covariate means or proportions between those experiencing an arrest

and those not experiencing one across each of the trajectory groups, any estimates of the trajectory group-specific treatment effects would be untrustworthy—as is the case with the “bivariate” associations between official intervention and subsequent violent behavior. For the arrest treatment, the propensity score matching procedure created excellent bias for the total sample and for two latent classes permitting trajectory group-specific treatment effect estimates for non-offenders and low offenders. Unfortunately, however, the data do not support causal inference for high offenders.

Treatment Model for Police Contact

Total sample

Table 9-9 reports treatment and control group means and standardized bias statistics before and after matching for the police contact treatment for the total sample. Focusing on the pre-matching statistics, well over half of the variables exhibit substantial imbalance. In addition to these 24 imbalanced covariates, there are eight additional covariates that have standardized bias statistics greater than 10. Overall, the typical covariate is out of balance by approximately 26% of a standard deviation and the logit of the propensity score is imbalanced by more than 90% of a standard deviation.

There are some noticeable trends in covariate imbalances pre-matching among the subcategories of variables which give some insight into the types of covariates that pose the greatest validity threats. For instance, none of the means for the treated and potential control groups for the neighborhood variables are substantially out of balance (though covariate balance could be improved). Contrast this with prior behavior variables which all show substantial imbalances between the two groups. A number of the covariates in the family, school, peer, and values and mental states sub-areas are imbalanced in directions consistent with expectations suggesting the importance of

accounting for these variables in sanction effects research as well. Not surprisingly, those with a police contact during the treatment period experienced more prior justice system contact. In sum, the two groups are not equivalent with respect to a number of important variables that could confound the observed relationship between police contact and subsequent violent crime.

Before discussing the post-matching balance statistics, it is important to point out that there is a large degree of consistency in the factors that are imbalanced for the police contact and arrest treatment variables prior to matching (cf. Tables 9-5 and 9-9). There are, however, a couple of notable differences between the pre-matching imbalances. For one, Blacks are overrepresented among the arrest treatment group but this is clearly not the case for the police contact variable. There are also differences in the imbalances of race at the macro-level (proportion African American). Self-image seems to be imbalanced for the police contact treatment but not for the arrest treatment and there are a few other more minor differences throughout. By and large, however, there is a good deal of similarity between the variables that tend to be imbalanced between the two different official intervention measures.

Once the treatment and control groups are matched based upon their propensities to experience a police contact, every covariate exhibits not only acceptable balance but excellent balance. Specifically, the means of any covariate do not differ by more than 7.25% of a standard deviation. The bias reduction statistics indicate that all except four of the variables underwent a bias reduction as denoted by the positive %BR values. The average percentage bias reduction was 46% and the median percentage bias reduction was 86%. The average covariate is now imbalanced by less than 4% of a

standard deviation and, moreover, the group means of the logit of the propensity scores are essentially identical. The full optimal matching procedure has created two groups that are very comparable for the aggregate sample.

Trajectory groups

Tables 9-10 through 9-12 contain the pre- and post-matching covariate means and standardized bias statistics along with percentage bias reduction values for the high offenders, non-offenders, and low offenders, respectively. The high offenders have 25 covariates that have standardized bias statistics greater than 20 and an additional 10 covariates that have standardized bias statistics greater than 10 (see Table 9-10). Three of the four demographic factors are imbalanced and two of the six neighborhood factors are also out of balance prior to matching. Two-thirds of the family variables exhibit non-trivial differences in covariate means with parental supervision being the covariate most out of balance. Four out of five school factors are substantially imbalanced. Four of the seven peer influence variables are out of balance with the other three variables fairly close to the conventional cutoff. Self-image and delinquent values are two additional variables that have non-trivial differences in their covariate means. Most of the prior behavior covariates are imbalanced as is the prior justice system contact variable. The average covariate is out of balance by approximately 28% of a standard deviation. The means of the logit of the propensity score are different by nearly two standard deviations.

Following the matching procedure, the treatment and control groups are not sufficiently balanced. While the covariate imbalances of some variables have been reduced to a large degree or essentially entirely eliminated (e.g., aggression and Blacks), the imbalance for many other variables is far worse post-matching than it was

pre-matching. In several cases, these negative bias reductions are quite substantial taking variables from acceptable levels of imbalance pre-matching to unacceptable levels post-matching (e.g., attachment to parent, gang involvement, and depression). In fact, the average bias reduction value is -0.78 indicating a bias increase. The median bias reduction for a covariate is 24%. The differences in the logit of the propensity scores across the treated and control groups was reduced by 85% but this global summary balance measure is still out of balance by nearly 29% of a standard deviation. After matching, the average covariate differs by 22% of a standard deviation. Similar to the high offending group for the arrest treatment, the data cannot support good causal inference.

Non-offenders have 16 covariates that are out of balance but another 14 that have less than optimal levels of imbalance meaning that only ten covariates have excellent balance pre-matching. Age is the only demographic variable imbalanced and none of the neighborhood characteristic variables is different between groups. The covariate means for three of the six family covariates, three of the five school factors, and four of the seven peer influence variables differ by more than 1/5 of a standard deviation. General delinquency, aggression, and prior justice system contact are imbalanced by this criterion as well. The covariate means for a typical variable for the non-offending trajectory group differ by approximately 19% of a standard deviation. And, the means for the logit of the propensity score diverge by 1.33 standard deviations.

After matching, all but two covariates have acceptable levels of covariate balance. Violent crime at times two and four have standardized bias statistics that exceed 20.[40]

[40] Violent crime variables were omitted from the propensity score models due to quasi-complete separation of data points. Firth's regression was attempted but did not yield better bias reduction overall.

It is important to note that any differences in violent crime for the non-offending group were created through the MI procedure. Recall, trajectories were estimated with up to two waves of missing data. Following trajectory group estimation and prior to the estimation of the logistic regression models, data were imputed and in a few instances an individual was assigned a non-zero score—even though they were classified as a non-offender by the LCGA modeling procedure. Thus, the high standardized bias statistics for the violent crime measures are products of extremely small standard deviations of these prior violent crime measures since nearly every one has a value of zero by definition. Thus, even though two variables are "technically" out of balance according to conventional cutoffs, this has little influence on our ability to make a causal inference in this case. Indeed, the mean scores on these measures are essentially identical. With that said, the average covariate is now imbalanced by only about 7% of a standard deviation and the logit of the propensity score is out of balance by approximately 5% of a standard deviation. These values are down from 19% and 132%, respectively. In short, the matching procedure for the non-offending trajectory group for the police contact treatment was successful. .

Finally, Table 9-12 reports the pre- and post-matching covariate balance statistics for the low offending trajectory group for the police contact treatment. The covariate means for a total of 15 of the measures are substantially different across the two groups. Another 15 variables have less than ideal covariate balance. None of the demographic or neighborhood variables are out of balance. Half of the family variables, two-thirds of the school factors, five-sevenths of the peer association measures, and two-thirds of the values and mental states variables are out of balance. The majority of

prior criminal behavior covariates are imbalanced but the prior justice system contact variable is not. Overall, the average covariate imbalance is about 19% of a standard deviation and the logit of the propensity score is out of balance by slightly more than one standard deviation.

After matching, all forty covariates have standardized bias statistics below 10. The average covariate is now imbalanced by less than 5% of a standard deviation. Three variables had greater bias post-matching but even these variables are still easily within acceptable levels of covariate balance. The average bias reduction was 32% and the median bias reduction for a covariate was 73%. The logit of the propensity score underwent a 99% bias reduction leaving the covariate means to differ by less than 1% of a standard deviation. The optimal full matching procedure has created a treatment and control group for the low offending trajectory in which the treatment effect of police contact on subsequent violent offending can be estimated with confidence.

With respect to covariate imbalance across the three violent criminal offending trajectory groups, there are again some similarities and also some important differences. Eight variables had acceptable levels of covariate balance pre-matching across all trajectory groups (e.g., neighborhood disorganization, sexual activity under fifteen) whereas nine variables had unacceptable levels across all trajectory groups (e.g., risky time with friends, general delinquency). The remaining 23 variables were out of balance for either one or two of the latent classes. While the particulars of the variables tended to vary some across the trajectory groups, there were a large number of pre-matching differences for all trajectory groups leading one to question whether the bivariate results will hold once bias has been removed. Unfortunately, the matching

procedure only successfully removed bias in the covariates for the non-offending and low offending trajectory groups. It is for these two groups which reasonable trajectory group-specific treatment effect estimates can be obtained. I now turn to the primary analysis, which quantifies effects for the arrest and police contacts treatments in both the short- and long-runs.

Treatment Effect Estimates of Official Intervention on Subsequent Violence

Three types of treatment effects are generally available for post-matching analysis. Most importantly, the average treatment effect (ATE) is the average effect of a treatment on an outcome for all individuals in the sample (or subsample). In addition to this overall effect, two other treatment effects are often of interest to policy researchers. The first of these and the one more often reported is the average treatment effect on the treated (ATT), which is the average effect of some treatment on an outcome for individuals who are typically treated. Second, the average treatment effect on the untreated (ATU) is the average effect of some treatment on an outcome for individuals who typically avoid treatment.

Each of these treatment effects seek to provide an estimate of the counterfactual; that is, ATE, ATT, and ATU estimate average outcome differences of individuals being treated and not treated. However, they differ with respect to their reference populations. To help understand how these treatment effect estimates are dissimilar, it is useful to consider their meaning in reference to the counterfactual for a single individual (for a discussion, see Morgan & Winship, 2007). In the present case, ATE is defined as the expected observed difference in violent crime for a randomly selected individual from the entire sample if the individual could have experienced both the treatment condition of official intervention and the condition of no official intervention. ATT is the expected

observed difference in violent crime for a randomly selected individual who experiences official intervention if the individual could have experienced both treatment conditions, whereas ATU is the expected observed difference in violent crime for a randomly selected individual who does not experience official intervention if the individual could have both experienced an official intervention and not experienced one.

Because the interest here is in the general effects of arrest and police contacts on subsequent violent behavior, I focus on the average treatment effect (ATE) of official intervention on subsequent violent offending. All of the build-up involving bivariate assessments and assessments of covariate balance culminates with the straightforward estimation of ATE using the Hodges-Lehmann aligned rank test for matched sets. I begin with arrest and estimate the treatment effects of this official intervention on subsequent violent offending behavior in the short-run for the total sample and then across trajectory groups. Similarly, I estimate the average treatment effects for the police contact official intervention in the short-run. This is followed with an exploration of whether any of these treatment effects persist or differ in the long-run.

Treatment Effects in the Short-run

Recall, the bivariate relationship between arrest and subsequent violent offending behavior at wave nine for the total sample was statistically significant (see Table 9-1). With a matched sample, the treatment effect estimate of arrest for the total sample remains statistically significant ($p \leq 0.05$). Table 9-13 contains the details of the Hodges-Lehmann test of statistical significance. It should be noted that the Hodges-Lehmann estimates reported in the table are averages of the five MI data sets and the standard errors that are reported were calculated using appropriate procedures for handling MI data sets (this also applies to Tables 9-14 through 9-16). To aid in interpreting the

treatment effects, the column at the far right of the table reports Cohen's *d* statistic. While the short-run treatment effect of arrest for the total sample is statistically significant, it is best characterized as a small effect. Thus, while arresting someone may exacerbate their violent offending behavior in the short-run there are certainly other factors that are probably much more important in explaining subsequent violent behavior. Nevertheless, the fact that arrest predicts later violent crime at all after successfully matching on forty covariates is important.

Table 9-13 also contains the results of the trajectory group-specific analyses. The treatment effects were not estimated for the high offending trajectory group since substantial amounts of covariate imbalance remained. Even though only the bivariate relationship for low offenders was statistically significant, recall that the expected counts of violence in the short-run were higher for those arrested compared to their counterparts across all trajectory groups in the bivariate analysis (see Table 9-1). Similar to the bivariate analysis, the treatment effect estimates for the non-offending group fails to be statistically significant. What is more, however, is that the observed effect is now in the opposite direction. Once individuals are successfully matched on the covariates, those experiencing an arrest appear less likely to commit acts of violence during wave nine but, to be clear, this relationship is not statistically significant. Conversely, the treatment effect for the low offending trajectory group is still in a direction that would support labeling theory. Like the non-offending group, however, this treatment effect estimate is also not statistically significant. Thus, the significant bivariate relationship between arrest and short-run violent crime for the low offending trajectory group does not hold after matching.

Table 9-14 contains the Hodges-Lehmann short-run treatment effect estimates and Cohen' *d* values for the police contact official intervention. Beginning again with the full sample, the aligned rank test shows that individuals self-reporting a police contact during waves seven and/or eight are likely to engage in more distinct acts of violent crime in wave nine. This treatment effect is statistically significant with an alpha level equal to 0.01. Similar to the short-run violent outcome results for the treatment of arrest, the treatment effect is relatively small (d=0.30). Thus, there is consistent evidence of a fairly small labeling effect across both types of official intervention for the sample as a whole.

The trajectory group-specific analysis reveals mixed support for the labeling perspective. While the bivariate relationship between police contact and violent crime during wave nine was statistically significant for the non-offending trajectory group, the treatment effect estimate for this relationship fails to achieve statistical significance. However, the treatment effect for the low offending trajectory group is statistically significant ($p \leq 0.05$). It should be noted that this effect is the strongest of all of the significant treatment effects across all models. Still, this effect is probably best described as small (d=0.35). For low offenders, experiencing a police contact results in an observed short-run outcome that is supportive of the basic tenants of labeling theory. This is not the case for non-offenders ($p > 0.05$) and, unfortunately, the data do not permit us to discern with any definitiveness whether this is true for high offenders—the trajectory group that is probably most important for policy. At best, one can attempt to glean information from the unmatched and matched results for the low offending trajectory group and the pre- and post-matching covariate balance assessments. It was

found that the bivariate findings held for the low offending trajectory group in the matched sample. For both low and high offenders pre-matching, the average covariate was out of balance by approximately one-quarter of a standard deviation. And, it was true that the difference in wave nine violent crime means was largest for the high offenders. Given this information, it is reasonable to think that the bivariate relationship for high offenders may have also held. However, this is quite speculative and the author is certainly in no place to draw any scientifically sound conclusions in this regard.

Treatment Effects in the Long-run

Table 9-15 contains the treatment effect estimates for the arrest condition for the total sample as well as the non-offending and low offending trajectory groups. For the sample on the whole, the Hodges-Lehmann aligned rank test is not statistically significant indicting there is no treatment effect of arrest. Likewise, the treatment effect estimates for the non-offending and the low offending groups are not statistically significant. This is not especially surprising given the fact that all of these relationships were not statistically significant at the bivariate level. Thus, the total sample treatment effect of arrest in the short-run is not sustained over the long haul and no trajectory groups suddenly have a labeling or deterrent treatment effect emerge in wave ten.

Figure 9-16 reports the long-run treatment effects of the police contact official intervention. Recall, the bivariate relationships between violent crime at wave ten and police contact were statistically significant for the total sample as well as for the low offending trajectory group (see Table 9-4). Once individuals are matched based upon their propensities to experience a police contact during the treatment period, there are no longer any significant relationships between police contact and subsequent violent offending. In sum, labeling effects on the whole or for any trajectory group specifically

are not sustained in the long-run. In addition, labeling effects do not appear to need time to run their course; in other words, there is no evidence in this sample that short-run deterrent or null effects eventually become long-run labeling effects as Sampson and Laub's (1997) discussions of the influences of official intervention across the life-course suggest as an empirical possibility.

Table 9-1. Bivariate associations between arrest and violent crime in the short-run for the total sample and by trajectory group.

	Sum of Scores	Expected Under Ho	Mean Score	Z
Total Sample				
Not Arrested	134209.5	138272.0	289.24	3.77**
Arrested	43100.5	39038.0	329.01	---
Trajectory Groups				
High offenders (C1)				
Not Arrested	1298.0	1380.0	32.45	1.22
Arrested	1048.0	966.0	37.43	---
Non-offenders (C2)				
Not Arrested	23415.5	23500.0	117.07	0.69
Arrested	4079.5	3995.0	119.98	---
Low offenders (C3)				
Not Arrested	32075.5	32928.0	143.19	2.01*
Arrested	10995.5	10143.0	159.35	---

** p≤0.01; * p≤0.05; † p≤0.10; Wilcoxon-Mann-Whitney test of statistical significance.

Table 9-2. Bivariate associations between police contact and violent crime in the short-run for the total sample and by trajectory group.

	Sum of scores	Expected under Ho	Mean score	Z
Total Sample				
No Police Contact	113527.0	121286.0	278.94	6.43**
Police Contact	63783.0	56024.0	339.27	---
Trajectory Groups				
High offenders (C1)				
No Police Contact	1002.5	1173.0	29.48	2.51*
Police Contact	1343.5	1173.0	39.51	---
Non-offenders (C2)				
No Police Contact	21939.5	22207.5	116.08	1.97*
Police Contact	5555.5	5287.5	123.45	---
Low offenders (C3)				
No Police Contact	25256.5	27048.0	137.26	3.71**
Police Contact	17814.5	16023.0	163.44	---

** p≤0.01; * p≤0.05; † p≤0.10; Wilcoxon-Mann-Whitney test of statistical significance.

Table 9-3. Bivariate associations between arrest and violent crime in the long-run for the total sample and by trajectory group.

	Sum of Scores	Expected Under Ho	Mean Score	Z
Total Sample				
Not Arrested	137778.0	138272.0	296.94	0.42
Arrested	39532.0	39038.0	301.77	---
Trajectory Groups				
High offenders (C1)				
Not Arrested	965.5	966.0	34.48	0.00
Arrested	1380.5	1380.0	34.51	---
Non-offenders (C2)				
Not Arrested	23510.0	23500.0	117.55	0.04
Arrested	3985.0	3995.0	117.20	---
Low offenders (C3)				
Not Arrested	33055.5	32928.0	147.57	0.29
Arrested	10015.5	10143.0	145.15	---

** p≤0.01; * p≤0.05; † p≤0.10; Wilcoxon-Mann-Whitney test of statistical significance.

Table 9-4. Bivariate associations between police contact and violent crime in the long-run for the total sample and by trajectory group.

	Sum of Scores	Expected Under Ho	Mean Score	Z
Total Sample				
No Police Contact	116284.0	121286.0	285.71	3.77**
Police Contact	61026.0	56024.0	324.71	--
Trajectory Groups				
High offenders (C1)				
No Police Contact	1131.0	1173.0	33.26	0.64
Police Contact	1215.0	1173.0	35.74	---
Non-offenders (C2)				
No Police Contact	22035.0	22207.5	116.56	0.74
Police Contact	5460.0	5287.5	121.33	---
Low offenders (C3)				
No Police Contact	25521.5	27048.0	138.70	3.04**
Police Contact	17549.5	16023.0	161.00	---

** p≤0.01; * p≤0.05; † p≤0.10; Wilcoxon-Mann-Whitney test of statistical significance.

Table 9-5. Treatment and control group covariate means and bias statistics before and after PSM for the arrest treatment (total sample).

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Demographics							
Race							
Black	0.74	0.59	31.74	0.71	0.72	3.65	0.89
Hispanic	0.19	0.18	3.65	0.20	0.20	1.79	0.51
Age	16.92	16.35	77.71	16.83	16.88	7.33	0.91
Family poverty	0.31	0.30	2.95	0.30	0.33	5.68	-0.93
Neighborhood characteristics							
Proportion African American	0.59	0.51	27.69	0.56	0.56	2.32	0.92
Proportion in poverty	0.36	0.32	27.99	0.35	0.35	3.08	0.89
Neighborhood disorganization	1.69	1.66	4.56	1.69	1.66	5.47	-0.20
Neighborhood integration	2.16	2.21	6.86	2.16	2.18	3.31	0.52
Neighborhood satisfaction	2.82	2.89	10.89	2.86	2.86	3.31	0.70
Neighborhood arrest rate	4.53	3.86	34.40	4.30	4.28	1.92	0.94
Family							
Family structure	0.24	0.36	24.48	0.28	0.28	3.26	0.87
Parental supervision	3.44	3.54	20.79	3.48	3.49	5.44	0.74
Attachment to parent	3.43	3.37	13.79	3.40	3.39	5.31	0.61
PC's edu. expect. of child	1.89	2.40	63.43	2.00	2.02	7.50	0.88
Attachment to child	3.32	3.52	43.36	3.38	3.38	3.62	0.92
Consistency in discipline	2.34	2.36	4.24	2.32	2.29	6.57	-0.55
School Factors							
Commitment to school	2.99	3.07	18.96	3.01	3.02	5.15	0.73
Aspiration for college	3.08	3.41	37.19	3.13	3.16	3.73	0.90
Prosocial activities	1.55	1.71	26.60	1.55	1.56	4.96	0.81
Educational expectations	2.45	2.64	24.89	2.44	2.44	2.17	0.91
School clubs with friends	1.10	1.26	32.13	1.12	1.11	2.42	0.92
Peer Associations							
Peer delinquency	1.48	1.28	37.11	1.46	1.40	10.25	0.72
Peer drug use	1.55	1.37	34.31	1.55	1.54	2.27	0.93

Table 9-5. Continued.

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Peer delinquent values	1.39	1.29	22.44	1.38	1.36	4.97	0.78
Dating	0.56	0.44	24.57	0.55	0.54	3.07	0.88
Sexual activity under 15	0.28	0.36	16.20	0.31	0.30	3.36	0.79
Risky time with friends	2.41	2.10	42.47	2.38	2.36	3.83	0.91
Gang involvement	0.44	0.22	48.21	0.40	0.40	4.54	0.91
Values							
Self-image	3.19	3.21	6.39	3.20	3.20	1.59	0.75
Depression	2.00	1.98	4.78	2.00	2.01	5.16	-0.08
Delinquent values	1.50	1.37	28.24	1.47	1.45	6.99	0.75
Prior criminal behavior							
Violent crime (Time 1)	0.80	0.51	31.69	0.77	0.78	2.79	0.91
Violent crime (Time 2)	0.76	0.41	38.67	0.64	0.66	3.22	0.92
Violent crime (Time 3)	0.74	0.40	36.84	0.67	0.64	4.38	0.88
Violent crime (Time 4)	0.55	0.32	28.33	0.49	0.48	6.88	0.76
Violent crime (Time 5)	0.57	0.30	33.81	0.55	0.50	6.40	
General delinquency	2.26	1.13	45.24	2.15	1.93	8.90	0.80
Drug use	0.23	0.13	19.54	0.21	0.20	1.83	0.91
Aggression	0.50	0.35	41.73	0.47	0.45	5.95	0.86
Prior Justice System Contact							
Previous contact or arrest	0.67	0.25	93.98	0.58	0.58	2.35	0.98
Global Covariate Balance Stats.							
Mean	---	---	29.32	---	---	4.42	0.70
Median	---	---	28.11	---	---	3.78	0.87
Maximum	---	---	93.98	---	---	10.25	0.98
Logit propensity score	-0.27	-2.25	147.72	-0.70	-0.70	0.26	1.00

Notes: The following is relevant for Tables 9-5 through 9-12. M_{Xt} = treatment group covariate mean before matching; M_{Xp} = potential control group covariate mean before matching; d_X = standardized covariate bias before matching (percentage of a standard deviation); M_{Xtm} = treatment group covariate mean after matching; M_{Xcm} = control group covariate mean after matching; d_{Xm} = standardized covariate bias after matching (percentage of a standard deviation); %BR = percentage reduction in standardized bias through propensity score matching.

Table 9-6. Treatment and control group covariate means and bias statistics before and after PSM for the arrest treatment (High offenders).

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Demographics							
Race							
Black	0.71	0.78	13.96	0.78	0.78	1.55	0.89
Hispanic	0.18	0.15	7.72	0.14	0.13	4.19	0.46
Age	16.82	16.72	11.81	16.77	16.69	9.95	0.16
Family poverty	0.16	0.30	32.64	0.17	0.26	19.87	0.39
Neighborhood characteristics							
Proportion African American	0.61	0.55	23.71	0.58	0.57	4.07	0.83
Proportion in poverty	0.35	0.31	26.86	0.35	0.34	3.61	0.87
Neighborhood disorganization	1.71	1.73	5.79	1.77	1.78	2.22	0.62
Neighborhood integration	2.12	1.97	22.12	2.15	1.97	28.05	-0.27
Neighborhood satisfaction	2.65	2.93	39.04	2.57	2.83	37.42	0.04
Neighborhood arrest rate	4.91	3.80	59.20	4.46	4.46	0.04	1.00
Family							
Family structure	0.25	0.23	5.88	0.25	0.25	0.35	0.94
Parental supervision	3.32	3.46	22.30	3.43	3.29	23.70	-0.06
Attachment to parent	3.52	3.32	42.05	3.50	3.22	50.97	-0.21
PC's edu. expect. of child	1.57	2.07	63.86	1.62	1.97	44.37	0.31
Attachment to child	3.31	3.37	11.94	3.38	3.27	22.10	-0.85
Consistency in discipline	2.38	2.27	22.72	2.34	2.10	47.03	-1.07
School Factors							
Commitment to school	2.86	2.93	16.35	2.85	2.85	1.52	0.91
Aspiration for college	2.89	3.03	13.51	2.87	2.84	2.98	0.78
Prosocial activities	1.39	1.75	58.20	1.39	1.72	53.90	0.07
Educational expectations	2.43	2.50	8.65	2.46	2.48	2.61	0.70
School clubs with friends	1.02	1.25	49.08	1.03	1.27	51.72	-0.05
Peer Associations							
Peer delinquency	1.88	1.77	15.27	1.77	1.73	4.94	0.68
Peer drug use	1.96	1.93	5.88	1.89	1.89	0.47	0.92

Table 9-6. Continued.

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Peer delinquent values	1.69	1.59	20.66	1.60	1.58	3.46	0.83
Dating	0.71	0.78	13.96	0.67	0.79	27.10	-0.94
Sexual activity under 15	0.39	0.45	11.59	0.44	0.51	15.48	-0.34
Risky time with friends	2.70	2.70	6.43	2.69	2.59	13.11	-1.04
Gang involvement	0.96	0.83	46.58	0.98	0.78	62.50	-0.34
Values							
Self-image	3.13	3.14	3.96	3.12	3.11	1.12	0.72
Depression	2.12	2.08	10.10	2.13	2.15	3.39	0.66
Delinquent values	1.80	1.66	28.86	1.72	1.74	4.59	0.84
Prior criminal behavior							
Violent crime (Time 1)	1.54	1.90	29.97	1.60	2.08	40.35	-0.35
Violent crime (Time 2)	1.94	2.15	17.69	2.02	2.09	5.75	0.68
Violent crime (Time 3)	1.89	2.16	22.02	1.96	2.02	5.25	0.76
Violent crime (Time 4)	1.79	1.48	29.39	1.63	1.57	5.44	0.81
Violent crime (Time 5)	1.54	1.28	22.13	1.33	1.43	8.44	
General delinquency	3.89	4.20	7.91	3.43	5.07	42.28	-4.34
Drug use	0.39	0.58	23.14	0.44	0.67	29.15	-0.26
Aggression	0.54	0.46	23.24	0.50	0.46	8.69	0.63
Prior Justice System Contact							
Previous contact or arrest	0.79	0.35	97.94	0.78	0.45	72.07	0.26
Global Covariate Balance Stats.							
Mean	---	---	24.85	---	---	19.14	0.17
Median	---	---	22.07	---	---	8.57	0.46
Maximum	---	---	97.94	---	---	72.07	1.00
Logit propensity score	1.22	-1.47	314.82	1.24	-1.38	306.27	0.03

Table 9-7. Treatment and control group covariate means and bias statistics before and after PSM for the arrest treatment (Non-offenders).

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Demographics							
Race							
Black	0.82	0.50	72.77	0.77	0.72	14.69	0.80
Hispanic	0.12	0.21	25.15	0.14	0.17	14.09	0.44
Age	16.86	16.27	89.79	16.80	16.82	2.60	0.97
Family poverty	0.32	0.28	9.45	0.35	0.32	15.36	-0.63
Neighborhood characteristics							
Proportion African American	0.58	0.47	39.53	0.54	0.54	6.19	0.84
Proportion in poverty	0.34	0.31	19.66	0.33	0.33	8.32	0.58
Neighborhood disorganization	1.56	1.64	12.55	1.58	1.57	13.47	-0.07
Neighborhood integration	2.17	2.22	7.09	2.14	2.10	13.59	-0.92
Neighborhood satisfaction	2.96	2.91	7.90	2.88	2.89	6.72	0.15
Neighborhood arrest rate	4.41	3.67	36.37	4.07	4.18	8.01	0.78
Family							
Family structure	0.26	0.42	33.18	0.26	0.25	5.64	0.83
Parental supervision	3.58	3.60	4.31	3.62	3.65	8.97	-1.08
Attachment to parent	3.53	3.45	19.32	3.52	3.49	12.97	0.33
PC's edu. expect. of child	2.10	2.52	53.11	2.24	2.19	13.44	0.75
Attachment to child	3.44	3.62	39.86	3.55	3.57	9.16	0.77
Consistency in discipline	2.39	2.45	11.72	2.39	2.35	13.34	-0.14
School Factors							
Commitment to school	3.17	3.15	5.57	3.20	3.17	13.86	-1.49
Aspiration for college	3.38	3.51	15.36	3.38	3.38	16.29	-0.06
Prosocial activities	1.72	1.76	4.70	1.75	1.76	9.20	-0.96
Educational expectations	2.44	2.74	38.82	2.50	2.51	7.38	0.81
School clubs with friends	1.27	1.32	7.35	1.32	1.26	7.89	-0.07
Peer Associations							
Peer delinquency	1.27	1.09	40.51	1.14	1.14	5.50	0.86
Peer drug use	1.24	1.19	13.79	1.17	1.20	12.12	0.12

Table 9-7. Continued.

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Peer delinquent values	1.26	1.16	24.15	1.19	1.23	12.23	0.49
Dating	0.35	0.33	5.91	0.31	0.34	7.07	-0.20
Sexual activity under 15	0.32	0.24	19.83	0.27	0.27	7.31	0.63
Risky time with friends	2.05	1.84	32.18	2.00	1.98	11.46	0.64
Gang involvement	0.03	0.04	5.79	0.04	0.04	2.41	0.58
Values							
Self-image	3.25	3.22	6.92	3.34	3.29	11.00	-0.59
Depression	1.81	1.92	22.20	1.79	1.80	3.51	0.84
Delinquent values	1.32	1.24	21.82	1.29	1.30	3.83	0.82
Prior criminal behavior							
Violent crime (Time 1)	0.00	0.01	13.03	0.00	0.00	1.28	0.90
Violent crime (Time 2)	0.01	0.00	13.40	0.00	0.00	3.11	0.77
Violent crime (Time 3)	0.00	0.00	6.53	0.00	0.00	0.68	0.90
Violent crime (Time 4)	0.00	0.00	6.00	0.00	0.00	6.68	-0.11
Violent crime (Time 5)	0.00	0.00	0.00	0.00	0.00	0.00	
General delinquency	0.47	0.30	23.89	0.39	0.47	12.65	0.47
Drug use	0.00	0.04	25.65	0.00	0.07	41.70	-0.63
Aggression	0.31	0.26	18.95	0.26	0.25	6.35	0.66
Prior Justice System Contact							
Previous contact or arrest	0.56	0.14	99.44	0.43	0.39	11.45	0.88
Global Covariate Balance Stats.							
Mean	---	---	23.84	---	---	9.54	0.27
Median	---	---	19.13	---	---	8.64	0.58
Maximum	---	---	99.44	---	---	41.70	0.97
Logit propensity score	0.25	-4.03	183.69	-0.77	-0.95	7.71	0.96

Table 9-8. Treatment and control group covariate means and bias statistics before and after PSM for the arrest treatment (Low offenders).

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Demographics							
Race							
Black	0.71	0.64	14.42	0.66	0.66	6.21	0.57
Hispanic	0.23	0.15	20.45	0.24	0.23	5.04	0.75
Age	16.99	16.35	84.88	16.89	16.85	5.46	0.94
Family poverty	0.37	0.32	10.82	0.37	0.37	2.43	0.78
Neighborhood characteristics							
Proportion African American	0.58	0.55	14.48	0.55	0.55	5.13	0.65
Proportion in poverty	0.37	0.33	32.60	0.35	0.35	5.64	0.83
Neighborhood disorganization	1.74	1.67	12.45	1.72	1.69	7.04	0.43
Neighborhood integration	2.17	2.23	9.69	2.23	2.23	3.32	0.66
Neighborhood satisfaction	2.82	2.87	7.06	2.86	2.87	4.01	0.43
Neighborhood arrest rate	4.43	4.04	20.56	4.18	4.14	7.14	0.65
Family							
Family structure	0.23	0.32	20.12	0.27	0.30	7.68	0.62
Parental supervision	3.42	3.50	18.22	3.48	3.46	7.35	0.60
Attachment to parent	3.35	3.31	9.10	3.33	3.33	3.71	0.59
PC's edu. expect. of child	1.92	2.35	53.70	2.01	1.96	6.49	0.88
Attachment to child	3.26	3.46	40.96	3.34	3.33	3.80	0.91
Consistency in discipline	2.29	2.29	1.52	2.26	2.25	5.66	-2.72
School Factors							
Commitment to school	2.96	3.02	14.84	2.99	2.98	9.12	0.39
Aspiration for college	3.01	3.39	43.28	3.14	3.08	10.76	0.75
Prosocial activities	1.52	1.67	25.36	1.48	1.46	5.54	0.78
Educational expectations	2.46	2.58	15.52	2.44	2.37	12.40	0.20
School clubs with friends	1.04	1.21	39.61	1.06	1.06	2.23	0.94
Peer Associations							
Peer delinquency	1.43	1.35	14.27	1.39	1.38	3.83	0.73
Peer drug use	1.54	1.43	20.83	1.56	1.55	2.61	0.87

Table 9-8. Continued.

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Peer delinquent values	1.32	1.35	6.33	1.34	1.34	3.94	0.38
Dating	0.59	0.47	24.43	0.56	0.55	2.95	0.88
Sexual activity under 15	0.22	0.45	51.09	0.26	0.25	6.05	0.88
Risky time with friends	2.46	2.22	34.38	2.48	2.47	3.32	0.90
Gang involvement	0.43	0.28	33.46	0.39	0.40	5.75	0.83
Values							
Self-image	3.18	3.22	9.57	3.22	3.20	7.91	0.17
Depression	2.05	2.01	7.00	2.02	2.04	6.05	0.14
Delinquent values	1.47	1.44	5.24	1.44	1.43	3.81	0.27
Prior criminal behavior							
Violent crime (Time 1)	0.90	0.71	22.59	0.89	0.90	4.86	0.78
Violent crime (Time 2)	0.66	0.45	29.31	0.55	0.54	1.92	0.93
Violent crime (Time 3)	0.63	0.43	28.79	0.52	0.46	8.85	0.69
Violent crime (Time 4)	0.31	0.40	15.68	0.30	0.33	4.66	0.70
Violent crime (Time 5)	0.46	0.39	10.35	0.41	0.43	5.37	
General delinquency	2.48	1.33	50.55	2.03	2.03	5.94	0.88
Drug use	0.28	0.14	26.46	0.25	0.25	6.06	0.77
Aggression	0.57	0.42	42.28	0.50	0.53	8.00	0.81
Prior Justice System Contact							
Previous contact or arrest	0.68	0.33	74.93	0.56	0.56	0.99	0.99
Global Covariate Balance Stats.							
Mean	---	---	25.43	---	---	5.48	0.60
Median	---	---	20.50	---	---	5.50	0.75
Maximum	---	---	84.88	---	---	12.40	0.99
Logit propensity score	0.22	-2.65	167.12	-0.61	-0.65	2.27	0.99

Table 9-9. Treatment and control group covariate means and bias statistics before and after PSM for the police contact treatment (total sample).

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Demographics							
Race							
Black	0.63	0.62	0.74	0.63	0.64	4.84	-5.54
Hispanic	0.19	0.17	4.41	0.19	0.19	1.82	0.59
Age	16.60	16.41	23.65	16.55	16.53	4.89	0.79
Family poverty	0.30	0.31	2.37	0.30	0.32	5.52	-1.33
Neighborhood characteristics							
Proportion African American	0.54	0.53	4.43	0.53	0.54	5.23	-0.18
Proportion in poverty	0.34	0.32	17.09	0.34	0.34	4.42	0.74
Neighborhood disorganization	1.67	1.67	1.27	1.68	1.70	3.70	-1.92
Neighborhood integration	2.16	2.21	7.58	2.17	2.15	2.84	0.63
Neighborhood satisfaction	2.83	2.90	11.62	2.87	2.87	3.21	0.72
Neighborhood arrest rate	4.22	3.91	15.42	4.11	4.13	5.21	0.66
Family							
Family structure	0.31	0.34	5.38	0.34	0.34	3.05	0.43
Parental supervision	3.44	3.55	23.63	3.49	3.48	2.88	0.88
Attachment to parent	3.33	3.41	16.12	3.39	3.37	4.07	0.75
PC's edu. expect. of child	2.05	2.40	41.70	2.19	2.16	4.18	0.90
Attachment to child	3.34	3.54	40.83	3.43	3.42	2.83	0.93
Consistency in discipline	2.30	2.38	15.15	2.34	2.31	6.35	0.58
School Factors							
Commitment to school	2.94	3.10	40.35	3.00	3.01	4.10	0.90
Aspiration for college	3.14	3.43	33.57	3.25	3.23	3.96	0.88
Prosocial activities	1.61	1.71	15.13	1.65	1.63	4.68	0.69
Educational expectations	2.45	2.67	28.85	2.52	2.50	3.19	0.89
School clubs with friends	1.18	1.25	12.74	1.20	1.19	2.18	0.83
Peer Associations							
Peer delinquency	1.49	1.25	45.95	1.37	1.38	2.86	0.94
Peer drug use	1.57	1.34	44.47	1.47	1.47	2.38	0.95

Table 9-9. Continued.

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Peer delinquent values	1.40	1.26	32.69	1.36	1.35	2.86	0.91
Dating	0.58	0.41	34.91	0.54	0.54	1.88	0.95
Sexual activity under 15	0.35	0.34	3.04	0.36	0.38	7.25	-1.38
Risky time with friends	2.36	2.08	40.27	2.28	2.28	4.44	0.89
Gang involvement	0.40	0.21	41.62	0.33	0.33	2.61	0.94
Values							
Self-image	3.13	3.24	27.94	3.17	3.17	4.45	0.84
Depression	2.04	1.95	19.17	2.02	2.02	5.02	0.74
Delinquent values	1.54	1.33	47.50	1.46	1.46	1.22	0.97
Prior criminal behavior							
Violent crime (Time 1)	0.74	0.50	26.27	0.67	0.66	3.75	0.86
Violent crime (Time 2)	0.63	0.42	24.36	0.55	0.57	4.68	0.81
Violent crime (Time 3)	0.66	0.38	31.67	0.54	0.56	3.13	0.90
Violent crime (Time 4)	0.57	0.28	39.93	0.46	0.46	1.96	0.95
Violent crime (Time 5)	0.56	0.27	37.73	0.42	0.43	1.70	
General delinquency	2.41	0.90	61.95	1.67	1.64	2.53	0.96
Drug use	0.29	0.09	35.11	0.19	0.20	3.78	0.89
Aggression	0.49	0.33	45.46	0.43	0.43	2.16	0.95
Prior Justice System Contact							
Previous contact or arrest	0.46	0.29	35.71	0.39	0.39	2.70	0.92
Global Covariate Balance Stats.							
Mean	---	---	25.94	---	---	3.61	0.46
Median	---	---	27.10	---	---	3.46	0.86
Maximum	---	---	61.95	---	---	7.25	0.97
Logit propensity score	-0.25	-1.17	92.37	-0.63	-0.63	0.23	1.00

Table 9-10. Treatment and control group covariate means and bias statistics before and after PSM for the police contact treatment (High offenders).

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Demographics							
Race							
Black	0.65	0.85	48.95	0.73	0.73	0.00	1.00
Hispanic	0.21	0.12	24.14	0.24	0.26	4.62	0.81
Age	16.71	16.81	12.97	16.59	16.78	24.36	-0.88
Family poverty	0.18	0.29	26.40	0.43	0.20	51.50	-0.95
Neighborhood characteristics							
Proportion African American	0.59	0.56	11.57	0.66	0.64	6.44	0.44
Proportion in poverty	0.35	0.31	27.61	0.37	0.33	33.13	-0.20
Neighborhood disorganization	1.74	1.69	13.71	2.03	1.76	40.93	-1.99
Neighborhood integration	2.08	2.00	12.55	1.93	1.87	8.78	0.30
Neighborhood satisfaction	2.86	2.80	8.74	2.57	2.65	11.33	-0.30
Neighborhood arrest rate	4.75	3.76	52.66	4.28	4.45	9.00	0.83
Family							
Family structure	0.29	0.18	28.01	0.26	0.31	11.42	0.59
Parental supervision	3.21	3.60	70.74	3.48	3.71	42.04	0.41
Attachment to parent	3.39	3.43	11.89	3.49	3.36	23.59	-0.98
PC's edu. expect. of child	1.73	2.00	33.08	2.31	1.92	48.19	-0.46
Attachment to child	3.29	3.39	21.65	3.36	3.44	16.54	0.24
Consistency in discipline	2.31	2.34	7.39	2.32	2.23	17.62	-1.38
School Factors							
Commitment to school	2.82	3.00	45.89	2.98	2.84	36.52	0.20
Aspiration for college	2.79	3.15	36.47	3.34	2.89	46.53	-0.28
Prosocial activities	1.48	1.72	37.78	1.88	1.85	5.55	0.85
Educational expectations	2.44	2.50	7.13	2.87	2.71	19.92	-1.79
School clubs with friends	1.09	1.23	27.13	1.47	1.48	2.48	0.91
Peer Associations							
Peer delinquency	1.96	1.67	40.48	1.89	1.58	44.73	-0.10
Peer drug use	2.02	1.86	27.27	1.76	1.74	5.01	0.82

Table 9-10. Continued.

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Peer delinquent values	1.68	1.59	17.06	1.66	1.52	26.65	-0.56
Dating	0.79	0.71	20.48	0.76	0.81	12.74	0.38
Sexual activity under 15	0.38	0.47	17.91	0.47	0.47	0.12	0.99
Risky time with friends	2.81	2.60	30.40	2.60	2.66	9.06	0.70
Gang involvement	0.85	0.91	18.33	0.77	0.94	48.97	-1.67
Values							
Self-image	3.08	3.19	25.01	3.34	3.28	13.49	0.46
Depression	2.11	2.09	3.82	2.11	2.21	20.59	-4.39
Delinquent values	1.89	1.54	82.16	1.73	1.61	27.07	0.67
Prior criminal behavior							
Violent crime (Time 1)	1.75	1.76	1.38	1.75	2.24	39.39	-27.47
Violent crime (Time 2)	1.99	2.15	13.85	2.19	2.03	14.11	-0.02
Violent crime (Time 3)	1.85	2.24	33.72	1.97	1.81	13.21	0.61
Violent crime (Time 4)	1.71	1.50	19.84	1.74	1.46	26.56	-0.34
Violent crime (Time 5)	1.56	1.21	30.23	1.71	1.12	50.05	
General delinquency	4.82	3.32	39.08	4.57	3.33	32.37	0.17
Drug use	0.65	0.35	36.22	0.53	0.42	23.59	0.35
Aggression	0.56	0.43	35.81	0.42	0.41	1.28	0.96
Prior Justice System Contact							
Previous contact or arrest	0.65	0.41	48.51	0.53	0.49	9.22	0.81
Global Covariate Balance Stats.							
Mean	---	---	27.70	---	---	21.97	-0.78
Median	---	---	26.76	---	---	18.77	0.24
Maximum	---	---	82.16	---	---	51.50	1.00
Logit propensity score	-1.07	1.50	184.93	-0.13	-0.52	28.59	0.85

Table 9-11. Treatment and control group covariate means and bias statistics before and after PSM for the police contact treatment (Non-offenders).

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Demographics							
Race							
Black	0.62	0.53	18.93	0.60	0.58	7.10	0.63
Hispanic	0.16	0.21	13.22	0.16	0.14	9.93	0.25
Age	16.61	16.29	42.04	16.54	16.51	6.43	0.85
Family poverty	0.27	0.29	5.76	0.28	0.29	7.11	-0.23
Neighborhood characteristics							
Proportion African American	0.53	0.48	18.08	0.51	0.50	7.88	0.56
Proportion in poverty	0.32	0.31	4.44	0.32	0.32	5.75	-0.30
Neighborhood disorganization	1.62	1.63	3.41	1.60	1.59	7.80	-1.28
Neighborhood integration	2.15	2.23	11.14	2.14	2.17	7.22	0.35
Neighborhood satisfaction	2.91	2.92	3.07	2.89	2.87	3.44	-0.12
Neighborhood arrest rate	4.00	3.73	13.66	3.88	3.86	5.08	0.63
Family							
Family structure	0.38	0.40	4.99	0.40	0.39	5.90	-0.18
Parental supervision	3.63	3.59	8.96	3.62	3.60	10.74	-0.20
Attachment to parent	3.50	3.45	13.02	3.52	3.52	3.86	0.70
PC's edu. expect. of child	2.21	2.52	39.49	2.28	2.29	5.93	0.85
Attachment to child	3.49	3.62	31.16	3.54	3.55	3.27	0.89
Consistency in discipline	2.35	2.46	20.83	2.37	2.36	6.71	0.68
School Factors							
Commitment to school	3.01	3.19	48.77	3.05	3.08	10.05	0.79
Aspiration for college	3.24	3.55	35.89	3.34	3.31	5.96	0.83
Prosocial activities	1.71	1.76	6.90	1.74	1.77	5.59	0.19
Educational expectations	2.47	2.75	37.06	2.52	2.57	9.35	0.75
School clubs with friends	1.40	1.30	15.22	1.37	1.40	5.70	0.63
Peer Associations							
Peer delinquency	1.22	1.09	31.31	1.17	1.13	9.18	0.71
Peer drug use	1.26	1.18	20.53	1.23	1.21	7.00	0.66

Peer delinquent values	1.28	1.15	31.50	1.26	1.25	4.40	0.86

Table 9-11. Continued.

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Dating	0.38	0.32	12.69	0.37	0.40	6.18	0.51
Sexual activity under 15	0.31	0.23	17.67	0.31	0.33	5.62	0.68
Risky time with friends	1.98	1.85	22.01	1.96	1.98	9.97	0.55
Gang involvement	0.04	0.04	3.75	0.03	0.03	5.06	-0.35
Values							
Self-image	3.15	3.24	22.41	3.18	3.20	6.68	0.70
Depression	1.96	1.89	15.88	1.93	1.95	7.23	0.54
Delinquent values	1.30	1.24	16.45	1.29	1.27	5.77	0.65
Prior criminal behavior							
Violent crime (Time 1)	0.00	0.01	13.41	0.00	0.01	14.19	-0.06
Violent crime (Time 2)	0.01	0.00	11.69	0.00	0.01	22.94	-0.96
Violent crime (Time 3)	0.00	0.00	6.72	0.00	0.00	2.27	0.66
Violent crime (Time 4)	0.00	0.00	6.17	0.00	0.00	21.39	-2.46
Violent crime (Time 5)	0.00	0.00	0.00	0.00	0.00	0.00	
General delinquency	0.53	0.27	30.82	0.47	0.50	4.52	0.85
Drug use	0.07	0.03	17.97	0.07	0.08	5.43	0.70
Aggression	0.33	0.25	30.01	0.30	0.30	5.77	0.81
Prior Justice System Contact							
Previous contact or arrest	0.33	0.16	39.94	0.32	0.31	7.12	0.82
Global Covariate Balance Stats.							
Mean	---	---	18.67	---	---	7.29	0.34
Median	---	---	16.17	---	---	6.31	0.63
Maximum	---	---	48.77	---	---	22.94	0.89
Logit propensity score	-0.49	-2.42	132.55	-0.85	-0.93	5.21	0.96

Table 9-12. Treatment and control group covariate means and bias statistics before and after PSM for the police contact treatment (Low offenders).

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Demographics							
Race							
Black	0.62	0.68	11.67	0.65	0.65	3.21	0.73
Hispanic	0.20	0.15	13.04	0.18	0.17	4.11	0.68
Age	16.57	16.46	13.20	16.49	16.50	4.18	0.68
Family poverty	0.35	0.32	5.69	0.35	0.37	5.69	0.00
Neighborhood characteristics							
Proportion African American	0.53	0.57	15.37	0.55	0.54	5.55	0.64
Proportion in poverty	0.35	0.33	18.51	0.34	0.34	6.03	0.67
Neighborhood disorganization	1.67	1.70	5.18	1.70	1.70	1.75	0.66
Neighborhood integration	2.19	2.24	7.38	2.19	2.22	5.82	0.21
Neighborhood satisfaction	2.79	2.90	17.38	2.85	2.84	2.13	0.88
Neighborhood arrest rate	4.14	4.13	0.40	4.14	4.09	4.59	-10.35
Family							
Family structure	0.29	0.30	2.35	0.32	0.31	6.51	-1.77
Parental supervision	3.44	3.51	15.78	3.46	3.44	7.91	0.50
Attachment to parent	3.25	3.36	22.90	3.31	3.29	5.97	0.74
PC's edu. expect. for child	2.10	2.34	30.02	2.21	2.20	2.23	0.93
Attachment to child	3.30	3.48	35.29	3.38	3.38	3.88	0.89
Consistency in discipline	2.28	2.30	4.17	2.29	2.29	3.30	0.21
School Factors							
Commitment to school	2.95	3.03	20.29	2.99	2.99	2.90	0.86
Aspiration for college	3.20	3.36	18.65	3.30	3.31	3.38	0.82
Prosocial activities	1.61	1.65	6.69	1.62	1.61	2.97	0.56
Educational expectations	2.44	2.62	22.73	2.53	2.52	4.27	0.81
School clubs with friends	1.11	1.20	18.41	1.12	1.13	3.37	0.82
Peer Associations							
Peer delinquency	1.45	1.33	24.90	1.39	1.38	5.42	0.78
Peer drug use	1.55	1.40	29.91	1.49	1.50	4.79	0.84

Table 9-12. Continued.

	Before Matching			After Matching			
	M_{Xt}	M_{Xp}	d_X	M_{Xtm}	M_{Xcm}	d_{Xm}	%BR
Peer delinquent values	1.37	1.33	11.19	1.37	1.36	5.23	0.53
Dating	0.60	0.45	30.51	0.54	0.56	5.30	0.83
Sexual activity under 15	0.36	0.42	12.48	0.39	0.39	2.21	0.82
Risky time with friends	2.38	2.22	23.52	2.33	2.33	4.26	0.82
Gang involvement	0.40	0.26	30.67	0.34	0.35	4.49	0.85
Values							
Self-image	3.13	3.25	29.29	3.18	3.14	8.85	0.70
Depression	2.06	2.00	12.44	2.04	2.07	9.69	0.22
Delinquent values	1.53	1.40	30.91	1.50	1.51	8.48	0.73
Prior criminal behavior							
Violent crime (Time 1)	0.74	0.77	4.25	0.72	0.75	6.97	-0.64
Violent crime (Time 2)	0.47	0.52	7.80	0.48	0.47	4.20	0.46
Violent crime (Time 3)	0.56	0.43	19.37	0.49	0.46	6.06	0.69
Violent crime (Time 4)	0.46	0.34	20.92	0.41	0.40	2.80	0.87
Violent crime (Time 5)	0.48	0.37	16.42	0.44	0.45	3.74	0.77
General delinquency	2.44	1.11	63.50	1.82	1.76	3.96	0.94
Drug use	0.27	0.11	27.78	0.22	0.20	4.59	0.83
Aggression	0.54	0.40	37.29	0.48	0.50	5.46	0.85
Prior Justice System Contact							
Previous contact or arrest	0.45	0.39	11.82	0.42	0.44	4.56	0.61
Global Covariate Balance Stats.							
Mean	---	---	18.75	---	---	4.77	0.32
Median	---	---	17.90	---	---	4.53	0.73
Maximum	---	---	63.50	---	---	9.69	0.94
Logit propensity score	0.05	-1.01	101.15	-0.40	-0.41	0.71	0.99

Table 9-13. Post-matching short-run treatment effects of arrest for the total sample and by trajectory group.

	Est.	SE	t	d
Total Sample	3346.28	1692.02	1.98*	0.26
High offenders	---	---	---	---
Non-offenders	-164.08	190.80	-0.86	-0.12
Low offenders	862.37	568.00	1.52	0.31

** p≤0.01; * p≤0.05;

Table 9-14. Post-matching short-run treatment effects of police contact for the total sample and by trajectory group.

	Est.	SE	t	d
Total Sample	5696.27	2078.11	2.74**	0.30
High offenders	---	---	---	---
Non-offenders	378.50	312.55	1.21	0.18
Low offenders	1722.42	743.64	2.32*	0.35

** p≤0.01; * p≤0.05;

Table 9-15. Post-matching long-run treatment effects of arrest for the total sample and by trajectory group.

	Est.	SE	t	d
Total Sample	-503.31	1610.89	-0.31	-0.07
High offenders	---	---	---	---
Non-offenders	-285.96	269.49	-1.06	-0.33
Low offenders	125.47	475.17	0.26	0.04

** p≤0.01; * p≤0.05;

Table 9-16. Post-matching long-run treatment effects of police contact for the total sample and by trajectory group.

	Est.	SE	t	d
Total Sample	2538.03	1895.17	1.34	0.12
High offenders	---	---	---	---
Non-offenders	-48.19	329.39	-0.15	-0.07
Low offenders	1455.22	858.37	1.70	0.25

** p≤0.01; * p≤0.05;

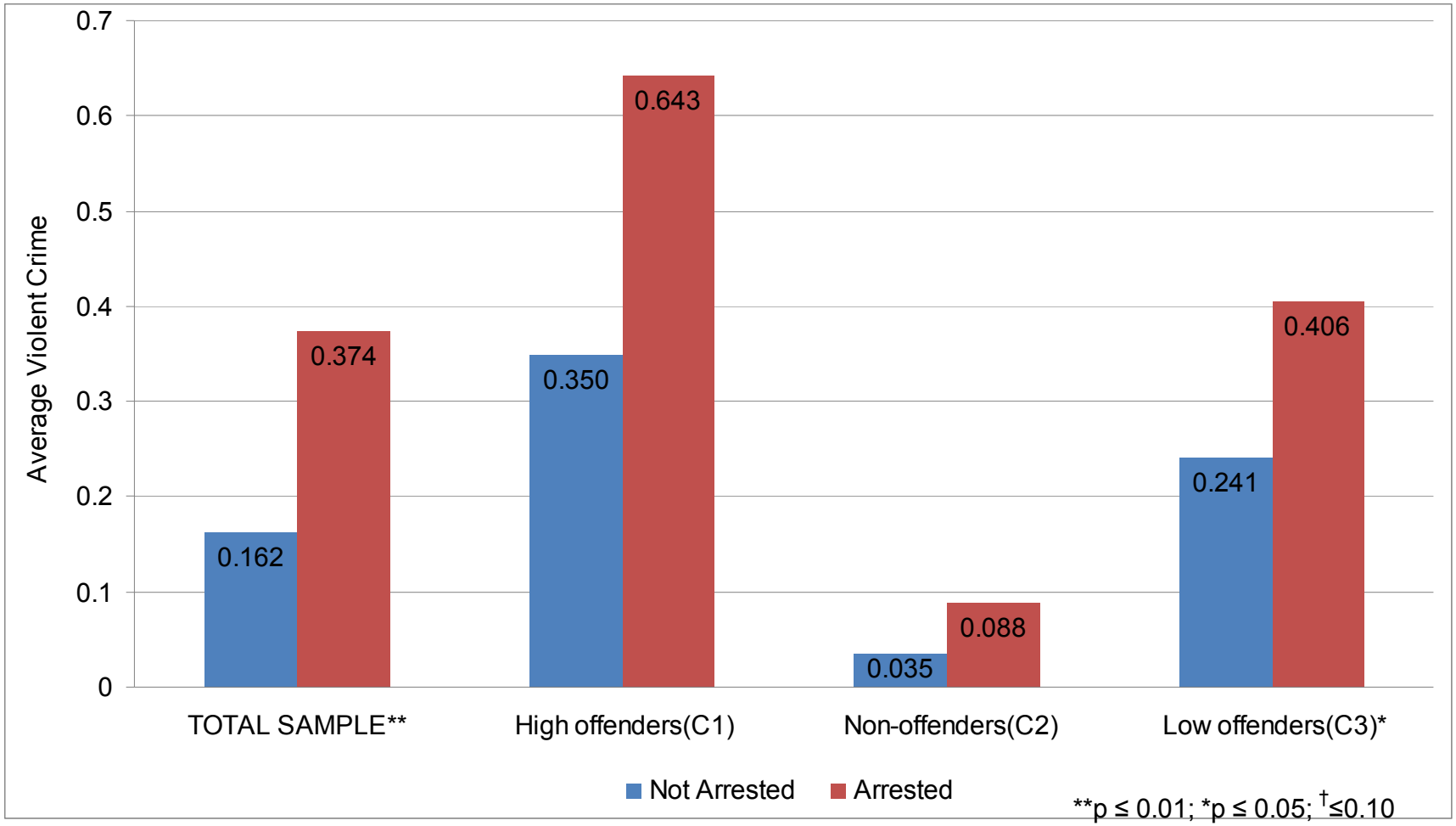

Figure 9-1. Short-run violent crime mean comparisons of those arrested and not arrested for the total sample and by violent crime trajectory group before matching.

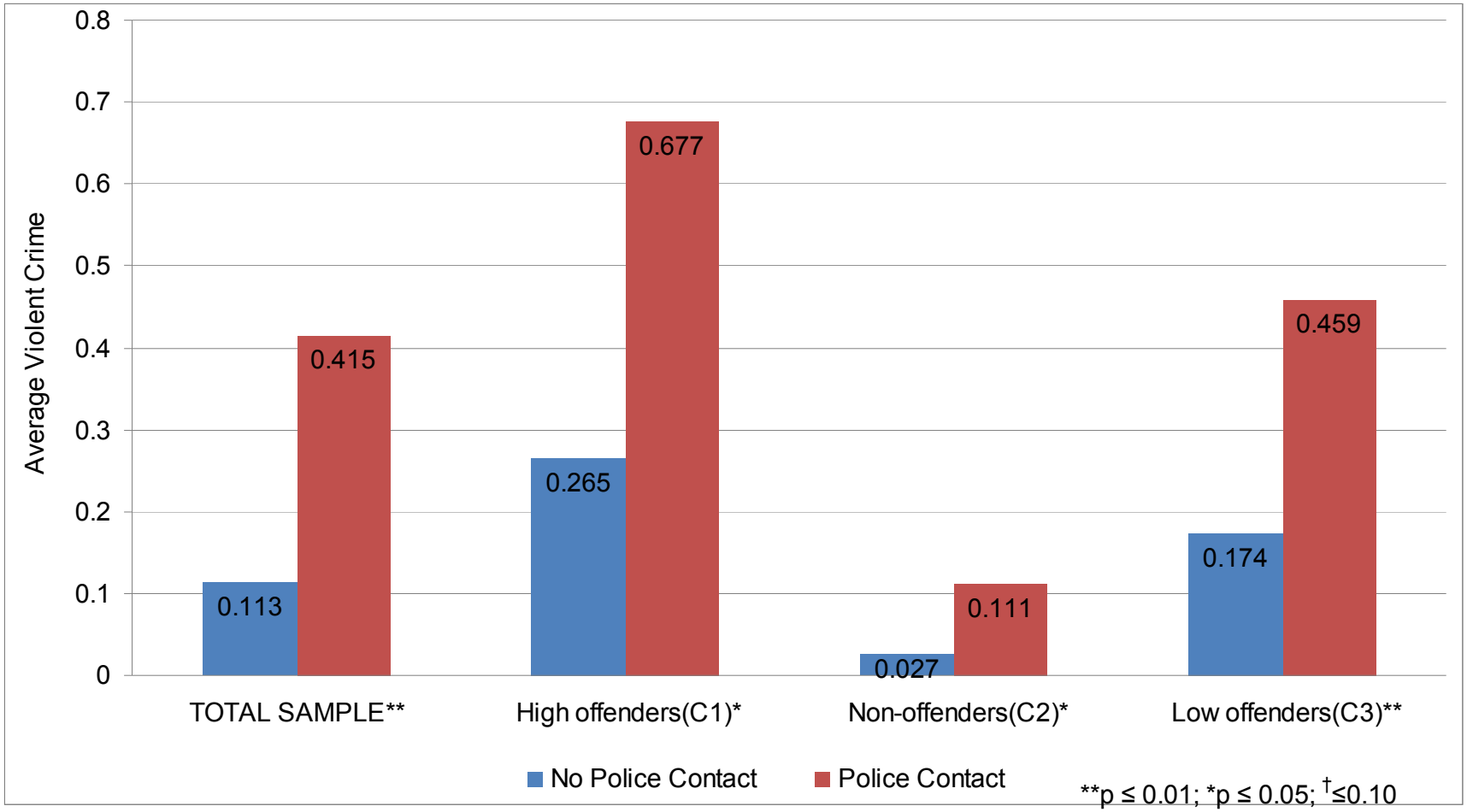

Figure 9-2. Short-run violent crime mean comparisons of those with and without a police contact for the total sample and by violent crime trajectory group before matching.

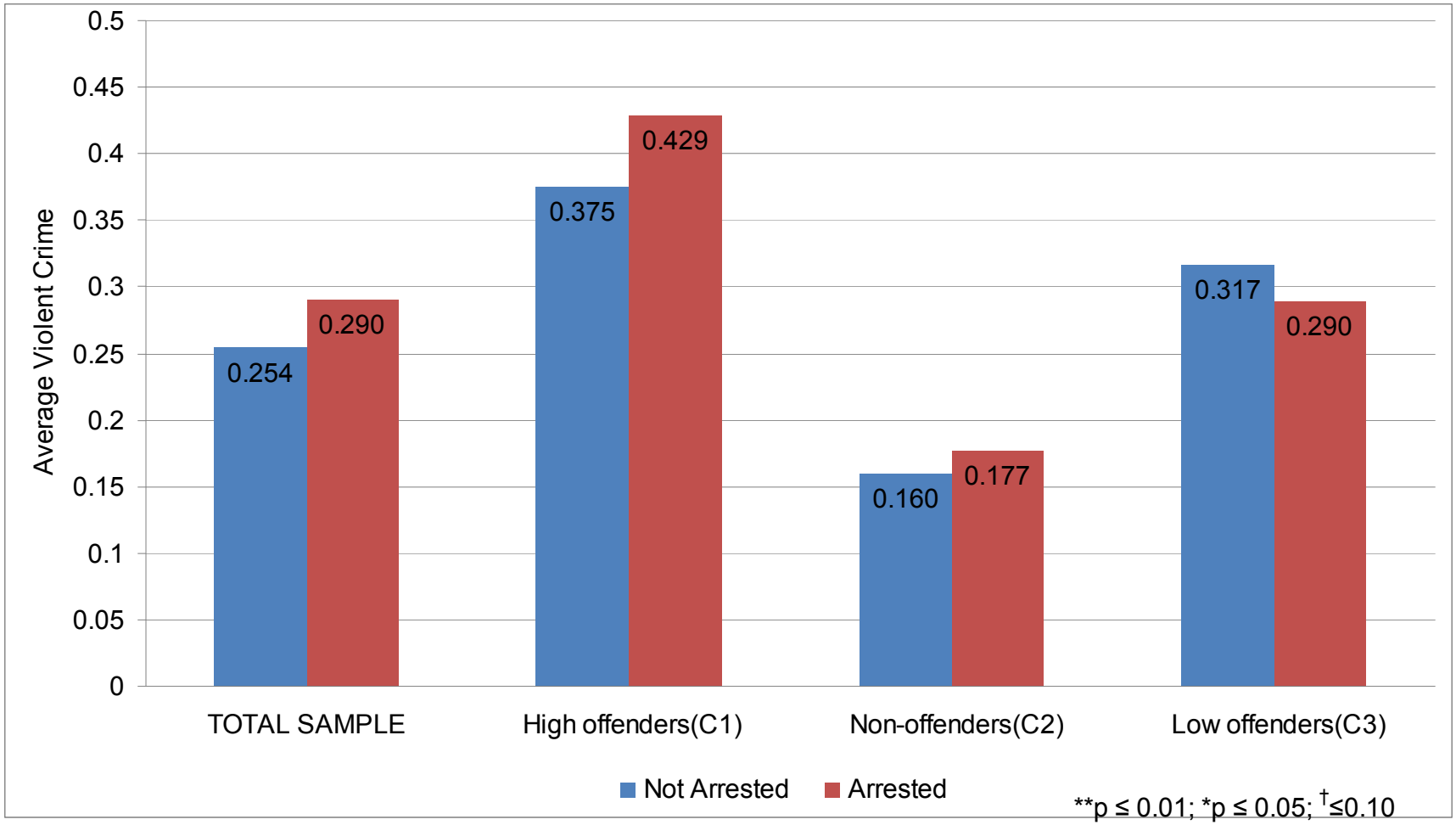

Figure 9-3. Long-run violent crime mean comparisons of those arrested and not arrested for the total sample and by violent crime trajectory group before matching.

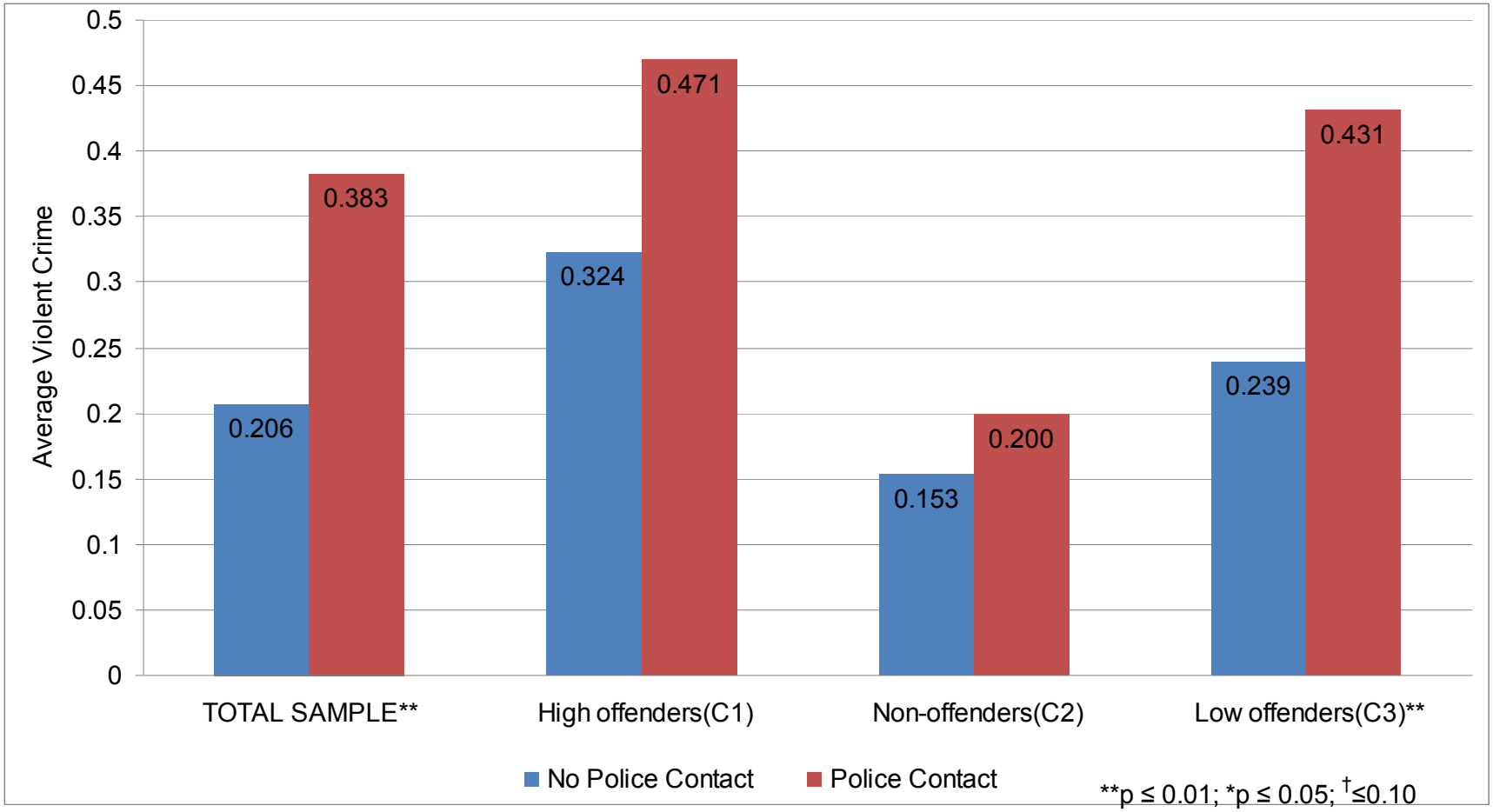

Figure 9-4. Long-run violent crime mean comparisons of those with and without a police contact for the total sample and by violent crime trajectory group before matching.

CHAPTER 10
CONCLUSIONS

Chapter 10, which is the final chapter in this work, provides a summary of the principal research findings and subsequently considers the limitations of the study. With these qualifications in mind, the implications of the study that can be safely drawn are discussed; more specifically, the relevancies of the findings for theories on sanctions effects and policies aimed at controlling violent crime are considered. Finally, Chapter 10 concludes with recommendations for future research endeavors seeking to examine the effects of official intervention experiences on subsequent crime in general and within a life-course and developmental framework specifically.

Research Summary

Using data from the male subsample of the RYDS, the current study examined the extent to which official intervention, such as officially recorded arrests and self-reported police contacts, influenced subsequent violent crime differently or similarly across violent offending subpopulations in both the short- and long-runs. To address the empirical aims of the current research, a three-stage LCGA-PSM integrated methodological approach was employed. First, latent class growth analysis was used to estimate the number and characteristics of violent offending subpopulations. Next, propensity score matching was employed to create quasi-equivalent treatment and control groups in the total sample as well as within each trajectory group specifically. In the current study, full optimal matching procedures were used to attempt to reduce or eliminate pre-matching biases on forty covariates. Finally, total sample treatment effects as well as trajectory group-specific treatment effects were estimated using the Hodges-Lehmann aligned rank test on the matched samples.

LCGA was employed to empirically determine the number of trajectory groups that best characterized the violent offending patterns in the data. Given the use of a count measure of violent crime as well as sufficient time points to estimate changes in offending over time, a Poisson LCGA was employed with all-quadratic polynomials. The results indicated that there was essentially a zero probability that a one, two, or five class model captured the offending patterns in the data. There was a non-trivial probability that a four class model was the correct model (28%) but the best evidence favored a three class solution (72%).

Following selection of the three class model, the order of polynomials was reduced such that two latent classes were characterized by linear and quadratic terms and one latent class was characterized with only an intercept. Empirical evidence strongly favored the more parsimonious three class model (2,0,2) over the more complex all-quadratic three class model (2,2,2). The mean posterior probabilities for trajectory group assignment for the parsimonious model were all greater than 0.81, which is well above the conventional cutoff of 0.7. Other model selection criteria also suggested the model was adequately precise in its classification.

Of the total analysis sample (N=595), approximately 11.4% were classified as high offenders, 39.3% as non-offenders, and 49.2% as low offenders. The offending trajectory of high offenders followed an inverted parabolic shape with offending peaking during wave three. Across all waves, the means for the high offenders were above one and this latent class was consistently the most problematic violent offending trajectory group across all time points. The trajectory of the non-offending group was flat by definition and the model estimated essentially zero acts of violence for this group across

all waves. Non-offenders are the trajectory group whose offending patterns, or lack thereof to be precise, were adequately captured with an intercept only. The low offending trajectory followed a j-shape decline. That is, the expected counts for the low offending trajectory group declined from waves two through four and then largely stabilized thereafter. Overall, there was strong evidence of between-individual stability in offending at the same time there was evidence of within-individual change in offending over time for the high and low offending trajectory groups.

Results of one-way ANOVAs and corresponding Tukey tests for specific group mean differences revealed that 30 of the 40 covariates were associated with trajectory group membership. There were noteworthy trends in the relationships between the covariates and the latent classes. For instance, there were minimal differences between the three trajectory groups with respect to the six neighborhood characteristic covariates but all family variables, all peer association measures and, not surprisingly, all prior behavior variables exhibited differences among some or all of the groups. The demographic, school factors, and values and mental states subcategories had a mix of their variables associated with trajectory group membership. Supporting continued between-individual stability in offending, the average short-run violent crime outcome was significantly the highest for the high offending group, second highest for the low offending group, and lowest for the non-offending group. This same general pattern of results is maintained in the long-run.

A sizeable proportion of the males experienced official interventions in the form of officially recorded arrests and self-reported police contacts. Specifically, 22% of the total sample was arrested and 32% reported a police contact during the treatment

period. High offenders experienced official interventions at the highest rate; 41% of this group was arrested and 50% had experienced a police contact. For the low offenders, approximately 24% was arrested and 37% had a police contact. Not surprisingly, non-offenders had the lowest proportion of individuals who had run-ins with the law. The proportion of individuals in this trajectory group that had an arrest or police contact was nearly 15% and 19%, respectively.

Bivariate relationships between official intervention experiences and subsequent violent behavior provided split support for labeling and null effects. No evidence was found that supported a short-run or a long-run deterrent effect of either measure of official intervention. In the short-run, all mean differences in violent crime for the total sample and across the three trajectory groups were in a direction consistent with labeling theory; however, some of these relationships did not reach statistical significance. Arrest was significantly associated with short-run violent crime for the total sample as well as for the low offending trajectory group. Police contact was significantly related to short-run violent crime for the total sample as well as for all three of the violent offending subpopulations.

In the long-run, all except one mean difference in violent crime (i.e., arrest treatment for the low offenders) were in a direction consistent with labeling theory; however, most of these relationships were not statistically significantly. Arrest was not significantly associated with violent crime in the long-run for any trajectory group, nor was it for the total sample. And, police contact was only associated with violence for the total sample and the low offending trajectory group. Thus, there is stronger evidence of

labeling theory in the short-run as compared to the long-run and no evidence that these types of official interventions curb subsequent violent offending behavior.

The optimal full matching procedure was highly successful in creating covariate balance for the aggregated sample for both the arrest and police contact treatments. Pre-matching mean covariate imbalances were approximately 29% and 26% of a standard deviation for the arrest and police contact treatments, respectively. The corresponding post-matching mean covariate imbalances dropped to approximately 4% of standard deviation in both cases. No covariates had variables that were substantially imbalanced following the matching procedure.

Matching was less successful in obtaining group equivalences within trajectory groups. On the one hand, very good covariate balance was achieved for the non-offending and the low offending trajectory groups for both the arrest and police contact treatments. For all of these balancing models, at least 96% of the bias in the logit of the propensity score was eliminated. And, the mean and median covariate imbalance was below ten in all cases. On the other hand, the full optimal matching procedure was unable to create treatment and control groups that were similar on the forty covariates for the high offending trajectory group. This was the case for both the police contact and the arrest treatment. Thus, treatment effect estimates are only presented for the total sample as well as the non-offending and low offending trajectory groups.

The main findings of the current research include mixed support for labeling effects in the short-run followed by consistent null effects in the long-run. For both the officially recorded arrest and self-reported police contact treatments, there are statistically significant short-run effects of official intervention on violent crime for the

total sample. The effect sizes are 0.26 and 0.30, respectively, which are both characterized as relatively small effects. Nevertheless, the fact that official intervention influences subsequent violent behavior in the short-run after accounting for forty covariates is noteworthy. The only trajectory group-specific effect is that of police contact on short-run violent crime for the low offending trajectory group. All other trajectory group-specific effects in the short-run are not statistically significant and all long-run effects, for the total sample and for all trajectory groups, are also not significant.

In sum, when individuals are successfully matched on forty covariates there is only evidence for a short-run labeling effect of the police contact treatment for the low offending trajectory group. For the matched, aggregated sample (which includes high chronic offenders who could not be matched within their trajectory group) there is evidence for short-run labeling effects for both measures of official intervention. Perhaps the clearest conclusion is that the experience of police contact and arrest appears to not have any crime reducing effects in the short- or long-run; this is the case for the total sample generally and each of the distinct violent offending trajectory groups specifically.

Limitations

While the current study has attempted to address important policy-relevant questions, which were illuminated when placing sanction effects in a developmental and life-course perspective, using both a high-quality longitudinal data set and sophisticated statistical analyses, it is certainly not without limitations. While it was argued that the RYDS data were among the best available data sets to address the empirical aims of the current study, the measurement of certain variables could have been better. Recall,

for instance, that there were few differences in neighborhood characteristics across the violent offending subpopulations, which seems to counter to the basic idea that social structure should be related to individual criminal behavior (e.g., see Akers, 1998). Several neighborhood factors are determined solely by the primary caregiver's perceptions of the neighborhood. Specifically, disorganization, satisfaction, and integration are neighborhood level phenomena that are being estimated by a single individual. These perceptions probably do not fully represent the neighborhood on the whole. Therefore, the extent to which these variables truly are accounted for in the analysis is unknown. While these neighborhood measures are indeed relatively weak indicators of the neighborhood level constructs, they do permit some degree of control over these factors. As such, one is probably better off including these variables in the propensity score models than excluding them. Other neighborhood measures are calculated from US Census or other official data so the aforementioned criticism does not apply to all of the neighborhood characteristics that were controlled.

More important than these more minor issues concerning the measurement of certain covariates are the limitations with the conceptualization, operationalization, and measurement of the two most important variables in the study, namely violent crime and official intervention. A variety index was selected as the variable to measure violent crime. This was selected given the fact that variety indexes have been found to be among the most reliable forms of self-reported measures of crime and deviance (Belson, 1986; Hindelang, Hirschi, & Weis, 1981). However, there are other conceptualizations of violent crime including prevalence (i.e., whether one commits any acts of violence at all) and frequency (i.e., the total number of violent acts one commits).

There are naturally some interrelationships between these different facets of the criminal career though they are distinct concepts. And, some have maintained that these different conceptualizations of offending behavior may have different causes (Blumstein, Cohen, & Farrington, 1988). Future research might seek to compare the effects of official intervention on prevalence and frequency in addition to variety as it could be the case. In addition, while there were practical reasons for focusing the current investigation on violent crime, there is a need to examine how the effects of arrest and police contact may influence other types of crime and delinquency. In short, additional research should consider both which types of criminal behavior and which specific dimensions of the criminal career official intervention experiences may influence.

One of the strengths of the current study was that it examined two types of official intervention. However, the police contact and arrest treatment variables came from two different data sources. To some extent, this limits the ability to compare the differences in the gradation of offending. Ideally, one would want to directly compare the effects of police contacts and arrests on subsequent offending behavior that come from the same source such as official records. In addition, it would have been ideal to have a large enough sample that one could have also examined the effects of stronger sanctions including incarceration. While examining the overall effects of incarceration on crime is often possible with general population samples, it is difficult to estimate trajectory group-specific effects given the relative infrequencies of these official intervention experiences. This was the case with the current study and therefore it is limited in this regard. Future research should seek to address this problem.

An additional point of consideration regarding the treatment variable is the fact that justice system contact is often not simply a one-time event. It is important to recall that contact with the justice system for this sample prior to wave seven was considerable (see Table 6-2), which of course means that for some of these individuals the official intervention experience during the treatment period was not a novel one. Then, an alternative and probably more realistic conceptualization of official intervention is to consider it as a repeatable treatment, rather than a static one.

The research questions necessitated the establishment of some period that would constitute a treatment period. However, it should be acknowledged that official interventions are repeatable treatments possibly occurring before, during, and after the designated treatment period. For some individuals, the ramifications of official intervention such as deviance amplification may have already unfolded. While propensity score analysis permits one to match individuals on previous treatment experiences and thereby obtain an unbiased estimate of current treatment experiences, the method does not necessarily capture first time treatment effects which might be more, or less, pronounced than those occurring during the selected treatment period. This limitation is not unique and is relevant to numerous other studies on sanction effects including those not focused on assessing how prior criminal history may moderate the effects of official intervention on criminal behavior.

The current study has addressed a first-order issue: does the effect of official intervention vary across violent offending subpopulations and are any significant effects different in the short- and long-runs? While there is some support for the deviance amplification hypothesis in the short-run, lacking from this research is an assessment of

the specific mechanisms by which arrests and police contacts may lead to increases in subsequent violent behavior. Labeling theory is explicit in the identification of mediating pathways that involve blocked access to conventional opportunity, alteration of self-identity, and acquisition of deviant peers (Paternoster & Iovanni, 1989). Without focusing on these pathways in particular, the current research can only identify that there is mixed support for the basic idea of deviance amplification and cannot speak to whether the data truly support a labeling process as specified by the theory. In addition, using propensity score matching analysis to address first-order causal issues may complicate secondary efforts to obtain information regarding which covariates might explain away the bivariate association between official intervention and subsequent violent offending. For instance, it might be a specific variable that renders the bivariate relationship between a treatment and an outcome non-significant. Regression analyses more easily permit an assessment of the influence of adding specific variables on the relationship between the independent and dependent variables of interest. Nevertheless, propensity score matching methods mimic experimental designs with observational data, providing a scientifically sound and transparent approach to identify whether non-spurious relationships exist. And, this is arguably the more important question for policy if not also for theory.

While the integrated research methodology has supported strong causal inference for the total sample and for two of the three trajectory groups, treatment effects could not be estimated for arguably the most important offending subpopulation, namely high offenders. High offenders are a small proportion of the population but they are the most problematic with respect to their offending and, therefore, the group that is central to

policy efforts aimed at controlling violent crime. With respect to these high offenders, it is known that the treatment(s) and outcome exhibit relationships at the bivariate level in the short-run that are clearly in a direction consistent with labeling theory (see Figures 9-1 and 9-2); in one case, this bivariate relationship not only appears large visually but also reaches statistical significance (see Table 9-2). Since the current study could not create adequate balance across the forty covariates for the high offending trajectory group, however, it remains unknown whether arrest and/or police contact experiences is causally related to subsequent violent behavior.

A related point of discussion, which some might argue is a limitation of the current analysis, deals with the procedure of selecting between two LCGA models that have non-trivial probabilities of accurately representing the data. Recall, the estimated four class model essentially split the low offenders into a rising and declining group. The author is on solid ground for selecting the three class model for two reasons. Empirically, the three class model was the best choice given the model selection and model adequacy assessment criteria. Practically, the three class model was more parsimonious and helped to ensure trajectory groups were as large as possible. This latter point proved to be important as treatment effects could not even be estimated for the high offending group. A four class model may very well have created matching difficulties for individuals classified in either of these two alternative groups. Nevertheless, estimating trajectory group-specific treatment effects using the three class model rather than the four class model could be seen as running counter to a main goal of the study. For those on a declining trajectory, for example, perhaps official intervention experiences might hasten the desistance process (deterrence) or cause the

individual to reverse this trend toward conformity (labeling). Important policy-relevant knowledge might be obtained if one were to successfully match individuals on official intervention experiences in these alternative groups.

The limitations of prior research that the study attempted to address were many and varied. In addressing these limitations and by placing official intervention effects research in a developmental and life-course context, the current study has done much to organize theoretical thinking about how sanctions may influence subsequent behavior and has provided an advanced statistical analysis to test these ideas. Despite going to great lengths to overcome limitations of prior studies, the current study has a number of issues that lead us to accept the conclusions with some degree of caution. Unfortunately, many of the aforementioned limitations could not be overcome with the data brought to bear on the research question and/or the methodological approach to the topic of study. Fortunately, many of these issues that were raised lead to additional avenues of research, some of which have been discussed here briefly and all of which will be discussed in the final subsection of this work.

Discussion and Implications

Notwithstanding the limitations of the study, there are important implications of the findings for theories on sanction effects and for policies centered on controlling violent crime. The results of the current study would seem to be somewhat damning to specific deterrence theory. There was no empirical evidence, even at the bivariate level, which supported the deterrence doctrine for the total sample or for any trajectory groups in particular. These results were consistent in the short- and long-runs and across two different measures of official intervention including self-reported police contacts and officially recorded arrests. One limitation of the current study may shed some light on

the lack of support found for the deterrence doctrine. The study used arrest and police contact variables as official intervention treatments. While certainty of punishment has been the primary focus of empirical research, deterrence theory calls for certainty, severity, and celerity of punishment and these elements may interact to influence behavior (Stafford et al., 1986). Perhaps, an official intervention in the form of an arrest or police contact is insufficient by itself to trigger the desired behavioral response. That is, it could be the case that an actual punishment received by a judge might be necessary to invoke specific deterrence because an arrest alone is technically not a punishment. Perhaps, researchers should examine the interactions among the elements of punishment (Stafford et al., 1986) and/or explore the effects of more serious official interventions. Regarding the latter, sanctioning a youth for his or her violent offense might make a labeling effect more likely. Thus, there is reason to believe that both deterrent and labeling effects may be higher as justice system involvement increases.

While the data did not support the main hypothesis of specific deterrence theory, mixed support was found for ideas underlying labeling theory. That is, individuals who experienced a police contact fared worse than those who did not. As discussed, however, this support was equivocal as well as dependent on the outcome assessment time period and on trajectory group membership. Before discussing the trajectory group-specific effects and differences in the findings in the short- and long-runs, some attention should be given to whether the results generally support the principal idea of "deviance amplification." The deviance amplification hypothesis maintains that an individual's offending patterns worsen as one goes from primary deviance to secondary

deviance (Lemert, 1951). The mediating pathways—blocked access to conventional opportunities, alteration of self-identity, and acquisition of delinquent peers—suggest that offending behavior should increase in quality and quantity (Link et al., 1989; Paternoster & Iovanni, 1989). This occurs because an individual now has little choice but to offend, comes to see oneself as a criminal, and is surrounded by individuals who are likely to exposure him or her to techniques and reinforce their violent behavior. Overall, this suggests official intervention will result in a turning point in a criminal trajectory that results in more problematic offending behavior than at the time of the official intervention (see Figure 1-1). When considered in a developmental and life-course perspective, however, this implies that there should be significantly more violence than would be expected at some time in the future had the individual not experienced justice system contact. Thus, it is important to consider what the offending trajectory was prior to the intervention such as whether it was trending upward or downward.

For the aggregated sample, the mean number of distinct violent crime acts was 0.36 in wave six, which was down from a high of 0.56 in wave two. Hence, the trajectory for the aggregated sample was declining between waves two and six (see Figure 8-1). The average number of distinct acts of violence that was committed during wave nine was 0.374 for the arrest group and 0.162 for the non-arrest group (see Figure 9-1). For the police contact treatment, these mean numbers were 0.415 and 0.113, respectively (see Figure 9-2). Both of these average values at wave nine for the treatment groups represent modest mean increases over the total sample mean at wave six. When viewed in this light, deviance amplification is not especially large.

When considering the wave nine outcomes, there are statistically and practically significant differences in the mean violence levels for those who experienced official intervention and those who did not. For the total sample, these relationships held when accounting for forty covariates in the short-run for both outcome measures (see Tables 9-13 and 9-14). Taken together, this provides evidence for a causal effect of official intervention on subsequent violent offending in the short-term. Criminal sanctions make matters worse in the short-run for this sample on the whole. Whether these findings are also indicative of an "amplification" effect is subject to interpretation. For some, this issue may be paramount to fully evaluating the efficacy of labeling theory and for others this may be largely irrelevant. Future theoretical discourse should expand upon this discussion.

The findings reveal no evidence that official intervention serves as a different type of turning point for individuals following along distinct violent offending trajectories. That is, arrests or police contacts do not exacerbate violent behavior for some offending subpopulations but alleviate problem behavior for others. Nevertheless, there is support for short-run trajectory group-specific treatment effects such that official intervention serves as a turning point in the criminal trajectories of low offenders but not for non-offenders (treatment effect estimates could not be obtained for high offenders).

Why might there be a statistically significant labeling effect for a low offender but not a non-offender? Recall, there were non-trivial differences between the low and non-offending groups for at least one or more of the covariates across the demographic, neighborhood, family, school, peer association, values and mental states, prior criminal behavior, and justice system contact subcategories (see Table 8-7). These statistically

significant mean differences are largely in directions that are consistent with expectations from a variety of criminological theories. To name a few of these differences, non-offenders had more intact family structures, greater attachment to his or her parent, higher aspirations for college, lower delinquent peers, weaker delinquent values, and lower prior justice system contact. Collectively, these factors might indicate that a youth has a greater stake in conformity and/or resources to thwart adverse effects of deviant labels. Those who are low offenders are, by definition, on an offending trajectory that is sandwiched between high offenders and non-offenders. These two types of individuals may have largely subscribed to nonconformity and conformity, respectively. Alternatively, low offenders might be more easily swayed one way or another. The evidence suggests that low offenders' violent offending trajectories take a turn toward nonconformity following the experience of an official intervention.

It is unlucky that treatment effects could not be estimated for the high offending group. Some suggest that the effect of a deviant label will level off once an individual has charted a course of problematic behavior (Paternoster & Iovanni, 1989). The idea is that while chronic offenders may be less susceptible to official intervention effects in later adolescence, this does not necessarily mean that these individuals are not responsive to punishments in earlier developmental stages.

Bivariate evidence suggests that if the independent variable of interest had any causal effects on violent crime it would make the violence of high offenders worse. While the author is unable to discuss treatment effect estimates for the high offending group, bivariate results provide a certain degree of relevant information (see Figures 9-1 and 9-2). While there are statistically significant short-run bivariate effects for low

offenders across both official intervention measures, there is only a statistically significant short-run bivariate effect for high offenders for the police contact treatment—although the practical mean difference between the treated and control groups for the arrest treatment is also quite large. Then, labeling effects may still be observable even for those who are fairly committed to violent behavior, but future research is needed that addresses this specific issue.

Sampson and Laub's (1997) discussion regarding the short- and long-run effects of criminal sanctions over the life-course is an important theoretical advancement. They suggest that labeling is a process that unfolds over time and, as a consequence, one might be more likely to observe differences in behavior once sufficient time has passed for one's conventional opportunities to be blocked, self-identity to be converted, and peer influence to become problematic. The empirical findings are not consistent with their predictions regarding a long-run labeling effect. Rather, they are supportive of short-run labeling effect followed by long-run null effects for the total sample. Additionally, there is partial support for this same pattern of results for the low offending trajectory group.

Why do short-run labeling effects vanish in the long-run when theory predicts that labeling effects in the long-run should actually be stronger? Aside from the possibility that the theory is simply wrong, there are some reasons why these findings may not be entirely surprising. Police contacts and arrests are repeatable treatments. This means that approximately three years had passed from the treatment period to the long-run violent crime observation period. During this time, individuals may or may not have experienced additional arrests and/or police contacts. The treatment and control groups

that were highly balanced across the forty covariates including prior justice system contact and violent offending behavior become less comparable as time passes. In essence, the treatment and control groups can be thought to be blending together in a sense, which leads to the washing out of any long-run treatment effects.

The issue of repeatable treatments is probably greater for police contacts and arrests relative to criminal sanctions such as incarceration. Hence, incarceration is a less common experience and there would be less mixing of the treatment and control groups long-term. Incarceration is also a more serious type of sanction that can be seen to more easily block one's access to conventional opportunities (e.g., reporting of felony status on job applications), alter one's identity (e.g., one is sentenced for a crime), and expose one to deviant peers (e.g., socialization in jail/prison). In short, it might be expected that long-run sanction effects would be more likely to be observed for more serious types of official intervention. In short, what seems to be clear from the analysis is that labeling effects are confined to the short-term, which is probably a product of the fact that official intervention, especially arrest and police contact, is a repeatable treatment. Testing the long-run effects of official intervention on subsequent offending might be best accomplished by employing incarceration as the treatment experience and/or running additional analyses on subsamples of individuals who did not crossover with respect to their treatment status.

The analysis raises another poignant question requiring discussion: why is the labeling process triggered by a police contact for low offenders but not an actual arrest? Even at the bivariate level, there were statistically significant differences for all three trajectory groups in the short-run for the police contact treatment (see Table 9-2) but

only for low offenders for the arrest treatment (see Table 9-1). Thus, there seems to be more evidence for labeling with a treatment condition that is conceivably less serious. These findings may be the result of different sources of data. Recall, arrests are officially recorded whereas police contacts are self-reported. There were more individuals who self-reported a police contact than experienced an officially recorded arrest (cf. Figures 8-3 and 8-4). The self-reported measure may have captured official intervention experiences that were missed using official data. For instance, if an individual was arrested outside the relevant jurisdictions then this official intervention experience would not be examined and this individual might be classified as not have received official intervention. However, the self-reported police contact measure would register any police contact. Future research that uses either official records or self-reports is needed to more directly compare the effects of different types of official intervention on crime and violence.

The results suggest that intervening in the lives of youth that are more or less experimenting with violence (i.e., low offenders) may lead to the augmentation of this form of criminal behavior. While this might be true for the other trajectory groups as well, this study did not support these conclusions for non-offenders when accounting for forty covariates and did not provide the opportunity to examine treatment effect estimates for high offenders. Ironically, controlling violent crime for low offenders might be best accomplished by leaving this group to their own devices. The findings of a significant labeling effect echo Moffitt's (1993) concerns that adolescence-limited offenders might become "snared" by the criminal justice system causing the normal desistance process that occurs for this group to go awry.

If police contacts are sufficient to set in motion the labeling process, the result is a catch twenty-two. While police can use their discretion to decide whom or when to make an arrest, contacting an individual who is suspected to have committed a crime is the mark of sound police work. Failing to perform job functions is obviously problematic in many ways but this research suggests that actually performing them might very well be problematic as well—at least from the point of view of controlling subsequent violent crime. There are issues pertaining to justice and fairness that support official intervention in all instances in which a crime has occurred. And, for sanctions beyond those studied here, incarceration might hold promise for controlling crime, albeit temporarily, via incapacitation.

The implications of the current research for policy are such that nonintervention should be considered as a response to the violent offending of low offenders if the goal is to minimize, or more precisely fail to worsen, their offending behavior. The findings here suggest that official intervention is the antithesis of a panacea for the violent crime problem. In other words, efforts aimed at controlling violent crime should look well beyond traditional approaches to criminal justice. For instance, taking a public health approach to the study of violence might help to redefine the problem and involve more resources and agencies in the process of devising and implementing sustainable solutions to the problem at hand. With that said, there are a number of avenues for research that address the limitations of this study and studies that have been conducted to date that should be explored before official intervention is deemed an ineffective approach to curbing crime. These are now discussed.

Directions for Future Research

The current study provides us with some answers to several important research questions pertaining to the effects of official intervention on subsequent violent offending behavior. However, these answers are certainly not the final word on the matter; rather, the study as a whole is probably better seen as a framework around which future research can build. While additional studies are needed on the moderating role of various factors including race, gender, and stakes in conformity, the potential for policy-relevant information stemming from theoretically driven investigations on the moderating role of criminal history helps to ensure this is an area that will continue to receive empirical attention in the future. Assessments of the ideas advanced by various deterrence, labeling, and developmental and life-course scholars might better inform public policies on the control of crime and violence and provide stronger tests of theory if they adhere to some basic recommendations. These suggestions for future research are primarily geared toward observational studies.

Future research is needed that compares different gradations of official intervention to better assess whether labeling or deterrent effects are more likely at different stages in the justice system. Ideally, a study would examine four different official interventions including police contact, arrest without a formal sanction, arrest followed by an alternative sanction, and arrest followed by a stint in a jail or prison. Doing so would help to determine whether labeling and/or deterrent effects were triggered with none, one, some, or all types of justice system contact. To prevent any differences being attributable to methodological artifacts, similar sources of data, such as official records, should be used whenever possible to examine the effects of the different treatments on subsequent offending behavior.

Future research should also compare the results of analyses that use official records with those that identify official intervention experiences with self-report data. Both methods of measuring official intervention have certain limitations. Official records are probably quite valid for arrests and stronger sanctions that follow an arrest. However, if one has data on official justice system contact only from a single agency then certain criminal sanctions may go unanalyzed. In addition to this problem, the validity of official records may be problematic for police contact experiences specifically. These may be documented less consistently and systematically by police officers. Self-reported data might be more valid in some instances but a variety of factors could influence a respondent's answers to various survey items. Then, future studies should use both forms of data when possible. Despite the limitation of self-report information in general, offending behavior is better captured with this type of behavioral measure. Official records will substantially underestimate the true number of offenses that are being committed. As a general rule, future research should seek to use self-report data to measure offending behavior as well as other covariates that serve as control variables. Researchers should use official records data for official intervention experiences and, when available, supplement this with self-report data for comparison purposes.

Studies examining sanction effects should also attempt to investigate the effects of an official intervention on prevalence, variety, and frequency of offending within the same study. Employing these different behavioral measures allows one to assess to what extent justice system contact terminates a criminal career, to what extent it limits the number of distinct acts that an individual engages in, and to what extent it reduces

the total number of offenses an individual commits, respectively. This may be considered in conjunction with the aforementioned considerations regarding the exploration of different gradations of criminal sanctions. For example, deterrence may lessen the frequency or variety of offending only if an individual is actually sanctioned with a fine or community service (not just arrested) but may actually result in the cessation of offending altogether if the individual experiences a highly undesirable outcome such as incarceration. As another possibility, a criminal sanction might differentially influence frequency and variety of offending. Future research should consider these possibilities.

Research on sanction effects needs to continue to examine behavior other than violence and compare differences in the effects of official intervention across different types of behavior. While certain data sets may not be able to support such specificity, the effects of criminal sanctions would ideally be examined both on the specific behavior one was sanctioned for and on offending behavior more generally. That is, if an individual was arrested for the crime of assault, one might seek to determine the deterrent or labeling effect of the official intervention on several behavioral outcomes including assault, violent crime, and criminal behavior in general. This speaks to the specificity or the generality in the effects of official intervention on subsequent offending behavior.

All of these recommendations thus far have not considered the developmental and life-course aspects of sanction effects research. Still, there is a healthy set of issues to address already. I conclude this work by making some recommendations for future studies that seek to explore differential sanction effects for fundamentally different types

of offenders and/or to examine divergent short-run and long-run effects. Of course, the general considerations discussed to this point should also be kept in mind for investigations into sanction effects that are informed by ideas from developmental and life-course criminology.

While the current research did not result in the conclusion that sanction effects operated in opposite directions for distinct violent offending subpopulations, there was some evidence that sanctions mattered more for low offenders relative to non-offenders. Given the acknowledgment that a small proportion of individuals are responsible the bulk of the crime problem (Wolfgang et al., 1972), it is highly important to evaluate the causal relationship between official intervention and subsequent offending behavior. As it turned out, the treatment effects for high offenders could not be estimated given adequate covariate balance could not be achieved. Obtaining group equivalences on covariates for chronic offenders may be most challenging without a fairly large sample size since they only represent a small proportion of offenders (see also Haviland et al., 2007; 2008). Therefore, future research should seek to use other data sets that may have larger sample sizes or might be able to create covariate balance for small, high offending trajectory groups. Relatedly, in cases where the data are not stretched beyond capacity, future research might explore trajectory group-specific effects of alternative latent class growth analysis models when these alternative models have a sizeable probability of representing the data, albeit one that may not the largest. For instance, researchers might consider reporting the results of analyses from both a three class model and a four class model should there be a non-trivial probability that each model is the correct one and the more complex model yields information may be

relevant for public policy. In short, research should continue to consider official intervention experiences as life transitions that may be turning points, for better or worse, in the lives of different types of offenders.

Future research should find promise in the integrated methodological approach used in this study that blends latent class growth and propensity score matching analyses. This methodology is especially well-suited to investigate whether a life transition serves as a turning point in a criminal trajectory for different types of offending subpopulations. However, it was acknowledged that official intervention is technically a repeated treatment and the integrated LCGA-PSM analysis does not directly deal with this problem. To address this concern, an alternative methodological approach to the one taken here might be to combine latent class growth analysis with inverse probability-to-treatment weighting (IPTW). To my knowledge, no study to date in the field of criminology that has employed this methodological design, but it might be fruitful for sanction effects researchers to consider doing so. IPTW permits a researcher to assess whether there is intra-individual change in offending behavior that is a function of intra-individual changes in treatment status while weighting the data to address confounding (Samspon, Laub, & Weimer, 2006). At the same time, the question as to whether official intervention serves as a turning point in a criminal trajectory may not be as straightforwardly answered with this approach. Thus, considering official intervention as a repeatable treatment will result in the need to carefully consider the implications for testing theories on sanction effects in a developmental and life-course perspective, but this is no reason to shy away from this potentially important empirical endeavor.

The issue of repeatable treatments also has implications for investigating long-term sanction effects. There are difficulties in estimating the effects of criminal sanctions in the long-run. As was previously discussed, individuals who did not experience an official intervention during the treatment period may experience one later and those who did experience one during the treatment period may not subsequently experience one. The groups that were formed during the treatment period are probably less comparable the later in time one examines the effects of official intervention. Still, this doesn't change the fact that investigations into long-run sanctions effects are theoretically important (Sampson & Laub, 1997). Future research might conduct additional analyses to determine the extent which these diminishing effects with time are the result of temporally sensitive deterrent or labeling effects or whether they are simply observed due to treatment experiences following the designated treatment period. Haviland and colleagues (2007; 2008) provide some guidance for conducting such an analysis.

Finally, future research should continue to employ strong methodological approaches such as propensity score matching analysis in conjunction with latent class growth analysis to address similar research questions raised herein. While propensity score analyses support strong causal inference in general, little is known about the best methods to use when the technique is integrated with latent class growth analysis. Propensity score methods give the researcher a multitude of options, which may be overwhelming at times. There is more than one way to develop a treatment model (e.g., theoretically-driven, empirically-driven), more than one approach to estimate propensity scores (e.g., logistic regression, probit regression), more than one strategy to address

confounding (e.g., matching, weighting, stratification), more than one way to employ a chosen strategy such as matching (e.g., optimal or greedy; 1:1, 1:K, or full), and more than one approach to evaluate the successfulness of the propensity score analysis for creating equivalent treatment and control groups (e.g., standardized bias statistics, t-tests). It would appear then that these advanced, integrated methods are in need of additional research and possibly simulation analysis to help guide future research endeavors. While some simulation analysis has been conducted on propensity score methods generally, it might be helpful to conduct these informative studies on data that employs LCGA-PSM integrated methods. Ultimately, propensity score analysis hinges on its ability to create covariate balance on the covariates used to predict treatment status but some approaches may do this better than others in the context of trajectory group-specific treatment effects and future research should seek to determine this to make the statistical analysis more accessible to researchers.

This discussion has outlined an agenda for sanction effects studies that touches on many concerns of other scholars and introduces several new issues to consider. Taken together, there is a demonstrated need for future research on the topic to address certain limitations of the current study as well as weaknesses of sanction effects studies in general. Notwithstanding the discourse on future research, the current study has taken an important step in the right direction by raising policy-relevant, developmentally sensitive empirical questions regarding the effects of official intervention on subsequent violent offending behavior and addressing them with carefully chosen data and quantitative techniques that are well-suited for the task.

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BIOGRAPHICAL SKETCH

Jeffrey T. Ward is a Maryland native who earned his Bachelor of Science (2005) degree from Rutgers, The State University of New Jersey, where he graduated *summa cum laude* and double majored in physics and psychology. He earned his Master of Arts (2007) and Doctor of Philosophy (2011) degrees from the University of Florida, where he majored in criminology, law and society. He was also awarded a Social Science Methodology Graduate Certificate (2011) from the University of Florida. His substantive areas of research include: sanction effects; violence and gangs; developmental and life-course criminology; and space, place, and crime. In addition to these substantive research areas, he is also highly interested in quantitative methodology and psychometrics. He is a recipient of the prestigious Harry Frank Guggenheim Dissertation Fellowship and his work has been published in some of the leading journals in the field of criminology. He begins his academic career as an assistant professor in the Department of Criminal Justice at the University of Texas at San Antonio.