

ABSTRACT

Title: DETENTION AND DOSAGE: UNDERSTANDING THE EFFECTS OF INCARCERATION ON FIRST-TIME ARRESTEE JUVENILE DELINQUENTS

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Master of Arts, 2009

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This thesis examines the relationship between detention and future recidivism for first-time arrestee juvenile delinquents. Labeling and deterrence theorists have each hypothesized and investigated the effects that official sanction will have on future delinquency. Empirical evidence has presented results both supporting and refuting the labeling effect of incarceration. However, many prior studies have not adequately taken the problem of selection bias and temporal ordering into account. In addition, past research often measures all detention experiences as equal with no consideration that the ‘dosage’ of time spent in detention has a varying effect. The current study follows first-time juvenile offenders in New York City for 18 months. Utilizing propensity score matching to balance the punishment groups on preexisting characteristics, results indicate that while an incarceration effect could be observed prior to matching, this effect disappears once the samples are balanced.

DETENTION AND DOSAGE: UNDERSTANDING THE EFFECTS OF
INCARCERATION ON FIRST-TIME ARRESTEE JUVENILE DELINQUENTS

By

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Thesis submitted to the Faculty of the Graduate School of the
University of Maryland, College Park, in partial fulfillment
of the requirements for the degree of
Master of Arts
2009

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ACKNOWLEDGMENTS

I would first like to express my thanks and gratitude to Dr. Raymond Paternoster, my mater's thesis committee chair and mentor, for among other things suggesting that I use propensity score matching and focus on only first-time juvenile delinquents in this research study. I would also like to thank the other members of my thesis committee, Dr. John Laub and Dr. Brian Johnson, for the vast amount of knowledge, comments, and suggestions they gave me throughout this experience.

Additionally, I would like to thank the members of the Graduate Student Discussion Group Smackdown!, in particular Sarah Boonstoppel and Bianca Bersani. This group provided me with the opportunity to present portions of my thesis and to receive comments and suggestions from my peers. Thank you for your several extra pairs of eyes and your input. I would also like to thank Dr. Laura Dugan and Dr. Alex Piquero for their comments and advice regarding data analyses.

Finally, thank you to Chris Kozey for his constant encouragement and enthusiasm, and for listening to my 'hmpfs' throughout this process. And last but certainly not least, thank you to my parents Michael and Pamela Noe for their incredible, unvarying support, coffee gift certificates, and meat orders, all of which were essential to the completion of this thesis.

TABLE OF CONTENTS

| | |
|---|----|
| CHAPTER ONE: Introduction | 1 |
| CHAPTER TWO: Review of the Relevant Literature | 9 |
| Deterrence: Punishment's Role in Reducing Deviance | 12 |
| Prizonization and Early Literature on the Effects of Imprisonment | 13 |
| Labeling Theory | 14 |
| Revitalization of Labeling Theory: Effects of Official Intervention | 18 |
| Decarceration: Closing Massachusetts Reformatories & Training Schools | 21 |
| Why Only Examine First-Time Arrestees? | 25 |
| The Effect of Incarceration Length: Does 'Dosage' Matter? | 25 |
| Selection Bias Issues Facing Research | 29 |
| CHAPTER THREE: Data and Methodology | 34 |
| Data Collection | 34 |
| Description of the Sample | 38 |
| Dependent Variable: Measurement of Recidivism | 48 |
| Control Variables | 51 |
| Examining the Effect of 'Dosage' | 51 |
| Addressing the Problem of Selection Bias | 53 |
| Propensity Score Matching | 54 |
| Estimation of the Propensity Score | 59 |
| CHAPTER FOUR: Results | 62 |
| CHAPTER FIVE: Discussion, Limitations, and Conclusion | 71 |
| REFERENCES | 79 |

LIST OF TABLES

| | |
|--|----|
| Table 1: Research on the effect of incarceration on future recidivism | 19 |
| Table 2: Prior official legal histories of all youths receiving disposition, Spring 2000 | 35 |
| Table 3: Description of sample by disposition type of youths in NYC, Spring 2000 | 38 |
| Table 4: Demographics of youths receiving disposition in NYC, Spring 2000 | 41 |
| Table 5: Operational definitions of control variables | 43 |
| Table 6: Descriptive legal and extra-legal characteristics of youths by disposition type | 46 |
| Table 7: Description of sample's rearrest information at 18 months- by disposition type | 50 |
| Table 8: Frequency of placement youths at each sentence length (N=145) | 51 |
| Table 9: Comparison of balance between punishment groups (means), before PSM | 56 |
| Table 10: Treatment effect of incarceration before and after matching | 62 |
| Table 11: Comparison of balance between punishment groups (means), before and after PSM | 64 |
| Table 12: Standardized bias statistics for covariates previously out of balance | 66 |
| Table 13: Rearrest information at 18 months for placement youths- by length of time served | 67 |
| Table 14: Logistic regression of rearrest status with placement youths | 69 |

LIST OF FIGURES

| | |
|---|----|
| Figure 1: Gottfredson et al., 1977: The inverted u-shaped relationship between time incarcerated and future offending | 26 |
| Figure 2: Orsagh & Chen, 1988: The u-shaped relationship between time incarcerated and future offending | 27 |
| Figure 3: Length of sentence for placement youths- by offense type | 52 |
| Figure 4: Length of sentence for placement youths- by offense severity | 53 |
| Figure 5: Ranges of Pr(Placement)- by punishment type | 60 |

For a punishment to attain its end, the evil which it inflicts has only to exceed the advantage derivable from the crime; in this excess of evil one should include the certainty of punishment and the loss of good which the crime might have produced. All beyond this is superfluous and for that reason tyrannical.

Cesare Beccaria, *On Crime and Punishments* (1764)

Chapter One: Introduction

The U.S. juvenile justice system was originally formed with the belief that youths should receive treatment which would help “socialize” them, and enable them to change their delinquent behaviors (Katkin, Hyman, and Kramer, 1976:261). In New York City, the first juvenile detention facility was called the House of Refuge which itself suggests that officials believed providing youths with refuge from their adverse living situations would offer the opportunity for their behavior to be corrected and reformed (Pickett, 1969). For more than a century, the philosophy of *parens patriae*¹ dominated the actions and proceedings of juvenile courts, and as such many court officials believed that any steps could be taken so long as the best interests of the child were kept in mind. In other words, youths who entered the juvenile justice system were receiving a trade-off; in exchange for receiving the care and protection of the system, there was no need for the system to provide them with constitutional protections.

However, in the late 1960s and early 1970s the Supreme Court handed down several decisions that began to shift the consideration of juvenile delinquents away from them being considered wards of the courts, and toward them being held more

¹ *Parens patriae*: (Latin) “father of the people.”

accountable for their actions. This shift came primarily in the form of slowly granting juveniles the legal rights and protections that adults experience in the justice system. Despite the rhetoric of *parens patriae* the Court recognized that the juvenile justice system was in fact punishing these youths, and therefore constitutional rights needed to be applied. In the Supreme Court case *In re Gault*, the decision stated that “the constitutional guarantee of due process applies to proceedings in which juveniles are charged as delinquents.”(387 U.S. 1). Additionally the decision for *In re Winship* declared that juveniles had to be convicted by the same standards as adults, meaning they had to be found guilty “beyond a reasonable doubt” (397 U.S. 361-368). In addition to these changes in rights and protections, juvenile court systems began to differentiate between categories of juvenile offenders, and the most serious juvenile delinquents could be judged to be criminally responsible at the age of thirteen.

These rulings came during a period of time in the 1960s when public sentiment continued to convey a desire to rehabilitate and help juveniles. However, the decisions shifted attention away from the intentions and towards the actual performance of the juvenile courts (Bernard, 1992). While many still advocated treatment and aid for the juveniles, the performance of juvenile court now emphasized that the juveniles in their jurisdiction were in fact being punished and not just saved. Advocates for changing the juvenile justice system believed that with these adaptations it could be possible to punish juveniles in a fair, systematic process for all youths (Bernard, 1992; McDermott & Laub, 1986). However, sentiment changed in the middle of the 1970s as the number of serious violent youth crimes

jumped to record levels. As this was happening, individual cases captured the media attention in major US cities, prompting juvenile justice officials and lawmakers to re-examine the issue.

In New York City, public outcry over increases in juvenile violence was facilitated by the media frenzy surrounding the serious violent offenses of juvenile Willie Boskett.² These forces influenced both public perceptions about adolescent offenders and legal treatment with the passing of two laws, the Juvenile Justice Reform Act (1976) and the Juvenile Offender Law (1978). Both of these laws were aimed at identifying the most serious violent juveniles and allowing them to be given longer sentences in detention facilities. The Juvenile Justice Reform Act offered a new label of “designated felony” for violent crimes, and the law included language shifting the concern of the court away from rehabilitation toward one expressing a “need for the protection of the community” as motivation for allowing more serious juvenile offenders to be given longer sentences (N.Y. Fam. Ct. Act § 711). The Juvenile Offender Law was the first to dictate that a serious crime committed by a youth when they are 13, 14, or 15 could allow them to be considered criminally responsible, and tried in adult criminal court.

While these laws may have been the most severe juvenile justice reforms to be enacted at the time, several other states followed with new laws written to allow

² Willie Boskett was 15 years old when he killed two people on the subway in New York City in 1978. Under the new Juvenile Justice Reform Act (1976) he was charged with a “designated felony” and was subject to the longer 3 – 5 year sentence in detention (N.Y. Fam. Ct. Act § 753-a (McKinney Supp. 1976 – 1980)). Boskett received the maximum five years, which the general public felt was not severe enough. His case sparked public outcry and was influential in the passing of the Juvenile Offender Law that year.

for more severe treatment of juvenile delinquents. In the 1990s, juvenile court proceedings became more structured and uniform, sentencing options were expanded and lengthened, and during the 1990s the number of juveniles in detention facilities reached an all-time high (Snyder & Sickmund, 2006). While the incarceration rate is currently not at its 1990s peak, we still incarcerate more juvenile offenders than we did in the previous decades.

Exploring the impact which incarceration has on juvenile recidivism is vital for ensuring that our punishments stay within ethical and practical bounds. Concern over the effect of incarceration was originally brought about by the convergence of two thoughts. First, that prisons act as schools for crime, an idea promoted in the 1950s and 1960s with books written about life inside prisons around the country (Clemmer, 1958; Sykes, 1958). Second, adolescent youths may be particularly susceptible to the influence of their peers and their environment in shaping their behavior, an idea popular with increasingly influential theories of stages in psychological development (Erickson, 1950; Kohlberg, 1975).

The idea that incarceration as it operates now may actually cause juvenile delinquents to commit additional crimes has been a hotly debated topic over the past several decades. It is possible to find research on both adult and juvenile populations concluding that offenders who are imprisoned leave the experience with a greater proclivity towards crime than they did upon entering the system. It is also possible to find research concluding that there is no measurable effect of detention on future recidivism. However, research on the effects of incarceration continues to stumble around the issue of selection. The question is, does detention *cause* crime;

does it make things worse or does detention only capture those who are more serious offenders and who would commit future crimes anyway. The need still exists to make a clean causal inference about what the treatment effect of detention is, when this treatment is not randomly assigned.

In most data on official sanctions (also the case in this study), juveniles are not randomly assigned to receive either incarceration or probation; their punishment is based on a review of numerous legal and individual characteristics. The more dangerous, serious juvenile delinquents are generally the ones who are sent to detention facilities. Since it seems that those who are initially determined to be more criminal are the ones sent to detention, it becomes difficult to conclude that incarcerated juveniles re-offend more than non-incarcerated youth because of their time in detention facilities and for longer periods of time. It may simply be the case that incarcerated juveniles re-offend more because they were more delinquent the first place, and the incarceration experience is no more important than other negative facets of their lives. Several publications have identified this problem and discussed ways to address the selection bias (Berk, 1983; Bushway et al., 2007; Smith & Paternoster, 1990; Stolzenberg & Relles, 1997).

Another problem in examining the relationship between incarceration and recidivism is the issue of 'dosage'. If prison is understood to be a 'treatment' of juvenile delinquents, then the length of time spent in detention is an issue of dosage. Several studies examining the impact of incarceration treat all detention experiences as equal without taking into consideration that different lengths of incarceration

might have different effects (Bernburg & Krohn, 2003; Spohn & Holleran, 2000; 2002; Thomas & Bishop, 1984).

Therefore, the goal of this study is to provide methodological and substantive contributions to the field of research investigating the effects of incarceration on juvenile delinquents. First, this study will investigate the effect of incarceration on subsequent criminal behavior in *first-time* juvenile delinquents. In the original analysis of this dataset, Jeffrey Lin (2007) examined the effects of detention and included all juveniles in the analysis, including those with prior arrest and detention records. In the subsequent chapters I will extend his research by isolating the effects of detention for only those who are experiencing their first arrest and sentence. With regards to selection bias, several recent publications have provided insight into the appropriate statistical techniques involved in addressing the selection bias in samples such as this. The current study provides an opportunity to put this methodology into practice so that if incarceration has an effect on recidivism, it can be analyzed and better understood. Second, this study will examine the relationship between length of time incarcerated and time until future recidivism. Examining whether or not the length of time in detention matters will continue to broaden our understanding of the effectiveness of both incarceration and its alternatives.

If the nineteenth century reformers who built the juvenile justice system were able to evaluate where we are today, they would find that the children whom they once thought in need of refuge and reform are now considered juvenile delinquents who are in need of punishment and, at times, removal. The purpose of this study is to evaluate one of the principle forms of punishment facing juvenile delinquents,

incarceration. It may be that there is a relationship between incarceration and recidivism. However, it may also be possible to conclude that the effect of incarceration disappears after controlling for selection bias, and juvenile delinquents will recidivate regardless of punishments they experience. Either result can suggest policy implications both for those who design the incarceration experience and for those who make the decision to incarcerate particular delinquents.

These data are taken from Jeffrey Lin's (NYU) data collection *Impact of Institutional Placement of Delinquent Youth in New York City, 2000 – 2003*. Data collection was conducted by the Vera Institute of Justice and was obtained from the Inter-university Consortium for Political and Social Research (ICPSR). The dataset consists of all 414 first-time juvenile delinquents processed in New York City's Family Court from April – May in 2000 and these juveniles were then followed for 18 months after release. Included are case records from Family Court and their corresponding records from the Office of Children and Family Services (OCFS). Five juvenile cases had to be omitted because they did not have complete OCFS files, leaving 409 for analysis. The data includes juveniles who received institutional placement and those who received community-based sanction (probation), and recidivism is measured by whether or not the juveniles were re-arrested within the time frame. I will focus my data analyses on the number of total juveniles who were re-arrested by the 18-month period, because all of the subjects who recidivated during those 18 months will be captured in this variable.

There are two hypotheses being investigated in this study. First, after balancing the samples detention will have no significant effect on the probability of

future recidivism. While an effect may be observable initially, accounting for selection bias will essentially make any observable effect of incarceration disappear. Second, the 'dosage' of time spent in detention will not have a significant effect on probability of future recidivism. Juvenile who spend long and short amounts of time in detention facilities will have similar recidivism rates.

Chapter 2: Review of the Relevant Literature and Research Hypotheses

Introduction

This research is relevant to the long-standing examination of the effectiveness of rehabilitation for juvenile delinquents. It is historically significant

to sample juvenile delinquents from New York City, because it was here that the first juvenile reform school was developed in the United States.

In July of 1824, the Society for the Reformation of Juvenile Delinquents was formed with the aid and support of New York City Mayor Stephen Allen. Allen was extremely active with institutions such as the American Discipline Society and the New York Hospital and Lunatic Asylum, both geared towards the growing problem facing big cities such as New York of numerous people living in poverty and on the streets (Pickett, 1969). While Allen supported strict and severe treatment for adult offenders, he strongly believed that children³ committing crimes did so as a direct result of living on their own on the streets of New York, with no moral or educational guidance. Allen contributed to one of the Society's first official reports, writing that he believed these children could be taught, put to work, and given moral and religious guidance that would "afford a prompt and energetic corrective of their vicious propensities, and hold out every possible inducement to reformation and good conduct." (SRJD, 1824).

In this way the House of Refuge in New York was designed, with the intention of operating like a school and not resembling adult prisons already in place. Mayor Allen was certain that a juvenile who came to understand the risks of getting caught and the rewards for good behavior would come to avoid deviant and criminal acts. Joseph Curtis, the Refuge's first superintendent believed that the reform schools should have the primary purpose of preventing those receiving them from

³ Allen asserted that girls between the ages of seven and eighteen, and boys ages seven to twenty-one who commit crimes should be punished and reformed separately from adult offenders (Pickett, 1969)

committing crimes again. Creating these institutions with reformation, education, and protection as the most important goals would, Curtis and Allen believed, give these youths an opportunity to be changed and saved from their prior lifestyle (Mennel, 1973; Pickett, 1969).

Deterrence: Punishment's Role in Reducing Deviance

Those forming the House of Refuge were tapping into the ideas of rehabilitation in shaping their treatment of juvenile delinquents. They viewed the detention experience as something that could be beneficial, and they viewed this not as punishment, but as opportunity to save troubled and disadvantaged youths from their poor living conditions. A contrasting viewpoint from what guided the creators of juvenile justice is the question of whether or not punishments actually effective elements of deterrence. Deterrence theory reflects one of the most basic ideals our adult legal system is based upon, that punishment causes subsequent conformity to the laws and norms. Origins of this ideal can most markedly be observed in Beccaria's *On Crime and Punishments*. The central tenets of deterrence can be summarized as follows:

1. As humans, we are motivated to act in ways that advance our personal best interests.
2. We act based on having the free will to make a choice between available options.
3. We will *avoid* a course of action outside the norms if we perceive that some combination of certain, severe, and swift punishment will follow. (Thomas & Bishop, 1984: 1228).

Some deterrence theorists have described the process of committing a crime as a cost-benefit analysis undertaken by an individual before choosing to engage in a criminal activity. Essentially, the argument is that people only make the decision to commit a crime and act in a way that is outside of the accepted norms if they believe the benefits they could achieve from the act outweigh the costs and risks involved in doing it (Andenaes, 1974). With regards to imprisonment, lawmakers hope and assume that incarceration is a punishment that offenders will perceive as being too high a cost to risk committing a crime in the future. In the late 1960s, as the number of offenders sent to prison began to rise, researchers began to empirically examine this assumption.

Charles Tittle investigated this issue by asking the basic question, “Are negative sanctions instrumental in inducing conformity to norms?” (Tittle, 1968: 409). He was especially interested in the three characteristics of punishment believed to be important in an offender’s perception of the situation: the certainty, swiftness, and severity of the punishments. Tittle found that the certainty of punishment seems to be associated with decreases in the crime rate over time, but he did not find as much support for the idea that swiftness or severity of punishments had a negative effect. However, this research was conducted on an aggregate level where the certainty of punishment was assessed by computing a rate of imprisonment, and then comparing this to the crime rates for various offenses.⁴

⁴ In Tittle’s study (1968) the rate of imprisonment was obtained by dividing the total number of new prison admissions for 1959 – 1963 and then dividing this by the total number of crimes known to the police from 1958 – 1962.

Tittle's study was important in recognizing that these three ideas (certainty, swiftness, severity) might have distinct impacts, but it is impossible to conclude from his research that any specific individual has perceived sanctions and has been influenced by them, which is an important requirement for a person to be deterred from crime by a punishment (Saltzman et al, 1982). It may not be practical to assume that even though these general rates of imprisonment exist any one individual knows about them, or factors them into their own decision-making. This was demonstrated in 1968 when the California Assembly Committee on Criminal Procedure asked state residents to report their knowledge of penalties that followed a variety of crimes, and most residents were unaware of the actual correct punishments (CACCP, 1968).

The idea that perception of threat of sanction matters was expanded in the research of deterrence theory, and people began to assess how perception of punishment influences individual offenders. In the 1970s several studies emerged which offered support for the idea that when individuals perceive a high amount of certainty of risk, this is associated with a moderate deterrent effect (Anderson et al., 1977; Jensen et al., 1978; Tittle, 1977). There was also initial evidence that the perceived severity of the punishment has an effect on criminal conduct, with Grasmick and Bryjak (1980) concluding that when offenders perceive the certainty of punishment to be high, they are less likely to engage in future criminal conduct if they also perceive this certain punishment to be severe. In this way, Grasmick and Bryjak concluded that both certainty and severity matter and may be associated with a decrease in criminal conduct. However, reviews of this research point to the

problem of temporal ordering. In their reexamination of perceived severity, Paternoster and Iovanni (1986) point out that it is impossible to gauge the effect of perceived severity on deterrence and future offending if the offenders are asked to report on the perceived severity *after* committing additional crimes, and when proper attention to the temporal ordering of events is considered, perceived severity of punishment does not have a significant deterrent effect.

'Prizonization' and Early Literature on the Effects of Imprisonment

In contrast to the deterrence theorists, there was a growing interest in the mid- 19th century in the idea that spending time in prison might have a criminogenic affect. In 1958, Donald Clemmer published an investigative exposé into cultures inside an Illinois penitentiary, and he is noted as being the first to suggest that “prizonization” occurs when inmates socialize and adopt a more criminal culture in prison. Clemmer was a strong believer in the notion that this prizonization process is associated with the recidivism of prison inmates, although his book was mainly observational and did not statistically examine this issue (Clemmer, 1958). In his influential text *Asylums* written, Erving Goffman reinforces the idea of prison being a “total institution” where prisoners must adapt and change their behaviors and norms in order to survive, and suggests that this change (he calls institutionalization) in behavior could be maintained upon release and make recidivism more likely (Goffman, 1961).

The overall rhetoric emerging from prizonization literature was that incarceration does not rehabilitate, as its originators had hoped, nor does it deter. Rather, public sentiment began to support the idea that incarceration can have lasting negative effects on offenders, making them more delinquent than they were upon entering the system.

Labeling Theory

The examination of the effectiveness of different types of punishment, and the possible negative impact of detention on juveniles has roots in the work of labeling theory. Early in the 1960s researchers began collecting data to research the idea that having contact with the criminal justice system might actually foster criminality, and not deter it. They became known as labeling theorists because their central belief was that when we as a system publicly label someone as a delinquent or a criminal, most obviously by incarcerating them, this has a significant impact on their ability to return to society and maintain a non-criminal life.

While not originally identifying himself as a 'labeling theorist,' Edwin Lemert believed that societal reactions to those who experience an official sanction do impact future criminal behavior (Lemert, 1951). Lemert described this as *deviance amplification*, and described the increased likelihood to re-offend as *secondary deviation* (in contrast to primary deviation, which explains the initial act of delinquency). In his original work, he states that secondary deviation can be a "means of defense, attack, or adaptation to the overt and covert problems created by the societal reaction to primary deviation" (Lemert, 1967:17).

Almost two decades after Lemert's first examination of societal reactions, Edwin Schur and Howard Becker gained the attention of the criminological field by developing a 'labeling' theory and including the earlier works of Lemert as the foundation for the perspective. Schur discusses the secondary deviation process as role engulfment, where the delinquent is so severely stigmatized by their criminal role that they continue to be delinquent based on their new expectations and others' expectations of the role they can have in society (Schur, 1971). Howard Becker developed a slightly different aspect of the labeling process- instead of just examining how people get labeled, Becker was interested in *who* and *what* gets labeled (Becker, 1963). He is responsible for developing the "conflict perspective" in labeling theory, which is an examination of how groups in power make laws that have the ability to punish the actions of those who are not in power, thereby labeling them as deviant and maintaining their control.

Some of the early work on juvenile labeling was done investigating youths who experienced police contact. Although not all youths who are picked up by the police are arrested, the argument was that being singled out and having this formal contact with law enforcement might be enough to increase a juveniles' orientation towards delinquency. Ageton and Elliott (1974) conducted a study examining the effect of police contact on juveniles, and followed them for four years after their initial police contact. Over the four years, youths who initially had contact with the police were significantly more likely to have "delinquent orientations" which was determined through self-reports of additional delinquent actions and official police records (Ageton & Elliott, 1974:97). They also observed that white youths were

more susceptible to the effects of this labeling experience, which was consistent with similar research of the time (Gould, 1969; Jensen, 1980). The general conclusion from this research was that while a labeling effect does exist, minority youths may be more accustomed to being labeled as deviants or troublemakers, therefore official sanctions have less of an impact on their continuing delinquency.

David Farrington focused on the labeling of juvenile delinquents, and developed a more methodologically rigorous approach to the idea of deviance amplification. Farrington tested one of Becker's primary hypotheses, that "individuals who are publicly labelled will increase their deviant behaviour as a result." (Farrington, 1977: 112). In Farrington's study, an official finding of guilt in court was the public labeling experience, and he conducted a prospective longitudinal study design administering questionnaires to the juveniles at ages 14, 16, and 18 years old. In addition, their official arrest records were obtained during this time period.

Farrington discovered that the labeled youths had higher levels of both self-reported and official records of deviant behavior. However, he is one of the first in the field of labeling research to directly address preexisting differences between those who initially were found guilty in court and those who did not experience this. Referencing his prior research, Farrington collected information at age 14 about the troublesome behavioral history of all the juveniles from teachers and peer ratings, as well as five other background characteristics.⁵ While he was able to conclude that

⁵Based on prior research (West & Farrington, 1973) the five background characteristics were: criminality of parents, family income, family size, I.Q. of juvenile, and a global index of parental behavior.

significant differences existed between the two groups, Farrington did not find that these differences explained the deviance amplification of the group who was initially labeled (Farrington, 1977). His overall conclusion was that public labeling has a definite impact on a juvenile's future deviance.

Although these early studies seemed to provide some evidence of a relationship between sanctions and future offending, the labeling perspective came under harsh criticism in the late 1970s and early 1980s. The theory was all but dismissed by theorists who believed that the concepts in the original theory were vaguely defined and empirically sound testing did not provide supportive evidence that offenders were actually more criminal after being labeled. In Walter Gove's book of assessment and critique, he and his colleague Charles Tittle evaluated research such as the Ageton & Elliott study described above and expressed doubt that the labeling experience has been measured in a way that adequately isolates its effect (Gove, 1975). Another dismissive evaluation came from Travis Hirschi, who refuted the theory by highlighting the importance of individual criminogenic factors for determining propensity for recidivism. He concluded that any consideration of the impact of labeling without thoroughly taking into account prior characteristics is "off the mark" and when individual factors are considered, the impact of labeling disappears (Hirschi, 1975: 198).

Revitalization of Labeling Theory: Effects of Official Intervention on

Recidivism

While initially these critiques quieted work from the labeling perspective, the criticisms by Hirschi and others were eventually important guiding principles when

research on labeling theory was revitalized. In the late 1980s, our national policies towards crime became more strict and severe, with official sanctions being given at higher rates than ever recorded (Mosher et al., 2002). It was during this period that researchers began to question the effects that punishment might have on offenders, bringing attention to labeling theory and specifically deviance amplification once again (Paternoster & Iovanni, 1989). However, theorists who began to re-examine the ideas of labeling theory understood that if they were going to attempt to identify amplification in deviance, they first needed to empirically show that the mechanism causing a continuation of criminal activities involved official sanction.

Spohn & Holleran (2002) have written some of the most recent work evaluating the effect of imprisonment on future recidivism. Using data on felony drug offenders who were convicted and either sent to prison or placed on probation, they followed these offenders for 48 months and recorded whether or not the offenders had new charges filed against them. Their results demonstrated a higher rate of recidivism for those sent to prison, and also a shorter time until recidivism, with drug offenders being the most negatively impacted by time spent in prison (Spohn & Holleran, 2002). This paper also addressed the issue of selection bias between the two punishment groups, and attempted to control for the problems of selection by including an indicator of each person's predicted probability of incarceration in the final logistic regression model.

| Table 1 | | | | | |
|---|-------------|--------------------------|------------------------------|-------------------------|-----------------|
| Research on the effect of incarceration on future recidivism | | | | | |
| Author | Year | Juv./Adult Sample | Measure of Recidivism | Follow-up Period | Findings |

| | | | | | |
|-----------------------|------|----------|------------------------------|--|---|
| Ageton & Elliott | 1974 | juvenile | arrest, self-report | 4 years | Youths with police contact more likely to have "delinquent orientations"; White youths more susceptible to official labeling. |
| Farrington | 1977 | juvenile | arrest, self-report | 4 years | Labeled youths had more self-reported & official records of delinquency |
| Gould | 1969 | juvenile | self-report | - | White youths more susceptible to effects of official labeling. |
| Jensen | 1980 | juvenile | official (n.d.), self-report | - | White youths more susceptible to effects of official labeling. |
| Osgood & Weichselbaum | 1984 | juvenile | self-report and rearrest | - | Diversion programs associated with less perceived stigma; younger juveniles vulnerable to labeling |
| Palamara et al. | 1986 | juvenile | arrest, self-report | 5 years | Police/mental health intervention has independent and interactive effects increasing deviance; Impacts differ by form of deviance. |
| Petersilia et al. | 1985 | adult | arrest, conviction | 40 months | Routine probation for serious felony offenders leads to increases in recidivism. |
| Sampson & Laub | 1993 | juvenile | Arrest | Until age 32 | Length of time incarcerated no direct deterrent or criminogenic effect. Longer time spent incarcerated associated with poor future job stability. |
| Spohn & Holleran | 2002 | adult | new charges filed | 48 months | Higher recidivism rates for those sent to prison; drug offenders are most effected. |
| Thomas & Bishop | 1981 | juvenile | self-report | 9 months | No significant results supporting labeling or deterrence theory |
| Thornberry | 1971 | juvenile | arrest, dispositions | - | Only a moderate effect found for detention leading to increased recidivism. |
| Turner & Petersilia | 1992 | adult | arrest, incarceration | 1 year | Intensive Supervision Programs (ISPs) focus on high surveillance, result in more recidivism than regular parole |
| Wooldredge | 1991 | juvenile | arrest | cohorts 12 - 17; followed until age 21 | Probability of recidivism exists at various ages regardless of age at 1st intervention. |
| n.d. = not defined | | | | | |

Many modern researchers have also begun to explicitly examine the effect of incarceration on juvenile delinquents. Research by Osgood and Weichselbaum (1984) evaluated the effectiveness of diversion programs for juveniles, compared with detention. The evaluation included the recidivism rate of the juveniles and self-reports from the juveniles regarding their perception of social stigma following the

official intervention. Their conclusions supported diversion programs as being associated with less perceived stigma by the juveniles. However, the researchers also indicated that the younger the juvenile, the more vulnerable they are to the labeling effects of official intervention (Osgood & Weichselbaum, 1984).

John Wooldredge (1991) followed up on this research by examining the relationship between age at first intervention and subsequent recidivism. Specifically, Wooldredge was curious about what matters more: a juvenile's age itself, or their age at first intervention. In other words, he questioned if a juvenile's likelihood for recidivism is more influenced by their current age than their age at first official intervention. Building off the development of the age-crime curve, indicating that offending is strongest when an offender is between 16 and 19 years old (Cohen & Land, 1987), Wooldredge concluded that the probabilities of recidivism at various ages exist regardless of age at first intervention (Wooldredge, 1991).

Another examination of the effect of sanctioning on juveniles was conducted by Thomas and Bishop (1984), in which they include a review of past research and the methodological problems that are common to labeling research. Their main point of criticism of past research is that researchers have remained too vague in their interpretation of a negative labeling experience and how reactions to it can be observed and measured (Thomas & Bishop, 1984). However, their own research design does not allow them to properly investigate the effect of sanction on future delinquency, because their data did not indicate when in time specific events occurred. Thomas and Bishop's research methods failure to account for temporal

ordering of events means that it is still difficult to conclude from their research that the juvenile delinquents experienced the effects of being negatively labeled after their first official sanctioning experience (Thomas & Bishop, 1984).

Past research on labeling theory and the effect of incarceration on juvenile delinquents has shown some support for the idea that incarceration may have a distinct negative impact on juvenile delinquents, especially on the very young. However, several examinations have been limited in their methodological rigor and in measuring the effects of sanction and incarceration in a way that isolates the incarceration effect from other possible influences. The goal of the current study is to examine the effect of incarceration on recidivism for juvenile delinquents, paying attention to the balance of the punishment groups and the temporal ordering of events.

Decarceration: Closing Massachusetts Reformatories and Training Schools

As prison exposé literature became more popular, people across the country began to pay attention to the conditions and operations of juvenile institutions. It was during this time at the end of the 1960s that Massachusetts embarked on one of the most extreme and relatively sudden changes any juvenile justice system has experienced in this country: the decarceration of juvenile delinquents.

In the late 1960s civic organizations and academics began looking into reports coming out of reformatories, and expressed their outcry at the wretched living conditions at several juvenile reformatories and training schools in Massachusetts. The call for change grew with newspaper articles in the *Boston*

Globe describing appalling living conditions, heavy emphasis placed on militant obedience to rules, and lack of emphasis on the correction and rehabilitation of the youths living in these facilities (Ohlin, 1973). In 1969, the governor of Massachusetts, Francis Sargent, appointed Dr. Jerome Miller as commissioner of the newly formed Department of Youth Services (DYS). Miller entered the position knowing that he was expected to serve as a pivotal figure in changing Massachusetts juvenile justice (Miller, 1991).

From the beginning, Miller showed a strong interest in the development of community-based alternatives to training schools and reformatories (Stolz, 1984). He believed that past attempts made by institutions to reduce the recidivism of juveniles were mostly failures, and after evaluating several problems relating to the physical structures, personnel, and policies at work, Miller and his team came to the conclusion that the problem was simply having institutions at all (Armstrong, 2002; Miller, 1991; Ohlin, 1973). He therefore made the decision to begin closing down reformatories and training schools, and focusing on three goals for juvenile corrections: regionalization, privatization, and community integration (Bakal 1998: 110). Starting with the Bridgewater Correctional Unit in August of 1970, Miller began closing facilities throughout the state, sending those who had previously been incarcerated to parole or group housing. In April of 1971 the daily population of juveniles in detention had dropped from 1,200 to 400 youths, and the average stay in a facility was 3 months (Knopp & Reiger, 1976). By July of 1974 the last juvenile institution was closed, and all juvenile delinquents were handled through community-based alternatives. Private organizations competed for contracts to

handle various community-based services, a process which is still partially in place today (EOHHS, 2009).

Evaluations of this endeavor began quickly amongst criminologists; questions arose as to whether community-based alternatives to detention such as those implemented in Massachusetts were both cost-effective and could also be shown more effective in reducing recidivism. In the case of Massachusetts, studies showed an increase in recidivism for youths who were punished in the new DYS system, as opposed to the previous system that included incarceration (Coates et al., 1978; Ohlin et al., 1977). However, community-based alternatives were studied elsewhere as well, and other reviews suggested that community-based alternatives can be equally as effective (Empey & Erickson, 1972; Gottfredson, 1987) and in some cases were found to be significantly more effective than incarceration in reducing juvenile recidivism (Barton & Butts, 1990). The issue of community-based alternatives to detention was addressed in Robert Martinson's seminal paper assessing what works in prison reform; he concluded that although both detention and community-based sanctions had similarly limited effects on reducing juvenile recidivism, community-based alternatives have been shown to be more cost-effective, and therefore should be explored as a practical alternative to detention (Martinson, 1974)

In 1988, the Maryland juvenile correctional facility Montrose Training School was closed, and its occupants were sent to community-based alternative sanctions. Recognizing the similarity of this situation with decarceration efforts of the past, the impact of this deinstitutionalization was examined by Gottfredson and

Barton (1993) measuring recidivism for youths who spent time at Montrose and youths who had not spent time in an institution. Similar to the results in Massachusetts, the group who had not been institutionalized had significantly higher recidivism rates than those who had been at Montrose. This would seem to be evidence in support of institutionalization. However, the researchers point out developments that have occurred to increase the supervision in community-based sanctions, which could have both positive and negative consequences. Increased supervision may mean that the youths are given more help and guidance in their re-entry process, but it could also simply make them more likely to get caught for minor violations of their release. They conclude that one reason why alternatives did not work after Montrose's closing was the poorly designed and implemented programs at work for these deinstitutionalized youths. Therefore, instead of abandoning either form of punishment, the focus should be on improving the quality and design of programs both inside and outside of detention (Gottfredson & Barton, 1993).

Why Only Examine First-Time Arrestees?

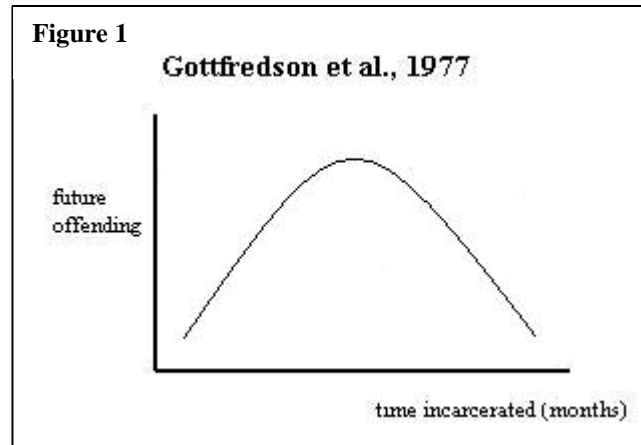
The decision to identify and examine only those youths who were experiencing their first official arrest was made based on the desire to establish a clear temporal ordering of causal relationship which may be at work. It has been suggested that the first sanctioning experience for a juvenile may have a strong deterrent quality, and policymakers have designed programs such as "Scared

Straight” to bring youth into prison facilities to meet and observe the lives of everyday prisoners. However, contrary to the goals of policymakers, evaluations of these programs by Petrosino and colleagues reveal that they are associated with increases in juvenile delinquency, and not reduction (Petrosino et al., 2000; Petrosino et al., 2003). The first sanctioning experience is an appealing place to measure the effects of punishments due to the desire to evaluate these effects with clear temporal ordering. By observing the process for only first-time arrestees, I can be more confident that any observed effects of punishment are not artifacts of past official sanctions, and a logical temporal ordering of events is being measured. Whether or not a labeling or deterrent effect is observed, the hope in studying first-time arrestees is that this effect will be stronger than examining the combined group of juveniles with and without prior records.

The Effect of Incarceration Length: Does ‘Dosage’ Matter?

One of the principle goals of the current study is to examine whether or not the amount of time spent incarcerated has a differential effect on future delinquency. If prison is understood to be a ‘treatment’ for offenders, then it is important to gain an understanding of what the appropriate ‘dosage’ should be. There is a limited body of research examining the relationship between length of time served and recidivism, and results from these studies have been mixed. Most research has also been done primarily on adult offenders; for the most part the issue of dosage for juveniles has been untouched (Gainey et al, 2000; Gendreau et al, 2002; Gottfredson et al, 1977; Orsagh & Chen, 1988; Smith et al., 2002).

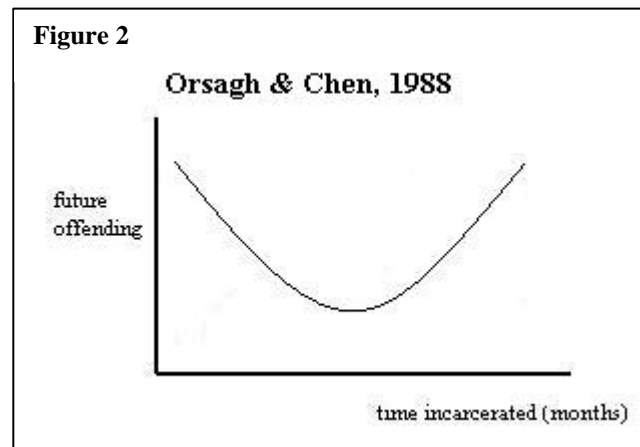
Gottfredson et al. (1977) followed adults released on parole, having recorded how many months they had stayed in prison. Their results indicated that recidivism (measured as rearrest) tended to increase with time served, however they did notice that for those offenders who were in



prison for extremely long periods of time (over 50 months) offending actually decreased. These results led Gottfredson et al. to conclude that an inverted U-shaped relationship exists between time spent in prison and future offending, as displayed in Figure 1. One problem with this study is their decision to only follow individuals for one year after release; this may cause the recidivism numbers following incarceration to appear lower than they actually are. It has been suggested that the effect of time served does not absolutely deter people, but rather extends the time between releases and rearrest, so only examining offenders for one year may be enough (DeJong, 1997). Gottfredson et al. also restricted their study to researching adult offenders, making it difficult to assert that delinquents will be similarly affected. This inverted U-shaped relationship was investigated later with only first-time offenders, and while there was some evidence for recidivism increasing with time-served but then declining for those with longer sentences, these results were not significant (Gainey et al., 2000).

A later study examined the same issue, but its results were opposite to Gottfredson et al. In a paper also examining adult offenders, Orsagh and Chen (1988) suggested that there is actually a conventional U-shaped relationship between time incarcerated and recidivism, and this is due to the economic and social conditions that are affected by time imprisoned (see Figure 2). While a large amount of those who re-offend had short

incarceration lengths, this is followed by a decline in recidivism for slightly longer sentences, and then those incarcerated an extremely long time have higher recidivism



levels. The researchers hypothesized that longer time in prison more severely ends previous economic and social prospects, such as employment and relationship with family members (Orsagh & Chen, 1988). Another interesting point raised by Orsagh and Chen is that there may be an optimal length of time served for an offender which leads to a reduction in recidivism. All else being equal, they found that serving approximately 1.2 years in prison seems to be the optimal sentence for reducing future arrest.

Meta-analyses have been conducted by Smith et al. (2002) on the effects of both sanction and time served on recidivism for multiple countries. Both were conducted dividing time served into the categories of 'more' or 'less' time in detention. They concluded that sanction itself does not promote a decrease in

recidivism (Smith et al., 2002). However, there was evidence that increased lengths of incarceration are associated with increased recidivism. The idea of an optimal length of time served was not supported, and unlike both Gottfredson et al. and Orsagh and Chen, they did not find evidence supporting either a U-shaped or inverted U-shaped relationship between time served and recidivism.

Research that has been conducted examining the issue of detention dosage for juveniles has yielded mixed results. Some evidence has been found indicating that first-time offenders have a greater likelihood of recidivism if their first incarceration experience was long (Myner et al., 1998). Others have suggested that the effect is a very small one, and after a few months the effects of imprisonment length are not observable (Visher et al., 1991). There are currently extreme differences in opinion that about the effects of different lengths of imprisonment and very few studies have examined these effects with juvenile delinquents (Gottfredson et al, 1977; Orsagh & Chen, 1988). Given that past research has yielded conflicting results of both positive and negative relationships, the following hypothesis is proposed for the current study:

Hypothesis 1: The relationship between length of time spent in detention and future recidivism is null; the length of time spent in detention has no effect on the probability of future recidivism

In their examination of the Glueck men, Sampson & Laub (1993) investigated the possibility that the total amount of time a juvenile spends incarcerated in adolescence may have an impact on their future success. No direct

relationship was found; time spent incarcerated did not seem to directly affect criminal activity in adulthood (Sampson & Laub, 1993; 1995). However, Sampson & Laub highlighted other aspects of the young men's lives which were impacted by incarceration and how these in turn influence adult offending. The researchers describe the incarceration experience as having the effect of "mortgag[ing] one's future, especially later life chances molded by schooling and employment in adulthood." (Sampson & Laub, 1993: 165). Given the potential for time spent in detention to take a toll on other aspects of a juvenile's development, such as friendship networks, education, and employment, this area of juvenile justice deserves additional investigation.

Selection Bias Issues Facing Research on the Effects of Incarceration

In the 1980s the revitalization of labeling theory research involved not only a re-examination of the core theoretical framework, but also a closer look at methodological problems frequent in the past. Of particular concern was the issue of selection bias which is present and unavoidable in research on the effects of incarceration.

The bias that exists in criminological data is largely due to the fact that as researchers we have very little control over the assignment of individuals to particular groups of interest. This problem is especially important to consider in studies examining the impact of official interventions, because the decisions made by criminal justice officials to arrest someone or not arrest them, and to incarcerate them or divert them into probation, is not a random decision but is based on

information about the individual's past. For juveniles, the decisions made about them are based on files compiled by individual case-workers. Judges base their decisions on information in these files, which includes extra-legal factors such as the child's history of poor school attendance, whether or not they live with both of their parents, and participation in extra-curricular activities.

The juvenile justice decision-making process is structured in this way to promote the treatment of each juvenile offender on an individual basis; a method which the founders of juvenile corrections in New York City believed produced the best results (Pickett, 1969). However, it is difficult to assess the results of placing a juvenile in detention if those juveniles sent to detention are significantly different from those who are diverted and placed on probation. In other words, if juveniles who are sent to detention come out and commit more crimes than juveniles who were punished with community-based sanctions such as probation, it is not valid to conclude that the negative labeling experience of going to detention had any kind of a causal effect without taking prior characteristic variables into account.

In 1983 Richard Berk wrote about this problem with sociological data in general. He discusses the problems that arise when people are non-randomly selected into sample groups, and states that while this has obvious problems for the external validity of the study, it also presents problems with the study's internal validity (Berk, 1983).

The problem with external validity concerns the exclusion of a non-random subset of individuals into the treatment group of a study. In the current study,

juveniles who were not incarcerated were diverted because of specific characteristics about their offense, their behavioral history in school and with their parents, and many other background characteristics. Berk asserts that making any conclusions about the effect of incarceration cannot be made without taking these individual background characteristics into account.

While Berk was one of the first to directly address and suggest remedies for the selection problem in all sociological research, this problem was specifically addressed for the ideas of labeling and deviance amplification by researchers in the early 1990s. In their influential article, Smith and Paternoster (1990) directly address the question of how this idea of ‘deviance amplification’ is measured, and they discuss an appropriate methodological treatment of a selection artifact in this kind of research questions (Smith & Paternoster, 1990). Stolzenberg and Relles (1997) wrote a piece which provides the tools necessary for researchers to break down and better understand the selection bias present in sample data, and also to better understand the appropriate situations to apply these techniques (Stolzenberg & Relles, 1997).

All of these past approaches have involved gaining statistical control over the confounding variables using logistic regression. An alternative technique is also being applied to samples in criminological research and will be applied in the current research, and that is the use of Propensity Score Matching (PSM). The goal of PSM is somewhat different from previous approaches; the focus is on creating balanced matched samples in each comparison group. In this way, after the groups have been balanced, each individual has a similar chance of receiving the treatment. Therefore,

the effect of placement on those who receive it is not confounded by preexisting characteristics that selected them into their punishment group (Rosenbaum & Rubin, 1983). A more detailed description of how the problem of selection bias can be resolved with PSM in the current research will follow in the sections describing the data and analyses used. However, it is important to note the strong developments that have taken place in this area of analysis, leading to a questioning of past research which modeled the relationship between incarceration and recidivism without accounting for selection bias.

The hope in using propensity score matching for this study is to come as close as possible to answering the counterfactual question. That is, if the same juveniles who were put in detention had actually been sent to probation, would their recidivism outcome be the same? This is a question that is itself contrary-to-fact and impossible to actually know, but using propensity score matching creates samples of youths in each disposition group that are as closely matched as possible. In Lin's (2007) initial analysis, he used propensity score matching for all juveniles in the sample and concluded that when the groups have been balanced detention has no observable effect on future recidivism. This study examines a subset of Lin's initial sample, first-time arrestees, to determine whether or not detention matters more for these 'naïve' offenders. Creating samples where youths have similar chances of getting sent to placement removes the confounding effect of the covariates. Therefore, while we can't know how those exact youths sent to placement would have fared, we do know how a group of youths very similar managed on probation.

This balancing of the sample allows for the following hypothesis to be proposed:

Hypothesis 2: Incarceration has a null effect on future recidivism, as measured by rearrest. After creating balanced samples on all covariates, any effect of detention will disappear; matched samples will take away the effect of detention for juvenile delinquents.

Smith and Paternoster concluded their examination of deviance amplification and selection bias with the hope that “increased attention to possible selection bias in empirical tests of the deviance amplification hypothesis will lead to more conclusive evidence regarding the effects of sanctions on future criminal activity.” (Smith & Paternoster, 1990: 1129). The present study attempts to contribute to this call for attention and expand our understanding of the relationship between incarceration and recidivism for juvenile delinquents.

Chapter 3: Data and Methodology

The following chapter will offer a description of the dataset being used for the current analysis, and an explanation of the methodological approaches I have chosen. I will include a discussion of the data collection process by investigators at the Vera Institute of Justice, a description of the youths in the sample, and a description of the statistical methods utilized.

Data Collection

These data were collected by a research team from the Vera Institute of Justice in April and June of 2003. The data were made publicly available on January 8, 2008 by Jeffrey Lin, a doctoral student at New York University who participated in the data collection with the Vera Institute. The data can be downloaded through the Inter-University Consortium for Political and Social Research (ICPSR) (study no. 20347). Data files can be downloaded in STATA, SAS, and SPSS formats. For the purposes of this study, analyses were primarily done in STATA 9.0 and 10.1.

The impact of incarceration on future recidivism was evaluated for this data set by Jeffrey Lin, and written up in a report submitted to the National Institute of Justice. His results indicated that contrary to the predictions of either deterrence theory or labeling theory, incarceration had a null effect on future recidivism. However, Lin's research was conducted examining all of the juvenile offenders arrested and adjudicated during the time period, including those with extensive prior records of arrest, disposition, and time served in detention (Lin, 2007). One of the primary assertions of early labeling theorists is that the first official sanction experienced is the pivotal societal reaction which leads to stigma being attached, and delinquent characteristics moving to the forefront of the juvenile's self-concept (Lemert, 1952). It is after this first official sanction that the labeling process unfolds and deviance amplification is said to take place. Therefore, including juveniles with several past official sanctions in a study where one is measuring the impact of a recent arrest on future recidivism runs the risk of attempting to measure 'secondary deviance' when in fact the juvenile has long since moved past that point.

Table 2 shows that out of the original 698 juveniles arrested, approximately 41% of them had been arrested at least once before the current offense. While only 4% of the total sample have a prior history of placement in detention, this may not be surprising considering that many juveniles exit detention and age out of the Family Court jurisdiction.

| Table 2 | | |
|--|----------|----------------|
| Prior official legal histories of all youths receiving disposition, Spring 2000 (N=698) | | |
| | n | Percent |
| Prior Arrests on Record | 284 | 40.69% |
| Previously Placed in Detention | 28 | 4.01% |

It is for this reason that I have chosen to expand on Jeffrey Lin’s research with the current data set, and only include juveniles who have just experienced their first official arrest. Evidence of a null effect of incarceration on this subset of first-time juvenile delinquents will provide a more complete answer to the question of the incarceration effect.

The sample consists of all first-time arrestee juvenile delinquents who were processed and given a disposition through New York City’s Family Court in the months of April, May, and June of 2000. This includes all five boroughs of New York City- Manhattan, Brooklyn, Queens, Staten Island, and the Bronx. Since this study is focused on the activities of ‘juvenile delinquents,’ it is first important to understand how this term is operationalized in the NYC Family Court system as of

2000. In New York, a juvenile delinquent is defined as anyone from the ages of seven to fifteen at the time of the offense who is charged with an offense that would otherwise be considered a crime if committed by an adult. This does not include those juveniles who committed the most serious violent offenses and were waived to adult court as juvenile offenders.⁶ Being a ‘first-time’ juvenile delinquent indicates that the arrest leading to this juvenile’s disposition was his or her first arrest ever recorded. Not included are previous complaints for status offenses that did not lead to an official arrest. Status offenses are defined as “chronic or persistent truancy, running away, possession of graffiti materials, violating curfew laws, or possessing alcohol or tobacco.”(28 C.F.R. § 31.304h).

New York City’s Family Court processes juvenile delinquents in several steps before reaching the disposition stages. Thus, the current group is comprised of juveniles who have been arrested for the first time and whose case was not dismissed or adjusted to avoid a disposition. For a detailed view of the steps in the juvenile justice processing system in Family Court, refer to Appendix C.

Each juvenile has a personal case file compiled by their case worker in the fact-finding stage of processing. With the exception of rearrest information these files contain information that serves as a ‘snapshot’ of the juvenile at the time of their arrest and disposition. Information in the case files includes the youth’s demographic profile (age, gender, Race/ethnicity), legal history and case processing information, family environment information (guardian information, history of past

⁶ Juvenile offender: A youth ages 13, 14, or 15 who has committed a serious violent offense (murder, arson, robbery) and is waived to the adult system.

abuse/neglect) and other extra-legal characteristics such as peer relationships, participation in extra-curricular activities, and mental health history.

Due to the nature of data collection, official data reviewed and then recoded by researchers from the Vera Institute, it is important to understand the steps taken by the data coders to ensure reliability and consistency in recoding the case files throughout the Family Courts in the five boroughs. Data coders were rotated amongst the five locations, and a senior staff member at the Vera Institute reviewed the coding for each file as it was completed from the researchers. In addition, information in the case files was checked by comparing the information with other state and local legal and administrative databases, ensuring the most accurate re-arrest information possible. This process no doubt is responsible for a very low ‘drop-out’ rate of juveniles from the sample; only five cases had to be excluded because their rearrest information was unknown (they did not finish their probation sentence, but were not recorded as being rearrested).⁷

Description of the Sample

A total of 414 first-time arrestee juvenile delinquents were processed in Family Court from April-June. Five cases had to be omitted due to incomplete rearrest case files leaving a total of 409 for analysis. The data includes juveniles who were sentenced either to placement in a detention facility (hereafter referred to as ‘placement’) or were sentenced to community-based sanctions. Receiving a community-based can come in three main forms for juveniles. Those in need of

⁷ If a juvenile aged out of Family Court jurisdiction during the 18-month follow-up period, any arrest recorded as an adult was recorded and included in this dataset.

more strict community supervision are handled by the Juvenile Intensive Supervised Probation (JISP) or the Center for Alternative Sentencing and Employment Services (CASES). With these two programs, juveniles at high risk for recidivism and other risky behaviors are diverted and sent to programs such as drug and alcohol treatment, and mental health counseling. Educational opportunities are also enhanced to combat the high drop-out rates of juveniles who have been detained.⁸ Less serious community-based sanctions are given in the form of normal probation with the usual requirements of drug testing, staying arrest-free and attending school. Due to the similarity in both appearance and time requirements, for the purposes of this study all of the juveniles sent to any form of probation will be considered as one group (hereafter referred to as ‘probation’).

As shown in Table 3, 145 youths were sentenced to placement in detention, comprising 35% of the total sample. There are three detention centers where the youths could have been sent: Horizons, Bridges (located in the Bronx) and Crossroads (located in Brooklyn). Horizons Juvenile Center and Crossroads are the newest facilities, opened in 1998. Bridges was formerly known as Spofford, and has been a juvenile detention facility since the mid-1970s. After renovations in 1999 Spofford was renamed Bridges Juvenile Center. All three of these facilities are ‘secure detention facilities’ which means that juveniles are under lock-and-key at night and their activities are closely monitored by guarded officials. Generally, those who have received longer sentences are sent to either Horizons or Crossroads,

⁸ For more information on community-based sanctions in NYC :
<http://dpca.state.ny.us/familycourt.htm>

with Bridges also serving as a temporary holding facility for juveniles awaiting hearings. Attending school classes is a requirement at each facility, and the NYC Department of Juvenile Justice also has a Behavior Management Program (BMP) administered at each facility. Each juvenile is required to participate in this program which is focused on the youths taking responsibility for their actions. Good behavior and active participation in the program is rewarded with additional phone calls and access to the lounge and commissary.

| Table 3 | | |
|--|------------------|----------------|
| Description of sample by disposition type of youths in NYC, Spring 2000 | | |
| | Frequency | Percent |
| Probation | 264 | 64.55% |
| Placement | 145 | 35.45% |
| Total | 409 | 100.00% |

| Table 4 |
|---|
| Demographics of youths receiving disposition in NYC, Spring 2000 |

| | Frequency | Percent |
|------------------------------------|------------------|----------------|
| Gender | | |
| Male | 296 | 72.7% |
| Female | 113 | 27.6% |
| Race/Ethnicity | | |
| White | 30 | 7.3% |
| Black | 236 | 57.7% |
| Hispanic | 121 | 29.6% |
| Other | 22 | 5.4% |
| Age at Disposition | | |
| 12 and under | 45 | 11.0% |
| 13 | 70 | 17.1% |
| 14 | 133 | 32.5% |
| 15 | 161 | 39.3% |
| Family on Public Assistance | 187 | 45.72% |

Demographically, the current sample of first-time youth offenders is a heterogeneous population of youth involved with the family court system in NYC. The sample is predominantly male (72%) and over 90% of the sample are persons from a racial or ethnic minority, mostly black and Hispanic. The mean age of youths at the time of their initial disposition is 14 years old, and about 45% of the youths come from families that receive some form of public assistance. Table 4 below contains a breakdown of the basic demographic characteristics.

In addition to this demographic information, case files for each juvenile compile information on a variety of legal and extralegal characteristics. This information is taken from pre-sentencing Probation Investigation and Recommendation Reports (I & R), intake reports, school records, and arrest reports. All of this information together allows the judges a more comprehensive understanding of the juvenile's family, school, and behavioral background before assigning punishment. The operational definitions of these characteristics are displayed in Table 5 below.

| Table 5 | |
|---|--|
| Operational definitions of control variables | |
| Variable | Metric |
| Sex of the juvenile | (female=0 male=1) |
| Race of the juvenile | White (0,1); Black (0,1); Hispanic (0,1); Other (0,1) |
| Age at 1 st arrest | (12 and under; 13; 14; 15) |
| Family on Public Asst. | (0,1) |
| Previous PINS complaints | (0,1) |
| Type of Initial Crime | Violent (0,1); Property (0,1); Drug (0,1); Other (0,1) |
| Severity of Initial Crime | Other/DK (0,1); Misdemeanor (0,1); Felony C/D/E (0,1); Felony A/B(0,1) |
| Length of Disposition | (<12 months, 12 mo., 13 – 17 mo., 18 mo., >18 mo.) |
| History- Violent Behavior | (0,1) |
| History- Fire Starting | (0,1) |
| History- Animal Cruelty | (0,1) |
| History- Sexual Aggression | (0,1) |
| Good School Att. (>90%) | (0,1) |
| In Special Education | (0,1) |
| Current/past drug use | (0,1) |
| Gang affiliated | (0,1) |
| Sexually Abused | (0,1) |
| Obedient to Parents | (0,1) |
| Parents want youth in detention | (0,1) |
| Guardian Information | 2-parent home (0,1); 1-parent home (0,1); inst./homeless(0,1) |

The New York State Office of Children and Family Services (OCFS) provide information about any rearrests after the first punishment. This includes the time spent in detention, number of days the juvenile remained arrest-free after release, and whether or not the juvenile was rearrested. The current study utilizes a follow-up period of 18 months after release and assesses recidivism with a binary variable indicating rearrest within 18 months. This means that for juveniles who were sent to probation, the ‘clock’ starts upon release back into the community while on probation, whereas for youths sent to detention their 18 months of follow-up time begins after they are released from detention.

The extra-legal characteristics that complete the results in Table 6 were collected by case workers during the fact-finding phase of the court proceedings and information in this file was available for judges at the time of sentencing (see Appendix C). Most of the information in this file was collected by interviewing parents, teachers, and other adult supervisors of the youths, and the final result was a report on the youth’s history of past behavior both in school and at home. Some of the information about the youth’s history reports on possible delinquent acts, such as fire starting, violent and assaultive behavior, and sexual aggression. However, because this sample includes only youths who had never been arrested, we know that none of this potentially delinquent behavior was ever officially sanctioned.

With the exception of OCFS information about rearrests, all information in the case files provides a fairly comprehensive ‘snapshot’ of the juvenile at the time of their initial disposition. Table 5 summarizes these characteristics, and displays them both for the entire sample and divided by disposition type (Placement v.

Probation). Using an alpha-level of 0.05, t-tests were calculated to determine significant differences in characteristics between the two disposition types.

Table 6 reveals that the typical first-time juvenile offender is adjudicated on a violent charge; while their average sentence is a little over one year (12.8 months) some juvenile delinquents spend as long as 18 months in juvenile detention and over 18 months on probation. A large proportion of juveniles have a history of violent/assaultive behavior (70.2%) and a little over 7 in 10 juveniles in the sample have been coded as being affiliated with a gang.

Comparing the two groups showed that the groups are fairly similar with regards to gender, race/ethnicity, and age at the time of the first arrest. There were significantly fewer white youths in the group sent to placement (4 or 2.8% of the group) compared with those sent to probation (26 or 9.9% of the group) but the groups had similar compositions of minority youths. Each group was predominantly male, and the average age for each group was about 14 years old.

| Table 6 | | | | | | |
|--|-------------------------------|---------------------------------|---------------------------|---------------------------------|----------------------------|---------------------------------|
| Descriptive legal and extra-legal characteristics of youths by disposition type | | | | | | |
| | Total Sample (N = 409) | | Placement (n =145) | | Probation (n = 264) | |
| | N | Mean (SD) Proportion | n | Mean (SD) Proportion | n | Mean (SD) Proportion |
| Male | 296 | 0.724 | 109 | 0.752 | 187 | 0.708 |
| Race/Ethnicity | | | | | | |
| White | 30 | .0730 | 4 | 0.028* | 26 | 0.099 |
| Black | 236 | 0.577 | 87 | 0.600 | 149 | 0.564 |
| Hispanic | 121 | 0.296 | 47 | 0.324 | 74 | 0.280 |
| Age at First Arrest | | 14.00^ (1.00) | | 14.08^ (0.946) | | 13.96^ (1.03) |
| Family on Pub. Asst. | 187 | 45.72 | 121 | 45.83 | 66 | 45.52 |
| Any Prior PINS | 90 | 0.220 | 56 | 0.386* | 34 | 0.129 |
| Type of Charge | | | | | | |
| Violent Charge | 219 | 0.535 | 65 | 0.583* | 154 | 0.448 |
| Property Charge | 131 | 0.320 | 53 | 0.366 | 78 | 0.295 |
| Drug Charge | 36 | 0.088 | 18 | 0.124 | 18 | 0.068 |
| Other/DK Charge | 23 | 0.056 | 9 | 0.062 | 14 | 0.053 |
| Severity of Charge | | | | | | |
| Felony A/B | 7 | 0.017 | 2 | 0.014 | 5 | 0.019 |
| Felony C/D/E | 126 | 0.308 | 53 | 0.365 | 73 | 0.276 |
| Misdemeanor | 258 | 0.631 | 88 | 0.607 | 170 | 0.644 |
| Other/DK | 18 | 0.044 | 2 | 0.014* | 16 | 0.061 |
| Disposition Length | | 12.81 (6.061) | | 12.51 (6.504) | | 12.973 (5.809) |
| < 12 months | 36 | 0.088 | 13 | 0.089 | 23 | 0.087 |
| 12 months | 238 | 0.582 | 84 | 0.579 | 154 | 0.583 |
| 13 - 17 months | 10 | 0.024 | 1 | 0.007 | 9 | 0.034 |
| 18 months | 98 | 0.239 | 47 | 0.324 | 51 | 0.193 |
| > 18 months | 27 | 0.066 | 0 | 0 | 27 | 0.102 |
| History-Violent Beh. | 287 | 0.702 | 113 | 0.779* | 174 | 0.659 |
| History- Fire Starting | 15 | 0.037 | 6 | 0.041 | 9 | 0.034 |
| History- Animal Cruelty | 6 | 0.015 | 1 | 0.007 | 5 | 0.019 |
| History Sexual Aggr. | 32 | 0.078 | 13 | 0.090 | 19 | 0.072 |
| >90% School Att. | 61 | 0.149 | 7 | 0.048* | 54 | 0.205 |
| In Special Education | 114 | 0.279 | 50 | 0.345* | 64 | 0.242 |
| Current/past drug use | 157 | 0.384 | 88 | 0.607* | 69 | 0.261 |
| Gang Affiliated | 317 | 0.775 | 94 | 0.648* | 223 | 0.845 |
| Sexually Abused | 26 | 0.063 | 14 | 0.097* | 12 | 0.045 |
| Obedient to Parents | 224 | 0.548 | 38 | 0.261* | 186 | 0.704 |
| Par. Want Placement | 54 | 0.132 | 43 | 0.297* | 11 | 0.042 |
| Guardian Information | | | | | | |
| 2-parent home | 108 | 0.264 | 31 | 0.214 | 77 | 0.292 |
| 1-parent/guardian | 266 | 0.650 | 90 | 0.621 | 176 | 0.667 |
| Inst./Homeless | 35 | 0.086 | 24 | 0.166* | 11 | 0.042 |
| TOTAL | 409 | - | 145 | 0.355 | 264 | 0.645 |

^ the mean age will be slightly inaccurate because the '12 and under' category covers all juveniles ages 7 - 12

*difference is significant from probation group at $\alpha = .05$ level

When comparing differences between the two punishment groups, it is obvious that youths who were sent to detention are different on several characteristics from those sent to probation. Most of these differences indicate that detention was reserved for the more serious juvenile delinquents. A higher percentage of the placement youths had prior status offense complaints on file, called 'Persons in Need of Supervision' (PINS), and those who were sent to placement had a significantly higher percentage adjudicated for violent and drug crimes. With regards to the severity of offenses, the results are a little surprising; a slightly higher proportion of the youths who committed a class A/B felony (the most serious) were sent to probation, although this was only 5 youths compared to the 2 A/B felony youths sent to detention. A slightly larger proportion of placement youths committed class C/D/E felonies, although the difference was not significant. A much larger number of the juveniles who committed misdemeanor offenses were given probation, however in terms of proportions of each punishment group, the juveniles sent to probation were not significantly more likely to have committed a misdemeanor.

Examination of this prior history information reveals that a higher percentage of youths sent to detention compared to those sent to probation displayed a variety of characteristics and behavioral descriptions that put them at an elevated risk for delinquency. In particular, youths sent to placement were more likely to have been recorded as having current or past drug use (60.7%), disobedience to parents (26.1%), parents wanting their child to be placed in detention (29.7%), history of violent or assaultive behavior (79%), history of sexual victimization (9.7%), and

poor school attendance, a variable indicating whether or not the youth attends school at least 90% of the possible days of classes. For this variable, only 4.8% of the youths sent to detention had attendance records of at least 90%, compared with 20.5% of the youths sent to probation. The family structures of the youths from each group were similar; similar proportions of youths came from one- and two-parent homes.⁹ The only exception being that a higher percentage of youths sent to detention had previously been homeless or were living in an institutional setting: 16.6% compared with only 4.2% of the youths sent to probation.

Dependent Variable: Measurement of Recidivism

There have been a variety of ways in which recidivism has been measured for juvenile delinquents and adult offenders. In some cases, studies have defined recidivism as a juvenile having an additional police contact after the initial incident (Ageton & Elliot, 1974). Others have chosen to look farther into the process and measure recidivism at the disposition level, measuring whether or not the offender was placed back under correctional supervision (Toombs et al., 1997). However, there has been growing support amongst those interested in juvenile re-entry populations that utilizing rearrest information as a measurement of recidivism captures the most accurate body of those who commit additional offenses (Gruenwald & West, 1989; Visher et al., 1991).

Given the variety of methods used to divert juveniles, measuring recidivism only at the point of official sanction may not capture all of the juveniles who re-

⁹ 2-parent homes: Distinction was not made in the data as to whether or not this had to be an intact home, or if one of the two parents could be result of a divorce and re-marriage.

offended. Conversely, measuring any youth who has a subsequent police contact runs the risk of recording juveniles as recidivating when in fact they were merely picked up, questioned, and then released. Therefore, in the current study juvenile recidivism is measured as a dichotomous variable; if the juvenile offender was officially rearrested within the 18 month follow-up period after release they were coded with a '1.' All those who had not been rearrested 18 months after release were coded with a '0.' For juveniles sent to probation, the 18 months includes the probation period, whereas for juveniles in placement, the 18 months begins after release from detention. Measurement at this level attempts to avoid as many 'false-positives' and 'false-negatives' as possible, and obtain an accurate picture of the future delinquency of the juveniles. Blumstein and Cohen (1979: 565) have stated that measurement at the rearrest level is a better indicator than measurement later in the process, such as conviction or disposition, because "the errors of commission associated with truly false arrests are believed to be far less serious than the errors of omission that would occur if the more stringent standard of conviction were required." Only one rearrest was recorded for each youth in the current study, therefore the variable recording recidivism at 18 months includes all juveniles who were rearrested during that time frame. A limitation of the current study, however, is that it was not possible to ascertain whether juvenile on probation were rearrested for rule-violations specific to their probation, or for actual crimes.

Table 7 describes the rearrest information for the entire sample. Out of the entire sample of 409 youths, 154 (about 38%) of the sample were arrested again within 18 months. Recidivism rates for juveniles are difficult to maintain on a state

level, and there is no measure of a national juvenile recidivism rate due to the many differences that exist between states in their treatment of juvenile delinquents.

However, according to the 2006 National Report from the Office of Juvenile Justice and Delinquency Prevention (OJJDP), New York State has a juvenile recidivism rate of approximately 55% (Snyder & Sickmund, 2006: 234). Given the fact that we only have NYC first-time arrestees and from just 3 months out of the year, this difference may be due to sampling.¹⁰

| Table 7 | | | | | | |
|---|------------------------|---------|---------------------|---------|---------------------|---------|
| Description of sample's rearrest information at 18 months- by disposition type | | | | | | |
| | Total Sample (N = 409) | | Placement (n = 145) | | Probation (n = 264) | |
| | Frequency | Percent | Frequency | Percent | Frequency | Percent |
| Rearrested | 154 | 37.70% | 67 | 46.21% | 87 | 32.95% |
| Not Rearrested | 255 | 62.30% | 78 | 55.18% | 177 | 67.05% |

It is apparent from the preliminary evidence in Table 7 that a larger proportion of those sent to placement in detention get rearrested (46% compared with 33%). This initially suggests that incarceration has an impact on those who spend time in detention, but these differences have not yet taken the selection process into account.

¹⁰ The re-offending data in the OJJDP's 2006 National Report is measured after a 12 month follow-up period, and the information about New York is averaged together with information from Florida, and Virginia. For details, see Snyder & Sickmund, 2006: 234.

Control Variables

Table 5 displays the operational definitions of all control variables in the analyses. I have utilized important demographic, legal, and extra-legal characteristics as control variables in this study. The variable indicating length of disposition was coded using a scale ranging from a sentence less than 12 months to greater than 18 months. The only juveniles who received sentences of over 18 months were youths put on probation; the highest sentence a juvenile sent to placement in detention received was 18 months.

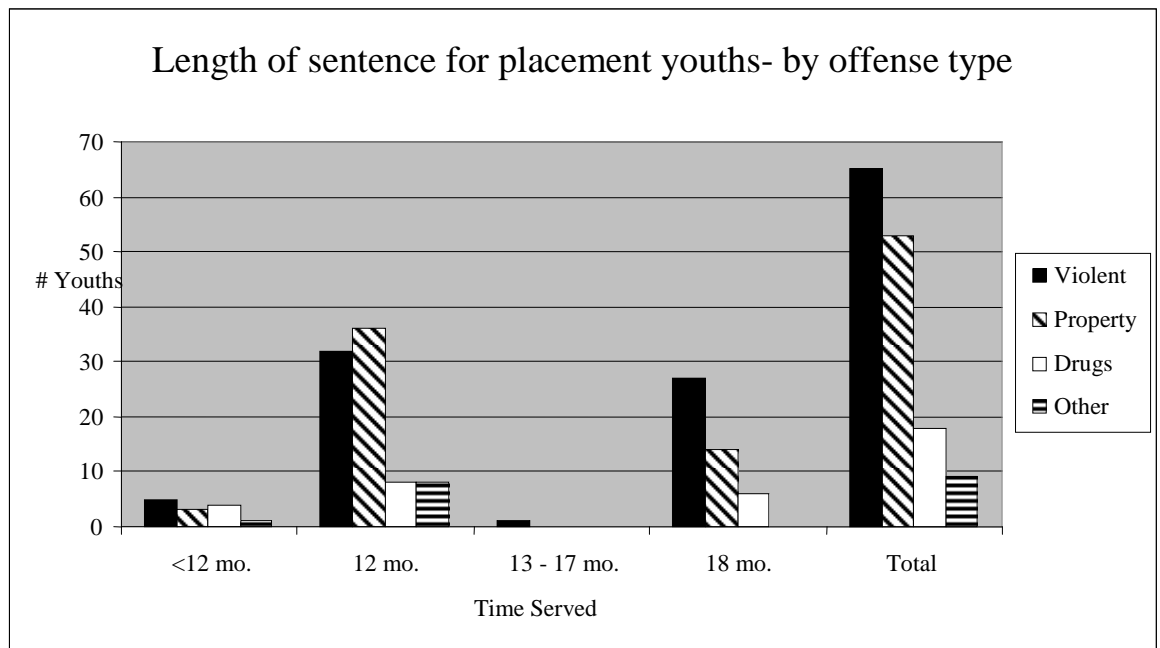
Examining the Effect of ‘Dosage’

The current dataset is limited with regards to analyzing the effects of time served in detention. Looking only at juveniles who were sent to placement (N = 145), the current data provide information on time spent in detention in the form of a scale: Less than 12 months; 12 months; 13 – 17 months; 18 months. Table 8 displays the number of youths serving each length of sentence:

| Table 8 | | |
|--|----------|----------|
| Frequency of placement youths at each length of time served (N=145) | | |
| | n | % |
| <12 mo. | 13 | 8.97 |
| 12 mo. | 84 | 57.93 |
| 13 - 17 mo. | 1 | 0.69 |
| 18 mo. | 47 | 32.41 |

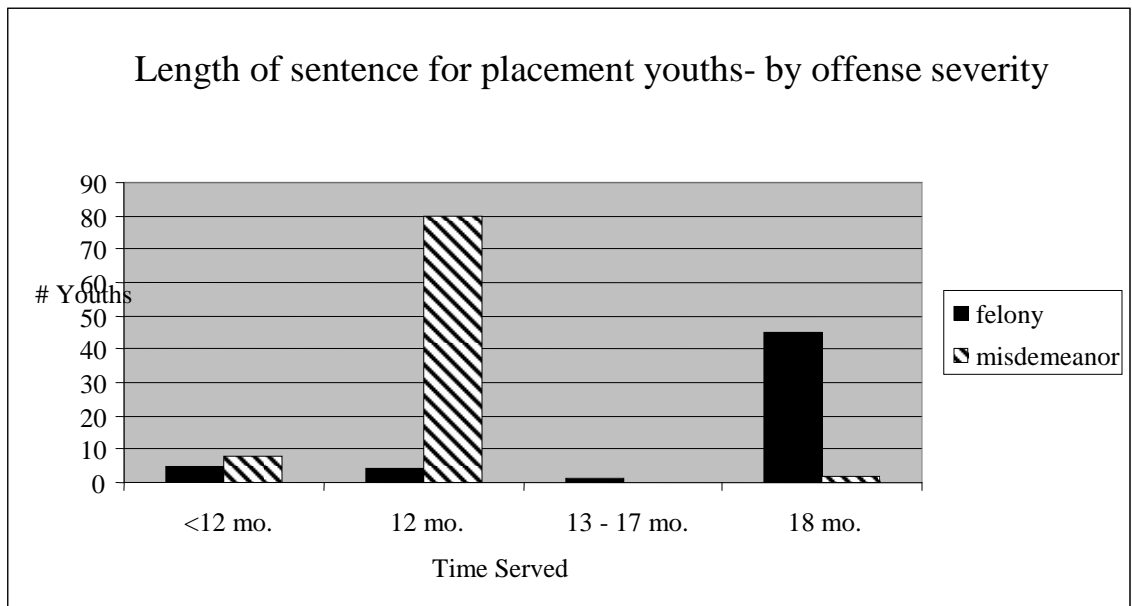
When examining the breakdown of time served by type of offense, Table 8 reveals that a large proportion (57%) of those with the most severe sentence of 18 months in detention committed violent offenses. Out of the total youths who committed property offenses and were sent to detention, over two-thirds received a sentence of one year, although 27% were sentenced to 18 months. There was only one juvenile sent to placement who received a sentence within 13 and 17 months, most likely this is due to Family Court guidelines, although the reason for the lack of sentencing within this range is not able to be ascertained in the current data collection. For the current analyses, the one youth who was given a 13-17 month detention sentence is combined with the youths who received a sentence of 18 months.

Figure 3



Not surprisingly, 45 out of the 55 youths who committed a felony offense were sentenced to the longest time in detention (18 months). In addition, Figure 4 reveals that almost all (89%) of the misdemeanor cases were sentenced to 12 months in detention. The case with a sentence between 13 and 17 months was previously identified as a violent offense, and Figure 4 also reveals that this case was adjudicated on a felony charge.

Figure 4



Addressing the Problem of Selection Bias

As indicated in the initial description of frequencies for characteristics amongst the two punishment groups, the current sample presents the problem of selection bias which must be addressed in the analysis. The case files reveal that on a variety of both legal and extra-legal characteristics, there are some significant pre-existing differences between the youths sent to probation and those sent to detention.

Namely, it seems that juveniles who have a more extensive violent/assaultive history, a record of status offense complaints, and who have committed a more serious crime were the ones selected to receive placement in detention by the judge.

While it is a practical function of the sentencing process that more serious offenders be given the more severe punishments, this selection process makes it difficult to analyze the effects of the punishment experience at face value. With two groups fundamentally unbalanced on so many prior observable characteristics, it is difficult to determine if juveniles who are sent to placement recidivate more because of the detention experience, or if they were simply ‘bad apples’ in the first place and would have been more likely to recidivate anyway. Several techniques have been utilized in the past to attempt to account for these unbalanced groups obtained through non-random assignment. In the current analysis, propensity score matching (PSM) is discussed and implemented.

Propensity Score Matching

The motivation behind using PSM as opposed to other methods of controlling for selection bias lies in the desire to get as close as possible to the counterfactual relationship. The counterfactual question that I would ideally like to answer is: *If the juveniles sent to placement had actually been sent to probation, would their recidivism rate be lower?* Obviously, it is not possible to go back in time and redirect these first-time offenders into probation and compare their rates. Therefore, the goal is to compare the juveniles sent to placement with juveniles sent to probation who best match them.

Referring back to Table 5, many of the observed characteristics out of balance amongst the two disposition groups are also characteristics known to be highly correlated to recidivism, such as prior status offense complaints, history of violent/assaultive behavior, etc. Propensity score matching offers a technique where the balance of observable confounding characteristics has been achieved (Rosenbaum & Rubin, 1983, 1984; D'Agostino & Rubin, 2000). The caveat when using this technique, however, is that balance can only be achieved for those characteristics which have been observed and measured. Unobserved heterogeneity may still be present amongst the two groups.

T-statistics comparing the two punishment groups on all covariates provides evidence of how unbalanced the groups are. Generally, a t-statistic greater than 2 yields a p-value that is significant at $\alpha = 0.05$, meaning that the groups are different on that covariate (Rosenbaum & Rubin, 1983). As shown in Table 9, the groups are out of balance on several characteristics, as evidenced by their significant p-values.

| Table 9 | | | | |
|--|-------------------|------------------|----------------|---------------|
| Comparison of balance between punishment groups (means), before PSM | | | | |
| Variable | Before PSM | | | % Bias |
| | Probation | Placement | p-value | |
| History of Status Complaints | 0.12879 | 0.38621 | 0.000 | 61.4 |
| Adj. on Violent Charge | 0.58333 | 0.44828 | 0.009 | -27.1 |
| Initial Crime is a Felony | 0.29545 | 0.37931 | 0.084 | 19.1 |
| History of Violent/Assault Behavior | 0.65909 | 0.77931 | 0.011 | 26.9 |
| Good School Attendance ($\geq 90\%$) | 0.20455 | 0.04828 | 0.000 | -48.3 |
| In Special Education | 0.24242 | 0.34483 | 0.027 | 22.6 |
| Current/past drug use | 0.26136 | 0.60689 | 0.000 | 74.2 |
| Gang Affiliated | 0.84470 | 0.64828 | 0.000 | -46.2 |
| History of Sexual Victimization | 0.04545 | 0.09655 | 0.043 | 19.9 |
| Obedient to Parents | 0.29545 | 0.73793 | 0.000 | 98.5 |
| Parents want Child in Placement | 0.04167 | 0.29655 | 0.000 | 72.1 |
| Living in institution/homeless | 0.04167 | 0.16552 | 0.000 | 41.4 |
| Table only highlights covariates near or completely out-of-balance | | | | |

Another suggested technique for determining how unbalanced the groups are prior to matching involves finding the standardized bias statistic (% Bias) for each original covariate (Rosenbaum & Rubin, 1983). This can be calculated as follows:

$$100 * \frac{\bar{x}_t - \bar{x}_c}{\sqrt{\frac{s_t^2 + s_c^2}{2}}}$$

In this equation \bar{x}_t is the sample mean for the treatment (placement) group and \bar{x}_c is the sample mean for the control (probation) group; s_t^2 and s_c^2 are the respective sample variances. A standardized bias statistic greater than 20 for any of the covariates is an indication that it is out of balance between the treatment and

control groups (Loughran et al., in press; Rosenbaum & Rubin, 1983). Table 9 above displays the percent bias statistics for all covariates initially out of balance.

Matching is useful in the current sample where there are almost twice as many youths sent to probation as those receiving the ‘treatment’ in this study, placement in detention. It is unlikely that we will find 145 youths in the probation group who are completely identical in background characteristics to each youth sent to placement. Given the fact that the juveniles are matched on about 20 characteristics, there are approximately one million different combinations of covariates that could exist in the data. Therefore, propensity score matching does not exactly match each youth on all of their characteristics, but rather they are matched on their conditional probability of receiving the treatment given the observed covariates (Loughran et al., in press; Rosenbaum & Rubin 1983). When the individuals receiving the treatment are matched with a control (probation) group individual with an identical propensity score, their treatment status is independent of the covariates observed and accounted for in the model.

Assuming the notation that y_1 denotes the probability of a youth getting rearrested given they were sent to detention, and y_0 denotes the probability of a youth getting rearrested without getting sent to detention, I am interested in $y_1 - y_0$ but cannot actually observe it. The expected effect of placement on recidivism for a random individual in the sample is $E(y_1 - y_0)$ and represents the average effect of treatment (ATE) which in this study refers to placement. Assuming that $Z=1$ denotes placement in detention and $Z=0$ denotes probation, the average effect of treatment on the treated (ATT) is represented as $E(y_1 - y_0 / Z=1)$. For the purposes of

the current study, I am interested in the ATT. It may be the case that some of the youths sent to probation won't ever be at risk for detention, therefore ATT is the more interesting and meaningful effect to measure.

I have conditioned the groups on the 20 covariates, which in each individual can be represented by \mathbf{x} , therefore making the ATT: $E(y_1 - y_0 / \mathbf{x}, Z=1)$. Rosenbaum & Rubin (1983) have proven that for $e(\mathbf{x}) = P(Z = 1 | \mathbf{x})$,

$$\mathbf{x} \perp\!\!\!\perp Z | e(\mathbf{x})$$

This indicates that the 20 covariates (\mathbf{x}) are conditionally independent of whether or not the youth was sent to detention (Z), given the function $e(\mathbf{x})$. Therefore the above equation gives us the predicted probability in the final model, the propensity score, $\hat{e}(\mathbf{x})$. Despite the fact that the covariates \mathbf{x} might strongly predict who will get sent to detention ($Z = 1$), for youths who have the *same value* or very closely matching values of the propensity score $\hat{e}(\mathbf{x})$, \mathbf{x} will *not* predict treatment assignment Z , and the groups are comprised of individuals who have the same probabilities for receiving the treatment, which in this study is getting sent to detention. The difference in assignment of youths to placement or probation is now only different by chance; the groups have been balanced on observable characteristics.

It is important to recognize that propensity score matching does *not* ensure that matched individuals have the same values of all the covariates represented by \mathbf{x} . Rather, their propensity score has been matched; the probability of being sent to placement is equal despite the fact that small differences in precise scores of \mathbf{x} exist

(Loughran et al., in press). In addition, it is important to acknowledge that when using PSM, it is only possible to balance the groups on *observed* heterogeneity. The limitations of this will be discussed in the results and conclusions section of this paper.

Estimation of the Propensity Score

The first step in using propensity score matching is to use a logistic regression model with the variable measuring punishment type as the dependent variable. Logistic regression is appropriate here due to the binary nature of the punishment variable (probation = 0, placement = 1) and has been proven effective in several previous studies to begin PSM (Rosenbaum & Rubin, 1984, 1985). In addition, the probabilities that can be obtained from logistic regression model are restricted to being within 0 and 1. All 20 covariates are included in this model and can be thought of as groups of demographic characteristics, legal controls, and extra-

legal controls, therefore:

$$\Pr(Z = 1 | x) = \frac{e^{x\beta}}{1 + e^{x\beta}}$$

where

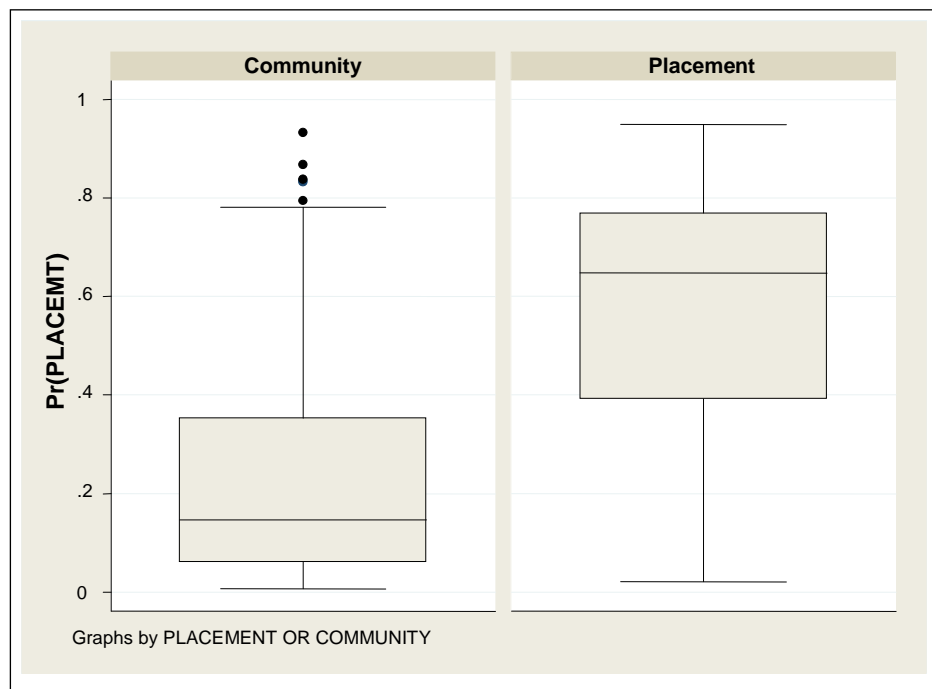
$$x\beta = \beta_0 + \beta_1 \text{DemographicCharacteristics} + \beta_2 \text{LegalControls} + \beta_3 \text{ExtraLegalControls}$$

The predicted probability that each youth received placement [their propensity score $\hat{e}(\mathbf{x})$] is then calculated based upon the 20 covariates included in the logistic regression model and common support is assessed. There must be adequate overlap in the predicted probabilities for each placement juvenile to have a

match in the control group, each placement youth having common support. If, for example there were very few juveniles sent to probation who had a similar propensity to be placed in detention, attempts to match and balance the two samples would be difficult. (McCaffrey, 2004; Rosenbaum & Rubin, 1983).

As shown in Figure 5, there is a considerable amount of overlap in the overall ranges of probability of placement for the probation and placement punishment groups. This aids in confidence that it will be possible to create a balanced sample based on each juvenile's propensity score.

Figure 5 Ranges of Pr(Placement)- by punishment type



Once the propensity scores have been estimated, nearest-neighbor matching is employed to create matched samples.¹¹ In nearest-neighbor matching, each individual t in the treatment group is matched with one control individual c as follows:

$$|p_t - p_c| = \min_{k \in \{D=0\}} \{|p_t - p_c|\}$$

where p_t and p_c are the propensity scores for the matched treatment and control individuals, respectively. The farthest acceptable distance between the propensity scores is designated a caliper; it is generally assumed that caliper distances farther than 0.05 between nearest-neighbors are unacceptable (Rosenbaum & Rubin, 1983). Maintaining small distances between the scores of matched pairs is essential because it establishes balance between the punishment groups. In this case, all 145 juveniles sent to placement were able to be matched with a juvenile from probation within the 0.05 distance, yielding a total matched sample of 290 youths.

The average effect of treatment on the treated (ATT) is estimated, because our control group now matches the original counterfactual $E(y_1 - y_0) / Z=1$. Therefore, estimation of ATT is as follows:

$$ATT = \frac{1}{n} \sum (y_1 - y_0)$$

¹¹ There are several possible ways to match individuals in PSM. For the purposes of this study, nearest-neighbor with 2 matches, caliper matching at 0.05, and matching without replacement were also explored. Results did not significantly differ from single nearest-neighbor matching, so only the results of this method are reported here.

Chapter 4: Results

The first hypothesis stated that after creating balanced samples on all covariates, any effect of detention on future recidivism will disappear. Table 10 displays the differences between the treatment effects before and after creating matched samples.¹² The initial t-statistic prior to matching indicates a positive and significant relationship between treatment and rearrest at 18 months (2.66). However, after controlling for the observable selection bias in the sample, the significant of the t-statistic is completely reduced and is slightly negative. This provides evidence that after balancing the samples the effect of being sent to placement is null; juveniles sent to placement do not appear to be any more likely to recidivate than their probation matches.

| Table 10 | | | | | | |
|--|-------------|---------|----------|------------|--------|--------|
| Treatment effect of incarceration before and after matching (means) | | | | | | |
| Variable | Sample | Treated | Controls | Difference | S.E. | T-stat |
| Rearrest: 18 mo. | Unmatched | 0.4621 | 0.3295 | 0.1325 | 0.0498 | 2.66 |
| | Matched-ATT | 0.4621 | 0.5241 | -0.0620 | 0.1053 | -0.63 |

order to determine if the controls used for matching actually did have propensity scores matching or in very close proximity to their similar placement juvenile, the absolute distance between their scores is calculated. The mean distance between the nearest neighbor matches was 0.006 (Std. Dev 0.007) and the largest was 0.035, well

¹² Prior to using PSM a logistic regression model was run to determine if detention had a significant effect prior to matching. The coefficient for detention was positive (0.558, s.d 0.212) and significant with a p-value of 0.008. This corresponds to the higher percentage of youths sent to placement who were rearrested, as shown in Table 6.

below the recommended 0.05 (Rosenbaum & Rubin, 1983). This evidence is consistent with the first hypothesis of the study, which stated that after creating balanced samples, any effect of detention will disappear.

Previously, Table 9 displayed all of the covariates originally out of balance. After PSM, balance has been achieved in nearly all of these covariates, as shown in Table 11. This table also includes all covariates present in the analysis, including those who were previously in balance. The only two covariates out of balance after PSM is applied are binary variables indicating enrollment in special education and knowledge of current or past drug use. It appears that PSM ‘overmatched’ the groups on current/past drug use and caused the probation group to have a higher proportion than the placement group. It also seems as though PSM did not increase the matching between the groups for enrollment in special education, and in effect caused them to be ‘undermatched’ on this covariate.¹³

¹³ The covariates indicating enrollment in special education and current/past drug use experienced a small increase in bias after PSM; after matching the probation group had a larger proportion of these than the placement group. The covariate for living in an institution or being homeless also bordered on being out of balance after PSM. In order to assess whether placement had any effect on the outcome after controlling these factors, a logistic regression analysis was run. Its results can be seen in Appendix A. After running this analysis, placement remains insignificant. Therefore the variables do not seem to significantly alter the results.

| Variable | Before PSM | | | After PSM | | |
|--------------------------------------|------------|-----------|---------|-----------|-----------|---------|
| | Probation | Placement | p-value | Probation | Placement | p-value |
| History of Status Complaints | 0.12879 | 0.38621 | 0.000 | 0.41379 | 0.38621 | 0.633 |
| Adj. on Violent Charge | 0.58333 | 0.44828 | 0.009 | 0.51034 | 0.44828 | 0.292 |
| Initial Crime: Class C/D/E | | | | | | |
| Felony | 0.27652 | 0.36552 | 0.062 | 0.40000 | 0.36552 | 0.547 |
| History of Violent/Assault. Behavior | 0.65909 | 0.77931 | 0.011 | 0.82759 | 0.77931 | 0.303 |
| Good School Attendance (>90%) | 0.20455 | 0.04828 | 0.000 | 0.06207 | 0.04828 | 0.608 |
| In Special Education | 0.24242 | 0.34483 | 0.027 | 0.23448 | 0.34483 | 0.038 |
| Current/past drug use | 0.26136 | 0.60689 | 0.000 | 0.73103 | 0.60690 | 0.025 |
| Gang Affiliated | 0.84470 | 0.64828 | 0.000 | 0.62759 | 0.64828 | 0.715 |
| History of Sexual Victimization | 0.04545 | 0.09655 | 0.043 | 0.11724 | 0.09655 | 0.570 |
| Obedient to Parents | 0.29545 | 0.73793 | 0.000 | 0.75172 | 0.73793 | 0.789 |
| Parents want Child in Placement | 0.04167 | 0.29655 | 0.000 | 0.27586 | 0.29655 | 0.698 |
| Living in institution/homeless | 0.04167 | 0.16552 | 0.000 | 0.08966 | 0.16552 | 0.053 |
| Male | 0.70833 | 0.75172 | 0.349 | 0.75172 | 0.75172 | 1.000 |
| Black | 0.56439 | 0.60000 | 0.487 | 0.54483 | 0.60000 | 0.344 |
| Hispanic | 0.28030 | 0.32414 | 0.354 | 0.36552 | 0.32414 | 0.460 |
| Other Race (white ref.) | 0.05682 | 0.04828 | 0.715 | 0.06207 | 0.04828 | 0.608 |
| Age at Arrest | 13.958 | 14.083 | 0.231 | 14.131 | 14.083 | 0.681 |
| Family on Public Assistance | 0.45833 | 0.45517 | 0.951 | 0.53793 | 0.45517 | 0.160 |
| Adj. on Property Charge | 0.29545 | 0.36552 | 0.147 | 0.36552 | 0.36552 | 1.000 |
| Adj. on Drug Charge | 0.06818 | 0.12414 | 0.056 | 0.09655 | 0.12414 | 0.455 |
| Initial Crime: Class A/B | | | | | | |
| Felony | 0.01894 | 0.01379 | 0.702 | 0.0001 | 0.01379 | 0.157 |
| Initial Crime is a Misdemeanor | 0.64394 | 0.6069 | 0.459 | 0.55712 | 0.6069 | 0.343 |
| Disposition Length | 13.958 | 13.890 | 0.823 | 14.083 | 13.890 | 0.586 |
| History of Fire Starting | 0.03409 | 0.04138 | 0.708 | 0.02069 | 0.04138 | 0.311 |
| History of Animal Cruelty | 0.01894 | 0.0069 | 0.334 | 0.1379 | 0.0069 | 0.563 |
| History of Sexual Aggression | 0.07197 | 0.08966 | 0.525 | 0.07586 | 0.08966 | 0.671 |
| Lives in One-Parent Household | 0.6667 | 0.62069 | 0.352 | 0.67586 | 0.62069 | 0.327 |

The standardized bias statistics in Table 12 also reveal a decrease in bias for all the variables whose statistic was previously greater than |20|. Several of the

variables whose percent of initial bias was originally below |20| experienced fluctuations that made them slightly more out-of-balance, but all stayed well within the desired range. For example, the variable indicating that the youth had a history of fire-starting experienced a 183.9% increase in bias, but this was actually only a jump in bias from 3.8 to 10.3.

| Table 12 | | | |
|--|--------------------------|-------------------------|------------------------------|
| Standardized bias statistics for all covariates | | | |
| Variable | % Bias Before PSM | % Bias After PSM | % Reduction in Bias |
| History of Status Complaints | 61.4 | -6.6 | 89.3 |
| Adj. on Violent Charge | -27.1 | -12.5 | 54.0 |
| Initial Crime Class C/D/E | | | |
| Felony | 19.1 | -7.4 | 61.3 |
| History of Violent Beh. | 26.9 | -10.8 | 59.8 |
| Good School Attendance (≥90%) | -48.3 | -4.3 | 91.2 |
| In Special Education | 22.6 | 24.3 | -7.8 |
| Current/past drug use | 74.2 | -26.6 | 64.1 |
| Gang Affiliated | -46.2 | 4.9 | 89.5 |
| History of Sexual Victimization | 19.9 | -8.1 | 59.5 |
| Obedient to Parents | 98.5 | -3.1 | 96.9 |
| Parents want Child in Placement | 72.1 | 5.9 | 91.9 |
| Living in institution/homeless | 41.4 | 25.3 | 38.7 |
| Male | 9.8 | 0.0 | 100.0 |
| Black | 7.2 | 11.2 | -55.0 |
| Hispanic | 9.5 | -9.0 | 5.6 |
| Other Race (white ref.) | -3.8 | -6.2 | -61.5 |
| Age at Arrest | 12.6 | -4.9 | 61.2 |
| Family on Public Assistance | -0.6 | -1.6 | -25.2 |
| Adj. on Property Charge | 14.9 | 0.0 | 100.0 |
| Adj. on Drug Charge | 19.0 | 9.4 | 50.7 |
| Initial Crime :Class A/B | | | |
| Felony | -4.0 | 10.8 | -168.0 |
| Initial Crime is a Misdemeanor | -7.6 | 11.4 | -48.9 |
| Disposition Length | -2.3 | -6.5 | -181.2 |
| History of Fire Starting | 3.8 | 10.8 | -183.9 |
| History of Animal Cruelty | -10.7 | -6.1 | 42.7 |
| History of Sexual Aggression | 6.5 | 5.0 | 22.0 |
| Lives in One-Parent Household | -9.6 | -11.5 | -20.0 |

The second hypothesis in this research study stated that the relationship between time spent in detention and future recidivism is null, having no effect on the

probability of future recidivism. Similar to prior studies of length of time served in detention and its varying effect on juvenile delinquents, information on length of stay was divided into two groups: those who received short stays in detention (“Less”: ≤12 months) and those who received long stays in detention (“More”: 13-18 months). While not ideal, this decision was made due to the clustering of most juvenile detention sentences at 12 months and 18 months. Only a small number of juveniles (13) received sentences smaller than 12-months and only one juvenile had a sentence between 13 and 17 months. None of the juveniles sentenced to placement received a sentence longer than 18 months.¹⁴

There were 67 youths from detention who were rearrested, and out of these 20 (~30%) had spent more than 12 months in detention, while the remaining 47 (70%) spent 12 months or less in detention.

| Table 13 | | | | |
|---|-------------------|----------|-----------------------|----------|
| Rearrest information at 18 months for placement youths- by length of time served | | | | |
| Time Served | Rearrested | | Not Rearrested | |
| | n | % | n | % |
| Less (≤ 12 months) | 47 | 70.1% | 50 | 64.1% |
| More (13 - 18 months) | 20 | 29.9% | 28 | 35.9% |
| Total (N=145) | 67 | 100.0% | 78 | 100.0% |

To assess differences in recidivism that may exist between those who spent more or less time in detention, a logistic regression model is used with rearrest at 18 months as the binary dependent variable, and the binary variable indicating more or

¹⁴ The reason for this consistent upper bound for time served is not able to be ascertained in the current data. It may be the case that most judges assume an 18-month sentence for a juvenile will release them at a point where they have aged out of the Family Court system.

less time served as the primary independent variable. All of the prior 20 covariates included in Table 5 were also included as controls in the model. Table 14 below shows that the coefficient for the binary variable indicating more time served in detention was negative but not significant (-1.446, std. error 0.931), indicating a slight but insignificant tendency for youths with less time served being more likely to get rearrested.¹⁵

¹⁵ The balance with respect to long vs. short amount of time served was also examined. Prior to matching, the treatment groups did not significantly differ with regards to time served. The balancing effects of PSM therefore did not significantly alter the balance of time served. Refer to Appendix B for the results of this analysis.

| Table 14 | | | |
|---|---------------|--------------|--------------------|
| Logistic regression of rearrest status with placement youths | | | |
| | β | (S.E.) | Exp β (Odds) |
| More Detention | -1.446 | 0.931 | 0.235 |
| Male | 3.229 *** | 0.884 | 25.245 |
| Black | -2.489 * | 1.420 | 0.083 |
| Hispanic | -2.604 * | 1.475 | 0.074 |
| Other Race (white ref) | -4.884 ** | 2.104 | 0.007 |
| Age at Arrest | 0.065 | 0.281 | 1.067 |
| Family on Public Asst | -1.058 * | 0.567 | 0.347 |
| Prior PINS | 0.043 | 0.512 | 1.043 |
| Violent Charge | -2.180 ** | 1.095 | 0.113 |
| Property Charge | -2.169 ** | 1.068 | 0.114 |
| Drug Charge | -1.465 | 1.173 | 0.231 |
| History: Violent/Assault | 3.559 ** | 1.181 | 35.113 |
| History: Fire Starting | -1.374 | 1.114 | 0.253 |
| History: Sex Aggression | -1.104 | 0.908 | 0.331 |
| History: Sexually Abused | -2.211 * | 1.291 | 0.109 |
| Good School Att. ($\geq 90\%$) | -1.035 | 1.192 | 0.355 |
| In Special Ed | 0.213 | 0.547 | 1.237 |
| Current/Past drug use | 1.044 * | 0.565 | 2.840 |
| Gang affiliated | 0.095 | 0.513 | 1.099 |
| Obedient to Parents | 0.141 | 0.526 | 1.152 |
| Parent wants youth placed | 1.145 ** | 0.526 | 3.142 |
| One-parent household | 0.760 | 0.640 | 2.138 |
| Homeless/Institution | -0.937 | 0.861 | 0.392 |
| Sample Size | 145 | | |
| LR χ^2 (25 df) | 97.62 | | |
| Pseudo R ² | 0.1802 | | |
| *p<.10 **p<.05 ***p<.001 | | | |

The lack of a significant relationship between time spent in detention and future recidivism does not provide adequate support for conclusions to be drawn about the effect of dosage on juvenile delinquents. The second hypothesis posed the effect of dosage is null, but confirming this null hypothesis seems to largely indicate that more detailed analysis is still needed. This will be discussed in the following

discussion of the limitations and restrictions involved in using this method of analysis for dosage, and recommendations for future research will be discussed.

Chapter 5: Discussion, Limitations, and Conclusion

Discussion

The results of the current study are interesting from both a methodological and substantive perspective. Methodologically, utilizing propensity score matching has revealed a null effect where initial evidence suggested an incarceration effect existed. Without balancing the punishment groups on their background characteristics, this could have lead to the erroneous conclusion that incarceration increases the probability of future recidivism. However, utilizing the current strategy provides evidence that this effect disappears using balanced matched samples.

There are several possible explanations as to why no incarceration effect was found. One is that the Family Court was sending the truly “bad apples” to detention, and they were more likely to recidivate in the first place regardless of their punishment. If this is the case, it can be concluded that even first-time juvenile offenders have already established patterns of behavior which increase their likelihood to offend before they have ever been caught, and the arrest and punishment experience does little to either deter or amplify their delinquency. For those who make the decisions to send specific juveniles to placement, this study provides preliminary evidence that they seem to be reserving detention for the more serious persistent offenders. For those who design and implement juvenile sanctions, however, a null effect is not necessarily good news. Detention is not shown here to have any observable effects, good or bad, on subsequent recidivism.

As evidence of the null incarceration effect continues to accumulate, perhaps this will eventually trigger efforts to implement new strategies in juvenile detention aimed at reducing future recidivism. The only program offered in New York City aimed directly at reducing recidivism seems to be the Behavior Management Program, which is focused on the juveniles taking responsibility for their actions. The effectiveness of this program, however, has not been evaluated. Given the results of the current study, an initial recommendation to New York City's Department of Juvenile Justice would be to begin an extensive evaluation of the programs offered in their detention facilities, to determine what works, what does not, and what shows promise.

Another explanation for the results of this study may be that the effect of detention is mediated by other factors after release and reentry into society. There has been some evidence to suggest that a re-commitment to education may act as a mediator (Dembo et al., 1999). If youths are released from detention and enter back into school, maintaining a good attendance level and academic achievement may reduce the negative effect of incarceration. Similarly, if a youth who has been incarcerated does not excel or regularly attend school, and drops out, this is associated with a gradual breakdown in opportunities which can lead to later criminal activity (Sampson & Laub, 1997). The current data did not collect any information about the youths after release other than their arrest records, making it impossible to explore this scenario.

Future research on the effects of incarceration would benefit from collecting a variety of post-release information about the juveniles, to determine whether or not

other aspects of their lives were affected by official sanction. In their follow-up of the original men from the Glueck sample, Sampson & Laub (1993) examined the salience of arrest, official labeling, and incarceration on several factors in addition to subsequent criminal activity into adulthood.¹⁶ They concluded that while initial results could be seen as evidence that the detention experience is not an important explanation of later criminal activity, the effects of these experiences may actually have an indirect cumulative effect. The stigmatizing effects of incarceration experiences can be seen as gradually affecting a young adult's ability to get a job, and to form conventional social attachments to non-delinquent peers. In their analyses, total length of time spent incarcerated as a juvenile had the largest effect on later job stability, regardless of prior characteristics (Sampson & Laub, 1993: 166). In the future, it would be useful to continue to follow juveniles released from punishment for periods longer than the 18 months that were gathered in the current data set, but more importantly it is important to gather additional information other than just subsequent criminal records. Incarceration may not have been shown to directly impact recidivism for these New York City youths, but the experience may have had a gradual effect of limiting opportunities in areas such as employment, peer relationships, and education. Future research would benefit from considering a wider range of possible areas affected by this type of sanctioning experience.

¹⁶ The original men from the Glueck sample refer to 500 white male delinquents ages 10-17 that were in one of two Massachusetts correctional schools. These males were matched with 500 non-delinquent (control) males according to age, race and ethnicity, neighborhood socioeconomic status, and IQ. To date, follow-ups have taken place when the men were at about ages 25, 32, and 70 (see also Glueck & Glueck, 1950; Sampson & Laub, 1993; 2003).

With regards to the examination of dosage, the data could only be divided into two main groups of length of time served, and this analysis revealed a slight although insignificant effect for those who spent shorter time periods in juvenile detention having higher probability of rearrest. It was not possible to properly test for either an inverted U-shaped relationship, as seen in Gottfredson et al. (1977) or a conventional U-shaped relationship, as suggested by Orsagh & Chen (1988). Dividing the time served into individual months, instead of the scaled data recorded in the present collection would help to uncover any subtle effects at work.

Limitations

There are several limitations to the current research which need to be addressed. Perhaps one of the most important is that when using propensity score matching, it is only possible to balance the sample groups based on observed and measured heterogeneity. The covariates used in the current study were collected and coded by researchers from the youth's case files as relevant information about their background characteristics. The goal was to get the most complete 'snapshot' of these youths prior to sanction as possible. However, the possibility exists that there are elements which did not get measured and remain out of balance amongst the placement and probation groups. The fact that using the current 20 covariates significantly altered the main result is evidence that this collection of information is relatively comprehensive, but the current study is limited to only studying the

covariates available, and future research could benefit from balancing the samples on additional characteristics.

Another critical limitation is the inability to separate juveniles by the specific type and location of their sanctions. For youths sent to detention, it would be useful to know whether or not those sent to the Horizons, Bridges, or Crossroads facilities have differential outcomes. If that is the case, an examination of the services and programs implemented during detention in these facilities could help answer the question of what is it *about* the detention experience at each facility that is leading to specific outcomes. For those sent to community-based sanctions, the programs available for these youths vary in terms of level of supervision. It would be useful to know, for example, whether the more strict supervision is benefiting those youths, or if it simply makes them more likely to get caught committing minor offenses that would have otherwise gone unnoticed.

The data were collected only to include those juveniles who received a sentence of probation or detention, however as seen in Appendix C there are several points prior to juvenile processing where their case could have been dismissed. This raises concern that the results of the current study are not generalizable for the entire population of juvenile delinquents. Future studies would benefit from including youths in the sample who were arrested but never reached the disposition stage, to determine if including them alters the results and conclusions.

With regards to the issue of dosage, the current study was also not able to utilize the balancing effect of propensity score matching in the examination of

dosage. In order to analyze differences among dosage groups with propensity score matching the recommended number of dosage groups has been placed at about four or five (Loughran et al., in press; Lu et al, 2001). In the future, gathering more specific data about the exact length of time served in weeks or months would be beneficial to determine if the preliminary results found in the current study remain true after balancing the samples.

Conclusion

The current study has found no evidence that an incarceration effect exists for juvenile delinquents. Contrary to both the labeling and deterrence perspectives, spending time in detention does not significantly amplify or deter youths from subsequent delinquency. Additionally, preliminary evidence suggests that youths who get sent to detention for short periods of time may be more likely to get rearrested within 18 months. This study contributes to the body of research that examines the effects of punishment on future recidivism, and offers an example of a slightly new technique to attack this question. This study also emphasizes the importance of establishing temporal ordering in the testing of deterrence or labeling hypotheses. Unlike the original research done with the current data, this study only analyzed youths who were experiencing their first official sanction, thus providing a clear temporal sequence of events from which an effect could be measured. The fact that no observable effect exists here is an important contribution to the field, but research should continue to delve into the precise nature of the incarceration and probation experiences to determine if there are lasting effects of punishment on the success of juveniles in a variety of facets of their lives.

Appendix A

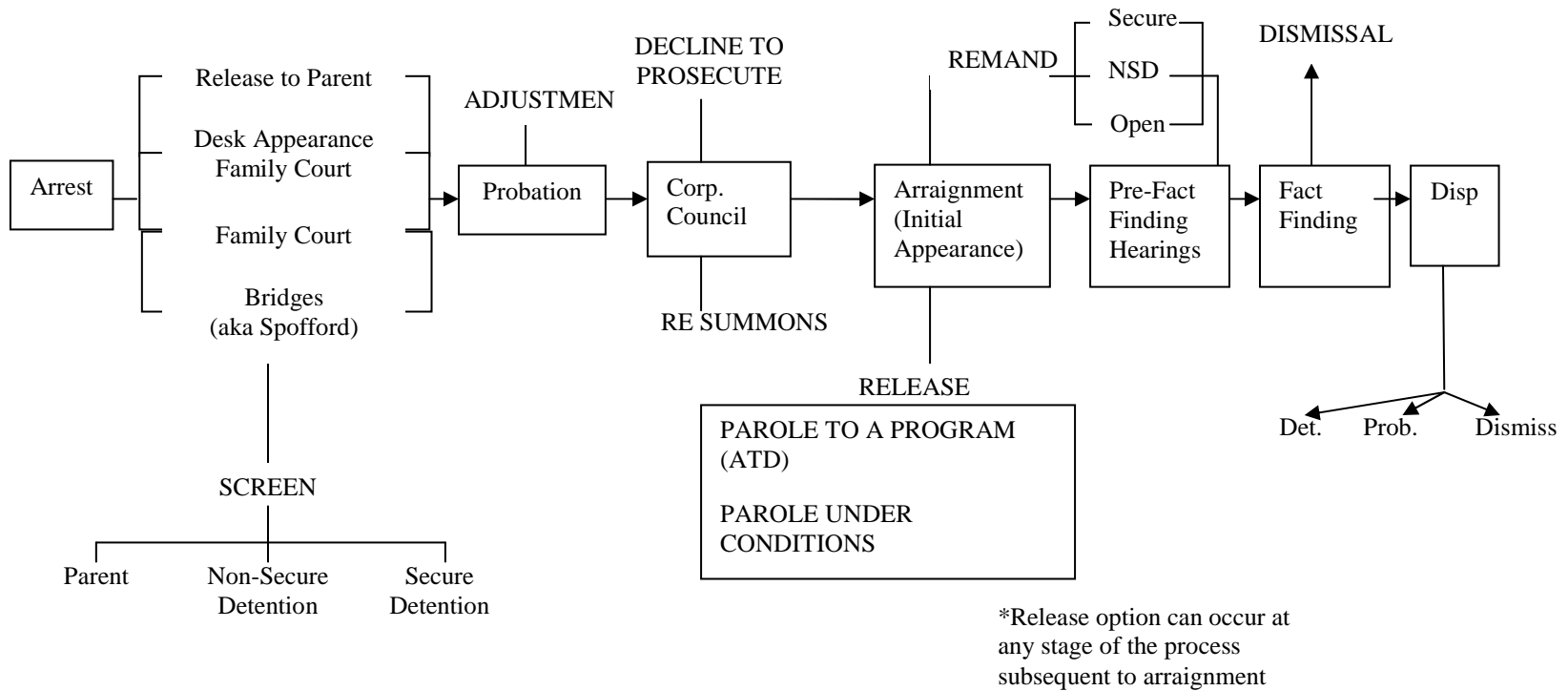
| Logistic regression analyzing overmatched covariates (In Special Education, Current/Past Drug Use) | | |
|---|-------------------|-----------------------|
| | β (S.E) | Exp β (Odds) |
| In Special Education | .442* (.242) | 1.556 |
| Current/Past drug use | .584** (.260) | 1.795 |
| Living in Institution/Homeless | - .883**(.421) | 0.413 |
| Pr(Placement) | 1.159* (.498) | 3.187 |
| Placement in Detention | .004 (.272) | 1.004 |
| <i>Sample Size</i> | 290 | |
| <i>LR χ^2 (5 df)</i> | 31.51 | |
| <i>Pseudo R²</i> | 0.0582 | |
| <i>* p<.10 **p<.05</i> | | |

Appendix B

| Treatment effect of more time served before and after matching (means) | | | | | | |
|---|-----------------|---------|----------|------------|--------|--------|
| Variable | Sample | Treated | Controls | Difference | S.E. | T-stat |
| Rearrest: mo. | 18 Unmatched | 0.4148 | 0.3576 | 0.0571 | 0.0510 | 1.12 |
| | Matched- ATT | 0.4148 | 0.4148 | 0 | 0.0895 | 0.00 |

Appendix C

Overview of New York City's Juvenile Justice System Processing



SOURCE: Rethinking Juvenile Detention in New York City: A Report by the Juvenile Justice Project of the Correctional Association of New York, 2002. Available online at: http://www.correctionalassociation.org/publications/download/jjp/rethinking_detention.pdf

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