ABSTRACT

Title of Thesis: HOW INCARCERATION AFFECTS JUVENILES: A FOCUS ON THE CHANGES IN FREQUENCY AND PREVALENCE OF CRIMINAL ACTIVITY

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There has been a longstanding debate over the effectiveness of correctional institutions. Some argue that incarceration deters offenders while others argue that the experience of being incarcerated causes individuals to continue in their life of crime. Resolving this debate is of particular importance for young individuals when there is a national push for the increased treatment of youth as adults. Using NLSY panel data, this study focuses on how the criminal offending of a sample of incarcerated youth changes over time in relation to incarceration while including a control group of youth who are not incarcerated but are similar in demographics. Close attention is paid to overcome past problems with validity. The findings suggest that incarceration does little to stop criminal paths or future contacts with the criminal justice system, but perhaps may even have harmful effects on youth, particularly drug sellers, over the short term.

HOW INCARCERATION AFFECTS JUVENILES: A FOCUS ON THE CHANGES IN FREQUENCY AND PREVALENCE OF CRIMINAL ACTIVITY

By

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INTRODUCTION

There is no debate that incarceration in the United States has rapidly increased over the last twenty years [1]. There is a debate, however, about the impact of incarceration on the future offending of punished individuals. Labeling theory suggests that sanctions negatively label offenders which amplify their criminal activity afterwards; this increase in behavior is often called deviance amplification. Instead of reducing criminal activity, sanctioning results in negative reactions from others and facilitates the offender in constructing a "deviant self image" thereby causing him to continue or increase his participation in crime (Farrington 1977). Sanctioning, particularly incarceration, acts as a severe reaction from society which aids an individual in accepting a deviant social status (Lemert 1951). Alternatively, specific deterrence theorists believe that incarceration deters an offender from returning to the same level of crime after his release. Specific deterrence occurs when "individuals who have suffered a punishment for a type of crime are deterred from future offending" (Gibbs 1975: 34). These theorists expect that sanctioning deters offenders from committing crime in the future by imposing more costs than benefits on rational offenders.

This debate is particularly salient for youth and young adults who are becoming involved with the criminal justice system for the first time. Labeling someone as deviant may have a greater effect on younger offenders and may not even apply to offenders with previous contacts with the system (Murray and Cox 1979). Similarly, Farrington, Osborn, and West conclude that labeling effects may "wear off after a number of years, whether or not further convictions occur" (1978: 283). The deterrent impact of

[1] Austin et al. (2001) estimate that the last two decades of increased imprisonment has resulted in two million people incarcerated, many as a result of the War on Drugs and parole violations.

incarceration may differ between younger and older offenders as well. For example, younger offenders may feel they have less to lose by committing a crime and therefore are less likely to be deterred by sanctions. Furthermore, some research suggests that incarceration has no direct effect on future criminal behavior, but instead, incarceration has an indirect influence on criminal behavior through its effect on future employment (Sampson and Laub 1993)[2]. Under these theories, the recent "get tough" policy of increased incarceration for youth and young adults (P. Smith et al. 2002) could lead to either increased or decreased criminal behavior or it could have no effect on criminal behavior at all[3]. It is estimated that over one hundred thousand juveniles are incarcerated in the U.S. (Austin et al. 2001)—a substantial amount of lives, which could be positively or negatively affected by incarceration policies. Resolving this question empirically is therefore interesting for both policy and theoretical reasons. This study will investigate whether incarceration deters young offenders from crime, amplifies their behavior, or fails to change their involvement in delinquency.

[2] They look at incarceration length on future arrests using the Gluecks' data on five hundred delinquent boys from Massachusetts.

[3] Between 1992 and 1995, forty states passed laws making it easier for juveniles to be tried as adults (Griffin et al. 1998) and nearly all states allow juveniles to be held in adult correctional facilities (Torbet et al. 1996).

CHAPTER I. REVIEW OF THE LITERATURE

Research on deterrence mainly focuses on the effects of general deterrence or how sanctions of a select few affect other potential offenders (for a review see Nagin 1998), while the limited research intended to look at specific deterrence often does not include actual behavior as a dependent variable. Instead specific deterrence research emphasizes how sanctions affect an offender's perceptions of certainty or threat of future punishment (Pogarsky and Piquero 2003; Bridges and Stone 1986) or how sanctions effect a person's projection of his committing future crimes (Piquero and Paternoster 1998; Piquero and Pogarsky 2002). Furthermore, the specific deterrence research that does look at criminal offending as an outcome often focuses on the effects of other sanctions than incarceration like arrest or court appearances (Farrington 1977; Smith and Gartin 1989; Smith and Paternoster 1990).

Specific deterrence research has for the most part focused on outcomes of cohorts released from prison. In 500 Criminal Careers (1930), Sheldon and Eleanor Glueck were the first to research how incarceration affects actual criminal behavior after release[4]. In this pioneering recidivism study, they find that about 80% of the men released from a Massachusetts reformatory are arrested or report committing criminal activity within five years of release. Although they include no comparison groups, this can be considered one of the first attempts at investigating specific deterrence. More recent research that focuses on specific deterrence effects of incarceration includes investigations of the recidivism of different types of offenders after release (Dejong 1997), comparisons of recidivism rates between incarcerees and probationers (Babst and Mannering 1965; Gray

[4] In addition to investigating effects on future criminal offending, the Gluecks' book includes information on how incarceration affects family relationships, employment, leisure habits, and the mobility of ex-offenders.

1994; Spohn and Holleran 2002), or comparisons of the effects of incarceration relative to the effects of other alternative sanctions (McCorkle et al. 1957; Stevenson & Scarpitti 1967; Gottfredson and Barton 1993). Only a rare group of deterrence studies compare offending before and after incarceration or other sanctions (Murray and Cox 1979; Empey and Lubeck 1971; Phillips et al. 1983).

There are two main problems with most of the specific deterrence research on the effects of incarceration: for the most part, they rely on a weak research design and they use official data to measure deviant behavior. The research designs fall into one of two types (as described by Shadish, Cook, and Campbell 2002): a posttest only design with nonequivalent groups, or the (improved but rarely used) untreated control group design with dependent pretest and posttest samples. From this point forward, these designs will be referred to as posttest-only and pretest-posttest designs respectively[5]. Moreover, particular studies using these designs may measure criminal offending with either official records or with self-report data. However, the majority of specific deterrence studies combine posttest-only designs with official measures (recidivism studies), which are problematic to studying the question at hand. Mainly, they do not provide any information on criminal offending before incarceration and therefore cannot provide information on how offending changes over time. A less popular method of studying specific deterrence—the untreated control group design with pretest and posttest—is actually better able to investigate a change in individual behavior and better suited to rule

[5] To simplify the language in this paper, unless explicitly stated, when I mention *posttest-only* designs I am referring to what Shadish, Cook, and Campbell (2003) state are posttest only designs with nonequivalent groups. Likewise, when I mention *pretest-posttest* designs it can be assumed that this design includes the comparison of an untreated control group to the experimental group. All the studies of specific deterrence mentioned here include comparison groups in their designs and therefore for simplicity I will refer to these labels.

out threats to internal validity. However, these types of studies have only relied on official data as well.

The use of official data as a measure of criminal behavior is problematic[6]. Studies that rely on the use of official data focus on extreme dichotomous outcomes (either the person recidivates or desists, fails or is successful) and do not consider declines in offending rates (Bushway et al. 2001). This decline is referred to as restrictive deterrence, which is the "curtailment of a certain type of criminal activity" in the belief that repetition is likely to result in more punishment (Gibbs 1975: 33). Essentially, recidivism rates are not suited to provide evidence of amplification or restrictive deterrence, but instead can only provide an imperfect measure of complete desistence (which is itself an unrealistic expectation for most offenders). Furthermore, the measurement problems associated with using official crime data to capture actual offending have been widely acknowledged (Mosher, Miethe, and Phillips 2002).

POSTTEST-ONLY DESIGNS

Research on How Incarceration Affects Adults

When comparing incarceration to probation, incarceration is consistently more criminogenic. Those offenders who are sent to prison exhibit higher failure rates than those who are sentenced to probation (Babst and Mannering 1965; Shannon 1980; Gray 1994; Spohn and Holleran 2002; McGuire 2002). Shannon (1980) was one of the first researchers to uncover an amplification effect by following a cohort of men from birth until age 25. He finds that more severe sanctions, like incarceration, result in more

[6] With the exception of Gottfredson and Barton (1993), studies of specific deterrence that have utilized posttest-only designs with non equivalent groups use official measures of crime, and studies utilizing untreated control group designs with a pretest and posttest have always used official measures of crime.

frequent and more serious police contacts. In general, these studies indicate that incarceration does little to deter individuals from committing more crime.

Some of the findings of the recidivism literature are applicable to youth who, as a group, tend to be inexperienced. Many of these studies do not investigate the effect of being a juvenile per se but they do note different outcomes for those offenders who are naïve versus those who have had numerous contacts with the criminal justice system. Two studies find that inexperienced offenders are more likely to recidivate after incarceration than repeat offenders (Babst and Mannering 1965; Dejong 1997). Dejong (1997) reports that first time arrestees when given incarceration are more likely to be re-arrested than those first-time arrestees that are not incarcerated[7]. Meanwhile, experienced offenders are not affected by incarceration. Likewise, incarceration in comparison to probation is more criminogenic for first time felony offenders than those with more experience (Babst and Mannering 1965). Even more alarming, one piece of research (Gottfredson 1999) suggests that juvenile correctional facilities are no less harmful than adult correctional facilities but perhaps even more so. After including controls for judges' selection processes and age of offender, Gottfredson (1999) finds that youth sent to juvenile facilities had a higher likelihood of being arrested following release (92.8%) than those sent to jail (81.2%) or to prison (81.2%)[8].

Research on How Incarceration Affects Juveniles

There exists a group of recidivism studies particularly focused on the incarceration experiences of youthful offenders. These studies use the posttest-only

[7] Dejong's sample consists of property, violent, and drug offenders.

[8] Gottfredson relied on careful analysis of how judges make sentencing decisions to include controls for selection processes including the perceived risk of rehabilitation, seriousness of offense, and prior record.

design with nonequivalent groups and use official data to measure offending. These studies compare groups of juveniles sent to a traditional juvenile correctional facility to those who received community sanctions (McCorkle et al. 1957; Stephenson and Scarpitti 1967)[9]. McCorkle and colleagues (1957) investigate the recidivism rates (either conviction or parole violation resulting in institutionalization) of those youth leaving the experimental program at Highfields with those leaving Annandale, a more traditional correctional institution. Findings show that 79% of youths from Annandale recidivate within 60 months of release compared to 56% of youth from Highfields. Similarly, Stephenson and Scarpitti (1967) find that cumulative recidivism was 55% among boys sent to Annandale within a three year follow up period[10]. Both studies indicate that incarceration does not deter youth more than community programs although it unclear what is happening to their individual offending patterns. Still, the results strongly suggest that African-American boys, those with less education, and those with low socio-economic status are more likely to recidivate.

Alternate Explanations for Findings

There are two main concerns when findings show larger failure rates among incarcerees than other groups. Selection bias and differential attrition could account for these findings and therefore undermine our ability to conclude that incarceration caused worse outcomes.

[9] The general theories driving these experimental programs for juvenile offenders are that youth need to learn in a social and interactive setting in which conforming is more rewarding than committing delinquent behavior and that youth benefit from avoiding the negative experiences of institutionalization (Gottfredson 1987). These practices are sometimes referred to as Guided Group Interaction (GGI).

[10] The experimental group (Essexfields) produced recidivism rates less than those exhibited by the group leaving Annandale. The program was similar to Highfields in that GGI was a main part of the treatment; however, boys were allowed greater freedom of movement in the community at night and on the weekends.

As a group, posttest-only studies suffer from selection problems because groups are not randomly sampled from the same population (Shadish, Cook, and Campbell 2002). Each study uses nonequivalent comparison groups resulting from the way that judges decide to sentence offenders to probation or incarceration. In the end, serious offenders are more likely to be sentenced to incarceration than to other non-incarcerative sanctions. In other words, unobserved factors affect both the independent variable (incarceration) and the dependent variable (frequency of future offending). From a statistical standpoint, Smith and Paternoster (1990) demonstrate that this selection problem leads to biased estimates of the effect of an intervention on future offending and may lead researchers to incorrect conclusions. Yet only a few researchers have made a concerted effort at addressing this problem. For example, Babst and Mannering (1965) control for prior felony offenses and seriousness of offense but much is left out.

Spohn and Holleran (2002) put forth the best effort to control for many of the offender characteristics that influence a judge's decision in sentencing. They find that felony offenders sent to prison recidivate at a higher rate than those given probation regardless of prior criminal record, background characteristics, and predicted probability of being incarcerated for the original offense. Still there are excluded factors from which a judge could select an offender for incarceration. Perhaps the judges in their study took into account the drug problems of the offender and decided to send those who he thought were able to be rehabilitated to probation programs which in Missouri, required drug treatment while prisons in the state did not. The point is that it is impossible to control for all factors that influence a judge's decision and therefore even the most carefully controlled study on incarceration will suffer from selection problems.

Posttest-only designs on juveniles suffer from the same selection problems. The general finding is that community sanctions produce smaller failure rates among cohorts than incarceration; however these studies cannot attest to the equivalency of the groups being compared. For example, the Annandale boys in the Highfields study tended to have earlier contacts with the criminal justice system (Gottfredson 1987) and the Annandale group in the Essexfields experiment had more economic disadvantage, prior delinquency, and psychological problems. Also, judges most likely assigned boys who posed a greater threat to society to the correctional facility where youth served sentences almost three times as long as those sent to Highfields (Gottfredson 1987).

Another problem that threatens the validity of findings using a posttest-only method is differential attrition. Treatment attrition, when individuals drop out of the sample during treatment, could result in less deviant youth being part of the control sample. For example, in studies where probationers and parolees are compared, it is not unlikely that probationers who violate may be sent to prison and excluded from the sample. (At the same time, it is nearly impossible for incarcerees to drop out of treatment!) Selection and differential attrition are plausible alternative explanations for incarcerated cohorts performing worse than other cohorts.

The Use of Official Data

By measuring delinquency with official data another type of attrition is possible. Measurement attrition is a problem when people drop out before the posttest. This is especially problematic when posttest-only designs use official data to measure criminal offending. Police may follow parolees closer than probationers (Phillips et al. 1983).

Increased surveillance of only one group of offenders may lead to higher failure rates that are unrelated to the actual experience of incarceration.

Studies using official recidivism measures may leave out important trends in individual offending (Williams and Gold 1973) and may provide a distorted picture of actual behavior. For example, Gottfredson and Barton (1993) compare official and self-report data *after* treatment and find deterrent effects only when using official measures[11]. The authors speculate this may result from a little overlap between the follow-up periods for official delinquency measures and self-reports of delinquency, only being able to interview non-random subsets of each group, and the reflection of minor crimes through self-reports compared to more serious crimes through official records. Still their findings suggest that conclusions formed using official data may be inconsistent with those formed when using a measure of self-reported delinquency.

PRETEST-POSTTEST DESIGNS ON JUVENILES

In an attempt to control for past behavior and to investigate trends in behavior as opposed to dichotomous outcomes, some researchers study the effects of incarceration on youth using a pretest-posttest design with an untreated control group, while using a measure of official delinquency (Empey and Lubeck 1971; Murray and Cox 1979; Phillips et al. 1983). These studies contribute fairly consistent deterrent effects to the literature and focus on young offenders. Empey and Lubeck (1971) compare a group of incarcerated juveniles (at Boys Republic) to an experimental group of juveniles participating in the Silverlake program. They find a reduced number of arrests after treatment by both groups when comparing one year prior treatment to one year following;

[11] They investigate the effects from the closing of Montrose Training School, a juvenile correctional institution, by comparing two groups that were institutionalized at Montrose to a group that would have gone to Montrose if the school had not been closed (post-closing group).

however, the boys who are placed in the more traditional correctional setting still reduce their offending by 71% as a group[12]. When excluding runaways and in-program failures, the reduction rate was even higher (85%) for those who completed the program.

Murray and Cox (1979) investigate the effects of the UDIS (Unified Delinquency Intervention Services) in Chicago by comparing rates of arrest before and after treatment. They find that incarceration of juveniles had the greatest suppression effect compared to alternatives such as probation and less restrictive, at-home punishments while controlling for prior arrests. They report that rates of arrest decreased for about 90% of the sample using an average follow up period of 16.8 months. Also using arrest rates, Phillips and colleagues (1983) find that 35% of youth in their sample completely desist and 30% have lower arrest rates post-incarceration[13]. In the same sample in which they find evidence of deterrence, Phillips and colleagues find that a portion (35%) of the youth in their sample actually exhibits higher arrests following incarceration. The authors assert that while other program evaluations show evidence of deterrence, their evaluation results "cast the effects of incarceration in a much darker light" (p. 249).

Alternative Explanations for Findings

The largest concern with these designs is the possibility that regression to the mean is occurring and that deterrence effects are not caused solely by incarceration. Because of this threat to validity, the methods used by the authors of the Chicago study have been severely criticized and their findings of a sharp drop in arrests have been

[12] The Silverlake program relied less on a traditional correctional setting and allows participants to retain more freedoms in the community. The boys in the Silverlake program have slightly better outcomes than those in the incarcerated group.

[13] They evaluate data from the Dangerous Offenders Project in Franklin County, Ohio, which includes youth committed to an Ohio Youth Commission facility and followed until their eighteenth birthday. Rates of arrest are relative to the length of pre and post-institutionalization ("street time").

questioned (Maltz et al. 1980). Likewise, Phillips and colleagues (1983) fail to rule out regression to the mean artifacts using controls for pre-intervention arrest rates. Both control and experimental groups in these studies were constructed following a judge's decision to send the prospective boys to an institutional setting. Maltz and colleagues (1980) argue that the process of sentencing youth to incarceration is based on the youth's prior offense history and therefore some of the deterrence effects are subject to a selection-regression artifact. In other words, because the boys in both groups were chosen for the intervention based on a certain number of prior arrests over a certain period of time, they are likely to be arrested at a rate above their means and once released from incarceration their arrest rate will naturally decrease even without an intervention.

Other threats to validity are also present in these designs. Authors (Murray and Cox 1979; Phillips et al. 1983) have been careful to control for age in their studies in order to rule out any maturation effects. Age should also be included since as juveniles become adults they are less likely to be arrested for certain status crimes (Empey and Lubeck 1971). By controlling for age, we can be confident that deterrence findings cannot be attributed to the incarcerated sample aging out of crime at a quicker rate than the comparison groups. However, a combination of two threats-- selection by maturation-- threatens our ability to conclude that incarceration deters youth. This threat posits that each group was already on a different trend even before the intervention. Perhaps the treatment group was already in a downward trend prior to treatment that the researchers are not aware of because they only have one pretest. Differential attrition is still plausible but at least this design allows the researchers to investigate what kind of offenders drop out of treatment. For example, Empey and Lubeck (1971) found that both

programs were dropping similar kinds of boys: those with high personal difficulty and not necessarily those with more serious criminal tendencies.

The Use of Official Data

The use of official data with a pretest-posttest design inhibits the designs ability to rule out attrition or mortality effects. Usually such a design is able to indicate any dropout between tests, but when the posttest measure of crime is collected by officials this problem becomes more complicated. One example is the Columbus study where Phillips and colleagues (1983) are unable to determine if they overestimated the number of desisters because their measure of offending (arrests in Columbus) does not capture offending occurring in other jurisdictions.

CONCLUSIONS FROM SPECIFIC DETERRENCE RESEARCH

As a whole, the empirical research on the deterrence-amplification debate is inconclusive. Posttest only studies using official data paint a bleak picture of incarceration especially for juveniles. Most studies indicate that inexperienced offenders are negatively affected by institutionalization and that failure rates are high compared to other sanctions like probation and community programs. Unfortunately, recidivism studies are useful for comparisons between sanctions but they cannot determine whether a person's criminal behavior changes after incarceration and they suffer from selection bias and differential attrition. Nevertheless, they suggest that incarceration does not have a specific deterrent effect. McGuire (2002: 202) concludes after his meta-analysis of criminal and psychologically based sanctions that the "hypothesis of deterrence fares particularly badly, and in some instances… punitive sanctions are associated with increased recidivism".

On the other hand, pretest-posttest studies produce results that are more consistent with a possibility of a deterrent effect but fail to rule out whether drops in offending result strictly from incarceration. Still, studies that use pre-post methods come closer to addressing the deterrence-amplification debate and are better equipped to investigate alternative explanations for changes in behavior. Regardless of the criticisms by Maltz and colleagues (1980), pre-post studies are better equipped than posttest-only designs to provide information on changes in offending relative to an intervention. Such studies permit a direct comparison of a certain measure of criminal activity over time. Still, no study to date has paired up this design with self-reports of delinquency.

ADDRESSING THE PROBLEMS OF PAST RESEARCH

Use of a Pretest-Posttest Design with Two Pretests and Two Posttests

This design will help me to address some threats to validity but not all. Selection by maturation can be better examined using a double pretest since I will be able to look farther back in time for any difference in initial trends between the groups. If the incarcerated group is already on a declining trend a couple years before the intervention I will be hesitant in concluding that incarceration has a deterrent effect. This design also helps me to investigate any differential attrition between groups by investigating the type of juveniles who drop out of the sample between the pretest and posttest. Finally, the use of this design also helps rule out maturation[14], history[15], and testing effects[16].

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[14] Maturation effects can also be ruled out by statistically controlling for age. To do this, I include a control group similar in age to the incarcerated group.

[15] Although history effects are possible, the plausibility of history can be reduced by "selecting groups from the same general location" and ensuring that the testing schedule is the same for both groups (Shadish, Cook, and Campbell 2002: 56). Since I am able to select a control group of youth who were arrested and an incarcerated group of youth who were also arrested, I assume that the groups of youth are similarly affected by events of which I am unaware. Also, since the surveys are given to all youth in the same years, youths in both groups are reporting their criminal activity in similar time periods

This project will combine the use of pretest-posttest design to better investigate the regression artifact identified by Maltz et al (1980). I have access to six years of data which allows me to not only look immediately before and immediately after incarceration, but it also allows me to look at criminal behavior two to three years prior to incarceration. In other words, if the occurrence of regression to the mean requires an abnormally high rate of criminal offending immediately prior to incarceration, it would be beneficial to ignore this time period and compare pre and post offending after the adjustment. Also, I can construct a control group of youth who exhibit high amounts of criminality by being arrested but not incarcerated which will help me to control for the general trend in crime.

Selection bias is inherent because incarcerated offenders are placed there because of their active criminal history. Sentencing decisions by judges will always be made with public safety in mind, and therefore those people who are perceived to be more serious will be more likely to be sentenced to correctional facilities. However, by using a pretest-posttest method with two groups, I will be able to investigate how the control group and experimental group differ initially. Finally, prior studies on specific deterrence only controlled for observed measures. By using panel data and a fixed-effect method that controls for stable individual characteristics, I will better control for unobserved time-stable characteristics that may influence the selection process (by judges

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[16] A testing effect is likely because youth are asked each year to report on criminal offending. With increased surveillance by surveyors, respondents may avoid criminal behavior in the future and therefore a deterrent effect may not be subject to incarceration. But there is no reason to think that youth in the different groups would have different testing effects and the use of an arrested group (who are receiving the same testing) helps to control for any changes in criminal activity due to testing effects.

determining incarceration). The combination of these methods to combat selection and regression to the mean represents a substantial improvement over the current literature.

The Use of Self-Reports of Delinquency

Unlike any pretest-posttest study to date, I will use self-report data which is not subject to differing probabilities of being caught by law enforcement. The use of official data in pre-post designs leads researchers to narrowly focus on repeat offenders by cutting out anyone without high arrest rates prior to incarceration (Phillips et al. 1983). Although those who are arrested numerous times are more likely to be incarcerated, having a history of arrests will not be a selection criterion. This way, offenders who may admit to committing higher amounts of deviance without high rates of arrest will be included in my sample. Furthermore, using this measure in combination with the pretest-posttest design helps reduce the threat of selection by maturation in that a youth's ability to avoid detection by police does not influence the outcome. Finally, since I am ultimately interested in how incarceration changes behavior, self-reports of offending may more directly tap behavior than measures of arrest.

In summary, I intend to test the theoretical debate between deterrence and amplification on a sample of young offenders who are most impressionable by the system while improving upon past methods. By using a non-equivalent comparison group design with multiple pretests and posttests and by measuring delinquency with self-reports, I will investigate whether the experience of incarceration increases, decreases, or has no effect on the frequency and prevalence of criminal offending among a sample of youth.

CHAPTER II. DATA AND METHODS

My sample is drawn from the National Longitudinal Survey of Youth collected by the U.S. Department of Labor, Bureau of Labor Statistics (BLS) in six waves between 1997 and 2002, which contains information on 8,984 youth born between 1980 and 1984 (Center for Human Resource Research 2003).[17] Although the survey was intended to collect data on labor force and education experiences, information on criminal behavior and criminal justice sanctions was also recorded. It is important to note that the NSLY over samples minorities and therefore minorities may be overrepresented in my samples as well.[18] Approximately 25% of the youth in my sample are taken from the supplemental survey sample. As a result, inferences to the national population of youth should be made carefully.

This data is appropriate to study the effects of incarceration for many reasons. First, the surveyors follow youth over many years even if they are incarcerated and released. I am able to study their behavior before and after incarceration since youth do not drop out of the survey once they are incarcerated and since they are not chosen into the sample by first being incarcerated (as is the case with most recidivism studies). The survey also asks youth each year about their delinquent behavior since the last survey in addition to collecting information on their contacts with the criminal justice system. BLS collects this sensitive information on anti-social behavior by administering a Youth Questionnaire which is a self-administered computer assisted interview. This

[17] BLS used a multi-stage stratified random sample to obtain youth aged 12 to 16 as of December 31, 1996: 6748 youth designed to be representative of the U.S. youth population plus a supplemental sample of 2236 Hispanic and black youths.

[18] The results from this study should not be inferred to the national population of youth because I did not assign each participant a sampling weight. For differences in characteristics between the youth in my samples and the youth in the national sample of NLSY see the appendix.

information allows me to quantify their amount of criminal activity over time and not just whether they recidivated back into the system. Furthermore, I can investigate the effects of incarceration on future arrest as well to determine how closely the youth's actual behavior correlates with a measure of getting caught by police.

From the NLSY youth, I will select two samples on which to concentrate in order to study the effects of incarceration on the offending of youth. The subset of incarcerated youth includes youth who are incarcerated in waves three or four for the first time, but may or may not be incarcerated again in waves five or six (N=124).[19] To be included in this group, a youth must never be incarcerated before wave three and must not be missing any surveys or reported crime information. By focusing on this group of incarcerated offenders (N = 90), I allow the most variation possible in crimes committed before and after the first experience of incarceration. Coding these variables in this way leaves out some incarcerated offenders, but this method is justified for the purpose of comparing the effects of *first experiences* of incarceration and whether deterrence or amplification occurs as a result. A comparison group of offenders who are arrested at least once in waves one through four but not incarcerated during this period will be used to demonstrate the effect of an absence of incarceration (N=1184).[20] Relying on the assumption that offenders who are arrested are likely to be offending at high rates, I will use this group (N = 1002) as a control to demonstrate the general trend of crime occurring in a sample of arrested youth.[21] Therefore, neither group experiences incarceration in the

[19] 124 youth were eligible for this group but 19 were dropped because they were not surveyed during waves 5 or 6. An additional 15 youth were missing crime information in any of the 6 waves.

[20] 1184 youth were eligible for this group but 114 were dropped because of missing survey information in waves 5 and 6. An additional 68 youth were missing crime information in any of the 6 waves.

[21] Attrition problems will be explored in a later section.

first two waves, the experimental group experiences intervention in either wave 3 or wave 4 while the control group does not, and there are no restrictions placed on either group during the outcome period (waves 5 and 6). Table 1 summarizes the trends in incarceration by each group. Thirty-one youth in the incarcerated group receive the intervention during wave 3, while fifty-three are incarcerated in wave 4, and six youth are incarcerated in both wave 3 and 4.

Table 1. Incarceration Patterns over Six Waves.

	1	2	3*	4*	5	6
	1997	1998	1999	2000	2001	2002
Incarcerated Group, N= 90	0	0	37 (41%)	59 (66%)	17 (19%)	8 (9%)
Arrested Group, N=1002	0	0	0	0	23 (2%)	36 (4%)

* 6 youth are incarcerated in both waves 3 and 4.

VARIABLES OF INTEREST

I will recode the incarceration variable to only include those youth who serve time in an institutional setting (jail, adult correctional facility, or juvenile correctional facility) and exclude those youth who were sentenced to community service or reform school.[22] A *group* variable represents whether a youth belongs to the group experiencing incarceration during waves 3 or 4 (group=1) or whether the youth belongs to the control group who did not receive the intervention (group=0).

Crime will be measured in two ways: how often a youth commits a crime (*frequency*) and whether the youth reports involvement in any crime (*prevalence*). The frequency of crimes committed includes the total reported number of crimes in the past year. These crimes include purposeful destroying of property, stealing something worth less than $50 (petty theft), stealing something worth $50 or more (including a car), other

[22] I also ran my analysis when including reform school youth in my sample but found no substantial differences from the results reported here.

property crimes[23], assaulting a person, and selling or help selling illegal drugs[24]. Since accounts of criminal behavior in the past year are highly skewed, I will use a recoded crime variable that is top-coded at the 99th percentile.[25] Also, since time between surveys varies by respondent, the crime variable will be weighted by exposure time. To investigate the change in prevalence of criminal activity over the six waves of the survey, I will use a dummy variable that indicates whether a person committed any crime in a given year or no crime at all. The data also allows me to investigate whether prevalence of arrest in a given year correlates with the prevalence of criminal activity in this sample of youth. An *arrest* dummy variable will represent prevalence of arrest in any given year. This variable is coded 1 if the youth was ever arrested in the current year and 0 otherwise.[26]

Since I will be comparing changes in behavior between years, I use two measures of time to capture these changes. Since I am interested in looking at the change in behavior in relation to an intervention, time is represented by dummy variables comparing wave 5 to wave 2 (0 if wave 2, 1 if wave 5) and comparing wave 6 to wave 1 (0 if wave 1, 1 if wave 6). Therefore, waves 1 and 2 will be referred to as "before" and waves 5 and 6 as "after" the presence (or absence) of the intervention in waves 3 or 4. Constructed in this way, these variables distinguish between a short term change and a long term change in criminal activity: the 2 to 5 comparison representing a three year

[23] Other property crimes include receiving, possessing, or selling stolen property and whether the youth has cheated someone by selling something for an unfair price.

[24] Illegal drugs include marijuana, hashish, heroin, cocaine, or LSD.

[25] The 99th percentile of frequency of crimes committed for all youth in the NLSY over six years is 84 crimes. Any responses above 84 crimes committed are recoded as 84.

[26] Since I cannot weight prevalence variables by exposure time between surveys, I added exposure as a control variable in all regressions and found no difference from the results reported here.

time span and the wave 1 to 6 comparison representing a five year time span. The data used in these analyses do not allow me to distinguish when incarceration occurred within each year (wave 3 or 4) and whether offending that year preceded, followed, or occurred during the intervention. Although the following graphs portray crime patterns over all six waves of the survey it is important that before and after comparisons focus only on the waves (not 3 or 4) in which I am certain do not overlap with the intervention of interest.

STATISTICAL MODEL

I will use a "differences in differences" method to investigate the criminal offending of NLSY youth who were incarcerated in either 1999 or 2000 (waves 3 or 4). Using a "differences in differences" method similar to the one outlined by Meyer (1995), I can investigate the effect of incarceration on the frequency of crimes reported while controlling for stable, unobserved individual characteristics. The effect of incarceration on someone experiencing incarceration for the first time is the difference in criminal offending of those who were incarcerated minus the differences in offending of those who were never incarcerated (see equation 1). In other words, I will compare the magnitude of the change in the incarcerated group to the magnitude of the change in the control group over time (i.e. compare the slopes). One way I can do this is to subtract the offending in wave five from the offending in wave two for the incarcerated group minus the offending in wave five from the offending in wave two for the control group. The same can be done using the differences between wave six and wave one in order to look at changes over a longer period of time. Let $_{t1}$ be the time before incarceration (either wave 1 or wave 2) and $_{t2}$ be the time after incarceration (either wave 6 or wave 5) for the incarcerated sample. Let $_{t1}$ be the corresponding year for the control group before waves

3 or 4 and $_{t2}$ be the corresponding year for the control group after waves 3 or 4. The "differences in differences" method is conceptualized in equation 1:

$$\beta_{dd} = [y^{incar}{}_{t2} - y^{incar}{}_{t1}] - [y^{control}{}_{t2} - y^{control}{}_{t1}] \quad (1)$$

Since the change in offending of the control group is subtracted from the change in offending of the incarcerated group, a negative outcome indicates a deterrent effect while a positive outcome indicates an amplification effect. We would expect that if incarceration does not have an effect on criminal offending the difference would be zero. Additionally, I will use the "differences in differences" to investigate the changes over time in the prevalence of criminal involvement and arrest (which are binary dependent variables). Again the prevalence of criminal activity is a measure of the proportion of youth who report committing at least one crime in a given year and the prevalence of arrest is the proportion of youth who report being arrested at least once in a given year.

The same outcome can be achieved by equation 2. The regression includes an interaction term that indicates the effect of being in the incarcerated group after the intervention on either the frequency or prevalence of criminal activity (or prevalence of arrest).[27]

$$Y = \alpha + \alpha_1 time + \alpha^1 group + \beta time*group + \varepsilon \quad (2)$$

where y is the outcome of a change in frequency or prevalence (of either criminal involvement or arrest), *time* is a dummy variable equal to 0 if in a wave before incarceration (either wave 2 or wave 3) and 1 if after incarceration (either wave 5 or wave 6) and α_1 is the way both groups together are influenced by time, *group* is a dummy

[27] This model is based off of Meyer's (1995) design for modeling the differences in differences technique using a before and after design with an untreated control group. OLS regression is used to estimate the effects of incarceration on the frequency of offending, while logistic regression is used to estimate the effects of incarceration on the prevalence of crime or arrest (binary dependent variables).

variable equal to 0 if in the arrested group and 1 if in the group experiencing incarceration, and α^1 represents which group exhibits greater frequency or more prevalence on average over all time periods (if $\alpha^1>0$, then crime is more frequent or prevalent in the incarcerated group, and if $\alpha^1<0$, then crime is less frequent or prevalent in the incarcerated group). By multiplying the *time* and *group* dummies and including the interaction in the model, I can investigate whether criminal frequency or involvement (or arrest) by the incarcerated group after incarceration is significantly larger net of the effects of time or group alone. In other words, does offending by the incarcerated group after intervention show effects above and beyond any offending occurring at the end of the survey period (waves 5 or 6) or any offending occurring by the incarcerated group at any time period (either before or after)? *Time*group* is a dummy variable equal to 1 if in the incarcerated group after it receives intervention, 0 otherwise and β is the "true causal effect of the treatment on the outcome for this group" (Meyer 1995: 155). That is β is the difference between the changes of the two groups or the differences between their slopes over time; hence we would expect β to be 0 when the slopes are equal or when both groups are changing in offending at the same rate over time. By doing this, I can control for time-stable characteristics (at time 1 and at time 2) which I assume to have no influence on the slope of the line. For interpretation of the "differences in differences" of the frequency of offending, the interaction term β in equation 2 produces the same coefficient as β_{dd} in equation 1. However, the coefficient of the effects of incarceration on the prevalence of criminal activity or arrest (using a logit model) cannot be interpreted in the traditional way so predicted probabilities will be calculated.

SAMPLE CHARACTERISTICS

Comparability of Samples

Table 2 indicates that the two groups being compared contain youth who are alike in age, race and prior drug use. The youth in both groups were about 14 years old on average during the first wave.[28] In terms of drug and alcohol abuse, the groups exhibit similar involvement prior to 1997. About half of the youths in each group admit to smoking cigarettes, a little less than half of each group report that they drank alcohol and less than a third of the youth in each group smoked marijuana. Most interesting perhaps, is that both groups admit to committing the equivalent amount of crimes on average in 1997 and 1998. The only significant differences between the groups are that the incarcerated group contains more males, less youth living in urban areas, and more school dropouts.

Table 2. Group Means of Individual Characteristics.

	Incarcerated	Control	Test Statistic
Age in 1997^	14.12	14.08	-0.30
% Male	0.84	0.66	-3.64*
% Black	0.29	0.30	0.19
% Lived in Urban area in 1997	0.68	0.81	3.03*
% Dropped out of school by 1998	0.27	0.14	-3.37*
% Ever smoked cigarettes prior to 1997	0.51	0.46	-0.99
% Ever drank alcohol prior to 1997	0.44	0.45	0.07
% Ever smoked marijuana prior to 1997	0.24	0.26	0.25
Number of crimes committed in 1997^	6.61	6.12	-0.27
Number of crimes committed in 1998^	5.61	4.92	-0.55

* significant at the .05 level (two-tailed test)
^Two sample t-test of means was performed; otherwise two-sample test of equal proportions was used for a z-score.

Table 3 summarizes the criminal history by type of crime of youth in each group prior to 1999 (the first year of possible incarceration). The table contains proportions of

[28] The groups' similarity in age is important in ruling out maturation effects. If the control group declines in criminal activity because of age we would expect the incarcerated group to decline in the same manner.

youth in each group who reported committing at least one crime disaggregated by offense type. The most popular type of crime committed by both groups is petty theft (worth less than $50). The involvement in other types of property crimes varies, but overall I find no significant differences in property offending. Likewise, the amount of involvement in selling drugs is also not different between the groups; in fact, only about a quarter of each group admits to selling drugs prior to 1999. A significant proportion of each group report assaulting another person during this time period, but the incarcerated group contains significantly more youth admitting such offenses. However, in general the incarcerated group and the control group look fairly similar on measures of frequency and prevalence of criminal activity prior to 1999. Overall, the two groups in this study look fairly similar in terms of demographics, drug use, and prior criminal activity; however, it is possible that the groups differ in ways in which I am unaware.

Table 3. Prior Criminal Involvement of Youth by Group.^

	Incarcerated	Arrested	Z-score
Property	0.42	0.42	-0.00
Petty Theft	0.61	0.60	-0.21
Theft > $50	0.27	0.20	-1.41
Other Property	0.20	0.19	-0.34
Attacked Someone	0.53	0.41	-2.29*
Sold Drugs	0.26	0.23	-0.56

* = significant at the .05 level (two-tailed test)
^ = Committed offense before 1999

Problems of Attrition

There are two reasons why a youth may be excluded from this study: he may not have answered all the crime questions or he may not have been surveyed in the last two waves of the survey.[29] Both sources of information are necessary: it is important to know how many of each crime a youth committed in order to compare frequencies across

[29] Both the incarcerated group and the arrested group were chosen based on incarceration status for waves 1 through 4 therefore the youth could not have missing surveys during this time. Missing surveys were possible in waves 5 or 6 (the outcome period), therefore I include them in my attrition analysis.

individuals and it is also important that survey information is available after incarceration in order to do a before and after comparison. Still, it is also necessary to acknowledge that the youth investigated here may differ somewhat from the youth who attritted from the study. Table 4 illustrates the significant differences found between the youth in the incarcerated group (N=90) and those youth who were excluded for either of these reasons (N=34).[30] Those youth who were excluded were much more likely to be black than those included in my sample (56% versus 29%, respectively) and were less likely to live with their biological parents (12% versus 31%, respectively). Surprisingly, the youth who were dropped from the sample were less likely to report prior cigarette smoking or alcohol consumption. Also interesting, for most crime types the dropped youth did not differ significantly from the youth in my sample and regarding prior involvement in petty theft and theft over $50, the dropped youth were actually significantly less likely to be involved in these criminal activities. However, previous research indicates that blacks are less likely to report their involvement in crime than whites (Farrington et al. 1996). Therefore it is quite possible that the dropped youth, who are more likely to be black, are underreporting their involvement in crime and are actually more criminogenic than shown here.

[30] My discussion and tables only include variables that produced significantly different means between the groups of included youth to excluded youth. All the variables that were investigated include: percentage male, black, and Hispanic, prior drinking of alcohol, smoking cigarettes or marijuana, % dropped out of school by wave six, % urban, highest grade completed by parent, % involved in prior property crime, petty theft, theft over $50, other property crime, or assault, % of youth involved in selling drugs, % peers belong to gang in 1997, and % living with biological parents.

Table 4. Differences between Incarcerated Group and All Dropped Youth.^

	Incarcerated N=90	Dropped N=34	Z-score
% Black	0.29	0.56	2.79**
% Ever smoked cigarettes prior to 1997	0.51	0.24	-2.76**
% Drank Alcohol prior to 1997	0.44	0.26	-1.83*
% Involved in prior Petty Theft	0.57	0.31	-2.47**
% Involved in prior Theft Over $50	0.09	0.00	-1.80*
% Living with Biological Parents	0.31	0.12	-2.23**

*Marginal Significance: Test statistic is significant at the .10 level.
**Significance: Test statistic is significant at the .05 level.
^ Dropped Youth (N=34) includes 19 youth who were dropped due to missing surveys and 15 youth dropped due to missing crime information.

Of these 34 youth who were excluded from the incarceration sample, 15 youth were excluded due to missing crime information and 19 were missing surveys in the waves after incarceration (see Appendix for additional tables). Those who were excluded for missing crime information were less likely to be male and were more likely to have dropped out of school by the end of the survey period. Nevertheless, it is encouraging that those youth dropped for missing crime information did not differ significantly from those included in their prior criminal activity. Those who were excluded due to missing surveys were more likely to be black and less likely to live with their biological parents, but they also were less likely to smoke cigarettes and to have committed prior petty theft. Overall, the dropped youth look very similar to those included in my sample in demographics and criminal offending while at times the excluded youth exhibit less involvement in certain crimes (theft) and other impulsive behavior (smoking and drinking). However, caution is required because the retention rate for this group is lower than the overall retention rate for the NLSY sample (72% versus 88% at wave 6) and the youth could differ in ways in which I am not aware. For example, although the criminal involvement of youth prior to the first survey is similar, the dropped youth may be more likely to underreport involvement (because over half of this group is black) and it is also

possible that the dropped youth involved in crime commit higher rates of crime (on which I have no information) prior to the first survey than those included in my study. If the dropped youth are more frequent or even more serious offenders, they may have had different post-incarceration experiences than the youth studied here.

The arrested sample (N=1002) was chosen based on having been arrested at least once but never incarcerated in the first four waves of the survey (individuals could be incarcerated in waves 5 or 6 as long as they were not incarcerated prior). Some youth were excluded because they were missing survey information in either waves 5 or 6 (N=114) and others were excluded for not providing answers to the crime questions posed by the interviewers (N=68) resulting in a retention rate of 84% by wave 6. Table 5 includes comparisons between the arrested group used in this study and those excluded youth for either of these reasons (N=182). The group of excluded youth contains more males, high school dropouts, and cigarette smokers. The youth not included in my analysis also are less likely to live with their biological parents and have parents with slightly less education. Although the groups report similar involvement in most criminal activities before the survey period, involvement in drug selling by the dropped youth is double that of the arrested youth included in my study.

Table 5. Differences between Arrested (control) Group and All Dropped Youth.

	Arrested N=1002	Dropped N=182	Test Statistic
% Male	0.66	0.73	1.81*
% Ever smoked cigarettes prior to 1997	0.46	0.53	1.89*
% Dropped out of school by 2002	0.25	0.32	1.66*
Highest Grade Completed by Parent	13.08	12.58	-2.21**
% Living with Biological Parent in 1997	0.40	0.32	-2.04**
% Involved in prior Selling of Drugs	0.06	0.12	2.85**

*Marginal Significance: Test statistic is significant at the .10 level.

**Significance: Test statistic is significant at the .05 level.

I also investigated the differences between the two types of missing youth and the arrested youth included in this study (see the Appendix for additional tables). Those youth who were missing surveys in the last two waves were less likely to live with their biological parents, more likely to be male and had parents who were less educated than those in my study. Although the groups' demographics are quite similar, I am unable to determine how similar the groups are on unknown characteristics. Still, it is possible that my control group contains less serious youth than those youth arrested at least once in the population. As expected, the excluded youth who did not answer all the crime questions reported higher rates of involvement in those crimes in which they shared information. The dropped youth were more likely to report prior property offenses, petty theft, and selling drugs. These youth were also more likely to drop out of school by the end of the survey period and have friends involved in gangs.

The differences between the groups included in the study and the youth who attritted from either group threaten the external validity of this study. By studying groups of individuals who are different from those found in the population, I am unable to make definitive conclusions about how incarceration would affect those youth not included in my samples with similar incarceration experiences. There are not many observable differences between those youth dropped from my incarceration group; however, I can never be sure that the groups do not differ on variables on which I have no information. There are differences in the criminal involvement of those dropped from my arrested sample which suggests my control group may underestimate the criminal involvement of youth who are arrested at least once in waves 1 through 4. Since I will be comparing the differences between a less criminogenic arrested group and the incarcerated group, it is

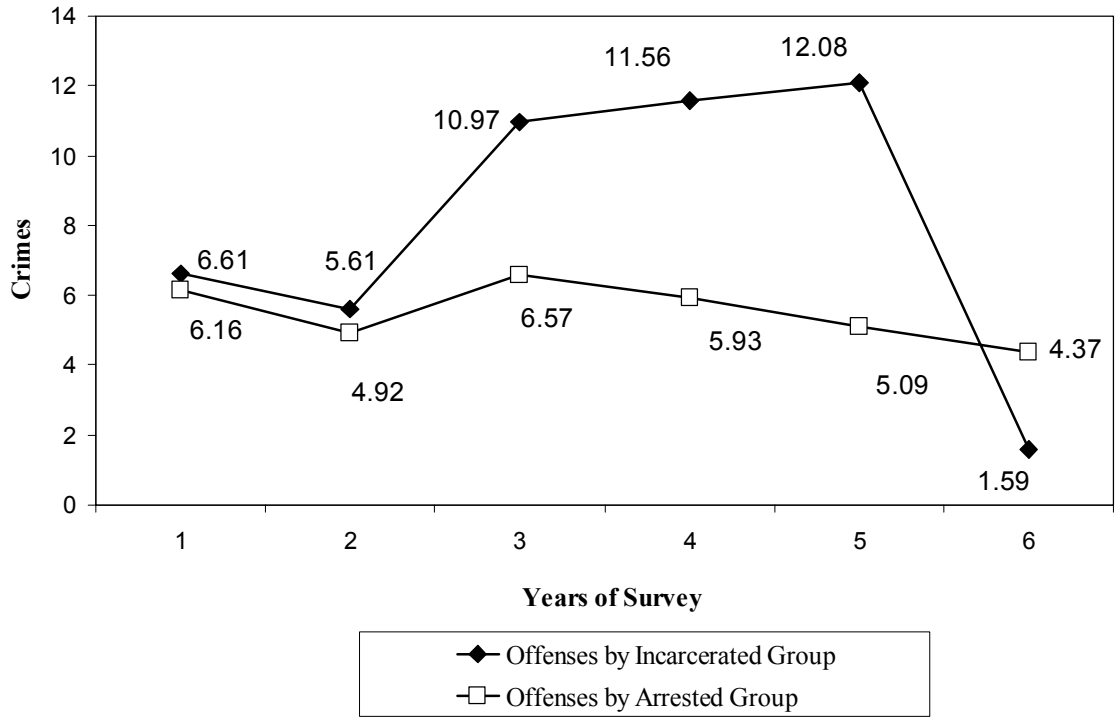
possible that any amplification effects exhibited by the incarcerated group could be overestimated and deterrent effects underestimated from what would normally occur in the population of youth surveyed. Furthermore, if the dropped youth from my incarcerated sample are indeed more criminogenic (despite their reports of less activity), the incarcerated group outcomes might not be over or underestimated (compared to the control group outcomes) but my ability to generalize to a larger group of youth in decreased.

CHAPTER III: RESULTS

FREQUENCY OF CRIMES COMMITTED

The rate at which each group commits crimes varies over the six waves (see Figure 1). This frequency count includes the total of property crimes, person crimes, and drug crimes committed by each youth. The arrested group's rate of crime remains around 5 crimes per year for all years while the incarcerated group exhibits a substantial increase and decrease in the rate at which they commit crimes.

Figure 1. Means of Crimes Committed by Group.



The first section of Table 6 summarizes the differences in frequency of all crimes between the group of youth experiencing incarceration and the group that did not. In general, over the long term (from wave 1 to wave 6) there is a significant decrease in the frequency of crimes committed by the average offender regardless of group affiliation (β=-1.80, p=0.01). Also, the incarcerated group on average commits more crimes than

the arrested group when looking at both before and after the intervention (*group* coefficients), although the differences are not statistically significant. This result should not be surprising given my intent to choose a control group that is similar in criminality to my experimental group but not similar in incarceration experiences. However, I am most interested in the effect of incarceration on the interaction term, *time*group*, which indicates if there is an effect unique to the incarcerated group after the intervention. Essentially, the incarcerated group experienced an increase of 6.3 more offenses from wave 2 to wave 5 than the arrested group. The incarcerated group increased their offending from 5.61 crimes to 12.08 crimes from wave 2 to wave 5 for an average increase of 6.47 crimes. The arrested group however, only increased their offending from 4.92 crimes in wave 2 to 5.09 crimes in wave 5, for an average increase of .17 crimes. The differences in differences of 6.3 crimes (6.47- 0.17) is statistically significant at the .05 level.

Table 6. Effect of Incarceration on the Frequency of Offending: "Differences in Differences".

	Time	Group	Time*Group
Any Crime			
Wave 5-2	0.17	0.69	6.30*
	(-0.64)	(1.27)	(3.00)
Wave 6-1	-1.80**	0.45	-3.22
	(-0.69)	(1.63)	(1.75)
Non-Drug			
Wave 5-2	-0.25*	0.96	-0.21
	(0.12)	(0.70)	(0.80)
Wave 6-1	-0.83**	0.27	-0.29
	(0.12)	(0.46)	(0.47)
Drug			
Wave 5-2	1.70**	0.37	3.65
	(0.58)	(1.00)	(2.49)
Wave 6-1	1.90**	-0.11	-3.16*
	(-0.61)	(1.18)	(1.32)

Standard errors in parentheses

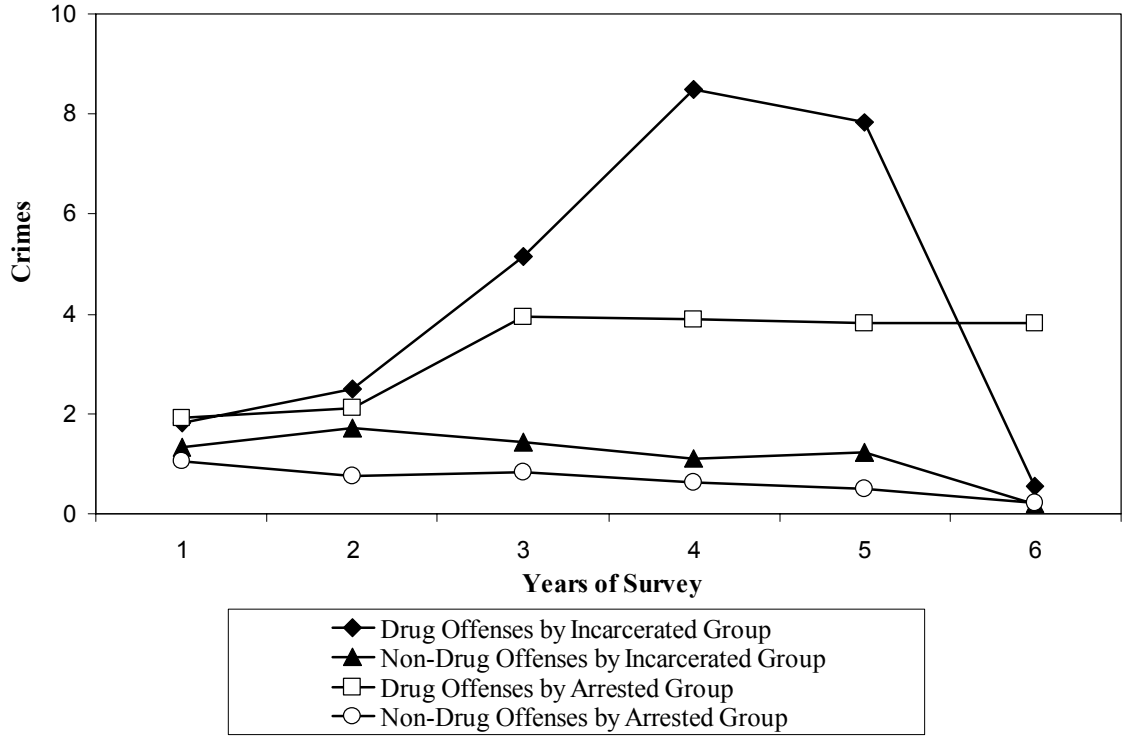
* p<.05, ** p<.01

However, as exhibited in Figure 1, the use of multiple pretests and posttests tells us that a conclusion of amplification is too simplistic. When comparing wave 6 to wave 1 (that is going farther back in time and farther ahead in time in relation to incarceration), the group coefficient indicates a slight deterrent effect although the result is not statistically significant from zero (β=-3.22, p=.07). Although Figure 1 portrays that the average offending of the incarcerated group drops below that of the control group, the change in offending attributed to incarceration is not significant and may be due to chance. However, the lack of statistical significance may result from small sample sizes and the lack of variability in the independent variables. Still, short term results suggest that youth who receive incarceration commit rates of crime immediately after release that are double the rate at which these youth may have offended otherwise. Although I do not include the frequency of offending at wave 4 in any statistical tests (because some youth are receiving the intervention here and therefore it is hard to distinguish which group is committing the offending), the similar high rates of offending compared to wave 5 provides some support for this argument since 34% of the incarcerated group are released after wave 3. In other words, the highest rates of offending over the six years occur during the years immediately following incarceration.

In order to investigate the changes in frequency of offending by the groups, I further distinguish between rates of drug crime and non-drug crime. Figure 2 depicts the trends in average offending by group and by type of crime. It is obvious from this picture that the drug crimes committed by each group are responsible for most of the variation in the group averages depicted in Figure 1. The two groups exhibit similar patterns over time in regards to their non-drug (property and person) offenses, but they differ when

comparing the average drug crimes committed over time. The incarcerated group commits more and more drug crime on average until wave 4 and between wave 5 and wave 6 the amount of drug crimes committed on average drops dramatically (from 7.83 crimes per year to less than 1). The arrested group in contrast, increases its average drug offending slightly from wave 1 to wave 3 but thereafter remains fairly stable (just under 4 drug crimes per year).

Figure 2. Means of Drug Crimes and Non-Drug Crimes Committed by Group.



Overall, the frequency of non-drug crimes committed stays relatively stable over time in comparison to the frequency of drug crimes committed. Figures 3 and 4 depict non-drug and drug crime trends separately. Non-drug crimes for both groups average less than 2 crimes per year for all of the six years of the survey. As seen in Figure 3, both groups generally decrease their frequency of offending over time when excluding drug selling. This general decline in offending by all youth is supported by negative and

significant *time* coefficients in Table 6. Except for wave 6, the incarcerated group of youth averages higher frequencies of crimes each year than the control group; however being in the incarcerated group alone does not have a significant effect on the frequency of offending. In the last wave, both groups report similar amounts of crimes committed on average (incarcerated mean = .21; arrested mean = .23). However when using the differences in differences method, the decrease in non-drug offending is not statistically significant between wave 1 and wave 6 (β=-0.29, p=.53) nor is it between wave 2 and wave 5 (β=-0.21, p=.80). In sum, although there is a downward trend in the frequency of non-drug offending over time, I cannot say with confidence that the incarcerated group is experiencing a deterrent effect on their property and person offending over the long term above the general decline in offending over time. Both groups tend to be declining in rates of non-drug crime regardless of whether they were or were not incarcerated.

Figure 3. Means of Non-Drug Crimes Committed by Group.

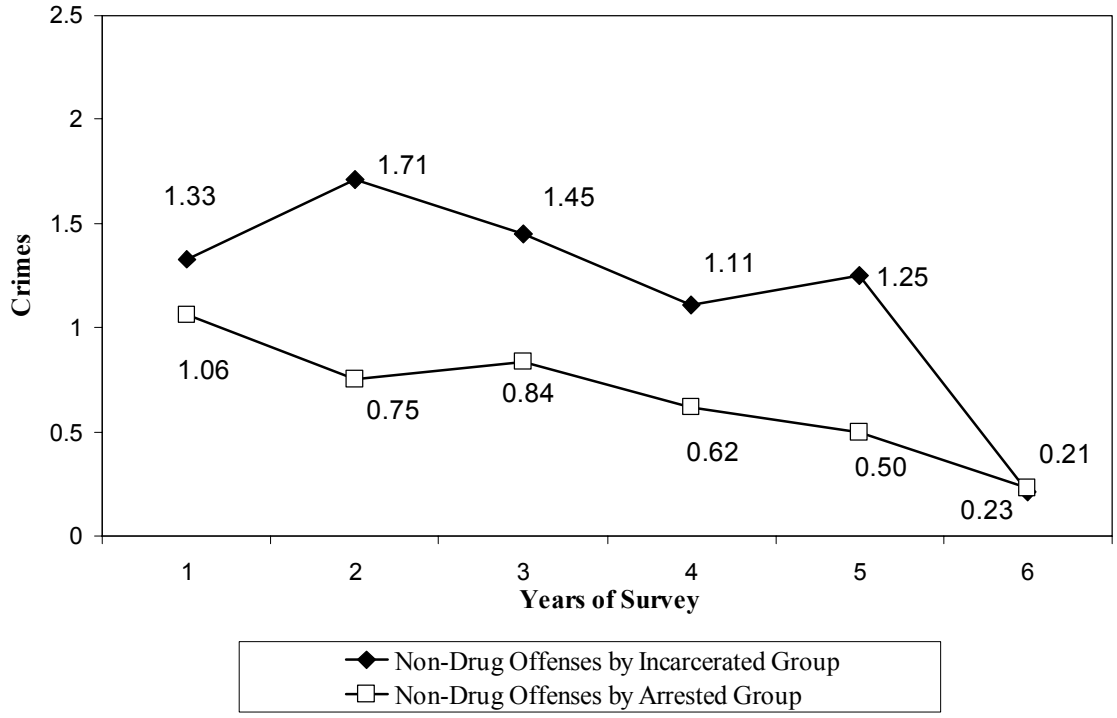


Figure 4 illustrates that drug selling by the youth in my samples is much more complex. The arrested group increases its average offending from wave 1 to wave 3 but then its offending levels off throughout the remaining waves. The average offending by the arrested group remains around 2 to 4 drug sales per year. However, the incarcerated group commits similar amounts of drug crime on average as the control group in the first wave (1.81 versus 1.92), but then increases its drug selling from about 2 crimes per year to around 8 crimes per year in each wave four and wave five. The group's average offending drops drastically in wave 6 (to less than 1 drug sale per year) to an average below that of the control group.

Figure 4. Means of Drug Crimes Committed by Group.

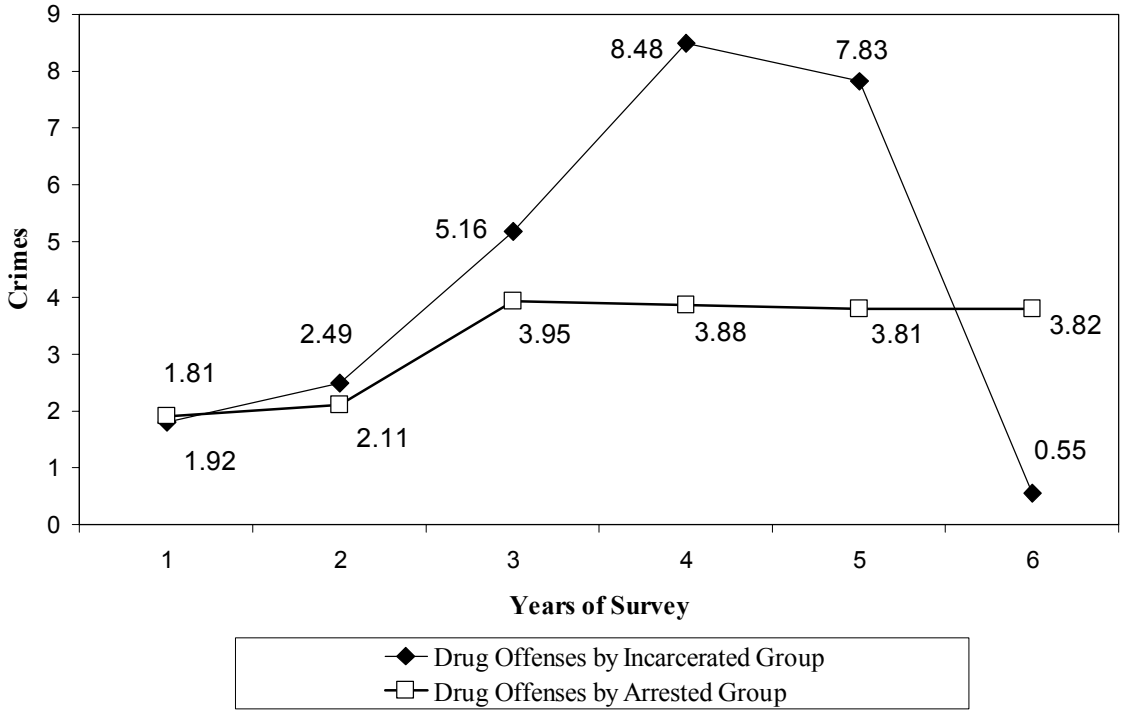


Table 6 also summarizes these changes in the frequency of drug crime between the groups using the differences in differences method. For the average offender, regardless of group, over time there is a significant increase in the frequency of

offending. However, over the short term, there is no significant amplification effect on the incarcerated group above and beyond this general upward trend: the coefficient comparing wave 2 and wave 5 is positive and but insignificant (β=3.65, p=.14). It is possible that the lack of significance could be a result of small sample size or the lack of variance in my dichotomous group variable and therefore, the increase in offending of the incarcerated group relative to the arrested group should not be overlooked. For example, the incarcerated group increases its average drug selling to 7.83 crimes in wave 5, for an increase of 5.34 crimes between wave 2 and wave 5, while the arrested group increases its average drug selling to 3.81 crimes in wave 5, for an increase of 1.70 crimes from waves 2 to 5. The incarcerated group exhibited a much larger increase in drug offending than did the arrested group in the same time period (5.34-1.70) resulting in a group coefficient of 3.65. In contrast, the change from wave 1 to wave 6, indicates a significant decrease in offending (β=-3.16, p=.02).

Overall, there seems to be no amplification or deterrent effect of incarceration on the frequency of non-drug crimes while the effects of incarceration on drug selling is unclear. There is a clear amplification of drug selling over the short term even though the differences are not statistically significant, but low statistical power may be to blame. The short-term amplification effect of incarceration on drug crimes is alarming and strongly implies that incarceration has harmful effects on youth in my sample. Furthermore, by wave 6 the differences indicate a significant deterrent effect of incarceration on drug selling although the large drop in drug offending from wave 5 to wave 6 warrants further scrutiny. It is also important to note that the rates of drug crimes by the incarcerated group seems to be driving the overall frequency of all crimes

committed by this group and that the rates of drug selling immediately after incarceration more than double the rates of selling prior to incarceration.

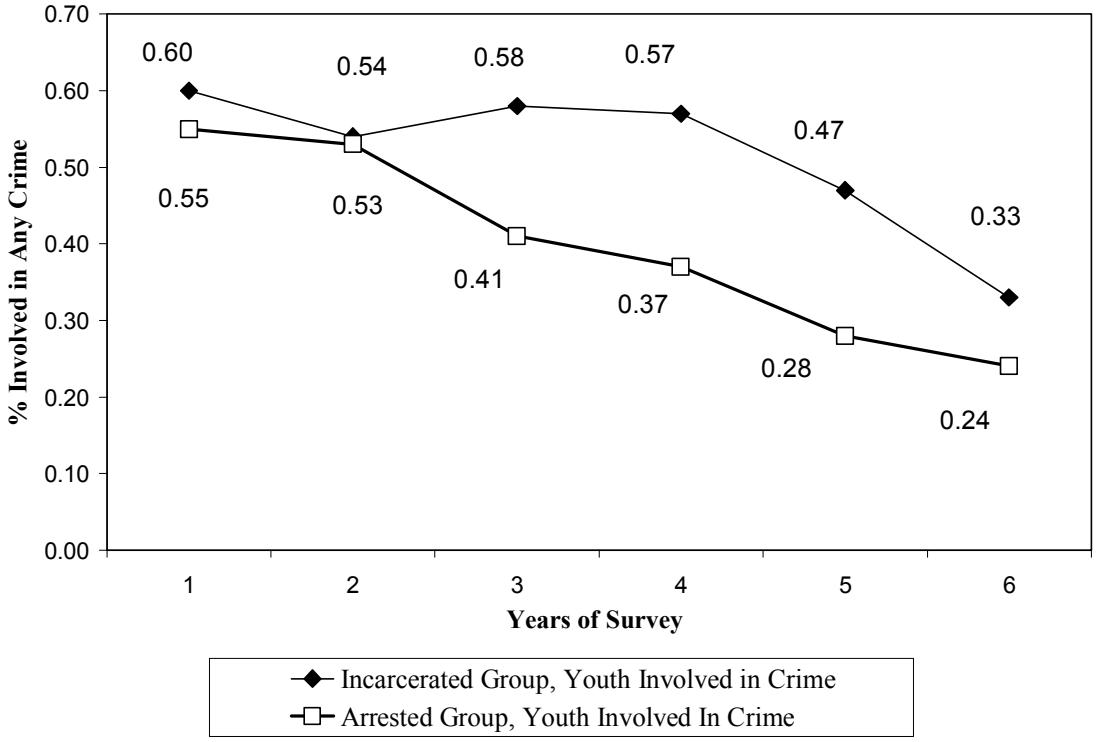
PREVALENCE OF CRIMINAL ACTIVITY

In addition to comparing the amount of crimes committed by youth in each group, it is also informative to investigate changes over time in how many of the youth in each group are involved in criminal activity. The prevalence of criminal offending is simply the percentage of youth who committed at least one crime out of all the youth in the sample. Here I am interested in whether youth who experience incarceration are more likely to stop offending completely (rather than merely reduce the rate at which they offend) or whether their involvement in crime is unaffected by incarceration. This variable provides an alternative way of measuring criminal activity which should not be largely driven by drug sales since only about a quarter of the youth in my sample are involved in drug crimes (see Table 3).

In general, a greater proportion of the youth in the incarcerated group report involvement in crime than the youth in the arrested group for every year of the survey (see Figure 5). But both group's prevalence in criminal activity are much more similar before the intervention period than afterwards. Over time, fewer youth in both groups stay involved in crime over the years of the survey. In the first year, about 60% of the incarcerated group and about 55% of the arrested group committed any crime, and by the last year, 33% of the incarcerated group and 24% of the arrested group committed any crime. However, there is clearly a larger drop in criminal involvement for the arrested group over the short term (they drop 25 percentage points while the incarcerated group only drops 7 percentage points from wave 2 to wave 5) while over the long term the

decreases in criminal involvement remain around 30 percentage points for both groups (from wave 1 to wave 6).

Figure 5. Prevalence of Crime Involvement by Group.



The effects of incarceration on the criminal involvement of youth are reported in the in Table 7 broken down by crime types. The *time* coefficients are significant and negative almost across the board, which supports the general decline in criminal involvement of both groups over time. Being in the incarcerated group alone does not have an effect on criminal involvement as indicated by non-significant group coefficients. The time*group coefficients which represent the causal effect of being in the incarcerated group after incarceration are only sometimes significant in the short term (wave 5).

Table 7. Effect of Incarceration on Prevalence of Criminal Involvement and Arrest: "Differences in Differences".

	Time	Group	Time*Group
Any Crime			
Wave 5-2	-1.02**	0.08	0.71*
	(0.09)	(0.22)	(0.31)
Wave 6-1	-1.33**	0.22	0.23
	(0.10)	(0.22)	(0.33)
Non-Drug			
Wave 5-2	-1.22**	0.07	0.63*
	(0.10)	(0.22)	(0.32)
Wave 6-1	-1.68**	0.22	0.19
	(0.10)	(0.22)	(0.34)
Drug			
Wave 5-2	-0.31*	-0.01	0.69
	(0.13)	(0.30)	(0.41)
Wave 6-1	-0.13	0.02	0.13
	(0.14)	(0.34)	(0.48)
Arrest			
Wave 5-2	-1.21**	-0.07	1.46**
	(0.12)	(0.24)	(0.34)
Wave 6-1	-1.48**	-0.07	1.27**
	(0.11)	(0.24)	(0.40)

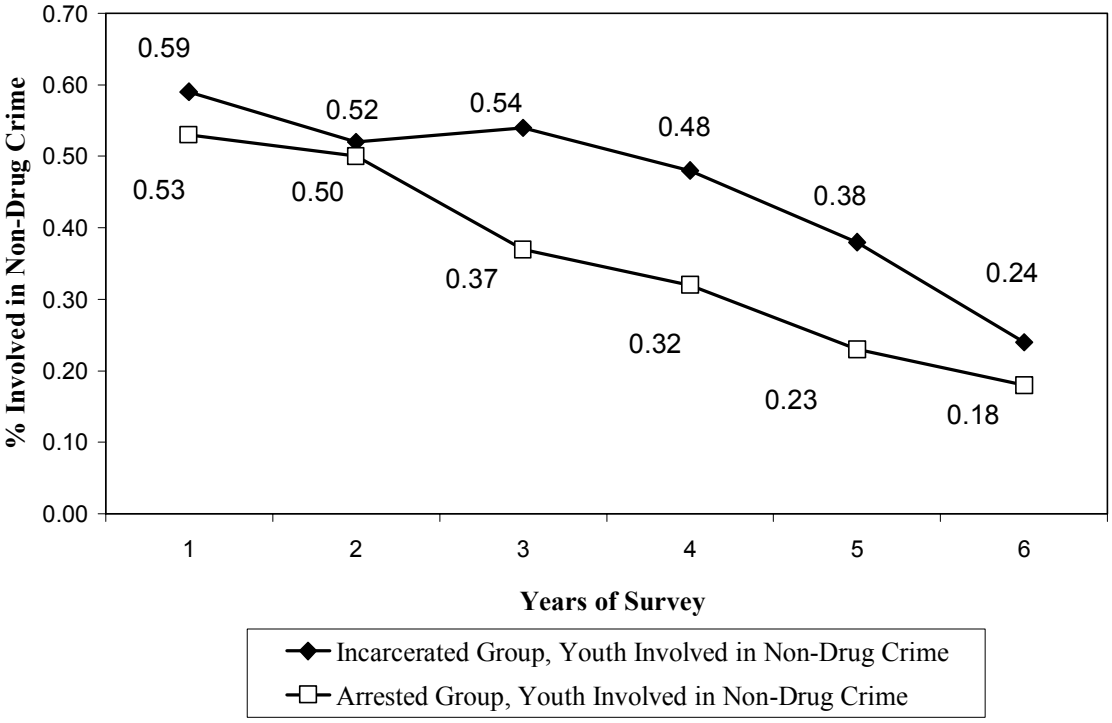
Logistic Regression.
Standard errors in parentheses
* p<.05, ** p<.01

The first two rows of Table 7 report effects of incarceration on the involvement of any crime. The interaction term *time*group* produces a positive and significant coefficient for involvement in any crime (β=.71, p=.02) when comparing short term outcomes (wave 5 to wave 2) but not for long term outcomes (wave 6 to wave 1; β=.23, p=.47). From wave 2 to wave 5, the incarcerated group experienced a decrease (7 percentage points) in criminal involvement that is significantly smaller than the decrease experienced by the arrested group (25 percentage points). So, even though both groups experience a significant decrease in involvement in crime over the short term, youth in the incarcerated group do not exhibit significantly different trends in criminal involvement from the arrested group in the long run. Nevertheless, a lack of statistical significance may result from low power as the groups still differ by about 9 percentage

points in the last wave. Still, the evidence suggests that youth who experience incarceration may stay involved in criminal activity for a longer period of time when otherwise some of them may have stopped offending directly after release.

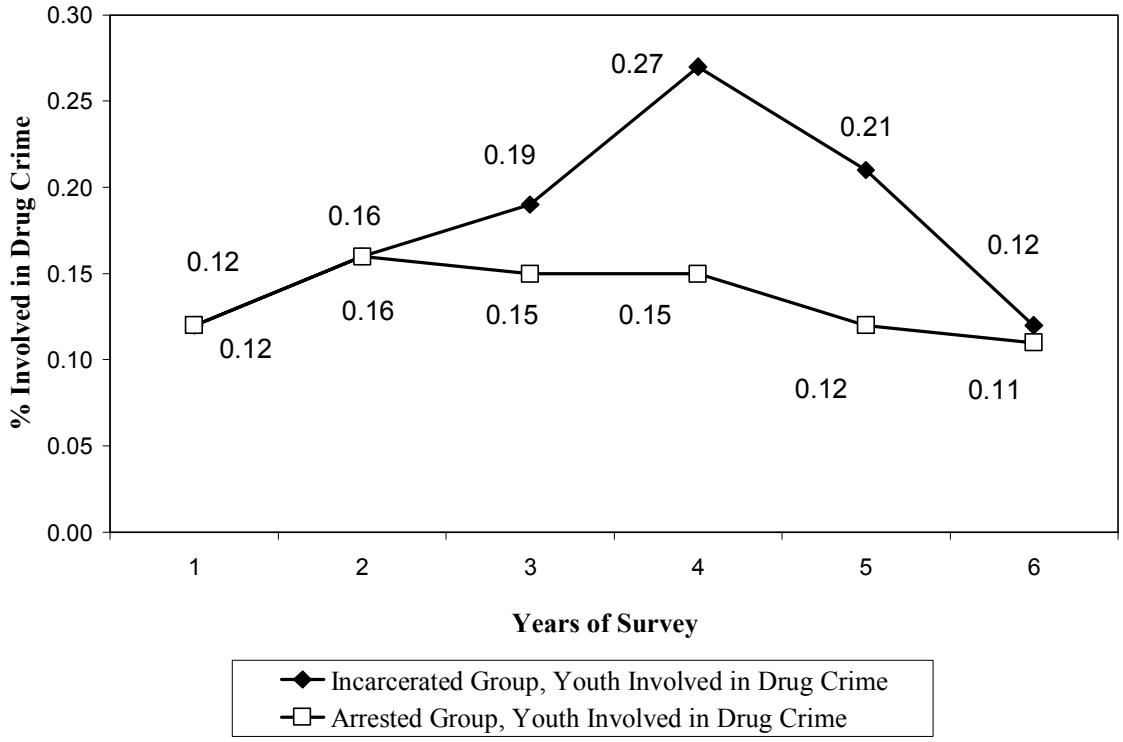
A very similar picture is illustrated in Figure 6 of the prevalence of non-drug crimes in each group. With each additional year, fewer youth in each group report committing a non-drug crime like property crimes or assaults. The effects of incarceration on the involvement in non-drug crimes like property and assault crimes reported in the third and fourth rows of Table 7. The effect of incarceration on the prevalence of non-drug crimes is only significant at wave 5 (β=.632, p=.04). The decrease in involvement from wave 2 to wave 5 for the incarcerated group (a decrease of 14 percentage points) is significantly smaller than the drop in involvement of the arrested group (a decrease of 27 percentage points). However, over the full six years being in the incarcerated group provides no significant change in non-drug crime involvement above that of the control group. Although fewer youth are committing non-drug crimes in later waves these results suggest that the experience of incarceration prolongs the desistence of criminal activity.

Figure 6. Prevalence of Non-Drug Crime Involvement by Group.



Like the frequency of drug crimes committed, the involvement of the arrested group in drug crime is much more stable over time than the involvement of the incarcerated group (Figure 7). Contrary to the frequency of drug crimes, the magnitude of involvement in drug crime by both groups is small in comparison to non-drug crimes, while the magnitude of the rates at which both groups commit drug crimes is large in comparison to non-drug crimes. It is important to note that at no time is more than 16% of the arrested group or 30% of the incarcerated group involved in drug selling at any time. That is, even though the frequency of drug crimes are on average higher than the frequency of non-drug crimes, the prevalence of drug crimes is smaller on average than the prevalence of non-drug crimes.

Figure 7. Prevalence of Drug Crime Involvement by Group.



In the first two waves, involvement in drug crimes by both groups is the same (12% in wave 1 and 16% in wave 2) but the prevalence of drug selling peaks at different times for each group. After wave 2 the arrested group's involvement in drug crime decreases while always remaining smaller than the prevalence of drug crime by the incarcerated group. For the incarcerated group, prevalence peaks at 27% in wave 4 but decreases to 12% by the last year. However, there is a striking difference between the groups prevalence in wave 5 when in wave 2 they were identical. There is clear amplification by the incarcerated group which increased its criminal involvement while the arrested group decreased its involvement slightly. By the last year of the survey both groups exhibit prevalence of crime almost identical to their prevalence in wave 1.

As reported in the fifth and sixth rows of Table 7, the effect of being in the incarcerated group after intervention (in either waves 5 or 6) is not significant at the .05

level (β=.69, p=.09; β=.13, p=.79, respectively). Again, the lack of significance, however, could be a result of small sample size or the lack of variance in my dichotomous group variable. Still, like the effect of incarceration on frequency, the short term amplification effect of incarceration on involvement in the selling of drugs should not be overlooked. Once again long term changes are not significant between the groups; in fact just as many youth in the incarcerated group sell drugs in wave 1 as in wave 6 (12%). Much like the huge drop in frequency by incarcerated group between wave 5 and 6, the involvement in drug selling by this group also declines rapidly. Involvement in drug crimes drops in the last three years, and those youth who remain selling do so at a lower rate.

Unlike the frequency of crime committed over time, the prevalence of crime is driven by non-drug criminal involvement. This is not surprising given that a larger proportion of youth in these samples commit non-drug crimes. These results suggest that incarceration prolongs criminal involvement in non-drug crimes while increasing involvement in drug crimes in the short term. The predicted probabilities of committing a crime (while holding all other variables at their means) are presented in Table 8. In order, predicted probabilities are reported for: being in the incarcerated group after incarceration, being in any of the other reference groups, being in the incarcerated group at any time, being in the control group at any time, being in either group before the intervention period, and being in either group after the intervention period. Overall, the probabilities of committing a crime are similar for the average youth in the arrested group and the average youth in the incarcerated group for drug and non-drug crimes. Again, this is expected given my selection of a control group similar in criminality to the

incarcerated group. Furthermore, general declines in prevalence are once again portrayed by decreasing probabilities of committing a crime by the average youth regardless of group affiliation (columns 5 and 6): all probabilities of committing a crime before waves 3 and 4 are higher than the probability of committing a crime after waves 3 and 4.

Table 8. Predicted Probabilities of Committing a Crime or Being Arrested.

	Inc Grp After	Other Group^	Inc Grp	Arr Grp	After	Before
	t*g=1	t*g=0	grp=1	grp=0	time=1	time=0
Any Crime						
Wave 5	0.58	0.40	0.42	0.41	0.29	0.53
Wave 6	0.44	0.39	0.44	0.38	0.25	0.55
Non-Drug						
Wave 5	0.51	0.36	0.38	0.36	0.24	0.51
Wave 6	0.38	0.33	0.38	0.33	0.18	0.54
Drug						
Wave 5	0.24	0.14	0.14	0.14	0.12	0.16
Wave 6	0.13	0.11	0.12	0.11	0.11	0.12
Arrest						
Wave 5	0.52	0.20	0.20	0.21	0.13	0.33
Wave 6	0.51	0.23	0.12	0.25	0.13	0.39

Wave 5 probabilities were calculated relative to wave 2.
Wave 6 probabilities were calculated relative to wave 1.
^All others include the probability of committing a crime by anyone in the incarcerated group before incarceration or in the arrested group in either time period.

Interestingly, being in the incarcerated group after incarceration is often associated with the highest probability of committing a crime in wave 5. Also, comparing the probabilities in the first column to those in the fifth column, the results suggest that criminal involvement by youth who have experienced incarceration is larger than that of the average youth up to two years after the intervention (for both waves 5 and 6). But when comparing the first column to the second column, it is over the short term (in wave 5) where the youth in the incarcerated group are predicted to be much more involved in any type of crime compared to any other combination of youth (incarcerated group before intervention, or arrested group at any time period). In fact the prolonging effect of incarceration on the prevalence of non-drug crime over the short term is

signified by the identical probabilities (.51) of the average youth before the intervention period and the average youth in incarcerated group after incarceration. Also, the amplification of incarceration on the prevalence of drug selling over the short term is apparent in that the average youth in the incarcerated group after incarceration is most likely to be involved in drug selling when compared to any other average youth. To summarize, the average youth in the incarcerated group after incarceration is the most likely to be involved in crime in wave 5, while in wave 6 the experience of incarceration does not change the likelihood that the average youth in the incarcerated group would commit a crime (wave 6; columns 1 and 3).

PREVALENCE OF ARREST

Finally, my dataset allows me the opportunity to determine whether youth who exhibit decreases in involvement in crime are experiencing decreases in the prevalence of arrest as well. Figure 8 maps the changes in prevalence of arrest among each group over the six years of the survey. Over the long term (from wave 1 to wave 6) the arrested group exhibits a much larger decline in the prevalence of arrest than the incarcerated group. The prevalence of arrest in the arrested group declines by 27 percentage points while the incarcerated group experiences a decrease of 3 percentage points.

Figure 8. Prevalence of Arrest by Group.

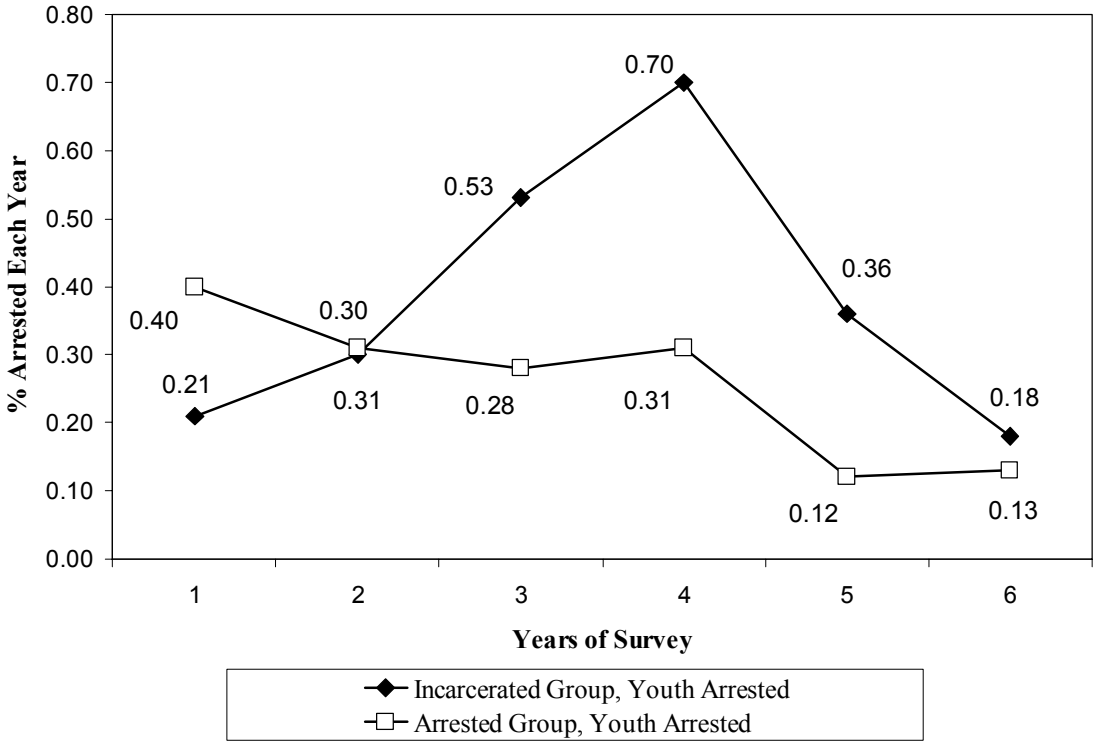


Table 7 reports the effects of incarceration on the prevalence of arrest for these two groups and the predicted probabilities are reported in Table 8. Both coefficients are significant indicating that incarceration produces a significant positive effect on arrest above and beyond the change in arrest of the control group. This can be seen most clearly in Table 8. An average youth in the incarcerated group after incarceration has a .52 probability of being arrested in wave 5 and a .51 probability of being arrested in wave 6, while the average youth in these waves has a .13 probability of being arrested in either wave. Although the arrested group starts out with higher prevalence of arrest in wave 1, by wave 2 each groups' prevalence in arrest is basically identical. Similar to the short term effects of incarceration on the frequency and prevalence of criminal behavior, incarceration also seems to have a substantial short term effect on the prevalence of arrest for those receiving the intervention. Although long term prevalence of arrest coefficient

is significant, it is obviously driven by the large differences between the groups in the first wave. An argument for amplification in prevalence of arrest would be better supported if both groups looked similar in the first wave.

It is unclear why youth in the arrested group are twice as likely to be arrested in wave 1 than the incarcerated group when both groups exhibit similar rates of activity (they average 6.16 versus 6.61 crimes that year) and similar involvement in crime (55% versus 60% respectively). But what is clear is that both groups exhibit similar prevalence of arrest before wave 3 while the youth who are incarcerated report the experience of arrest three times higher than the control group directly after incarceration. High prevalence of arrest is expected during the years in which these youth become incarcerated but the substantial difference in arrest during wave 5 parallels the substantial difference in criminal involvement between the groups in the same year. Large differences in the prevalence of arrest at wave 5 independently suggest that the youth who were incarcerated may be closely followed by police or parole officers after release. But in conjunction with the finding that the incarceration group also exhibits high prevalence of criminal activity, these findings may be merely a reflection of incarceration's impact of prolonging criminal activity after release. The findings on arrest prevalence may also be biased by the way I chose my samples: arrest is highest for the control group during the first four waves when arrest during this time period was a requirement, and arrest is highest for the incarcerated group during the same years that all the youth in this sample were required to be incarcerated. Finally, these arrest measures are self-reported by respondents and are not based on official records and therefore are subject to the same biases as self-reports of offending. Although the conclusions from

the arrest measure support my findings on criminal involvement, they alone are not as suitable a measure of how incarceration affects criminal activity.

OVERALL CONCLUSIONS

Overall the trends in the frequency and prevalence of criminal activity and the prevalence of arrest indicate that incarceration does little to stop criminal careers or future contact with the criminal justice system. In fact, there is evidence that over the short-term incarceration is harmful to youth, especially those involved in the selling of drugs. Although there is a substantial decrease in the frequency of drug selling by the end of the survey period, the same amount of youth are involved in drug offending as there was in the first year of the survey regardless of whether the youth was incarcerated or not. Furthermore there is evidence that incarceration may introduce some youth to the drug trade (at least temporarily) who otherwise would not have gotten involved in drug selling. On the other hand, incarceration does not increase the rate at which youth commit property and person crimes but it does prolong their involvement in such activity. Given that the youth in the incarcerated group are more likely to be involved in both drug and non-drug crimes after incarceration, it is not surprising that they are also more likely to be arrested than the comparison group. Although the long-term results may reflect the exceptionally high prevalence of arrest by the arrested group in wave 1, the short-term results provide strong evidence that higher prevalence in criminal activity leads to higher prevalence of arrest for the incarcerated group. In other words, incarceration increases or at least prolongs the criminal activity of youth in my sample and therefore, increases their chances of being arrested again in the future. Although there is some evidence that the frequency of drug selling decreases substantially for the incarcerated group after wave 5,

this lagged drop is suspicious and will be explored in the following section.

CHAPTER IV: DISCUSSION

EXPLAINING THE DROP IN DRUG OFFENDING

Perhaps the drastic drop in the frequency of drug selling in the last survey year can be explained by changing life factors for the individuals committing drug crimes. Research by Sampson and Laub (1993) indicates that changes in social ties over time like finding employment, getting married and having children reduces criminal activity while other research finds that drug use increases drug crime specifically (Horney et al. 1995). To do this, I compare the youth in each year that were included in the incarceration sample and who reported selling drugs that year (therefore each year will have a different cohort of youth who sold drugs, even though some youth will overlap). I find that as the cohorts are growing older their social ties and substance abuse patterns change. The group of drug sellers each year is growing older over the years of the survey (see Table 9). The average age of the cohort in the first wave is 15 years old (and no one is older than age 18) and the average age in the last year is just over 20 years (and no one is younger than age 18). There is also evidence that over time, smaller proportions of each year's drug cohort is involved in non-drug crime, with 91% of the cohort committing other crimes in wave 1 and 27% of the wave 6 cohort committing other crimes (see Table 10). This further suggests that changes in social ties as youth get older influence their involvement in deviant activities in general.

Table 9. Age of Youth in Drug Selling Cohorts by Year.

	Mean	Std Dev.	Min	Max	N
1	15.23	1.59	12.75	17.25	11
2	17.29	1.32	14.91	18.83	14
3	17.21	1.61	15.08	19.83	17
4	18.60	1.40	16.25	20.83	24
5	19.25	1.41	17.00	22.00	19
6	20.70	1.58	18.25	22.83	11

Table 10. Life Factors of Drug Cohorts by Year

	Non-Drug	Cohabit	Married	Children	Hours Worked	Alcohol	Marijuana	Cocaine*
1	0.91	0.00	0.00	0.00	465.09	1.00	1.00	-
2	0.86	0.07	0.00	0.00	551.86	0.86	0.71	0.64
3	0.82	0.00	0.00	0.06	620.88	0.94	0.82	0.53
4	0.67	0.08	0.08	0.17	839.50	0.88	0.88	0.63
5	0.58	0.21	0.11	0.16	793.10	0.89	0.74	0.63
6	0.27	0.27	0.09	0.18	1434.89	0.73	0.64	0.46

* The first survey did not include a question about cocaine use.

Next, I investigate the changes in marriage, children, employment, and drug use over the six cohorts to determine if any differences in social bonds or opportunity might explain why the group of drug offenders in wave 6 sell on average .55 drugs that year while the wave 5 cohort sells on average 7.83 drugs. [31] Later cohorts are more likely than earlier cohorts to cohabit with a romantic partner, get married, or have children over the six years of surveys. But the differences in these life factors are not really large enough to explain the drastic drop in drug selling from wave 5 to wave 6. However, the average hours worked by the wave 6 cohort is almost double that worked by the wave 5 cohort. That is while in wave 5 drug offenders average 793 hours of legal work that year (approximately a 15 hour work week), the drug offenders in wave 6 averages 1434 hours of legal work (approximately 27 hours a week). This is a sizeable increase which may suggest that as young adults spend more time in legal employment there is less time or motivation to sell large amounts of drugs. Finally, since drug users become involved in drug selling to support their habits (De Li et al. 2000), I expect that youth who do not use may be less likely to sell drugs frequently. Indeed, I find that illegal drug use (cocaine and marijuana) and alcohol use is the least prevalent for the wave 6 cohort than any of the

[31] I also investigated participation in military service. None of the youth in the incarcerated sample reported working in the military during the years of the survey.

other years. It is possible that a greater proportion of youth in the last cohort need not rely on frequent drug selling to support their drug habits.

Comparing differences in cohorts by year gives us some explanation for the reduced frequency of drug crimes sold each year. But what could explain the decrease in prevalence of drug selling over the last two years of the survey? That is why would individuals choose to commit a drug-crime in one year and not in the next? It is somewhat difficult to follow drug offenders over time in this sample because there is high turnover throughout the six years. In fact only thirteen youth are found to report drug selling in three or more years of the survey. Instead, I investigate the changes in marriage, children, and employment over the six years of the group of youth who sold drugs at any time (N=50)[32]. Of those that sell drugs, more and more youth cohabit with a romantic partner, get married, or have children over the six years of surveys (See Table 11). In wave 5, 20% (10/50) of the drug offenders report living with a romantic partner and 24% (12/50) report having children. In comparison, in wave 6, 34% (17/50) of youth report cohabiting and 36% (18/50) report having children. There is also a steady increase in the average number of hours worked over time although by wave 6 the youth only average about 18 hours a week in legal employment. Unlike the drug-selling cohorts, I do not find any substantial changes in drug use nor in the involvement of non-drug crimes over the time period. Still, it seems that some of this group is in fact gaining more social bonds in waves 5 and 6 when there is a decreased involvement in crime.

[32] This group of offenders includes those who sold drugs in at least one year of the surveys.

Table 11. Life Factors of an Incarcerated Sample of Drug Sellers.*

	Cohabit	Married	Children	Hours Worked
1	0.00	0.00	0.00	291.79
2	0.06	0.00	0.04	450.52
3	0.02	0.00	0.06	564.94
4	0.10	0.04	0.20	790.73
5	0.20	0.10	0.24	816.56
6	0.34	0.14	0.36	959.08

* Drug sellers (N=50) are those youth who sold drugs at least once during the survey years.

RELEVANT THREATS TO VALIDITY

There are many threats to internal validity that unfortunately keep me from concluding that my results are what Meyer (1995:155) calls the "true causal effect" of incarceration. Of particular importance in making conclusions is the issue with low statistical power resulting from small sample size in particular. Although, most short-term effects are significant, non-significant long-term effects should not be discounted simply for this reason.

It is unlikely that selection-interaction threats like regression-selection or maturation-selection are at work in this study. Although there is a jump in offending from waves 2 to 3 by the incarcerated group as predicted by the literature on regression-selection, there is no regression to the mean directly after the intervention (in fact the drop in offending is delayed at least one year after release). The artifact depends on *abnormally high rates of offending* immediately prior to selection into treatment (sentencing by judges into a correctional facility), and there is little evidence of this occurring. The average frequency of offending of the incarcerated group continues to climb until wave 5 when most of the group is no longer incarcerated. For this artifact to be an issue I would expect that the offending in waves 2 and 3 would be higher than all other waves (since the youth are incarcerated in either 3 or 4; see Table 1) and this is

obviously not occurring. Secondly, maturation-selection threats would be plausible if one group is on a different trend two years prior to the intervention—that is one group is maturing faster than the other regardless of treatment. The offending by both groups within the first two years of the survey seem to exhibit similar slopes regardless of type of crime, which increases my confidence that this maturation-selection is not threatening my results.

On the other hand, there is still one alternative explanation for my findings that I am unable to rule out. It is unclear whether a history effect is occurring that would cause the group of incarcerated youth to decrease its selling of drugs after wave 5. An overall history effect is not evident since the selling of drugs by the group of youth who were not incarcerated did not follow the same pattern. This may indicate, for example, that there was no national legislation that might have deterred all drug sellers regardless of incarceration experience. Further analysis also indicates that being reincarcerated during the outcome period (waves 5 and 6) does not influence the results reported here.[33] If increased watch by parole and police officers discouraged youth from selling drugs (most of which occurs in public places, see Jacobs and Woods 1999) we would instead expect to see the drastic drop occur in wave 5 when most of the youth are released but not in wave 6. Finding evidence of changes in social ties and drug use provide some confidence that a history effect is not occurring but I cannot entirely rule out this explanation.[34]

[33] For example, when adding a dummy variable for being incarcerated in wave 5, the interaction term coefficient stays at 3.65 and the t statistic changes from 1.47 to 1.49. Similarly, adding a wave 6 reincarceration variable produces no change in the relationship between incarceration and the change in the frequency of drug crimes committed over the long term.

[34] I investigated whether alternate groups of youth exhibited such a drop from wave 5 to wave 6 as well and find that this drop is unique to my sample of youth incarcerated in 3 or 4. There is no drop in drug or non-drug crime for 87 people incarcerated in 1 or 2, or the 139 youth incarcerated in 5 or 6.

Finally, the selection of representative samples of youth is a problem in this study for both internal and external validity. Selection inhibits my ability to conclude that the changes in offending found here result solely from the experience of incarceration and are not influenced by other factors. That is some omitted variables that a judge uses in his decision to send a youth to prison may be correlated with future offending independent of the sanction's effect on future offending and may bias results upward in favor of an amplification effect (Smith and Paternoster 1990). Although I was able to control for time-stable characteristics of the youth included in my samples to better combat the selection process by judges, I am limited by the available information on youth in the NLSY. It is still quite possible that selection by judges and the resulting omitted variables in this study may bias offending upward in favor of an amplification effect and therefore my short-term conclusions may be misleading.

External validity is threatened when the type of youth in a study are not typical of the youth in the general population. Attrition is almost always a problem in longitudinal survey data (Zagorsky and Gardecki 1998) and this is particularly problematic when the type of youth who drop out of a study on incarceration are the exact youth we want to know more about. My investigation suggests that the youth dropped from the control group were more likely to drop out of school, smoke, and sell drugs among other high risk factors. Also, although the youth dropped from the incarcerated group did not report higher criminal involvement, they were more likely to be black and possibly more likely to underreport deviant behavior. It is possible that the most criminogenic youth were excluded from this study and that had they been included, the short term amplification effects found here may even underestimate the true effects of incarceration on youth. It is

also possible that the excluded youth may have held less positive social ties to work and family and would not have exhibited a substantial decrease in drug offending in the last year of the survey.

When interpreting the results of this study, we must also recognize that offending may not be generalizeable to the larger population of incarcerated youth. My treatment sample was chosen based on first experiencing incarceration in the third and fourth waves of the survey. This limits my ability to make conclusions about youth who may have been incarcerated at a younger age (in waves 1 and 2 for instance when most youth were fourteen to fifteen years old). Also, since information was required on each youth for the first four waves of the survey (to determine incarceration experiences), youth who missed a survey during this time period are excluded. It is likely that these youth who were unable to be tracked down by surveyors are at higher risk of criminal behavior and are more likely to be in contact with the criminal justice system.

CONTRIBUTIONS TO THE LARGER EMPIRICAL DEBATE

Overall, the results from this study provide little support for amplification or deterrence over the long run, despite the fact that I used numerous measures of criminal activity and tested for incarceration effects on different types of offenses. Still, a lack of statistical power is likely to account for these null findings. Over the short term, however, there is evidence that incarceration may prolong criminal involvement in non-drug crimes or may even increase involvement in the selling of drugs by youth. Although there is evidence that incarceration may have a deterrent effect on the frequency of drug selling over the long run, further investigation suggests that this relationship may be spurious. I provide anecdotal evidence that social ties may play a large role in the sharp

decrease in rates of drug selling by the incarcerated group at wave 6. Finally, results from self-reports of arrest indicate that the harmful effects of incarceration on the criminal activity of youth may translate into additional contacts with the criminal justice system.

My findings concur with those of posttest-only studies that indicate high failure rates of inexperienced offenders when using official measures. Not only do I find that criminal involvement is prolonged or increased by incarceration, but I find that the youth in my sample—who are experiencing incarceration for the first time—are also at the highest risk for arrest. Although the evidence here cannot determine whether the youth in the incarcerated group are being closely monitored by police, it does indicate that the high recidivism rates of past studies (using official data) may indeed reflect the higher criminal involvement of youth leaving correctional facilities.

Besides the work of Phillips et. al (1983), my findings stand in direct contrast to the findings of the other two pretest-posttest studies (Empey and Lubeck 1971; Murray and Cox 1979). While they find some evidence of deterrence from incarceration, I find little evidence of deterrence over the long run when using multiple pretests and self-reports of behavior. When I use self-reports of arrest, the deterrence argument fairs even worse. It is possible however, that the differences lie in their use of rates of arrest as a dependent variable versus my prevalence of arrest variable. Still, my consistent results using numerous measures of criminality that incarceration does not deter future crime or arrest suggest that their findings may be more reflective of regression artifacts than deterrence effects.

My results also support previous research that indicates that incarceration impacts different types of offenders in different ways. Like Phillips and colleagues (1983) who found that a portion of youth exhibited increases in arrests after institutionalization while another portion showed decreases in arrests, I also find that some youth exhibit better outcomes than others. For example, the criminal involvement in drug selling increased while the involvement in non-drug crimes did not. A short-term amplification effect on the drug selling by youth compliments Spohn and Holleran's (2002) findings that drug offenders recidivated quicker and at a higher rate than non-drug offenders. Although I do not test whether the arrest patterns of drug offenders differ from non-drug offenders, this study provides some evidence that youth involved in drug crimes may need to be treated differently than those involved in non-drug crimes.

Furthermore, additional analysis into the drop in drug offending from wave 5 to wave 6 reveals that increases in social bonds, particularly employment, could be a main cause of this decline in offending as opposed to any deterrent effect from incarceration. This is consistent with research that finds increasing the hours of legal employment has a negative effect on criminal behavior for both adolescents and adults (see both Apel et al. 2005 and Blumstein et al. 1986) and with research that finds offenders who have higher stakes in conformity (job, family, education) are less likely to recidivate after incarceration (Dejong 1997).

While other research has found some evidence of amplification after incarceration (Sampson and Laub 1993; Bernburg and Krohn 2003), these effects disappear once social bonding variables are added to the model. Both studies suggest that incarceration effects future criminal activity indirectly through its effect on job stability or on educational

outcomes. My short-term amplification effects may disappear when controlling for changes in social bonds. However, both of these studies do not look at changes in the frequency or prevalence of offending in relation to a sanction but instead include measures of prior criminal activity as control variables. Also, one study's design is problematic to making a causal argument that incarceration effects future offending. For example, Bernburg and Krohn (2003) use a crime seriousness index during adolescence to predict crime seriousness and the frequency of drug selling in early adulthood (ages 19-22) and claim that juvenile justice interventions increase involvement in crime. However, their control measure of adolescent crime is taken from the same time period as the adolescent intervention! So it is not clear whether adolescent crime or the intervention occurred first. Two of the greatest strengths of the current study however, are its clear temporal precedence and its ability to look at the same measures of criminal behavior before and after incarceration. Although I do not investigate how social bonds may mediate the relationship between incarceration and crime (and it is quite possible that the amplification of drug crimes immediately after release could result from a lack of social bonds), the findings from this study suggest that at least for drug offenders, increases in legal employment play a large role in their decision to offend less.

POLICY IMPLICATIONS

If incarceration indeed has little effect on the criminal offending of youth then its only purpose is to temporarily separate offenders from society. The evidence presented here suggests that confining youth does not deter them from committing future crimes, particularly drug crimes. Other than serving the principle of retribution, locking up teenagers and young adults does not keep them from committing drug and property

crimes. In regards to policy, the increased treatment of youth as adults will not produce additional benefits. While, prior research on the effects of transferring youth to adult criminal court indicates that official recidivism is not reduced (Bishop et al. 1996; Winner et al. 1997), the findings here using self-reported data confirm this conclusion.

My findings that drug dealers are not affected by incarceration but perhaps may even be *more* likely to commit drug crimes, challenges the intentions of the drug war waged against society in the past two decades. Perhaps, for adolescents and young adults, the benefits of selling drugs after incarceration outweigh the risks of returning to prison. If accurate, the political push towards incarceration and away from less severe sanctions for drug offenders does not reduce crime. Furthermore, a short-term amplification of drug selling could have dire consequences for society. Although the group of drug sellers is small compared to non-drug offenders, impacts on society could be large given that this small group of offenders is responsible for higher frequencies of crime.

CONCLUSIONS

Most of the research that investigates whether incarceration increases or decreases criminal offending has been inadequate. There are numerous reasons for this: from the lack of available data to the inherent selection problem of studying a group of people who are sentenced by judges in the criminal justice system. Although some researchers took an important step when changing their research designs from posttest-only to pretest-posttest designs to address this question, problems with threats to validity halted their efforts. This study attempted to utilize data over a longer period of time in order to rule out some of these threats while capitalizing on a unique opportunity to use self-reports of offending that may better capture actual behavior. Although the selection problem cannot be entirely eliminated, I was able to control for time stable characteristics that may have been used by judges in their selection criteria. The question of how incarceration affects the frequency of offending and the prevalence of criminal activity by youth was addressed by using a "differences in differences" method that controls for time stable characteristics and by using a comparison group of youth who were arrested and were similar to the treatment group on most demographics.

This study found little evidence for deterrence but revealed that incarceration produced a short term amplification effect on the frequency and prevalence of drug offending and a delay in the decline of involvement of non-drug crime. Although the lack of statistical power may account for null findings over the long-term, if anything the findings reported here provide more support for the amplification argument than the deterrence argument. The findings of this study are limited by the lack of control variables (i.e. omitted variable bias) and the use of a sample of youth who may be non-

representative of those youth arrested or incarcerated in the actual U.S. population. It is necessary to combine the "differences in differences" method used here with pertinent control variables that change over time like drug abuse, family relationships, employment, and further contact with the criminal justice system. Time constraints also keep me from better defining the time each youth spends in confinement. Although more difficult, it is feasible and beneficial for future researchers to investigate the effects of incarceration while controlling for length of sentence and street exposure time

This study should serve as a starting point for other researchers to revisit the question of specific deterrence using data that is now available and methods that better combat threats to internal validity. Future researchers should continue to take advantage of self-report data collected by the NLSY especially once the 2003 survey information becomes available. Extending my analysis using the 2003 data (with additional controls) would provide more extensive information on whether the drastic drop in drug offending in 2002 holds over time. Furthermore, particular attention should be paid to drug offenders to see if a pattern of short-term amplification holds over numerous samples and sanctions. Future analysis should also take advantage of employment and educational variables available in the NLSY to further investigate the impact of incarceration on education and employment. Finally, this study only looks at one type of sanction imposed on youth. Further research should investigate whether changes in the frequency and prevalence of offending result from lesser sanctions like community service or probation.

APPENDIX

Characteristics of Youth in Study versus National Sample of NLSY Youth^^

	Samples*	NLSY**
Age in 1997^	14.08	13.96
Highest grade completed by parent^	13.02	13.75
% Male	0.67	0.49
% Black	0.29	0.16
% Hispanic	0.20	0.14
% Dropped out of school by 2002	0.26	0.11
% Living in urban area in 1997	0.80	0.71
% Youth living below poverty level in 1997	0.27	0.17
% Living with biological parent in 1997	0.39	0.55
% Ever smoked cigarettes prior to 1997	0.46	0.27
% Ever drank alcohol prior to 1997	0.45	0.27
% Ever smoked marijuana prior to 1997	0.26	0.11

^Two sample ttest of means was performed; otherwise two-sample test of equal proportions was used for a z-score.

^^All test statistics are significant at the .01 level.

*My sample includes those in either the treatment or control groups.

**NLSY sample includes the remaining 5963 youth from national sample that are not included in my samples.

Differences between Incarcerated Group and Youth Dropped for Missing Surveys.

	Incarcerated N=90	Dropped N=19	Z-score
% Black	0.29	0.53	2.00**
% Ever smoked cigarettes prior to 1997	0.51	0.21	-2.39**
% Involved in prior Petty Theft	0.56	0.26	-2.41**
% Living with Biological Parents	0.31	0.11	-1.85*

*Marginal Significance: Test statistic is significant at the .10 level.

**Significance: Test statistic is significant at the .05 level.

Differences between Incarcerated Group and Youth Dropped for Missing Crime Information.

	Incarcerated N=90	Dropped N=15	Z-score
% Black	0.29	0.60	2.37**
% Male	0.84	0.67	-1.66*
% Ever smoked cigarettes prior to 1997	0.51	0.27	-1.76*
% Drank Alcohol prior to 1997	0.44	0.13	-2.28**
% Dropped out of school by 2002	0.43	0.67	1.72*

*Marginal Significance: Test statistic is significant at the .10 level.

**Significance: Test statistic is significant at the .05 level.

Differences between Arrested Group and Youth Dropped for Missing Surveys.

	Arrested N=1002	Dropped N=114	Test Statistic
% Male	0.66	0.75	1.91*
Highest Grade Completed by Parent	13.08	12.60	-1.94*
% Living with Biological Parent in 1997	0.40	0.31	-1.90*

*Marginal Significance: Test statistic is significant at the .10 level.

Differences between Arrested Group and Youth Dropped for Missing Crime Information.

	Arrested N=1002	Dropped N=68	Z-score
% Dropped out of school by 2002	0.25	0.34	1.73*
% Involved in prior Property Offense	0.08	0.16	2.26**
% Involved in prior Petty Theft	0.51	0.64	1.92*
% Involved in prior Other Property Offense	0.05	0.12	2.50**
% Involved in prior Selling of Drugs	0.06	0.18	3.60**
% with Friends in a Gang	0.34	0.45	1.66*

*Marginal Significance: Test statistic is significant at the .10 level.

**Significance: Test statistic is significant at the .05 level.

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