ABSTRACT

Title: THE IMPACT OF SENTENCE LENGTH ON THE RECIDIVISM OF VIOLENT OFFENDERS: AN EXPLORATORY ANALYSIS OF PENNSYLVANIA DATA 1997-2001

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The current research seeks to answer the question whether tough sentences decrease the probability of recidivism. The specific deterrence literature posits that increased sentence severity decreases the probability of recidivism. The results of previous studies on the incarceration decision and recidivism were mixed with some studies claiming that sentence length has no impact on recidivism and some claiming that sentence length increases the probability of recidivism. Relatively few past studies have focused exclusively on the impact of time served in prison on the recidivism of serious violent offenders. I use judge assignment as an instrumental variable to correct for omitted variable bias. It is found to be an exogenous variable that is not related to recidivism, but is related to predicting time served in prison. Using two-stage least squares regression, I find that longer sentence length increases the probability of recidivism. Implications for specific deterrence and labeling theory are discussed.

THE IMPACT OF SENTENCE LENGTH ON THE RECIDIVISM OF VIOLENT OFFENDERS: AN EXPLORATORY ANALYSIS OF PENNSYLVANIA DATA 1997-2001

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The Impact of Sentencing Length on the Recidivism of Serious Violent Offenders:
An Exploratory Analysis of Pennsylvania Data 1997-2001

I. INTRODUCTION

In June 2004 former Attorney General John Ashcroft, while announcing a new Violent Crime Reduction Initiative, said:

> "The violent-crime rate has plunged to the lowest level in 30 years, but we view these impressive results as just the beginning. We are determined to drive down violent crime everywhere – especially in those places where habitual offenders are concentrated and communities live in fear of the violent and predatory …Our goal is to make an immediate impact in these communities by targeting repeat offenders with tough prosecutions, and tough sentences" (USDOJ Press Release June 2004)

Despite the fact that violent crime has experienced decreases in the last 30 years, politicians like former Attorney General Ashcroft continue to call for stiffer sentences to combat violent crime, especially crime committed by habitual offenders. In the past 30 years, while violent crime rates have experienced periods of decrease, incarceration rates have skyrocketed (Beckett and Sasson 2000). This unprecedented expansion of incarceration can be traced to policy changes enacted to do what Attorney General Ashcroft and other politicians are calling for, tougher sentences for violent offenders.

To politicians, recidivism is important because it presents a direct threat to public safety and is significant in the development, maintenance, and revision of correctional policy If the recidivism rate is low, a policy is viewed as deterring habitual offenders. If a correctional program or policy reflects a high rate of recidivism, or a rate equal to recidivism rates for alternative interventions, the program is considered ineffective in reducing recidivism (Smith & Akers 1993). The question remains, do stiffer sentences reduce the rate at which people become re-involved in crime?

Habitual violent offenders have been targeted by state sentencing commissions to receive relatively severe sentences (Kramer & Ulmer 2002). By and large the justifications given for these sentences are just deserts, incapacitation and most notably deterrence. To date there is no concrete evidence that stiffer sentences deter violent offenders. There have been several studies that address issues of the deterrent effect of sentence severity. These studies offer contradictory findings. Some found that the length of sentence has no impact on the rate of recidivism (see Baumer 1997 for a detailed list). Recently at least one study (Dejong 1997) has shown that sentence severity (as measured by sentence length) affects the timing of recidivism for habitual offenders.

Politicians have taken it upon themselves to increase the incarceration penalty paid by serious violent offenders. This direction is not supported by the research on sentence severity and recidivism. Most studies show that sentence severity has no effect on recidivism rates, and some even demonstrate that more severe sentences actually increase the probability of recidivism.

The current research is an attempt to answer the question whether tougher sentences have a specific deterrent effect and decrease the probability of recidivism. First, this study will review the state of general and specific deterrence literature. Second, I explore the current state of knowledge regarding recidivism and what factors increase the probability of recidivism. Also I will discuss the limitations of past research such as omitted variable bias. Third, I will focus on the recent studies that have attempted to control for the selection bias associated with offenders who are given severe sentences. I will also discuss alternative theories to explain the findings of prior studies. Fourth, I will introduce the use of instrumental variable estimation in order to correct for omitted variable bias. After discussing the proposed instrument, the research questions and the data used in this study will be described. Fifth, I will present the findings of this study and discuss the methodological and substantive conclusions. Finally I will discuss the policy implications of the findings.

II. LITERATURE REVIEW OF GENERAL DETERRENCE RESEARCH

From the time of Beccaria (1763), the idea of deterrence has been central to the formation and operation of criminal justice systems all over the world. The notion thatswi ft, certain and severe punishments are more effective in deterring people from committing crime is still popular today because it is intuitive and resonates with citizens. Though deterrence is logical, it is a theory that has driven much criminal justice policy without firm empirical evidence. It was not until the 1960's when Tittle (1969) and others proposed a concrete way to analyze deterrence that empirical research in this area really began.

Some criminologists attempted to distinguish between certainty, severity and celerity to determine which of these characteristics of punishment was the most important in terms of deterrence (Chiricos and Waldo 1970). According to Chiricos and Waldo (1970), Gibbs lead the way by operationalizing certainty and severity of punishment. Certainty was measured as the ratio of the number of offenders convicted of homicide to the number of homicides known to police. Severity was measured as the median number of months served by persons for homicide. Gibbs concluded in his analysis that "no evidence exists of a relationship between legal reactions to crime and the crime rate'" (Chiricos and Waldo 1970). Tittle (1969) also sought to investigate whether penal sanctions deter crime. Using similar operationalizations as Gibbs and using the seven Federal Bureau of Investigations (FBI) index offenses, he found that when the index offenses were considered together, certainty of punishment had a negative relationship on rates of deviance;

whereas severity of punishment had a positive relationship with rates of deviance for all index offenses except homicide. Tittle (1969) thus concluded that severity of punishment alone was irrelevant to the control of deviance as measured by crime rates.

Since these two early studies were conducted, the research designs, quality of data used, and theoretical questions asked of deterrence have all increased in sophistication. Nevertheless, the message regarding severity of punishment in general deterrence remained relatively constant(see Nagin 1978 for a comprehensive review of this literature). In a review of the research, Nagin (1978) concluded that recidivism is not affected by varying the severity of punishments. Paternoster (1987) has also drawn similar conclusions. After some early research, those employing aggregate-level data found no consistent inverse relationship between crime rates and objective levels of punishment severity. Thus, early objective deterrence studies consistently found little support for use of severe sentences as a means to reduce recidivism. Because of these less than optimistic findings, research on sentence severity lay dormant for some time. It was not until perceptual deterrence became in vogue that research on sentence severity began to reemerge.

The research on perceptual and general deterrence, while enlightening, does not address whether individuals convicted of a crime will be deterred based on the severity of sanctions they receive.

> " … it is suggested that future empirical research on the question of deterrence should not be based upon available macroscopic data. Rather, an attempt might be made to follow a number of individuals' cases through the criminal justice system to determine celerity, severity and certainty of punishment, and to assess the impact of that

> punishment upon behavior of the individuals and of his peers and significant others." (Chiricos & Waldo 1970, pg 215)

While the current research cannot address all the issues articulated by Chiricos and Waldo (1970), I do attempt to at least fill in the void regarding the severity of punishment and its impact on the future behavior of individuals.

III. LITERATURE ON SPECIFIC DETERRENCE, RECIDIVSM AND SENTENCE LENGTH

From the studies cited above, one might be led to conclude that severity of sentence is not important in deterring offenders on a macro level, but what about on an individual level? Simple specific deterrence theory proposes that individual offenders who have experienced a harsh sanction are more likely to desist from future offending compared to those who experience a less severe sanction (Dejong 1997)[1]. A substantial body of research has developed on recidivism and specific deterrence. Below is a brief description of some of that research as well as an analysis of the empirical evidence to date. I will also endeavor to raise some questions that still remain to be answered.

Recidivism

What do we know about recidivism? First, available evidence from the United States shows that about 35-45% of persons released from prison are reconvicted within six years of being released (Baumer 1997: 608). Second, measuring recidivism as specific deterrence is problematic. Research in the past has used various methods of operationalizing recidivism. The two most common methods are either analyzing subsequent arrests or using a new conviction as a measure of failure.

[1] Though, this is the most often tested version of specific deterrence theory (Dejong 1997, Spohn & Holleran 2002 etc) some scholars question whether specific deterrence actually posits that experiencing a sanction will reduce the likelihood of future offending by the one who experienced the sanction. Rather, they maintain that it is the threat of a severe sanction that plays the biggest role in the cost benefit analysis when a potential offender entertains the thought of committing an offense. That threat is what deters, not the actual experience of a punishment. This perspective would argue that severe sanctions would have a greater deterrent effect on others who now perceive the costs of engaging in crime as greater due to the sever sanction experienced by another person.

Both have disadvantages. Using rearrest as a measure of failure is a more liberal measure, but it may be inaccurate because offenders may be labeled by law enforcement and thus may become the "usual suspects" in crimes in their area. This bias may result in offenders, who are trying to desist, being constantly brought before the criminal justice system. Convictions, on the other hand, are a more conservative measure of recidivism. All the cases that presumably have no merit will not make it to the conviction stage of the criminal justice process. Conversely, cases where the defendant is guilty, but not prosecuted because of lack of evidence would also fall through the criminal justice cracks. In some states when an offender is already on parole and commits a new offense, rather than prosecute the new offense, the offender gets re-admitted to prison due to a parole violation. There are certainly errors with both measures of recidivism;previous research on the reliability of arrests versus convictions indicates that arrests may be preferable to convictions because use of arrest minimizes potential errors better than use of convictions (Woodredge 1988).

Third, the research on recidivism is difficult to synthesize because there have been a multitude of studies that measure many different aspects of the recidivism question. Recidivism studies can be divided into several categories: studies that deal with juvenile offenders (Wooldredge 1988: Auerhahn 1999; and Corrado et. al. 2003), studies that deal with different types of offenses (i.e. white collar, Weisburd et. al. 1995; or drug offenses Hepburn & Albonetti 1994; and Spohn & Holleran 2002), and studies that compare different kinds of interventions like electronic monitoring (Gainey et. al 2000), intermediate sanctions such as house arrest (Smith & Akers 1993), or combinations of parole, prison and treatment programs (Wooldredge 1988;

and Hepburn & Albonetti 1994). From these and other studies, there has developed a credible list of factors that can be called predictors of recidivism.

In a study assessing the impact of 12 juvenile court dispositions in eliminating recidivism for 2,083 offenders in four Illinois jurisdictions, Wooldredge (1988) found that serious offenders are more likely to recidivate regardless of the sentence imposed. Regarding sentence severity, his analysis showed that longer sentences coincide with higher recidivism rates.

> "These results do not overwhelmingly support the idea that longer terms of detention are generally harmful to specific deterrence, but they do suggest that longer terms of detention are not superior to shorter terms for reducing recidivism, and that in some cases it may be counterproductive. This recommends that if some term of detention is imposed, it should be limited to short terms (Wooldredge 1988: 283)."

He also found that county of residence holds the highest significance for explained variation in recidivism rates followed by gender, prior record, race, grade point average and learning disabilities.

Visher et. al. (1991) found similar results in their study of serious juvenile offenders. They analyzed data obtained from the California Youth Authority between July 1981 and June 1982 for 1,949 male offenders. From prior research, they choose to control for such variables as criminal history, current commitment, substance abuse, school problems, family background and environment. About 80% of the sample was rearrested during the 3 year follow up period. The average time to failure was 306 days. The variables that were significant in predicting recidivism were criminal history, current offense, youth's substance abuse, school problems, family background characteristics, county level property, violent crime, crime clearance rates, and county of commitment for each subject. Prior violent behavior and family problems (family violence and parents who are criminals) failed to influence time to rearrest.

With respect to the factors that influence recidivism, Wooldredge (1988) and Visher et. al (1991) provide factors that have been supported by much research since then. In his criminal career research, Greenberg (1991) shows that the major correlates of recidivism are race, age and gender. Minorities, the young, and males are all more likely to recidivate. Hepburn & Albonetti (1994) also find that gender, race, age and prior record significantly affect juvenile recidivism. In a summary of prior research, Baumer concludes that recidivism rates vary by offender and offense characteristics, including age and gender of the offender, the crime type, and offender's criminal record. Research in the U.S., England, Canada and Australia, has consistently shown that recidivism rates decrease with the age of the offender, are higher for males, and higher for those convicted of property offenses than among violent offenses. Also of importance is that the likelihood of recidivism increases with a longer criminal history. A smaller body of research finds that the risk of recidivism is slightly lower among those who are married and those who have higher levels of education (Baumer 1997).

The above list of factors appears to be consistent even in different cultures. In Baumer's (1997) study of recidivism in Malta from 1976-1994, 1,230 inmates released from Malta's only prison are followed for several years. Baumer (1997) demonstrates that even in a vastly different society, the predictors of recidivism are more or less the same. Malta is a highly integrated society with strong social structural institutions of family, church and polity. There is a strong sense of community identity, commitment to the conventional family, and adherence to religious morals and values. In this community context, Baumer finds that gender, age, number of previous convictions, offense type, and length of confinement are significantly related to the risk of re-imprisonment. Interestingly, Baumer (1997) discovers thatin Malta persons who serv ed longer sentences were less likely to be re-convicted. This last finding is contrary to many years of recidivism research conducted in the U.S. and other western societies.

The majority of studies in the U.S. and England say that length of confinement has little or no effect on the likelihood of recidivism while other studies report that persons who serve longer prisons terms are more likely to recidivate. "The ambiguity of prior research on the relationship between length of confinement and recidivism rates may be due to inconsistencies in controlling for various demographic and criminal history characteristics, or it may be that the relationship is nonlinear" (Baumer 1997, pg 609). Future research needs to be able to control for the various demographic and criminal history characteristics and the possibility of non-linear relationships in order to make a significant impact on the study of recidivism. The present study will attempt to make such a contribution. First I will explore what previous research has to say about the relationship between sentence length and recidivism.

Sentence Severity and Sentence Length

Most of the past research on sentence severity concerns itself with measuring the impact of incarceration versus probation and other sanctions on recidivism (Spohn & Holleran 2002; Weisburd et. al. 1995; Smith & Akers 1993) rather than measuring the impact of sentence length on recidivism. These studies reveal that imprisonment does not have a significant deterrent effect and, in fact, may even have a criminogenic effect and increase the probability of recidivism (Spohn & Holleran 2002).

One such study was conducted by Weisburd et. al. (1995). They studied a sample of white collar offenders because these offenders are generally believed to be more influenced by penal sanctions and are thought of as more rational than common law violators. They examine the impact of the predicted probability of imprisonment on 742 offenders convicted of white collar crimes in seven U.S. districts from 1976-1978. Half of the sample received a prison sentence (N=368), but nearly all of them served less than two years in prison. These offenders were followed for 10.5 years. The authors calculated differences in the likelihood of recidivism between a prison group and a no prison group. They controlled for various indicators that might affect recidivism such as prior record, type of conviction, district of conviction, demographic characteristics, amount of victimization, role in the offense, etc.

In general, it took this sample of white collar offenders a long time to recidivate, an average of over 3 years until they had a subsequent rap sheet entry. Weisburd and his colleagues determined that there was no significant difference in the recidivism rates of those who received prison sentences and those who did not. In fact, model estimates revealed that those in the prison sample had slightly higher failure rates than those in the no-prison sample though none of these were statistically significant.

While this might seem to be compelling evidence that prison does not have a specific deterrent impact upon the likelihood of rearrest, these results must be interpreted with caution. It has widely been acknowledged in the criminological literature that there is some inherent selection bias in many studies that attempt to predict deterrence or recidivism comparing probationers and incarcerated offenders. The probability of receiving a sentence of incarceration is not random (Smith & Paternoster 1990). Offenders who receive longer sentences for a specific crime differ from other offenders who receive a less severe sentence. They have other characteristics that are assumed to predispose them to recidivism, for example, longer criminal histories, the severity of the current offense, offender attitudes, amenability to treatment or stakes in conformity, etc (Weinrath & Gartrell 2001; Smith and Paternoster 1990). Weisburd et al.(1995) attempt to control for selection bias by matching offenders and using multivariate statistical models to control for extraneous variables. This is problematic because some of the variables that correlate with future offending when estimating the relationship between court sanctions and subsequent offending may have been left out of the analysis. Failure to take into account some of these factors may have weighty consequences for conclusions drawn concerning the association between sanctions and future offending (Smith & Paternoster 1990). When key variables are omitted from the regression equation, a biased and inconsistent estimator of the true slope parameter will result. Omitted variable bias is perhaps the most significant problem facing theorists who want to understand the impact of incarceration on future offending behavior.

Dejong (1997) also tested the propositions of specific deterrence theory. Data were obtained from 4,505 male offenders incarcerated in the Manhattan central booking facility of the New York City Jail between April and October of 1984.

Using multivariate analysis she examined the effect of a custodial sentence on time until re-arrest controlling for an individual's stake in conformity and type of offender, first time or experienced. Due to the nature of the New York court system, Dejong had to restrict her analysis to less serious offenders who were tried in criminal court because the more serious offenders are automatically transferred to the Superior Court system. Also Dejong has no precise measures of sentence length, instead she uses both the length of time sentenced to prison for the offense as well as one third the number of days sentenced to incarceration as a proxy for actual time served. She controls for demographic variables, offending history, current charge type, drug test result as well as some incident related variables (felony, property offense, violent offense etc.) stakes in conformity, and social bonds.

After a three year follow-up period, she concludes that it is unrealistic to expect incarceration to have a universal deterrent effect on alloffende rs. Instead offenders respond differently to incarceration and it would seem that incarceration stimulates restrictive and not absolute deterrence. Confinement seems to increase subsequent offending of naïve offenders, while experienced offenders are no more or less likely to recidivate following incarceration. Though, longer prison sentences do seem to lead to a longer time until re-offending for experienced offenders.

This study is important because it builds upon the work of Weisburd et. al. (1995) by articulating reasons why some offenders sentenced to prison are more likely to recidivate than others. Also she adds to the empirical evidence that incarceration in and of itself is not a universal deterrent. Perhaps her most significant contribution is the discovery that sentence length does have an effect on the timing of recidivism. But because Dejong groups offenders into naïve and experienced, the reader cannot make any inference as to what type of offenders are more likely to recidivate. According to her definition a naïve offender can be anyone from a small time thief to a high level drug distributor being arrested for his first offense. Most policies that are increasing incarceration time are targeted to serious violent offenders, a group which Dejong cannot address.

A study that attempts to address some of the limitations of the above studies is the study of the effect of imprisonment on recidivism rates of felony drug offenders by Spohn and Holleran (2002). The authors of this study collected data on 1,077 felony offenders in Jackson County, Missouri who were convicted in 1993. They compared those who were given probation to those who were sentenced to imprisonment for three types of offenders, drug offender, drug-involved offenders and non-drug offenders. The indicators of recidivism analyzed were a new file charged, a new conviction, or a return to jail or prison, and they also measured the time-to-failure in months. They controlled for someextraneous variables including the sentence type (probation or prison), the type of offender, gender, race, employment status, age, and number of prior felony convictions.

Through their analysis, Spohn and Holleran found that for each measure of recidivism prisoners had higher rates than probationers. They were significantly more likely to be charged with a new offense, convicted of a new offense and be sentenced to prison for a new offense. The probability of recidivism was not affected by employment status or predicted probability of incarceration. They state that "Contrary to deterrence theory, offenders who were sentenced to prison failed more quickly, in terms of being arrested and charged with a new offense, than offenders who were placed on probation. Recidivism also occurred more quickly for men, blacks, younger offenders, and offenders with more serious prior records," (pg. 345). Before such strong claims as these can be accepted on face value, it is prudent to examine the authors' analytic strategy.

Like Weisburd et. al. (1995), these authors recognize that offenders sentenced to prison are qualitatively different from those sentenced to probation. A judge's determination of the proper punishment reflects factors related to offender's seriousness and risk of recidivism. "Although our models control for offender characteristics that are linked to these assessments, they do not control for all of the factors that judges take into account in determining an offender's dangerousness and risk of recidivism," (Spohn & Holleran 2002, pg. 340). To attempt to control for these factors, they model the offender's predicted probability of imprisonment. Individuals predicted probability of incarceration was estimated controlling for seriousness of the instant offense, number of prior convictions, offender' prior criminal record, offenders' probation status at time of instant offense, whether the offense involved a weapon, the mode of conviction (guilty plea or trial), type of attorney, pretrial status, and demographic variables.

The authors' attempt at predicing the decision to incarcerate is a step in the right analytical direction. But the authors did not use Berk's (1983) procedures appropriately. Their attempt to control for selection bias fails to take into account that the instruments they are using to model the decision to incarcerate are not exogenous. Some of the variables are directly correlated with recidivism. As previous research has demonstrated, prior criminal record, offender's background, race, gender, age, marital status and drug use are all significantly related to recidivism (Spohn & Holleran 2002; Greenberg 1991; Baumer 1997; and Visher et. al. 1991). Also, because this study predicts the probability of incarceration rather than actual sentence length, it does not address the issue of whether increased sentence lengths themselves impact recidivism.

Spohn and Holleran (2002) declare that their results are contrary to specific deterrence theory, but this assertion is based on the assumption that specific deterrence theory posits an unambiguous negative relationship between sentence length and recidivism. In reality specific deterrence theory suggests that the effect of incarceration length can be positive or negative depending on how the sanction is experienced by the offender (Gainey et al 2000; Orsagh and Chen 1989). If the specific deterrence effect is positive then that effect will diminish as sentence length increases suggesting a curvilinear U-shaped effect (Orsagh and Chen 1988, Cook 1980).

Nonlinearity and Recidivism

How does sentence length work differently for different types of offenders? Why would there be an expected curvilinear u-shaped relationship between incarceration length and recidivism? Dejong's (1997) study reiterates previous research findings that suggest that the effect of sentence length on recidivism can vary for different types of offenders, i.e. naïve versus experienced offenders, young versus old, male versus female etc. This suggests that the association between time served and recidivism is more complicated than it appears at first glance.

Gottfredson et al. (1977) analyzed parole outcomes for a sample of men and women paroled in Ohio. They found that for a few specific classifications of offenders, time served was not related to parole outcome, but for other classifications there were complex patterns of association between sentence length and parole outcomes (Gottfredson et al. 1977). When they examine the total sample of parolees, the overall trend appeared to be with increasing time served, up to 50 months or more, the recidivism rate increased. After 50 months the recidivism rate decreased somewhat. After partitioning the sample into different subgroups based on Predictive Attribute Analysis, they find that there is no consistent pattern of increasing parole success associated with increased time served. Some subgroups had increased recidivism with increased time served, while another group experienced no difference in recidivism rates with increased time served. One group experienced decreased recidivism with increased sentence length and while another group experienced both increases and decreases of recidivism with increases in sentence length (Gottfredson et al. 1977). They then examined offenders who had been convicted of robbery and burglary (the two crimes which represented most of the incarcerated sample). Once again, the recidivism of these two groups of offenders was very different from one another. Robbers were more likely to recidivate with increased sentence length and the relationship was linear. For burglars, the relationship was nonlinear and those given shorter sentences were less likely to recidivate, especially young offenders given short sentences.

In addition to Gottfredson et al.'s (1977) research, Orsagh and Chen (1988) also add to the confusion regarding sentence length and recidivism. The theory they test posits that the specific deterrence effect of sentence length dominates other effects (i.e. social bonding, and employment) when the time served is relatively low. The additive effects of the lack of social bonds and disconnection from employment opportunities post release become more powerful than specific deterrent effects when time served is relatively long. They conclude that "different offender groups respond differently to different interventions, whether the intervention be a rehabilitation program, employment in prison industry – or time served," (1988, p. 166). When offenders are analyzed together in one large sample, these group differences may be obscured.

While the focus of the current study is not to determine subgroup differences in recidivism, it is crucial to remember that sentence length may not have the same impact on recidivism for different groups of offenders. Since I plan to examine the recidivism of a total sample of violent offenders, I recognize that different subgroups have different recidivism trajectories and that these multiple trajectories can manifest themselves in a curvilinear relationship between time served and recidivism when examining offenders in the aggregate. Most of the recent studies (Dejong 1997; Spohn & Holleran 2002) of sentence severity have failed to take non-linearity into account. The current study will test for non-linearity by including a squared function of time served to the model equations. This is especially important given the fact that based on data limitations; the current study is unable to classify offenders into meaningful subgroups. Based on the results from the reviewed studies, I expect that the relationship between sentence length and recidivism will be non-linear.

An examination of the handful of studies that have looked specifically at sentence length gives a glimpse of the puzzle that still remains regarding this issue. Is sentence severity unrelated to future offending as prior research in the U.S. has suggested? Or are these results simply reflecting the fact that more serious offenders, who receive longer prison sentences, are more likely to recidivate based on some underlying propensity to offend that is not related to the sanction? Also, research suggests that different types of offenders experience time served differently and thus their recidivism rates also differ as a result (Dejong 1997; Gottfredson et al. 1977). Many policy decisions are based on the version of specific deterrence that rests on the assumption that there is a negative relationship between sanction severity and future criminal activity (Orsagh & Chen 1988). But recall that the effect of specific deterrence may be positive or negative based on how the offender experiences the sanction. Specific deterrence is only one of several theoretical frameworks that attempt to explain the relationship between sentence severity and recidivism.

Alternative Theoretical Explanations

Prisonization

Different theoretical perspectives predict differing effects for the impact of time served on recidivism. Orsagh and Chen (1988) proposed that specific deterrence may operate for certain types of offenders who receive relatively short sentences. This is a perspective supported by other research (Dejong 1997, Gainey et al. 2000). As sentence length increases the effects may differ from the effects produced by shorter sentences. There is a criminological perspective that views institutions as "schools of crime". It follows that the more time an offender spends in this school of crime the more opportunity s/he has to learn to be a better criminal and consequently post-institutional criminal activity will increase. Donald Clemmer (1950) was one of the first sociologists to articulate this idea. He coined the term "prisonization" – which is a process similar to assimilation in which the codes, norms, myths and dogma in prisons are absorbed by inmates.

According to Clemmer (1950), prisons have their own culture and every inmate undergoes prisonization to some degree. The social world of a prison is described in the following way:

> Trickery and dishonesty overshadow sympathy and cooperation. Such cooperation as exists is largely symbiotic in nature. Social controls are only partially effective. It is a world of individuals whose daily relationships are impersonalized. It is a world of "I," "me," and "mine," rather than "ours," "theirs," and "his." Its people are thwarted, unhappy, yearning, resigned, bitter, hating, revengeful. Its people are improvident, inefficient, and socially illiterate. The prison world is a graceless world. There is filth, stink and drabness; there is monotony and stupor. There is disinterest in work. There is desire for love and hunger for sex. There is pain in punishment. Except for the few, there is bewilderment. No one knows the dogmas and codes notwithstanding, exactly what is important (Clemmer 1950, 314).

It is not simply the assimilation process of prisonization that is of importance, it is those influences that deepen anti-social beliefs. As a result of being in prison the inmate learns to reject society and accept a new images of themselves as a criminal.

In an empirical test of Clemmer's propositions, Wheeler finds that prisonization does occur. Inmates restore their self esteem by participating in a system that enables them to reject the people that have rejected them (Wheeler 1961).

He points out that the effects of prisonization are most apparent just prior to release. As an inmate prepares to leave the institution, s/he begins to shed their prison persona and look towards coping post-release. This may be where prisonization has its most deleterious effects. By shedding the rigid adherence to the prison code, inmates have to contend with the rejecting feelings that the prison code enabled them to discount. As they turn their attention to the outside, they have to make contacts with employers and relatives, while their status as offenders remains the same (Wheeler 1961). It is the reaction of society to offenders just released. When they are not accepted and find it difficult to find work, they may turn to their criminal friends for social support. Turning to those kinds of contacts may lead to re-involvement in offending behavior, even though that was not the intention of the offender when seeking out the support of others with the same offender status (Wheeler 1961).

Prisonization occurs for all incarcerated offenders. This presents the question, how can prisonization be used to explain differences in recidivism rates as a function of sentence length? As the research reviewed above suggests, perhaps prisonization has more deleterious effects for first time versus repeat offenders. If the first time offender is sentenced to a relatively short prison term then they have experienced the punishment and may be deterred from committing future criminal acts because they want to avoid future punishment. However if the first time offender is sentenced to a longer prison stay, longer than the optimal length to achieve specific deterrence, then s/he will undergo prisonization, develop a criminal self-concept, learn to do crime better than before this period of incarceration and be more likely to engage in future offending behavior in accordance with the new self concept. Repeat offenders have theoretically undergone the process of prisonization and so if they are incarcerated for a second time for a long period of time, there is no theoretical reason to expect that the length of their sentence will impact the probability of recidivism since they have already been prisonized from their first incarceration. So perhaps for repeat offenders there is no relationship between incarceration length and the probability of recidivism, which is what has been demonstrated in previous research (Dejong 1997).

Labeling Theory

Another theoretical perspective that deals with altered self concepts and predicts an increased probability of recidivism with increased time served is the symbolic interactionist perspective. Students of the labeling might argue that punishment – which is the public identification and treatment of an individual as a deviant – may also increase secondary deviance or recidivism (Harris 1975; Tittle 1975). These theorists posit "that the reaction of social control agents, through the application of a 'deviant' label, results in actor's being typified or 'cast' as a deviant," (Paternoster & Iovanni 1989, 375). Official intervention increases the probability of creating a commitment to deviance when the actors' commitment might have been low before the intervention. The casting process results in the actor adopting an image of themselves as a deviant person, an image that might not have been present until after the intervention. Once this transformation occurs, the labeled individual seeks out the company of those who have similar motivational systems that favor law violation. Associating with these similarly situated people leads to fewer legitimate opportunities and more illegal opportunities (Tittle 1975). Both of these factors may lead to increased future offending by those who have been officially labeled. Thus there are three consequences of being labeled a delinquent; 1) a change in one's personal identity, 2) segregation from conventional society, and 3) an increase in the probability of future law breaking behavior.

For my purposes, the consequence of labeling, that may provide insights as to why specific deterrence is ineffective, is that the altered self-conceptmay lead to acting out in deviant ways (Paternoster and Iovanni 1989). Thus, after violent offenders are convicted and have been labeled deviants, they may return home with an altered self-image as a criminal individual. Finding that their opportunities are blocked they may take this new master status and become more likely to engage in acts which are in line with the image of themselves as a criminal (Paternoster and Iovanni 1989, Tittle 1975).

Not everyone who is labeled deviant will accept that new identity. Scientists have identified several characteristics of the actor that will make a difference in who is more susceptible to secondary deviance as a result of labeling:

> "1) the degree of self-orientation, or the tenacity with which one holds one's personal identity …, 2) the degree of affective bonds; or emotional ties between actor and both normal and deviant others, and 3) the similarity between actor's and other's categories for describing an identity that is attributed to him," (Paternoster & Iovanni 1989).

For those offenders who serve lengthy prison sentences, all the above characteristics will be affected. In prison, it is difficult to have a personal identity, when everything that you do is done together with numerous others – from eating meals, taking exercise, showering and going to bed all together. With extended prison stays, inmates lose ties to the outside world and their ties with other inmates increase in number and strength. As these bonds increase, inmates begin to identify with one another and can attribute to themselves a criminal persona.

While theoretically, the concept of labeling is appealing, there has been little consensus in the empirical literature to unequivocally support such a perspective. Most of the research to date has assumed that any sanction or official act of negative classification amounts to labeling (Tittle 1975). This has lead to a plethora of labeling research done on various interventions such as arrest, conviction for various offenses, being placed on probation, imprisonment, and even hospitalization for mental illness (Tittle 1975). One issue that continues to be a point of contention in labeling research is whether the characteristics of sanctions themselves are important in determining if an offender is deterred from criminal activity or takes part in further rule violation. Tittle suggests that sanctions that are imposed by an in-group member or significant other are more likely to deter. Whereas sanctions that are public, certain, severe and made by a control agent may be crucial in the production of secondary deviance.

There have been few studies to examine the labeling process specifically in the context sentence severity. One early study indicates that there may be a link between increased future offending and sentence length. Harris (1975) surveyed 234 black and white inmates in a New Jersey correctional facility. He found with increased imprisonment, both races reported they would be more likely to engage in future criminal involvement upon their release.

Also, researchers have noted that the labeling perspective may also suffer from the selection bias problems that plague recidivism studies. It is reasonable to attribute future offending to some unobserved propensity to offend that existed in an offender prior to the casting process began. In a quasi-experimental study of British youth, Farrington (1977) concludes that public labeling as measured by criminal convictions increases future criminal behavior, which was measured by self-reported delinquency scores. Later scholarship calls into question Farrington's early conclusions. Is it possible that differences in later offending between those who are formally processed and those who are not simply reflects a preexisting propensity towards criminal behavior? In that case, what researchers have called deviance amplification would in fact be the result of a selection artifact. When employing three different statistical techniques to control for selection bias, Smith and Paternoster (1990) find that the evidence for a deviance amplification theory is not compelling.

Though the empirical evidence is inconclusive and has many shortcomings, it is not yet time to close the book on this theoretical perspective. One area of labeling that deserves more empirical theorizing and empirical attention is the link between labeling and sentence severity. From previous research (e.g. Harris 1975) we know that youthful inmates who are serving long incarceration terms are more likely to report a higher relative expected value for committing crimes in the future than inmates who are sentenced for shorter periods of time. They also found this relationship to be curvilinear in the expected direction. Shorter sentences produce a labeling effect that decreases somewhat, then reaches a trough and begins to rise again dramatically. The longer sentences produce a significant labeling effect, "an apparent strengthening of the motivational basis of 'secondary deviance," (Harris 1975; 82).

This suggests that labeling effects are dynamic and change depending on the severity of the punishment associated with the label of deviance. Rather than conceptualizing labeling as on and off switch, perhaps it would be more informative to consider labeling as a continuum. Offenders may adhere to the label less tenaciously when they have spent less time incarcerated with criminal others, and continue to have strong ties to significant others on the outside (Orsagh and Chen 1988). Offenders who experience longer periods of incarceration may be more wedded to the criminal label, less likely to have maintained bonds and support structures on the outside and therefore more likely to engage in future criminal behavior. While suggestive, the Harris study was of a youthful sample and examined perceived probability of future criminal behavior rather than concrete post-release behavior and thus cannot be generalized to a sample of violent adult offenders. The results are nonetheless suggestive of a possible process where labeling theory can account for the increase in recidivism after long periods of incarceration in a curvilinear fashion.

From the preceding summary of the recidivism, sentence length and sentence severity and alternative theoretical explanations, it is apparent that prior research has several limitations. First, investigators operationalize sentence severity as an incarceration sentence versus a probation sentence rather than examining actual sentence length. Second, because of poor record keeping and because offenders were sometimes released prior to the completion of their full sentence, it was difficult to get accurate data on the exact length of time an offender was incarcerated. These and other types of omitted variable problems severely impact scientists' ability to accurately model the process of recidivism. Third, there have not been many good quality studies that focus on the recidivism of adult serious violent offenders, one of the groups most often targeted for harsh sentences. Finally, the most important limitation is that previous research has failed to take into account the problem of selection bias. Those that receive harsher punishments are also those who are more likely to recidivate.

The present study is an attempt to unravel some of these issues. I take the advice of Baumer (1997), Nagin (1998) and Smith and Paternoster (1990) who urge researchers to take into account the selection bias in recidivism research and to address previous model specification errors. There continues to be conflicting evidence about the relationship between sentence length and recidivism. I attempt to fill this gap in the literature by analyzing a group of violent offenders released from several jurisdictions in Pennsylvania between 1997 and 2001. I also propose the use of instrumental variable estimation as an analytic technique that attempts to control for the possible omitted variable bias and bias related to selection in previous studies.

IV. RESEARCH QUESTIONS

In considering the research questions that I would like to address, I must first acknowledge the limitations of this current research. The data was provided to me by the Pennsylvania Commission on Crime and Delinquency (PCCD) and was from the Bureau of Prisons detailing primarily recidivism information. Because the data was from the Bureau of Prisons and not the Pennsylvania Commission on Sentencing (PCS), it contains a substantial amount of information regarding the actual sentence imposed, the jurisdiction, and a description of the current offense and most importantly the identifier of the sentencing judge. The data does not contain any information on the offender's criminal history or offense seriousness score. There are also a few of the key offender characteristics that are missing from the data such as offender's age and socio-economic status. I can address the missing offense seriousness scores by using other variables that I do have as proxies. These will be discussed more fully below. The literature does indicate that one of the major determinants of an offender's sentence is his or her criminal history. This is a serious limitation of the present study, which I am unable to address at the current time. For this reason, this study is simply an exploratory analysis. I cannot make any causal claims because of the limitations of the data, however, if I can show an effect, even a small one, by including the judge variables and examine if my predicted sentence length has an impact on recidivism with this limited data, then future research would be warranted with a more complete data set to determine of the effect continues to contribute to recidivism.

In the current research I hope to control for omitted variable bias in predicting recidivism using judge identifiers as an instrumental variable to predict sentence length. I hope to address the following research questions:

1) *Does the use of individual judge identifiers add to the ability to predict sentence length?*

2) *Is the relationship between sentence severity and recidivism non-linear?*

3) *Are offenders who are given longer sentences less likely to recidivate, more likely to recidivate or equally likely to recidivate than those given shorter sentences for violent offenses?*

Before I can use the judge identifier variables in a prediction equation it would be beneficial to ascertain what, if anything, the use of these variables adds to the ability to explain sentence length. This point will be further explicated in subsequent sections of this manuscript. The second question is the essential research question of this study. How does sentence length impact the recidivism of violent adult offenders?

V. DATA AND VARIABLES

The current research is an attempt to determine whether or not violent offenders who have received longer prison sentences are more or less likely to recidivate. I focus on violent offenders because in recent years, many policy decisions have been targeted towards them (Spohn 2002). Sentencing guidelines in most states have strived to increase penalties for violent offenders, and eliminate the possibility of parole and early release for good behavior. I analyze a group of violent offenders who received a custodial sentence so that I can focus explicitly on sentence length as an independent variable in predicting recidivism.

Data from Pennsylvania is useful for several reasons. First Pennsylvania has a unique sentencing framework. After reforms enacted in 1982, the state adopted sentencing guidelines whose main purpose was to reduce sentencing disparity and to reduce perceived leniency in the judicial system. The guidelines were used to establish a presumptive range of sentences that judges could impose. The guidelines underwent comprehensive revision in 1994, after smaller changes in 1989 and 1991, to reflect increased problems with prison overcrowding (Sabol et. al. 2002). There were also revisions made in 1997 and more recently in 2005. Because of the overcrowding issues, a policy shift occurred and the 1994 guidelines had the specific objective to increase the use of prison for violent criminals and reduce the use of prison for property offenders and drug offenders (Sabol et. al. 2002). Judges in Pennsylvania can depart from guideline recommendations simply by providing a reason for doing so. Judges have a great amount of discretion and leeway in determining appropriate sentences. These reasons make Pennsylvania an ideal location to examine the deterrent effect of sentence length on recidivism.

The data was obtained from the Pennsylvania Commission on Crime and Delinquency (PCCD) and originally contained information from 206,233 records for offenders sentenced from 1940 through January of 2004 in the state of Pennsylvania. The unit of analysis is the individual offender. Working with this data set was problematic for 2 major reasons. First, the data was provided in a series of access files. The first type of file was a large table of information on the offense, sentence, judge assignment, sentencing dates etc of over 206,233. There were additional files that were broken into 4 separate files for each month of each year from 1996-2001. The first file was a list of all offenders who had been released that particular month. The second file contained a list of offenders who had been released that month and then returned to the Bureau of Prisons custody within 6 months to 1 year after their initial release[2]. The third file contained a list of offenders who had been released that particular month and been returned to custody after 3 years. The final file contained information on offenders released that month who returned to custody after 6 years. Unfortunately the 6 year recidivism files were incomplete given that the offenders released in 2001 would not have had the full 6 years in which to recidivate. Thus the 3 year fixed follow-up period was chosen for purposes of this analysis. In addition to the name and identification number of the inmates who were returned to custody, the 3 and 6 yr returnee files contained demographic information, dates of parole and dates

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[2] Bureau of Prisons record keeping changed between 1996 and 1997. In 1996 the returnee file contained offenders who had recidivated after 6 months. After 1997 and until 2001 the first returnee file contains offenders who recidivate after 1 year.

returned to bureau of prison custody and the stated reason for return, new arrest / conviction or parole violation and basic demographic information.

After choosing the follow up period, I combined the 3 year recidivism files for each month with the records of those men who had been released during that month. This process was lengthy because it had to be repeated for each month of each year. After creating one file containing all the information on inmates released between 1996 and 2001 and their recidivism I then needed match those records with the table that contained all the other relevant sentencing data. I aggregated over year and created one large data file that finally contained demographic information on the defendant, the dates sentence started, dates released and paroled and if parole was violated, the length of time incarcerated, demographics and the judge identifier.

The Second difficulty with the data was the way the judge identifierwas coded. Rather than assigning judges with some unique number or series of numbers and letters the data came with the actually judges name as the judge identifier. This was problematic because there was no standard way to enter the judge's name. Judge John Smith could have been entered as:

- John A. Smith
- Smith, John A.
- Smith, J.
- J Smith
- Smith
- Smith, A. John

Because the task of matching the judge names was going to have to be done by hand, I wanted to reduce the sample size to make this task as easy as possible.

First I limited the areas of Pennsylvania that I would examine and choose to include only cases from the 33 Primary Metropolitan Statistical Areas (PMSAs or MSAs) in Pennsylvania as defined by the Census (N= 178,912)[3]. Also Pennsylvania instituted sentencing guidelines in 1982. I choose to analyze cases that were sentenced after 1985, which was 2 years after the guidelines went into effect. This 2 year buffer was chosen to ensure that the kinks had been worked out and that full implementation of the guidelines was taking place across the state. Also when examining the data it became apparent that the data from 1996 was different from the rest of the data in that it did not contain the demographic variables that the remaining years of data included. For this reason, inmates released in 1996 were excluded from the analysis. With the exclusion of 1996 data the remaining sample is composed of individuals sentenced after 1985 and released sometime between 1997 and 2001 (N= 31,966).

The remaining 31,966 cases had several thousand unique judge identifiers. I needed help to clean this data further and since this was beyond my expertise, I contacted Dr. Cynthia Lum who has had experience with this kind of data problem from the mapping research she does on hotspots. Using FoxPro and dropping punctuation, she helped me to cut down the list of over 5,000 variations in judge identifiers to just over 1,500. Then, by hand I examined the list and grouped those names that were most likely the same together using pretty conservative decision

[3] The general concept of a metropolitan statistical area is that of a core area containing a substantial population nucleus, together with adjacent communities having a high degree of social and economic integration with that core. MSAs can comprise one or more entire counties. In Pennsylvania, all the MSAs are individual counties.

rules[4]. After several iterations, it was determined that there were 308 unique judges. Dr. Lum was able to take the 308 judge names that had been identified and match them directly to all the different variations of judge names in the data.

The original data set included data from persons convicted of a variety of offenses including, fraud, drug possession, and forgery. Since the focus of this study is violent offenders, all cases not involving a violent offense were excluded from the analysis (excluded cases 18,431) (N = 13,535) [5]. Of the remaining cases 171 were escapees, 473 died either of natural causes, suicide or homicide, 626 (313 individuals) were duplicate files for offenders serving multiple sentences. The duplicate files, escapees, and dead individuals were all excluded from the present analysis (N=12,265).

Also to ensure that statistical analysis would be meaningful and have sufficient variation, the decision was made to analyze only the cases from judges who sentenced at least 20 offenders in the remaining sample (excluded cases N=3,449). One hundred thirty nine cases had missing information on the judge identifier variable and were also excluded. Also, one case containing an extremely large minimum sentence (70 years) was excluded because it is an extreme outlier (remaining N=8,676). The largest minimum sentence for the remaining data was 35

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[4] The rules were as follows:

1) All names that were clearly the same, first and last names identical but in different orders or including obvious spelling errors were grouped together.
2) All unique and distinctive last names that were the same were grouped together and counted as one judge if the first initial matched what was known about the judge compared to the results of the first grouping using rule #1.
3) For more common last names such as Smith and Brown, if only the first initial was present and there were multiple variations such as a Charles Smith and a Calvin Smith and several entries of a C. Smith, then judge name was left as C. Smith and all C. Smiths were grouped together only if they were from the same county.

[5] Violent offenses include: Murder, Manslaughter, Involuntary Manslaughter, Aggravated Assault, Indecent Assault, Rape, Robbery and Burglary.

years. As a result of this constraint on the judge assignment variable, several counties were excluded either because the county itself had less than 20 cases remaining in the sample or because all the cases in that particular county were sentenced by the same judge (excluded cases N = 429). These counties were also excluded from the present analysis[6]. In addition, there were 52 cases of individuals who had a negative value for the time served variable. These individuals had a sentence start date after the recorded sentence start date. There was no consistent and systematic reasoning for this finding and because it is impossible to determine exactly how much time these inmates spent in prison these cases were also excluded (Final sample size = 8,195). The final analysis includes data from 18 counties, See Table 2 for a complete list of counties included in the analysis.

Variables in the Analysis

Control Variables

Many variables have been identified as having an impact on sentence length. This analysis will attempt to control for those variables within the limitations of the data.

<u>Characteristics of the offense</u>: The data set includes statutory offense codes as well as a verbal description of what that code entails. Thus robberies that result in serious bodily injury are different from robberies with no injuries. (See Table 1 for a list of the offenses included and their descriptions.) These offense descriptions are included in the analysis as a series of dummy variables. As mentioned above, the

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[6] The following counties were excluded: Blair, Butler, Cambria, Carbon, Columbia, Cumberland, Lackawanna, Lebanon, Luzerne, Mercer, Northampton, Perry, Pike, Somerset, Wyoming)

data do not include offense seriousness scores from the Pennsylvania guidelines. Fortunately, in addition to the actual number of months each offender served (Year Served variable), I also have the minimum sentence that each offender could have been sentenced to which I will use as a proxy for seriousness. This is admittedly not the best measure of offense seriousness, but statutory minimum sentence is a crude indicator of the seriousness of the offense. This minimum sentence is measured in years.

Previous research has shown that the area a person lives in may have an impact on recidivism, in addition to limiting my analysis to PMSA counties; I also include the county as a control variable. These are included as a series of dummy variables for each of the 28 counties remaining in the sample.

The PCCD data includes identifying information on the specific judge who sentenced the offender and I include judge identifier as an independent variable in predicting the time served. The data contained the judge's name or some variation thereof. After the matching process, each judge name was replaced by a generic number and a series of dummy variables was created. Of the 308 judges identified, 157 remained after cutting judges who sentenced less than 20 cases and those judges in counties where they sentenced all of the cases.

As mentioned above the Pennsylvania sentencing guidelines underwent a substantial revision in 1991, 1994, and 1997. Some of these revisions reflected a significant policy shift with regards to sentencing violent offenders. To take into account the differences in sentence length that may be a result of these revisions in the guidelines, I include dummy variable s indicating whether or not the case was sentenced under the 1991, 1994 or 1997 guidelines.

Characteristics of the Offender: The above cited recidivism literature shows that certain characteristics of offenders are related to future recidivism, so to the extent that I am able, I want to control for these variables. The PCCD data includes information on the gender, race (defined as Black, White, Hispanic, Asian, Native American or Other), and marital status (Married, Single, Widowed, Divorced or Unknown).

This data is limited in that there was no information on offender's age, education, substance abuse, SES, employment, or family background. There was also no indication of the mode of conviction or other important courtroom process variables such as caseload. These are variables that have been shown in previous research to have an impact on sentence length. Because this data set does not have these variables, they could not be controlled for. Despite the fact that these variables are missing, they're impact will nevertheless affect the results of this study.

Dependent Variable

The dependent variable is recidivism. Research in the past has used various methods of defining failure in recidivism studies. The most common methods are rearrest or reconviction. Some researchers have been even more conservative and recorded failure as any new conviction in the follow up period. All three methods have advantages and disadvantages as discussed above. Using arrest as a measure of failure is a more liberal measure, but it may be inaccurate because offenders may be labeled as such by police and as Tittle (1975) points out, ex-convicts are more likely to re-arrested independent of their actual criminal behavior. Using parole violations as an estimate may also be inaccurate because some offenders slip through the cracks, their violations never discovered or reported. Thus far there is no perfect method for measuring failure. The PCCD data includes the actual date on which the offender was returned to prison, whether it was for a parole violation, or a new conviction. Thus my measure of recidivism is re-commitment to the bureau of prisons custody and I will use dummy variables to control for a parole violation or a new conviction.

The use of re-commitment to the bureau of prisons custody has advantages and disadvantages. Specifically if an offender was on parole at the time of a new arrest for another offense, they may be re-admitted to prison for the parole violation without the costly process of going through a trial again Also offenders may be re - admitted to prison on a parole violation that is not a criminal offense (Tittle 1975). For instance, one of the conditions for parole may be that an offender stay away from alcohol that s/he is an alcoholic or if s/he must stay away from school playgrounds if s/he is a sex offender. An offender may also have parole revoked for failing to report to a parole officer or for leaving the jurisdiction without prior permission from the parole officer. A violation of this nature is not necessarily a criminal offense, yet it may land a person back in prison if caught by a parole officer. The merit of this measure of recidivism is that it is like using a fishing net, it will detect those "big fish" who are more likely to be truly re-involved in crime as well as some of those "small fish" who are re-involved in some form of delinquent behavior. Also, because I include those that have been re-admitted to bureau of prisons custody because of a parole violation, I will also include offenders who were not formally processed for a new crime and simply returned to prison to serve out the remainder of their sentence.

The data set does include information on the reason for re-commitment, whether it was for a parole violation or for a new conviction. However because of the methods used to arrive at the present sample, there were only 27 individuals who were re-admitted to custody for a new conviction. Due to the small number of re-commitments for a new conviction, they are combined with individuals who were re-committed to for a parole violation for the purposes of this analysis.

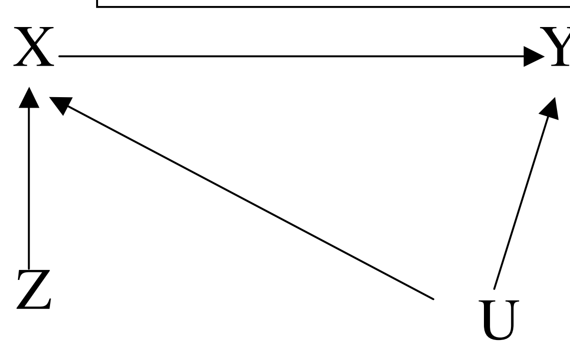
Proposed Analytic Method

I propose to follow the technique articulated by Angrist and Krueger (2001) as well as Smith and Paternoster (1990) and conduct instrumental variables estimation (IVE). This is an appropriate method for this data for several reasons. First, IVE is used for exploring the relationship between an outcome variable and a predictor variable in cases where the predictor variable is endogenous. Endogeneity in the predictor variable occurs when the predictor variable is correlated with the residuals in the regression model that describes its relationship with the outcome of interest. In the present study, length ofsentence is the predictor variable and recidivism is the outcome. There are many factors that go into the determination of length of sentence. Some of these factors I attempt to control for and the others appear in the error term.

Second, in focusing on recidivism I know that there are many factors that play a role in whether or not an individual will recidivate. I want to know specifically what kind of an impact does the actual length of sentence have on recidivism. Unfortunately, some of the factors that go into predicting time served may also be correlated with the error term in the equation. There is no guarantee that the independent variable (time served) and the error term of the equation will be uncorrelated and therefore no guarantee that an ordinary least squares estimate of predicted sentence length will be an unbiased and consistent estimator of sentence length. Also, there is no guarantee that I have included all the relevant variables that predict recidivism. Social scientists have developed instrumental variable estimation as a technique that can solve the problem of omitted variable bias.

IVE uses an additional variable, the instrument, to separate any exogenous variation in the endogenous predictor variable. There are 3 assumptions that guarantee the success of the IVE technique: 1) the instrument must predict the predictor variable; 2) the instrument must be uncorrelated with the residuals in the second stage model; and 3) the instrument must act on the outcome only through the question predictor and not directly by itself, See Figure 1 (Pindyck and Rubineld 1998).

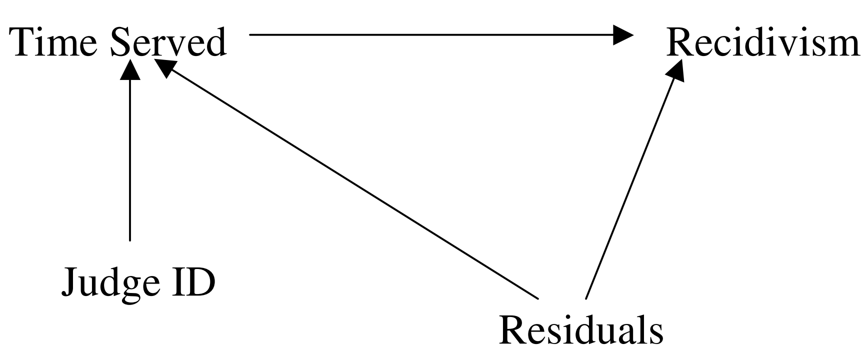
Figure 1. Sample Instrumental Variable Equation



The IVE method involves finding a new variable, Z, which is correlated with the X and at the same time uncorrelated with the error term in the equation, U.

Following the recommendations of Angrist and Krueger (2001) as well as Smith and Paternoster (1990), when utilizing the instrumental variable approach, the optimal method is to calculate the predicted length of sentence for this group of offenders. Then regress the predicted sentence length and all other control variables on the predicted recidivism rate. When predicting sentence lengths, I need to include as an instrument a variable that is not associated with the residuals in the regression equation, but is associated with sentence length. I was fortunate enough to find a data set that includes such an instrumental variable. Data from the Pennsylvania Bureau of Prisons included recidivism data as well as judge identifiers that can be used as an instrument. As will be demonstrated below, judge assignment has some impact on the determination of sentence length. At this time, there is no evidence that this assignment has any impact directly on recidivism or would be related to the error term in a regression equation.

Figure 2. Present Studies IVE Equation



This method is usually done using the two-stage least squares approach. In the first stage, the predictor variable, sentence length, is regressed on the instrument and the predicted sentence length is calculated for each individual in the dataset. I will calculate the predicted sentence length according to the equation below:

Equation 1. $\boldsymbol{\textit{predicted sentence length}} = \beta_0 + \beta_1\ \textit{counties} + \beta_2\ \textit{marital status} + \beta_3\ \textit{race} + \beta_4\ \textit{offense} + \beta_5\ \textit{min sentence} + \beta_6\ \textit{judge} + \beta_7\ \textit{1991 guidelines} + \beta_8\ \textit{1994 guidelines} + \beta_9\ \textit{1997 guidelines} + \beta_8\ \textit{gender}$

If the instrument is truly exogenous, then the predicted sentence length will only contain part of the variation in the original sentence length variable that can be attributed to judge assignment. This predicted sentence length can be used in place of the true sentence length in the second stage model to examine the relationship between recidivism and time served. Thus, with the predicted sentence length obtained from Equation 1, I can then attempt to predict the probability of recidivism using Equation 2:

Equation 2. $\textit{recidivism} = \beta_0 + \beta_1\ \textit{predicted sentence length} + \beta_2\ \textit{Counties} + \beta_3\ \textit{marital status} + \beta_4\ \textit{Race} + \beta_5\ \textit{offense} + \beta_6\ \textit{min sentence} + \beta_7\ \textit{predicted sentence length}^2 + \beta_8\ \textit{1991 guidelines} + \beta_9\ \textit{1994 guidelines} + \beta_{10}\ \textit{1997 guidelines} + \beta_{11}\ \textit{gender}$

VI. INSTRUMENTS IN PREDICTING SENTENCE LENGTH

What are the factors that make judge assignment a good instrument for predicting recidivism? Of significance for this research is that judges in Pennsylvania have an extraordinary ability to determine sentences even after the imposition of the guidelines, especially for violent offenders. As I shall illustrate below, despite the use of sentencing guidelines in Pennsylvania, individual judges matter in determining sentences. However, the assignment to a specific judge has no direct relation to the probability of recidivism that this researcher could uncover, making this one of the better variables to use as an instrument.

Why does assignment to a judge matter in the sentence length decision? There is some research to suggest that judge assignment does not matter. This body of research on judge effects generally examines the traits of court actors (Frazier & Bock 1982; Myers 1988; Spohn 1990; Steffensmeier & Hebert 1999). The bulk of this research finds no direct effect of judge social background characteristics on sentencing patterns (Frazier & Block 1982, Myers 1988). More recent research indicates that female judges may sentence repeat black offenders more harshly (Steffensmeier & Hebert 1999) and that Black judges sentence black defendants more harshly than white defendants in some instances (Spohn 1990; Steffensmeier & Britt 2001). The empirical evidence for judge effects in sentencing does not appear convincing at first glance. However most of the early research in this area examined judge characteristics independently and did not account for possible interactions between judge characteristics and courtroom contexts. There remain convincing theoretical reasons to expect judicial background characteristics to matter in judges sentencing decisions.

In perhaps one of the most thorough studies of the process of sentencing, Hogarth (1971) examines the complexity of the sentencing process when the legal and factual make up of the case, the social constraints imposed by membership in the courtroom workgroup, and psychological and sociological factors are given theoretical consideration. Hogarth compares a phenomenological model of sentencing to a input-output model. He finds that over 50% of the variation in sentencing can be accounted for by knowing certain pieces of information about the judge himself and incorporating that into the analyses. Some of these pieces of information are the usual suspects in judge effects modeling: jurisdiction of magistrates; background characteristics such as community of residence, class, age, religious affiliation, length of judicial experience etc; penal philosophies; attitudes concerning crime, sentencing and justice; magistrate caseload; and degree of legal training.

Hogarth takes the modeling of judge effects one step farther through in-depth interviews with over 70 judges and observational study of courtroom processes. He finds that there are other factors that influence the process of sentencing, for example the process through which magistrates collect, communicate and assess information regarding the offense and the defendant, or the complexity of the magistrates' thought process in sentencing. Hogarth (1971) concludes that sentencing is a dynamic process in which the facts of the cases, the constraints arising out of the law and the social system, and other features of the external world are interpreted, assimilated, and made sense of in ways compatible with the attitudes of the magistrates in the study.

In Pennsylvania, the sentencing structure is described as loose precisely because it is less restrictive on judicial discretion and provides greater latitude for judges to consider legal and extralegal factors (Steffensmeier & Demuth 2001). In fact, Pennsylvania's guidelines establish standard, aggravated and mitigated ranges from which judges choose a minimum sentence. They are allowed to depart from the guideline ranges simply by stating their reasons for doing so. Thus, judges in Pennsylvania have more discretion than similarly situated judges in other states.

Pennsylvania's guidelines mandate that severity of the convicted offense and the prior record are to be the major determinants of sentences. There is a growing body of literature that says that judges themselves have an effect in determining how long of a sentence an offender will receive above and beyond factors pertaining to seriousness of the offense and offenders criminal history (Johnson 2003; Hogarth 1971; and Bushway & Piehl 2001). Former Justice Marshall remarked that:

> "[E]xcessive discretion fosters inequality in the distribution of entitlements and harms, inequality which is especially troublesome when these benefits and burdens are great; and discretions can mask the use by officials of illegitimate criteria in allocating important goods and rights. (Tonry 1987, 371)

Judges are important members of the courtroom workgroup. They make bail decisions and sentencing decisions that have long term impacts on the lives of defendants. Their decisions are rarely only based on proved offenses, but also on their ideas about possible future offenses of the individual involved. Because judges are forced to make decisions with incomplete knowledge, they must rely on their own past experience, stereotypes, and prejudices and thus they develop a set of "patterned responses" that help them reduce uncertainty in the sentencing process (Albonetti 1991).

In her recent book, Spohn (2002) discusses the fact that when making sentencing decisions, judges rely on both legally relevant factors (offender criminal record and offense seriousness) as well as legally irrelevant factors. More and more research has demonstrated that judges are taking into account legally irrelevant factors in their decision making such as race of defendant, age of the defendant, gender of the defendant, social class of the defendant, and whether the defendant was represented by a public defender or private attorney (Ulmer & Kramer 1996; Spohn 2002; and Steffensmeier & Demuth 2001).

These findings must also be interpreted with caution because, as the literature has shown, legally irrelevant factors are correlated with legally relevant factors, for instance young African American males are more likely to have a longer criminal record and so the race effect in a sentence may be operating through this interaction (Bushway & Piehl 2001). Another reason for caution in interpreting these results is that there are many hurdles in the prosecution of an offender before s/he even reaches the sentencing stage, so observed disparities may reflect disparity in other stages of the system that are not solely attributable to the actor who actually imposes sentences (Spohn 2002; and Bushway and Piehl 2001).

To address some of these cautions and find out exactly what part of the variation in sentencing is due to judicial discretion, Bushway and Piehl (2001), using a sample of offenders sentenced in Maryland, test the hypothesis that sentencing departures are being used to disproportionately sentence African Americans to longer sentences. By modeling the Maryland sentencing grid and controlling for age, gender, guidelines recommended sentence, and the manner of conviction (trial or guilty plea), they find that the variation associated with the difference between judges' sentencing decisions of African Americans and whites is about 20 percent. Bushway and Piehl interpret their findings to suggest that judges in states with less restrictive guidelines (similar to the ones used in Pennsylvania) are using legal factors above and beyond their prescribed purpose, thus using an extensive criminal history to determine the minimum sentence and also departing to a higher sentence based on the same criminal history.

Recent research on sentencing guidelines has shown that sentence departures from the guidelines are another source of extralegal disparity. Johnson (2003) provides insight into the role of different courtroom actors in the sentencing process by investigating whether the effect of race and other extralegal factors on upward and downward departures are moderated by mode of conviction. According to Johnson, departures allow judges to reintroduce their personal judgment into the sentencing process. Using PCS data from 1996-1998, Johnson breaks down mode of conviction into four categories; negotiated pleas, non-negotiated pleas, bench trials and jury trials. Controlling for offense seriousness, criminal history, county courtroom factors, race, age, gender, and mode of conviction, he evaluates the likelihood that an offender will receive a sentence above or below the standard guideline range.

Johnson finds that judges consider multiple factors in sentencing. "Offenders who exercise their right to trial … Offenders who have racial/ethnic attributes that are tied to offender-based focal concerns, such as perceived dangerousness, increased culpability, or a lack of rehabilitative potential, are [at] a disadvantage," (Johnson 2003, 469). His research demonstrates that there are important differences in the effects of legal and extralegal factors across modes of conviction and more importantly for the present research, he demonstrates that the increased use of patterned responses by judges influence judges' assessments of blameworthiness and future dangerousness which leads to different likelihood of departure for certain categories of offenders.

The current study does not have the appropriate data to replicate these findings; but I think they clearly demonstrate that individual judges do have an impact on the variation in sentence length that is measurable. Assignment to judge can have an impact on the length of sentence one receives. Each county in Pennsylvania determines their own process for case assignment, but generally the president judge[7] of the district will assign one judge to determine the caseload for that district.

In larger jurisdictions of Pennsylvania, judges may specialize in certain types of cases. I have no direct data to control for this type of specialization, still judge specialization would provide a more conservative test of specific deterrence. If judge assignment is not truly random and some judges get assigned more difficult cases consistently for sentencing and they sentence them more severely, than judge assignment may be correlated with the error term in the regression equation which may also impact recidivism. Economists have named this kind of variable a quasi-

[7] There are 386 judges in 60 districts for 67 counties in the state of Pennsylvania. Each district has a President Judge, usually the person with the most years on the bench. The President Judge is the head administrator for the district.

instrument because it may not be truly exogenous (Goodliffe working paper). In Monte Carlo experiments, quasi-instruments, while still somewhat biased continue to perform better than regular OLS. Angrist (1991) demonstrates that with large samples, violations of the assumption of no correlation between the instrument and the unobserved correlates of the outcome of interest do not perform worse than the correctly specified estimator. In fact, just identified models only slightly underestimate the true treatment effect (Angrist 1991). On the other hand, OLS can extremely over estimate the relationship, sometimes up to four times as much (Angrist 1991). Thus, if judge assignment is correlated to unmeasured covariates in the equation, then I know that the treatment effect really underestimates the true effect of sentence length on recidivism, giving a conservative test of my hypothesis.

VII. Findings

Tables 3[8] and 4[9] present descriptive statistics for the variables in this analysis. Because of the data reduction methods utilized in this analysis, none of the results are generalizable to the population at large. I can only speak for this group of offenders from these counties in Pennsylvania. In general, these offenders served up to 17 years in prison. The average sentence length was 5.92 years. The majority of the cases come from Philadelphia (53%). This sample of ex-inmates is overwhelmingly male (91%) and over half are African American (61%). Most are single (69%). Over 30% were sentenced under the 1994 sentencing guidelines. The most frequently occurring offenses that resulted in their incarceration are aggravated assault, burglary and robbery. Thus the median offender in the sample would be a single African American male, who was arrested in Philadelphia for a robbery of some kind not involving serious bodily injury. Number of cases, average time sentenced, average minimum sentence and recidivism rates are broken down and reported by judge in Table 4. Average sentence ranges from a low of 2.19 years to a high of 12.56 years.

About 19% of the sample was returned to prison within the three year follow-up period. This is substantially lower than previous studies have reported (Wesiburd et al. 1995; Spohn & Holleran 2002 and Dejong 1997). These other studies have examined different types of offenders, drug offenders and white collar offenders

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[8] The models presented are linear probability models used because the estimation of instrumental variables does not operate yet in a Logit or Probit model.

[9] The minimum sentence variable has a minimum value of 0 for two reasons 1) because time was measured in years there is some rounding error involved. 2) There were 16 offenders who were given a life sentence and subsequently had their sentences vacated and reduced. They were included in this analysis because they met all the other sample requirements. Because their initial offense was recorded as a life sentence, their minimum sentence in the data was recorded as a 0.

whereas this research focuses on the recidivism of violent offenders. Violent offenses are often crimes of passion that are done in the heat of the moment and are causes largely by circumstances. Other types of offenders, such as property offenders are more opportunistic in their offending and more likely to re-offend than violent offenders (Baumer 1997). Also one can argue that drug offenders are physiologically predisposed to re-offending until they are able to kick their drug habit.

Table 5 presents the results of regressions predicting the probability of recidivism. In Model 1, I estimate a simple OLS Regression using sentence length as the target independent variable and controlling for, metropolitan statistical area, offense, marital status, race, gender, minimum sentence and sentencing under 1991, 1994, or 1997 guidelines. The estimates of only the key variables are presented in Table 5. According to Model 1, the OLS regression model, a one year increase in actual sentence length increases the probability of recidivism by .046. Figure 3 represents the predicted probability of recidivism under the OLS framework. The graph demonstrates that as additional years are added to a sentence, the predicted probability of recidivism increases dramatically. At a sentence length of 17 years, the predicted probability of recidivism is .84.

The OLS (Model 1) also indicates that the minimum sentence and guidelines variables are all significantly related to the probability of recidivism. For each additional year increase in the minimum sentence, the probability of recidivism *decreases* by -0.017. This is a counterintuitive finding as this analysis uses minimum sentence as a proxy for offense serious. Upon further investigation it was found that that minimum sentence is correlated with time served. If minimum sentence affects recidivism thru time served than this finding would make sense.

The guidelines variables appear to have the biggest impact on recidivism. Whether an inmate was sentenced under the 1991 guidelines was associated with a .17 percentage point increase in the probability of recidivism. The 1994 guidelines were associated with a .19 percentage point increase and the 1997 guidelines were associated with a .216 percentage point increase in the probability of recidivism. This finding may be more a reflection of an age effect than the actual guidelines themselves. Offenders sentenced under the 1997 guidelines are likely to be younger than the offenders sentenced under the 1994 guidelines just as offenders sentenced under 1994 are likely to be younger than those sentenced under the 1991 guidelines. And we know that previous research has demonstrated that the young are more likely to recidivate and so it follows that offenders sentenced under the 1997 guidelines would have the highest recidivism rates.

Offenders who were separated, divorced and single were all more likely to recidivate than the married offenders, though the widowed were less likely to recidivate than married offenders. As far as race, Asian offenders were less likely to recidivate than White Offenders. Native American, Hispanic and Black offenders were more likely to recidivate than White offenders. The Black offenders however had a highest probability of recidivism. None of the marital status or race variables were significant. Also males are more likely to recidivate than females, but this finding is also non-significant.

Overall, these findings are consistent with theprevious research that suggests that those who were given longer prison sentences have an increased probability of re-offending compared to those who were given less severe sentences. However, as some social scientists have suggested, perhaps the relationship between sentence severity and recidivism is not linear. I test for this in the next model. Model 2 incorporates a squared function of time served to account for non-linearity in the relationship between time served and recidivism using OLS regression. Including the squared term increases the probability of recidivism from about .045 to .079. The coefficient for time served squared is highly significant (p=.000). This indicates that since the coefficient of the squared term is significantly different from 0, there is a definite indication that the relationship between sentence length and recidivism is indeed non-linear. Also note that the relationship is negative. This non-linear finding is in line with theoretical predictions. It lends some support to the notion that the effect of sentence length on recidivism may vary for different types of offenders.

The regression equations for the OLS regressions from Model 1 and Model 2 are represented graphically in Figure 3. The lines were estimated using the median values for each variable. The regression line represents single black male offenders from Philadelphia County, who was sentenced for robbery under the 1994 guidelines. As can be observed from the graph, both models approximate the data very similarly. Both regression lines (the regular OLS Model 1 and Squared OLS Model 2) have similar slopes. The model with the squared term while in general shows a steady increase in the probability of recidivism, seems to begin to level off at 13 years of time served, indicating that the probability of recidivism may have a threshold at a sentence length of 13 years for violent offenses.

The same variables that were significant in the OLS model (1) without the squared parameter continue to be significant in the OLS model with the squared term; sentence length, minimum sentence, 1991, 1994,1997 guidelines and Asian offenders. They are remarkably similar in magnitude as well. The only difference is that the parameter estimate for whether an offender was sentenced under 1994 guidelines decreases by about half the percentage points when the squared term is added. The marital status, race and gender effects are essentially the same in Model 2 as they were in Model 1.

If a researcher were to examine these two models solely, s/he might be lead to make the conclusion that longer sentence lengths indeed increase the probability of recidivism. However, the OLS regressions are limited because by themselves, they fail to control for omitted variable bias. The possibility of omitted variable bias in an OLS framework leads to a biased estimator of the true slope parameter. Since this bias will not decrease as the sample size gets larger, omitting a variable from the regression equation also leads to an inconsistent estimator.

To control for bias and present a better specified model, I use instrumental variable estimation. This is a two-stage process. In the first stage, sentence length is predicted using the judge assignment, metropolitan statistical area, offense, marital status, race, gender, minimum sentence and whether or not an offender was sentenced under the 1991, 1994 or 1997 guidelines. These first stage results are presented in

Table 6. While this present study undoubtedly is missing some key variables, the above listed variables explain about 75% of the variance in sentence length.

Table 6 presents some findings that require explanation. First some of the judge identifier variables were dropped in this analysis and this is presumed to be a result of the co-dependency between the explanatory variables in the model. Judges are nested within counties and thus the judge assignment variables are highly correlated with county. Other possible explanations were for the dropped variables were investigated such as constancy in certain values of variables. Alternative explanations were systematically examined and ruled out, thus the drops were determined to be a result of multi-collinearity. Also, when the coefficients for the offense variables are examined, some of them appear to predict a longer sentence than the reference category, which is Murder. Recall that the sample was limited to those offenders who were sentenced after 1885 and released sometime between 1997 and 2001 and that we have minimum sentence in the model. When the offenders who committed murder in this sample are examined, they served less time relative to their minimum sentences than other offenders who committed offenses like forcible rape and sexual assault. Thus the coefficients for those variables are positive.

The indication that judge assignment is a good instrument comes from the first stage F statistic. Normally a low F-statistic, under 10 (Staiger 2002), or a non significant one would be an indication that the instrument is weak. In the first stage of the IVE the F statistic is $F = 105.580$, which is significant, $p=.000$. Based on the F statistic, I can conclude that judge assignment is indeed a good instrument. It is correlated with sentence length, which is the key independent variable.

Using the predicted sentence length from the first stage of the IV regression, I then estimate the probability of recidivism controlling for MSAS, offense type, marital status, race, gender, minimum sentence and sentenced under 1991, 1994, and 1997 guidelines. The instrumental variable regression results are presented under Model 3 in Table 7. Upon examination of this model, it is evident that some estimates changed. Comparing Model 3 with Model 1, it is clear that the probability of recidivism using predicted year served on recidivism dropped from .046 to .025. Though the beta coefficient dropped, it remains significant. In fact most of the variables that were significant in models 1 and 2 remain significant, i.e. 1991, 1994, 1997 guidelines, and Asians.

Two differences in the results are worth examining. First the effect of minimum sentence on the probability of recidivism becomes non-significant. It remains in the negative direction, but is of smaller magnitude when compared to the OLS models. This is likely an indication that minimum sentence affects recidivism through time served. By utilizing the predicted time served variable in the IVE estimation, that relationship was accounted for. Second, with the use of the instrumental variable, males have a significantly higher probability of recidivism than the females. This finding is in line with prior research.

Figure 4 graphically represents the predicted probabilities of recidivism from the results of the IVE regressions. Model 3 is the first IVE regression model run without the squared term and Model 4 includes the squared term. Similar to the OLS models discussed above, with increases in sentence length the probability of recidivism increases. Second, the increase in probability of recidivism is of a substantially lower magnitude than the OLS models.

Model 4 is an IVE model run with the squared term to take into account the non-linearity in the relationship between time served and recidivism. The results of this model differ slightly from the results from Model 3. According to Model 4, a one year increase in time served increases the probability of recidivism by only .042 percentage points. This finding is significant, $p<.05$. The sentence length squared variable is non-significant in this model, even though its addition increased the magnitude of the predicted time served variable. . In the other squared model, Model 2, where the squared term was estimated in a regular OLS regression, the estimate for year served was an 8 percentage point increase in the probability of recidivism. The instrumental variable estimation method cuts down that estimate by almost 50%.

Interestingly, comparing the two models that use the IVE method, both models show that time served has a significant impact on the probability of recidivism. However, when the squared term (Model 4) is included in the equation, it is not significant. This is a both an intriguing and confusing finding. Since use of the IVE method is an attempt to control for omitted variable bias, it may be that in Model 4, the instrumental variable of judge assignment was able to pick up on the omitted factor that caused the relationship between sentence length and recidivism to be non-linear.

A comparison of both squared models is graphically represented in Figure 5. This figure demonstrates that the OLS regression of the probability of recidivism on sentence length overestimates the relationship. The IVE regression demonstrates that the magnitude of the relationship is much smaller. Regarding the third research question, whether or not offenders given longer sentences are more likely to recidivate, the data demonstrate thatthose offenders in this sample who are given longer sentences are significantly more likely to recidivate than those offenders given shorter sentences.

VIII. DISCUSSION AND CONCLUSIONS

I must present one caveat before I begin interpreting findings, there are significant limitations to this study and the results must be interpreted with caution. This data does not include offender's criminal history nor does it include offense seriousness scores or several other important variables that have been demonstrated to predict recidivism. I have tried to account for these missing variables by incorporating proxies for offense seriousness such as minimum sentence and offense code. The minimum sentence variable will account somewhat for offense seriousness but it is not a complete approximation. That being said, these results are suggestive. The contributions of the present study can be divided into methodological contributions and substantive contributions. Each of these will be discussed in turn.

Methodological Contribution

The issue of omitted variable bias is one that plagues much of social science research. Instrumental variable estimation is a way to control for the presence of omitted variables; yet it is not a method that is used often because of the difficulty in finding a good instrument. The attractiveness of IVE is that it can be used to exploit quasi-experimental variation in research. The IVE method, which has been long used by economists, can address omitted variables problems in observational studies and address the statistical issues that result from imperfect criminological experiments (Angrist 2005). As a discipline, criminology has not been able to capitalize on this method. The discipline has only a handful of instrumental variables such as Levitt's (1997) work using electoral cycles as an instrumental variable to identify the causal effect of police hiring on crime rates or the work of Kling (1999) who uses assignment to federal judges to estimate the impact of length of incarceration on future employment and earnings.

Finding a good instrumental variable in criminology is difficult. The instrument should be correlated with the treatment and not correlated with the error term. With my limited data set, I found that judge assignment as well as the other control variables present in the data explained 75% of the variation in sentence length. The use of this instrument demonstrated that without controlling for omitted variable bias, a multiple regression approach to predicting recidivism may overestimate the probability by approximately 50% points. The development of judge assignment as an instrumental variable and the introduction of this technique to criminological questions is a significant contribution to the research of criminologists.

Substantive Contributions

In addition to the methodological contributions there are also substantive contributions that can be drawn from this research. The results of this study provide no support for specific deterrence theory as it has been operationalized in this study. In every regression estimated, increased severity as measured by increases in sentence length was significantly associated with an increase in the predicted probability of recidivism for this sample of violent offenders. This finding adds to the substantial body of research that suggests that increases in sentence severity also increases probabilities of recidivism (Dejong 1997; Spohn & Holleran 2002; Harris 1975; Wooldredge 1988; Visher 1991).

Some have suggested that these empirical findings against deterrence theory may be a result of an over-extension of the original intent of the theory. As has been discussed above, the original conception of deterrence theory was that if a sanction for a crime is certain enough, severe enough and swift enough, then those who contemplate engaging in said infraction would re-think that choice because the perceived cost of committing the crime would outweigh the potential benefits of committing the crime. This conceptualization of deterrence theory posits nothing about whether offenders will be deterred from committing future crime after they experience a sanction personally. A perception about the costs of experiencing a sanction and experiencing the sanction itself are different things. As we see from this study the experience of the sanction does not deter this sample of serious violent offenders.

Rather than a deterrence perspective, the current results seem to provide more support for a labeling / deviance amplification hypothesis. Since the probability of recidivism increases with longer sentence lengths, perhaps the longer time spent in prison corresponds to a greater commitment to a deviant label / lifestyle. Alternatively when one examines who are the offenders who are most likely to receive longer sentences, it would come as not surprise that repeat offenders are more likely to receive longer sentences. According to the recidivism literature repeat offenders, those with long criminal histories are also the group of offenders who are the most likely to recidivate. Due to the limitations of the data it is impossible to distinguish any distinct classes of individuals who might experience sentence severity differently. However, due to the non-significance of the squared term in the second IVE estimation, there is strong evidence that there are different classes of offenders who experience sentence severity differently.

Future research in addition to replicating these findings with better data may also investigate whether this effect holds true for all groups, or if it is different for experienced, versus naïve offenders such as DeJong (1997) finds in her research.

There is one other finding of the present research that is of particular significance. The relationship between sentence length and recidivism is non-linear. In Model 2 where a squared term was added to the equation, not only was it significant, it also increased the estimate of the magnitude of the impact of sentence length on the probability of recidivism. That relationship was not significant in Model 4, the IVE model. This is possibly a result of the power of the instrument in negating the effect of omitted variable bias. Despite the fact that in Model 4 the squared term was not significant, its addition nearly doubled the estimate of the association between sentence length and recidivism, from .025 to .042. Both the models including the squared term predicted recidivism better than the models that did not have the squared term. This clearly demonstrates that Baumer (1997) was correct when he hypothesized that the relationship between sentence severity and recidivism is not linear.

Limitations

Though the results of this analysis are highly suggestive, they are limited. The sampling frame necessary to conduct this analysis limits the generalizability of the results to only the largest MSA's in Pennsylvania. Also the present analysis is missing a large number of key variables needed to estimate sentence length and recidivism. These variables that the present study was unable to measure may be impacting the results for example; age may be a factor in the significant findings regarding the guidelines variables. Also prior criminal history is known to have a significant impact on sentence length and recidivism. Instrumental variable analysis is a powerful tool to account for omitted variable bias, however the use of other relevant predictors adds to the precision of the instrument. When important variables are left out, the model's precision may be questioned, as is the case in the current study.

Policy Implications

Based on the results of this study there is some evidence to suggest that increasing the length of time that individuals are sentenced for serious felonies does increase their probability of recidivism. This finding, if confirmed with better data would be a blow for specific deterrence theory and for those policy advocates who rely on deterrence theory to justify increased sentences for serious offenders. The evidence from this study suggests that shorter sentences are the most effective in reducing recidivism rates. Though implementing a policy that advocates shorter sentences for serious offenses would be tantamount to political suicide in a society so steeped in the crime control mentality, it is an avenue that would be worth considering if these results are replicable.

Table 1. Offenses and Offense Counts

Count	Description
30	Murder – Criminal Homicide
10	Murder – First Degree
5	Murder – Second Degree
471	Murder – Third Degree
172	Voluntary Manslaughter
86	Involuntary Manslaughter
1771	Aggravated Assault
11	Aggravated Assault with Serious Bodily Injury
4	Aggravated Assault with Serious Bodily Injury to Officer
6	Aggravated Assault with Deadly Weapon
5	Assault by Prisoner
407	Forcible Rape
253	Involuntary Deviant Sexual Intercourse
16	Sexual Assault
81	Aggravated Indecent Assault
88	Indecent Assault
2	Spousal Sexual Assault
1730	Burglary – General
3019	Robbery – General
28	Robbery with SeriousBodily I njury
8195	**Total**

Table 2. Metropolitan Statistical Areas (MSAS)

MSA Name	Frequency
Allegheny	1138
Beaver	55
Berks	258
Bucks	88
Centre	41
Chester	119
Dauphin	276
Delaware	295
Erie	120
Fayette	62
Lancaster	183
Lehigh	108
Lycoming	127
Montgomery	330
Philadelphia	4559
Washington	75
Westmoreland	57
York	304
Total	8195

Table 3. Descriptive Statistics for Variables

Dependent Variable	***N***	***Mean***	***S.D.***	***Min***	***Max***
Recidivism	8195	.19	.394	0.00	1.00
Control Variables					
Years Served		5.92	3.17	0.00	17.00
Years Served Squared		45.25	46.37	0.00	289.00
Minimum Sentence in Years		3.47	2.31	0.00	35.00
MSAS					
(reference) Alleghany		.14	.34	0.00	1.00
Beaver		.01	.08	0.00	1.00
Berks		.03	.17	0.00	1.00
Bucks		.01	.10	0.00	1.00
Centre		.01	.07	0.00	1.00
Chester		.01	.11	0.00	1.00
Dauphin		.03	.18	0.00	1.00
Delaware		.04	.19	0.00	1.00
Erie		.02	.12	0.00	1.00
Fayette		.01	.09	0.00	1.00
Lancaster		.02	.14	0.00	1.00
Lehigh		.01	.12	0.00	1.00
Lycoming		.02	.12	0.00	1.00
Montgomery		.04	.19	0.00	1.00
Philadelphia		.55	.50	0.00	1.00
Washington		.01	.09	0.00	1.00
Westmoreland		.01	.08	0.00	1.00
York		.04	.18	0.00	1.00
Offenses					
(reference) Murder		.00	.06	0.00	1.00
First Degree Murder		.00	.04	0.00	1.00
Second Degree Murder		.00	.02	0.00	1.00
Third Degree Murder		.06	.23	0.00	1.00
Manslaughter		.02	.14	0.00	1.00
Involuntary Manslaughter		.01	.10	0.00	1.00
Aggravated Assault		.21	.41	0.00	1.00
Aggravated Assault with Serious Injury		.00	.04	0.00	1.00
Aggravated Assault to an Officer		.00	.01	0.00	1.00
Aggravated Assault with a Deadly Weapon		.00	.03	0.00	1.00
Aggravated Assault by a Prisoner		.00	.03	0.00	1.00
Forcible Rape		.05	.22	0.00	1.00
Involuntary Deviant Sexual Intercourse		.03	.17	0.00	1.00
Sexual Assault		.00	.04	0.00	1.00
Aggravated Indecent Sexual Assault		.01	.11	0.00	1.00
Indecent Assault		.01	.11	0.00	1.00
Spousal Sexual Assault		.00	.02	0.00	1.00
Burglary		.22	.41	0.00	1.00
Robbery		.37	.48	0.00	1.00
Robbery with Serious Injury & Force		.00	.05	0.00	1.00

Table 3. Descriptive Statistics for Variables (cont'd)

Offenses (cont)	***N***	***Mean***	***S.D.***	***Min***	***Max***
Marital Status					
Single		.69	.46	0.00	1.00
(reference) Married		.14	.34	0.00	1.00
Separated		.04	.20	0.00	1.00
Divorced		.07	.26	0.00	1.00
Widowed		.01	.09	0.00	1.00
Unknown		.00	.04	0.00	1.00
Race					
Native American		.01	.03		
African American / Black		.66	.48	0.00	1.00
(reference) White		.24	.43	0.00	1.00
Hispanic		.07	.26	0.00	1.00
Asian American		.01	.07	0.00	1.00
Other		.00	.03	0.00	1.00
Gender					
Males		.91	.29	0.00	1.00
Additional Variables					
Sentenced Under 1991 Guidelines		.24	.43	0.00	1.00
Sentenced Under 1994 Guidelines		.33	.47	0.00	1.00
Sentenced Under 1997 Guidelines		.20	.40	0.00	1.00

Table 4. Descriptive Statistics for Judge Dummy Variables

Judge	***N***	***Average Time Served***	***S.E.***	***Average Min Sent***	***S.E.***	***Recidivism Rate***	***S.E.***
1	26	10.81	0.50	4.71	0.43	0.15	0.07
5	29	4.86	0.26	2.29	0.17	0.18	0.07
6	20	3.25	0.25	2.33	0.28	---	---
7	38	4.89	0.31	2.65	0.21	0.24	0.07
9	26	9.73	0.39	5.31	0.46	0.23	0.08
16	34	6.91	0.60	4.06	0.40	0.15	0.06
17	44	9.23	0.39	4.05	0.25	0.25	0.07
19	31	6.90	0.70	3.77	0.44	0.17	0.07
21	61	5.58	0.43	3.38	0.26	0.20	0.05
23	33	6.50	0.47	3.86	0.42	0.28	0.08
24	49	4.71	0.20	3.31	0.23	0.14	0.05
26	62	4.80	0.26	2.83	0.26	0.20	0.05
29	21	3.55	0.33	2.42	0.26	0.05	0.05
31	20	6.95	0.52	4.45	0.53	0.05	0.05
32	56	3.64	0.23	2.28	0.14	0.24	0.06
34	24	4.38	0.38	2.62	0.43	0.21	0.09
38	47	6.66	0.38	3.43	0.20	0.19	0.06
39	26	9.76	0.51	5.45	0.48	0.04	0.04
40	29	3.07	0.22	2.34	0.45	0.28	0.08
43	36	5.17	0.49	3.06	0.35	0.17	0.06
47	25	4.96	0.53	3.10	0.29	0.08	0.06
49	59	4.02	0.25	3.42	0.29	0.14	0.05
51	24	6.25	0.72	4.18	0.55	0.25	0.09
52	82	5.02	0.27	3.80	0.29	0.17	0.04
53	57	4.02	0.35	2.44	0.24	0.12	0.04
58	28	8.74	0.59	6.02	0.48	0.19	0.08
59	31	9.87	0.70	3.60	0.57	0.19	0.07
62	72	6.78	0.34	3.94	0.22	0.15	0.04
63	98	5.86	0.18	3.54	0.21	0.24	0.04
64	73	5.38	0.25	3.13	0.22	0.24	0.05
66	87	5.99	0.36	2.89	0.26	0.20	0.04
67	32	2.75	0.15	1.57	0.14	0.25	0.08
68	39	5.11	0.51	3.03	0.36	0.11	0.05
71	24	3.67	0.39	2.88	0.30	0.04	0.04
74	98	7.84	0.37	4.30	0.27	0.26	0.45
76	243	6.30	0.18	2.66	0.12	0.25	0.03
77	209	5.30	0.14	3.31	0.11	0.21	0.03
82	53	4.43	0.25	2.78	0.21	0.13	0.05
84	71	4.31	0.24	2.80	0.14	0.16	0.04
86	43	5.33	0.45	3.64	0.36	0.19	0.06
88	21	2.19	0.21	1.72	0.15	0.05	0.05
89	48	5.91	0.37	3.44	0.25	0.17	0.06
90	53	3.92	0.22	2.46	0.20	0.19	0.05
91	28	5.79	0.47	4.15	0.43	0.07	0.05
92	33	3.42	0.23	2.42	0.26	0.18	0.07

95	141	5.55	0.19	3.26	0.16	0.21	0.04
104	58	6.60	0.45	4.27	0.30	0.10	0.04
105	23	6.00	0.32	3.81	0.25	0.32	0.10
106	35	6.26	0.45	3.47	3.97	0.20	0.07
107	23	10.87	0.53	5.38	0.67	0.22	0.09
108	26	5.28	0.48	3.77	0.41	0.08	0.06
109	55	3.82	0.21	2.33	0.17	0.14	0.05
110	21	5.62	0.72	3.51	0.50	0.29	0.10
111	91	4.88	0.21	3.07	0.16	0.19	0.04
112	26	4.62	0.34	2.80	0.31	0.27	0.09
113	23	7.83	0.35	4.26	0.46	0.22	0.09
114	38	5.24	0.28	3.70	0.30	0.08	0.04
115	148	7.35	0.24	3.92	0.22	0.25	0.04
117	21	6.67	1.01	2.92	0.48	0.19	0.09
118	98	10.23	0.29	5.55	0.25	0.18	0.04
119	69	11.26	0.30	6.87	0.35	0.17	0.05
120	69	2.77	0.12	1.55	0.08	0.25	0.05
122	40	4.15	0.35	2.53	0.19	0.18	0.06
123	58	7.49	0.38	3.45	0.26	0.12	0.04
128	40	3.45	0.24	2.02	0.16	0.15	0.06
129	47	4.19	0.26	2.49	0.19	0.09	0.04
130	24	3.04	0.21	2.39	0.32	0.13	0.07
132	40	9.30	0.33	4.61	0.34	0.18	0.06
133	79	7.31	0.44	5.76	0.53	0.22	0.05
135	27	7.04	0.49	4.47	0.44	0.19	0.08
137	24	3.42	0.28	2.17	0.25	0.25	0.09
139	85	6.51	0.28	4.08	0.23	0.25	0.05
142	108	6.12	0.24	3.58	0.19	0.19	0.04
143	61	5.84	0.34	3.96	0.29	0.15	0.05
144	28	4.50	0.35	2.36	0.29	0.25	0.08
146	132	4.46	0.19	2.68	0.19	0.23	0.04
147	22	2.86	0.27	2.17	0.21	0.28	0.09
148	92	4.24	0.17	2.60	0.17	0.17	0.04
150	26	3.81	0.28	2.26	0.25	0.12	0.06
153	33	4.56	0.29	2.95	0.23	0.34	0.08
155	72	11.07	0.26	4.98	0.30	0.27	0.05
157	21	2.65	0.24	1.85	0.18	0.20	0.09
158	39	7.05	0.53	5.36	0.45	0.10	0.05
160	30	6.23	0.58	3.05	0.42	0.10	0.06
161	109	4.73	0.15	2.96	0.14	0.16	0.04
163	76	5.76	0.30	3.12	0.22	0.16	0.04
164	23	2.74	0.23	1.56	0.13	0.30	0.10
167	22	4.29	0.56	3.46	0.45	0.14	0.08
170	36	5.14	0.32	3.83	0.26	0.22	0.07
171	42	5.33	0.38	2.57	0.27	0.31	0.07
172	85	6.18	0.30	3.13	0.29	0.38	0.05
175	34	8.53	0.54	3.73	0.30	0.24	0.07
176	59	4.53	0.24	2.39	0.19	0.17	0.05
177	49	6.63	0.30	2.88	0.28	0.27	0.06
178	26	5.44	0.36	2.29	0.18	0.36	0.10

179	23	2.91	0.21	2.00	0.17	0.17	0.08
181	59	6.79	0.55	3.88	0.34	0.14	0.46
183	100	5.31	0.28	3.00	0.20	0.30	0.05
185	102	7.05	0.31	4.54	2.33	0.22	0.04
187	32	4.58	0.40	2.89	0.26	0.10	0.05
188	36	12.56	0.40	6.56	0.54	0.22	0.07
189	119	5.31	0.28	2.86	0.19	0.20	0.04
191	25	5.44	0.87	3.51	0.47	0.20	0.08
193	63	6.61	0.51	3.30	0.27	0.16	0.05
194	32	2.88	0.22	1.86	0.15	0.25	0.08
195	50	10.60	0.37	6.48	0.37	0.12	0.05
196	195	2.98	0.10	2.07	0.13	0.15	0.03
198	22	6.91	0.45	3.71	0.41	0.09	0.06
202	29	8.36	0.58	4.31	0.43	0.29	0.09
203	43	5.84	0.45	3.40	0.31	0.23	0.07
208	144	5.21	0.15	2.98	0.15	0.20	0.03
209	59	5.53	0.38	3.02	0.24	0.20	0.05
210	23	8.09	0.38	4.22	0.31	0.13	0.07
211	93	6.47	0.36	3.27	0.24	0.19	0.04
213	23	8.57	0.76	3.59	0.46	0.30	0.10
215	74	5.56	0.34	2.85	0.20	0.37	0.06
216	60	8.39	0.29	4.66	0.31	0.20	0.05
219	65	4.25	0.21	3.01	0.19	0.20	0.05
226	22	6.76	0.72	4.03	0.53	0.19	0.09
229	42	7.78	0.43	5.74	0.32	0.10	0.05
232	25	6.00	0.46	2.65	0.28	0.08	0.06
234	31	5.16	0.64	3.26	0.55	0.13	0.06
237	28	7.57	0.65	5.81	0.56	0.11	0.06
238	155	7.41	0.25	5.01	0.18	0.14	0.03
240	24	4.29	0.21	2.75	0.23	0.04	0.04
243	39	7.41	0.53	3.75	0.27	0.08	0.04
244	21	4.90	0.50	4.19	0.68	0.25	0.10
246	50	5.94	0.51	2.79	0.24	0.17	0.05
247	27	2.00	0.14	1.49	0.12	0.11	0.06
248	60	9.35	0.40	7.25	0.36	0.15	0.05
252	28	7.71	0.65	3.92	0.34	0.11	0.06
254	21	6.90	0.42	4.08	0.38	0.19	0.09
257	54	9.87	0.23	4.69	0.22	0.34	0.07
261	44	7.36	0.51	3.93	0.44	0.29	0.07
262	26	7.35	0.49	3.89	0.47	0.15	0.07
264	119	3.81	0.13	2.32	0.10	0.12	0.03
266	21	7.25	0.76	3.09	0.34	0.45	0.11
267	65	3.88	0.21	1.98	0.16	0.17	0.05
269	22	4.95	0.33	3.82	0.49	0.22	0.09
273	25	5.36	0.36	2.68	0.28	0.16	0.08
274	67	9.39	0.29	6.82	0.30	0.16	0.05
275	22	8.59	0.78	6.61	0.70	0.05	0.05
277	34	5.32	0.46	3.52	0.32	0.18	0.07
280	185	7.63	0.22	4.57	0.18	0.16	0.03
282	38	3.76	0.53	2.05	0.17	0.16	0.06

283	40	4.83	0.50	2.78	0.30	0.18	0.06
284	85	5.76	0.23	3.88	0.19	0.13	0.04
285	43	4.12	0.29	2.31	0.24	0.19	0.06
287	31	3.61	0.22	1.94	0.15	0.26	0.08
289	34	5.82	0.45	3.33	0.35	0.24	0.07
290	26	4.83	0.51	2.97	0.45	0.33	0.10
291	25	6.76	0.69	2.28	0.24	0.36	0.10
297	67	7.71	0.37	4.14	0.21	0.24	0.05
305	26	3.62	0.28	1.94	0.12	0.12	0.06
307	23	2.43	0.19	1.49	0.13	0.26	0.09
308	32	3.90	0.33	2.80	0.33	0.33	0.09

Table 5. OLS Regression Predicting Probability of Recidivism

Variables	***Model 1[a]***			***Model 2[b]***		
	b	***S.E.***	***Sig.***	***b***	***S.E.***	***Sig.***
Years Served (actual)	.046	.002	***	.079	.007	***
Years Served Squared	---	---	---	-.002	.000	***
Min Sentence (years)	-.017	.002	***	-.018	.003	***
1991 Guidelines	.111	.015	***	.090	.015	***
1994 Guidelines	.194	.018	***	.188	.018	***
1997 Guidelines	.216	.022	***	.237	.022	***
Separated	.007	.024		.005	.024	
Divorced	.014	.020		.013	.020	
Widowed	-.035	.048		-.027	.048	
Single	.004	.013		.003	.013	
Unknown Marital Status	.065	.113		.065	.113	
Asian	-.088	.159		-.087	.159	
Native American	.084	.147		.085	.147	
Hispanic	.083	.148		.082	.148	
Black	.095	.147		.095	.147	
Other Race	.001	.209		-.001	.208	
Male	.024	.023		.026	.023	
Model R-squared	.066			.069		
Adjusted R-Squared	.059			.063		

a. Model 1 is a simple OLS regression of probability of recidivism
b. Model 2 is also OLS regression but includes the squared term to account for non-linearity
c.*** $p<.05$

Table 6. First Stage Instrumental Variable Estimation Results

Variables	***b***	***S.E.***	***Sig.***
Minimum Sentence in Years	0.432	0.011	***[a]
1991 Guidelines	-2.338	0.058	***
1994 Guidelines	-3.952	0.063	***
1997 Guidelines	-5.164	0.073	***
PMSAS			
Allegheny	(reference)		
Beaver	-2.607	2.870	
Berks	-3.183	2.101	
Bucks	-3.931	2.356	**
Centre	-.161	2.365	
Chester	-1.997	2.247	
Dauphin	-3.035	3.851	**
Delaware	-2.242	2.358	
Erie	-2.458	1.700	
Fayette	-3.065	1.707	
Lancaster	-2.543	2.782	
Lehigh	-3.046	1.637	**
Lycoming	-3.402	2.095	
Montgomery	.003	1.842	
Philadelphia	-1.155	1.706	
Washington	-2.586	1.639	
Westmoreland	-3.285	1.709	**
York	-4.030	3.301	
Offenses			
Murder – Criminal Homicide	(reference)		
Murder – First Degree	(dropped)		
Murder – Second Degree	-0.889	.872	
Murder – Third Degree	-0.792	.327	***
Voluntary Manslaughter	-0.186	0.340	
Involuntary Manslaughter	-0.717	0.362	***
Aggravated Assault	-.0421	0.315	
Aggravated Assault with Serious Bodily Injury	-0.448	0.580	
Aggravated Assault with Serious Bodily Injury to Officer	-0.654	0.865	
Aggravated Assault with Deadly Weapon	-0.627	0.730	
Assault by Prisoner	-0.477	0.870	
Forcible Rape	0.263	0.324	
Involuntary Deviant Sexual Intercourse	0.054	0.331	
Sexual Assault	0.049	0.561	
Aggravated Indecent Assault	-0.048	0.361	
Indecent Assault	-0.346	0.362	
Spousal Sexual Assault	0.483	1.180	
Burglary – General	-0.409	0.316	
Robbery – General	-0.342	0.315	
Robbery with Serious Bodily Injury	-0.500	0.437	

Marital Status			
Married	(reference)		
Separated	-0.005	0.101	
Divorced	-0.015	0.084	
Widowed	0.092	0.202	
Single	-0.090	0.053	***
Unknown Marital Status	-0.236	0.470	
Race			
White	(reference)		
Native American	-0.429	0.660	
Asian	-1.213	0.612	***
Hispanic	-0.592	0.615	
Black	-0.356	0.611	
Other Race	-0.618	0.866	
Gender			
Females	(reference)		
Males	1.890	0.093	***
F-Statistic	105.580		
Prob > F	0.000		
R-Squared	0.746		
Adj. R-Squared	0.740		
Judges			
Judge 1	(reference)		
Judge 5	-1.845	0.439	***
Judge 6	-1.898	0.487	***
Judge 7	0.088	2.237	
Judge 9	-1.103	0.450	***
Judge 11	-0.852	1.712	
Judge 16	1.489	0.455	**b
Judge 17	-0.684	0.397	
Judge 19	1.390	1.683	
Judge 21	-1.400	1.688	
Judge 22	0.067	0.468	
Judge 23	(dropped)		
Judge 24	-1.939	0.400	***
Judge 26	1.618	2.352	
Judge 29	-2.223	0.486	***
Judge 31	-2.518	1.708	
Judge 32	-2.414	0.388	***
Judge 34	0.520	1.300	
Judge 38	-4.097	2.835	
Judge 39	-0.713	0.455	
Judge 40	0.417	1.295	
Judge 41	-1.739	1.703	
Judge 43	-0.849	0.432	***
Judge 47	(dropped)		

Judge 48	-0.892	1.711	
Judge 49	-1.982	1.691	
Judge 51	-1.733	1.710	
Judge 52	-1.968	0.368	***
Judge 53	-4.723	2.854	**
Judge 58	-1.418	0.445	***
Judge 59	0.612	1.701	
Judge 62	-1.347	0.372	***
Judge 63	-1.695	0.358	***
Judge 64	-1.602	0.370	***
Judge 66	0.059	0.336	***
Judge 67	-1.994	0.437	***
Judge 68	-3.227	0.805	***
Judge 69	-1.656	1.716	
Judge 71	-3.249	1.697	**
Judge 74	-0.939	1.684	
Judge 76	-1.596	0.334	***
Judge 77	-1.659	0.338	***
Judge 81	-0.558	0.385	
Judge 82	-4.478	2.855	
Judge 84	-1.934	1.686	
Judge 85	-1.424	1.700	
Judge 86	-0.686	2.235	
Judge 88	(dropped)		
Judge 89	-4.386	2.855	
Judge 90	-5.469	3.475	
Judge 91	-0.405	2.242	
Judge 92	-2.360	0.429	***
Judge 95	-1.846	0.347	***
Judge 97	1.664	2.369	
Judge 98	0.776	0.488	
Judge 99	(dropped)		
Judge 100	(dropped)		
Judge 101	-1.232	1.716	
Judge 104	0.652	1.644	
Judge 105	1.277	2.368	
Judge 106	-0.870	1.521	
Judge 107	-0.562	0.458	
Judge 108	-0.171	2.246	
Judge 109	-2.304	0.390	***
Judge 110	0.636	0.501	
Judge 111	-1.869	0.361	***
Judge 112	-1.324	0.456	***
Judge 113	-1.148	0.461	***
Judge 114	-1.704	0.416	***
Judge 115	-1.469	0.343	***
Judge 117	-2.128	1.629	
Judge 118	-0.875	0.356	***
Judge 119	-0.507	0.370	
Judge 120	-2.000	0.381	***

Judge 122	-2.925	1.683	**
Judge 123	-1.326	0.383	***
Judge 128	-5.854	3.477	**
Judge 129	-4.946	2.855	***
Judge 130	-2.012	0.482	***
Judge 132	-1.161	0.404	***
Judge 133	-1.002	0.372	***
Judge 135	-2.068	1.705	
Judge 137	-2.746	1.694	
Judge 139	-1.427	0.364	***
Judge 142	-1.405	0.355	***
Judge 143	-2.669	1.674	
Judge 144	0.164	1.296	
Judge 146	0.380	1.275	
Judge 147	-5.083	2.868	**
Judge 148	-1.739	0.363	***
Judge 150	-5.466	3.484	
Judge 153	-2.698	1.685	
Judge 155	-0.029	0.370	
Judge 157	-2.180	0.487	***
Judge 158	-1.881	0.415	***
Judge 160	-2.611	0.815	***
Judge 161	-2.053	0.354	***
Judge 163	-1.739	0.368	***
Judge 164	-1.586	0.469	***
Judge 167	-2.005	0.486	***
Judge 168	-5.539	3.487	
Judge 170	-2.128	0.420	***
Judge 171	-5.404	3.467	
Judge 172	-1.207	1.685	
Judge 175	-1.594	0.809	***
Judge 176	0.232	1.267	
Judge 177	-0.956	1.692	
Judge 178	-1.239	0.456	***
Judge 179	-0.725	1.542	
Judge 181	-1.418	0.387	***
Judge 183	-1.556	1.683	
Judge 185	-1.619	0.355	***
Judge 187	0.106	0.441	
Judge 188	0.825	0.416	***
Judge 189	-1.548	1.682	
Judge 191	1.159	2.318	
Judge 193	-0.993	1.688	
Judge 194	-1.837	0.436	***
Judge 195	-0.852	0.392	***
Judge 196	-2.120	0.343	***
Judge 198	-1.590	1.712	
Judge 202	-4.538	3.481	
Judge 203	-5.122	3.476	
Judge 208	-1.860	0.346	***

Judge 209	-2.806	0.780	***
Judge 210	-2.145	0.464	***
Judge 211	-0.938	1.684	
Judge 213	-0.776	1.709	
Judge 215	-1.360	1.686	
Judge 216	-1.345	0.380	***
Judge 219	-1.854	1.688	
Judge 226	0.077	2.248	
Judge 229	-1.618	0.409	***
Judge 231	-3.600	2.312	
Judge 232	0.390	1.296	
Judge 234	-0.361	2.308	
Judge 237	-1.610	0.445	***
Judge 238	-1.402	0.348	***
Judge 240	-1.046	1.534	
Judge 243	-1.294	1.696	
Judge 244	-3.589	0.851	***
Judge 246	-2.665	0.798	***
Judge 247	-1.960	0.454	***
Judge 248	-1.305	0.387	***
Judge 252	-1.863	1.630	
Judge 254	-1.913	1.697	
Judge 257	-0.969	0.386	***
Judge 261	0.809	1.262	
Judge 262	0.921	1.307	
Judge 264	-1.925	0.354	***
Judge 266	-2.296	0.841	***
Judge 267	-1.841	0.377	***
Judge 268	-0.309	0.487	
Judge 269	0.385	1.315	
Judge 273	0.315	2.244	
Judge 274	-1.534	0.372	***
Judge 275	-1.141	0.469	***
Judge 277	-0.829	1.524	
Judge 280	-1.370	0.338	***
Judge 282	-2.854	1.682	**
Judge 283	-2.368	0.790	***
Judge 284	-1.936	0.363	***
Judge 285	-5.466	3.476	
Judge 287	-4.446	2.861	
Judge 289	-2.883	0.809	***
Judge 290	-0.965	0.462	***
Judge 291	0.771	2.350	
Judge 297	-1.096	0.376	***
Judge 305	-2.609	1.691	
Judge 307	-2.017	0.482	***
Judge 308	-2.031	1.703	

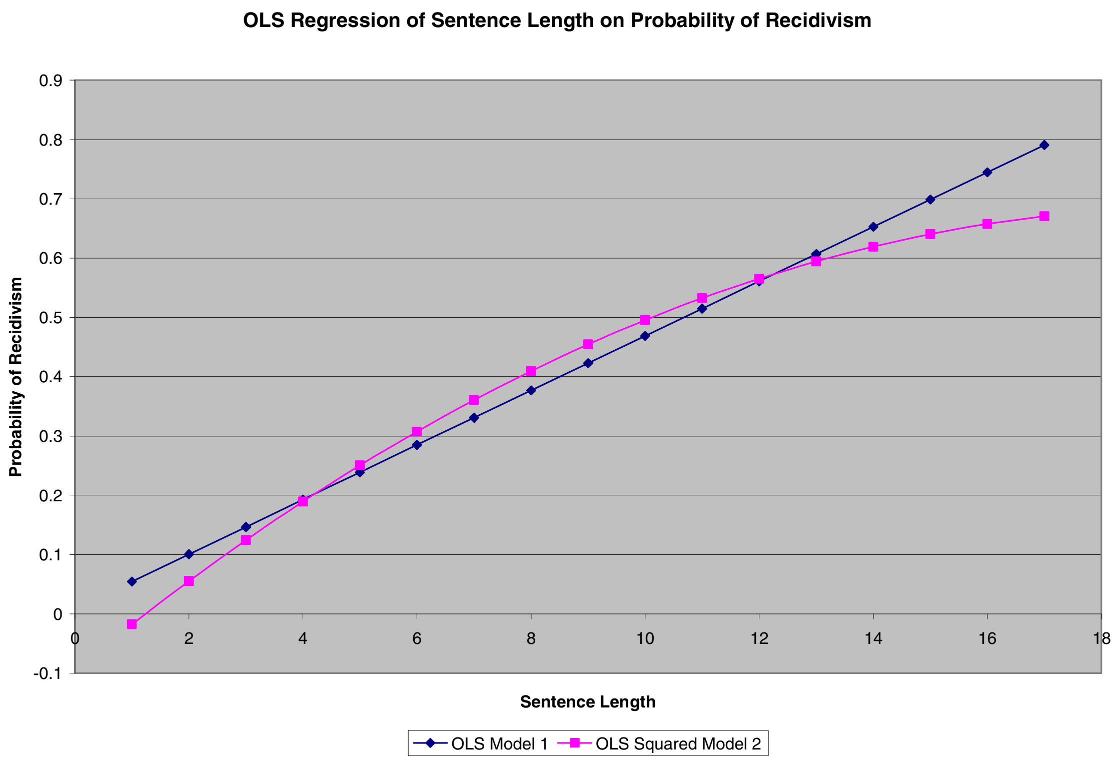
a. *** $p<.05$

b. ** $p<.10$

Table 7. Second Stage Instrumental Variables Estimation Results

Variables	***Model 3***[c]			***Model 4***[d]		
	b	***S.E.***	***Sig.***	***b***	***S.E.***	***Sig.***
Years Served (Predicted)	.028	.011	***	.042	.016	***
Years Served Squared (Predicted)	---	---	---	-.001	.000	
Min Sentence in Years	-.009	.005	**	-.009	.005	**
1991 Guidelines	.061	.031	**	.060	.031	**
1994 Guidelines	.114	.048	***	.121	.049	***
1997 Guidelines	.112	.074	**	.132	.065	***
Separated	.007	.024		.006	.024	
Divorced	.015	.020		.015	.019	
Widowed	-.030	.049		-.031	.048	
Single	.003	.013		.003	.012	
Unknown Marital Status	.031	.111		.062	.110	
Native American	.074	.141		.077	.150	
Asian	-.108	.061		-.103	.162	
Hispanic	.070	.019		.073	.151	
Black	.086	.012		.089	.150	
Other Race	-.008	.141		-.004	.213	
Male	.060	.031	**	.056	.032	**
Model R-squared	.0315			.0316		
Adjusted R-Squared	.0250			.0250		

Figure 3. OLS Regressions for Model 1 and Model 2[10]



[10] All other variables are held constant at the means in this and subsequent graphs.

Figure 4. IVE Regressions for Model 3 and Model 4

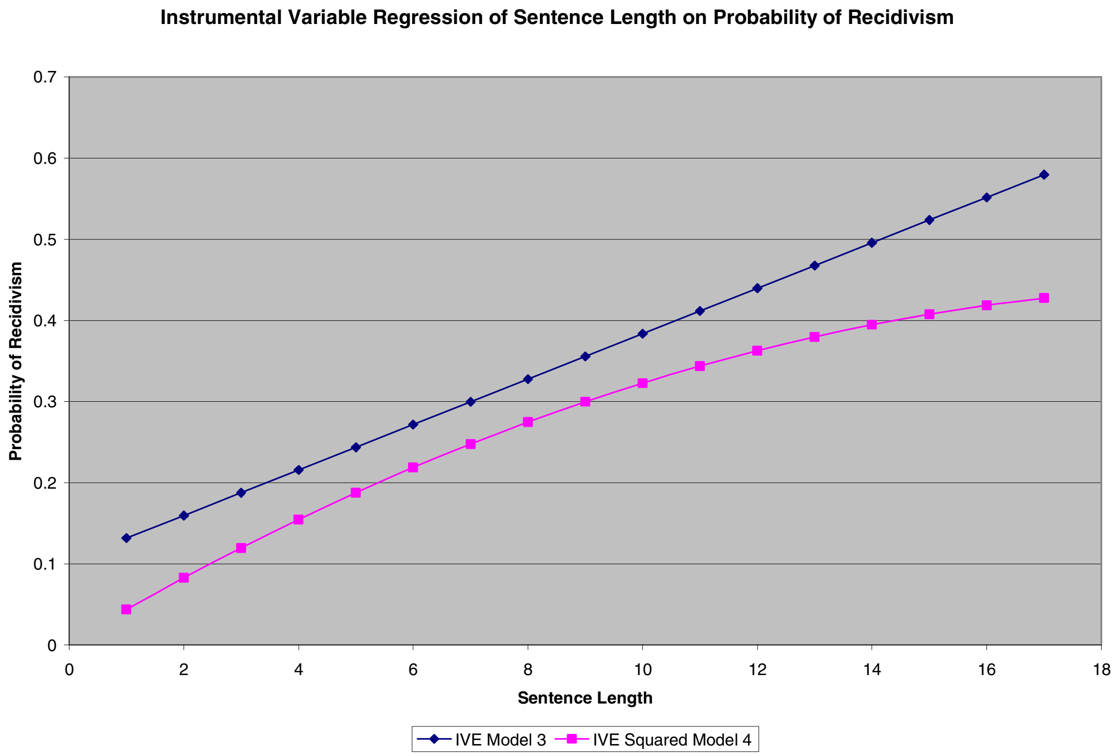
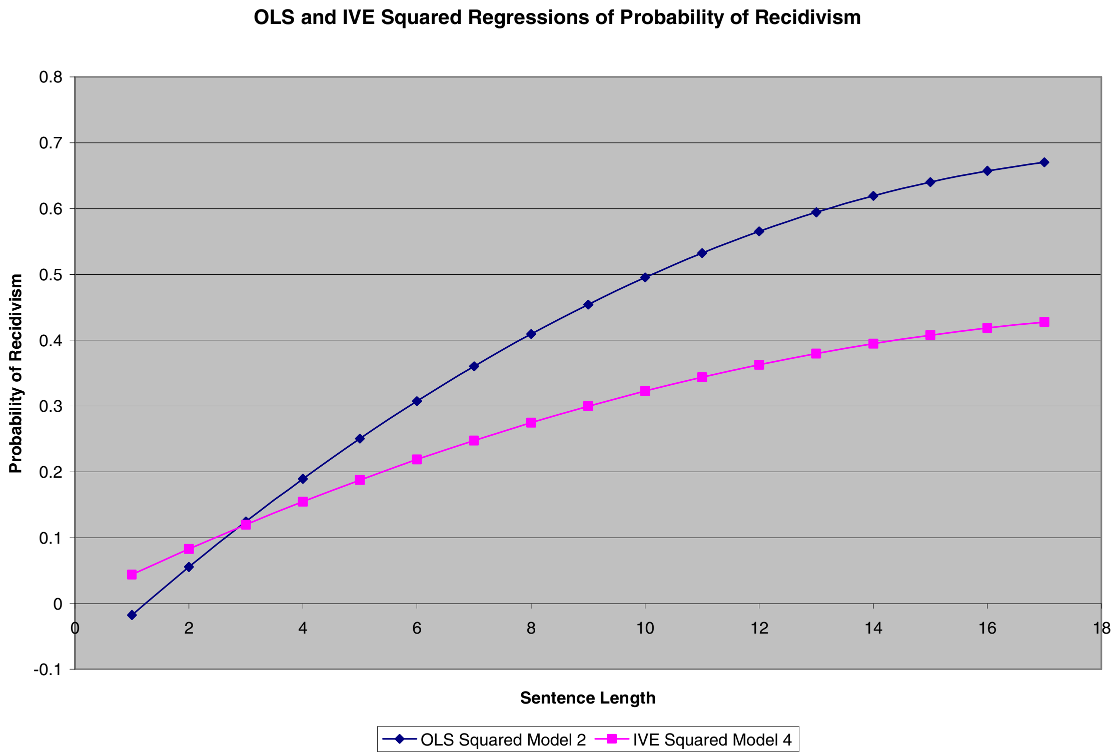


Figure 5. Comparison of Both Equations including the Squared Terms



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