ABSTRACT

Title of Document: SPECIFIC DETERRENCE REVISITED: A QUASI-EXPERIMENT ON SENTENCE SEVERITY AND RECIDIVISM

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This paper examines the utility of using a unique variation produced by a ruling in the Maryland sentencing guidelines as an instrumental variable in future research. The guidelines specify that at age 26 an offender's juvenile record is discounted from the calculations, resulting in a lower sentencing grid placement and a shorter sentence. I examine in depth the appropriateness of this treatment rule as an instrumental variable for research and find that it is an effective instrument. The study also examines preliminary results produced by using an instrumental variable to estimate a relationship between sentence severity and recidivism. The use of instrumental variables corrects for the selection bias present in other studies in this area by allowing me to compare individuals affected by the treatment rule. The results indicate a slight deterrent effect of imprisonment; overall, increasing sentence length decreases future rates of recidivism.

SPECIFIC DETERRENCE REVISITED: A QUASI-EXPERIMENT ON SENTENCE SEVERITY AND RECIDIVISM

By

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INTRODUCTION

One of the most enduring issues in criminal justice is the effect of sentence severity on recidivism. A primary issue in this debate is whether longer sentences increase or decrease future criminal behavior. Specific deterrence theorists argue that harsh sentences will deter an individual from committing further crimes upon release (Andenaes, 1968). Thus, more severe sentences will reduce individual and societal offense rates. Others (e.g. Petersilia, Turner, and Peterson, 1986; Spohn & Holleran, 2002; McGuire and Priestly, 1995), however, conclude that longer prison sentences will amplify future deviance. They claim that longer terms of incarceration will change an individual's social environment and negatively label offenders upon their return to society, resulting in negative reactions from others and the formation of a "deviant self image" (Farrington, 1977). Similarly, Sampson and Laub (1993) argue that longer prison sentences will increase offending by weakening conventional social bonds and negatively affecting future employment. At the same time, the most recent comprehensive literature review (Doob and Webster, 2003) suggests that no conclusive evidence exists to support any effect of sentence severity on recidivism. Similar reviews by Nagin (1978; 1998), Cook (1980), and Von Hirsch, Bottoms, Burney, and Wilkstrom (1999) support this view.

This issue is especially salient in light of the many developments in criminal sentencing that have occurred in this country throughout the past three decades. The number of people incarcerated has risen steadily and swiftly since the 1970's, only recently slowing. Currently, over two million people are incarcerated in federal, state and local institutions; this demonstrates an increase over previous years (Bureau of

Justice Statistics, 2005). In addition, recent policies have been enacted to make certain that the system remains "tough on crime". For example, mandatory minimum laws designate a minimum sentence length for all offenders of a specific crime. This requires that judges sentence at or above a certain level, ensuring that sentences will not be too lenient. Similarly, truth-in-sentencing laws enacted in the 1980's greatly limit early release; they require that offenders serve the majority of their sentence. Policy makers continue to increase penalties in an effort to reduce high crime levels.

The theoretical and practical implications of this issue necessitate further clarification of the incarceration-recidivism relationship. Despite the large amount of research produced in the past few decades, scholars remain unable to make clear and consistent conclusions about the effects of sentencing on recidivism. Increased severity has been found to increase, decrease, and not affect future rates of offending. The mixed literature results in large part from less than ideal research methods. Selection bias plagues research in this field. Though researchers have gone to great lengths to control and limit its effects, the selection artifact is still present in many studies. Considering the massive movement for incarceration, resolving this question is extremely important for both theoretical and political reasons. The nature of the issue requires further efforts to separate out the effects of selection bias.

In an attempt to examine the effects of sentence severity without the influence of selection, this study will serve as a preliminary analysis to determine the utility of utilizing a unique feature of the Maryland sentencing guidelines. The Maryland guidelines provide quasi-experimental conditions for assigning sentence length, with a treatment rule introducing a significant amount of exogenous variation, allowing for

a new approach the problem from a new angle. In the state of Maryland, offenders over the age of 26 do not receive any points for juvenile record. This places them in a lower sentencing grid (which would result in a lower sentence) than comparable offenders under this cut-off age.

I can use an instrumental variable estimation (IVE) to explore the use of the variation produced by the rule. Instrumental variable estimation can greatly eliminate many of the methodological weaknesses in earlier studies, specifically selection and omitted variable bias. It is an extremely useful, and remarkably underutilized, tool in research. Proper instrumental variables are very difficult to find in this field. I have found one such variable built into a treatment rule in the Maryland sentencing guidelines. This study purports to establish if this rule is, in fact, an appropriate instrument for the study of sentence severity and recidivism. In addition, I hope to provide preliminary evidence about the effect of sentence severity on recidivism without the influence of selection bias.

Thus, the purpose of my study is two-fold. I will first examine in depth the utility of the proposed instrumental variable to determine if it meets the necessary requirement and produces variation. Once this is established I will use the instrumental variable in a preliminary study to revisit the relationship between sentence severity and recidivism. This part of the study will determine if the presence of this guideline rule changes sentencing behavior. That is, if offenders over the age of 26 actually receive shorter sentences than equitable offenders who are 25 or younger. Then, using this information, I will determine if the different sentence lengths alter offender behavior in any way. While the main purpose of my study,

then, is to explicitly examine the effectiveness of the proposed instrumental variable, it will also investigate the unbiased effect of sentence severity on the probability of recidivism. Hopefully, this will bring additional attention to the issue of bias in research and inspire further investigations.

PART I. REVIEW OF THE LITERATURE

In spite of a large amount of empirical studies, criminological researchers have for the most part produced opposing and inconsistent conclusions about the effect of sentence length on recidivism. This section will discuss briefly the underlying theories as well as the existing literature on recidivism and specific deterrence.

RECIDIVISM

In order to fully understand deterrence and deterrence theory, it is important to first explicitly speak to recidivism. Researchers generally define recidivism as "the reversion of an individual to criminal behavior after he or she has been convicted of a prior offense, sentenced, and (presumably) corrected" (Maltz, 1984: 1). In other words, an offender recidivates if they commit an offense at any time after they have been sanctioned. The majority of offenders reoffend at some point after their punishment; most within the first few years of release (Beck and Shipley, 1987). Currently, approximately two-thirds of imprisoned offenders are reconvicted within six years of release (Bureau of Justice Statistics, 1994).

Unfortunately, recidivism can be extremely difficult to investigate. Researchers generally use the occurrence of a subsequent arrest or conviction to measure recidivism. Both of these measures have distinct advantages and disadvantages. Using arrest data to establish recidivism can often lead to an overestimation of events. For example, offenders who have already served time in prison may be monitored more closely upon release, leading to an inflated number of

arrests. Arrest estimates, therefore, may include a number of false or unjust arrests that would not truly indicate recidivism. Conviction data, on the other hand, provides a more conservative estimate of recidivism because it does not include falsely arrested offenders. At the same time, offenders who are actually guilty may escape conviction, which would bias this estimate in the opposite direction – it could become too conservative. In addition, both measures fall short of capturing a true estimate of recidivism because they are both official measures. Historically, official data is an inaccurate estimate of crime because it captures only those crimes known to the criminal justice system. There is a gap between the number of crimes actually committed and the number that are officially recorded (Mosher, Miether, and Phillips, 2002). Neither measure is perfect and researchers have produced conflicting opinions regarding the preferred, or more accurate, measure.

Furthermore, the existing research on recidivism can be hard to synthesize and interpret because scholars use a number of different definitions, research designs, and statistical techniques. Study results are often inconsistent and difficult to generalize. The findings do, however, consistently indicate a number of factors that consistently influence recidivism. Gender, race, age, and offense severity are the most commonly cited predictors. Gainey, Paine, and O'Toole (2000), for example, claim that sex strongly influences recidivism, with males more likely to reoffend than females. Race has also been cited as a strong predictor of future offending; non-white offenders, on average, have higher rates of recidivism than white offenders (Hepburn & Albonetti, 1994; Kruttschnitt, Uggen, and Shelton, 2000; Bushway and Piehl, 2001). This research is also supported by Greenberg (1991), who cites race, age, and gender as

the strongest predictors of recidivism. According to Greenberg, young minority males are the most likely to recidivate. A number of legal variables are also connected to recidivism. Offenders with criminal records reoffend more, on average, than offenders with no prior history of offense (Wooldredge, 1988; Hepburn & Albonetti, 1994). Further, as the number of prior convictions increase, so does the likelihood of rearrest (Wooldredge, 1988; Hepburn & Albonetti, 1994). In an examination of 1,949 juvenile males, Visher, Lattimore, and Linster (1991) determine that criminal history and instant offense are the two strongest predictors of recidivism. Offenders with the most serious prior history and the most severe current offense are more likely to recidivate upon release. Though researchers have identified a number of tangible factors that frequently affect recidivism, there are still unidentified influences.

A number of competing theories attempt to further explain the mechanisms that change the likelihood of recidivating. Significant factors cited in these theories generally center around the prison experience itself. Depending on how an offender experiences their time in prison it can make them either more or less likely to offend later. Deterrence theory, the main focus of this paper, argues that prison is a severe and uncomfortable experience for all offenders. Going through a period of incarceration, then, will decrease recidivism as offenders will try to not repeat the experience. Several other theories, however, imply that spending time in prison could alter an offender's social settings in a way that increases their likelihood to reoffend. These theories are discussed below.

DETERRENCE THEORY

Originally introduced in the eighteenth century by Cesare Beccaria (1963 [1764]), deterrence theory is greatly responsible for the creation and operation of the current criminal justice system. Deterrence theory has found long-lasting political and popular support and has greatly contributed to the increased use of incarceration in this country. The theory appeals to the population and to policy makers because it is logical, suggesting that individuals respond to swift, severe, and certain punishments (Beccaria, 1963 [1764]). Both policy makers and the general public perceive prison sanctions to be an effective deterrent of crime (Farrington, 1986). Thus, the use of severe sanctions as punishment for crime has been promoted for many years as an effective means to control crime (Wilks and Martinson, 1976).

The theory suggests that human beings act in a rational manner. Every action results from a careful weighing of the costs and benefits; individuals are most likely to do those things in which the perceived benefits outweigh the costs. Similarly, individuals will only commit crimes if they believe that the expected benefits are greater than the potential punishments. It follows that increasing the cost of crime (the punishment) will reduce the number of individuals committing crimes. Therefore, the theory suggests that the implementation of harsher sentences will result in lower rates of offending, all else being equal (Andenaes, 1968).

Deterrence theory is one of several theories responsible for the increase in prison population throughout the late twentieth century[1]. Policy makers have supported increasingly harsh sentences -- higher "costs" of crime -- in an effort to

[1] The theories of Just Deserts, Retribution, and Incapacitation have also been used as justification for the increased use of incarceration and harsh sanctioning.

increase the deterrent impact of punishment and decrease crime rates. Historically, these enhancements have resulted in determinate sentencing laws. More specifically, in the past three decades, policy makers across the country have introduced mandatory minimum laws, truth in sentencing laws, and "three strikes" laws (Bureau of Justice Assistance, 1996; Orsagh and Chen, 1988). This legislation has led to a corresponding increase in the incarceration rate and average sentence length (U.S. Sentencing Commission, 2004; Tonry, 1996). In light of these changes, it is important to closely examine the theory and its evidentiary support in order to establish if the increase in penalties has indeed reduced levels of crime and recidivism.

Deterrence theory can be divided into two areas of focus: general and specific. These two types of deterrence imply widely different mechanisms. General deterrence examines the effect of punishments on the overall population offense rate; in other words, how the punishment of an individual can change the perceived costs of the population. The research on general deterrence has produced consistently pessimistic results (see Nagin, 1978 for a review). Early reviews by Blumstein, Cohen, and Nagin (1978) and Cook (1980) do not provide convincing evidence of a deterrent effect of harsh punishments. More recently, Nagin (1998) and Von Hirsch et al. (1999) reached similar conclusions, demonstrating that the "sheer accumulation of studies over the past two to three decades have not supported the notion that variation in sentence severity affects crime" (Doob and Webster, 2001: 152). Despite large and enduring public support, general deterrence has received limited empirical support.

Though the research generated about general deterrence is pessimistic as a whole, it cannot speak to the effectiveness of deterrence on an individual level. Specific deterrence "is concerned only with individuals who have been punished for crimes and the effects of actual punishment" (Bridges and Stone, 1986: 208), and focuses on the future offending of a specific offender after they have been sanctioned. It is expected that "individuals who have suffered a punishment for a type of crime are deterred from future offending" (Gibbs, 1975: 34). In other words, once punished, individuals "will cease offending, commit less serious offenses, or offend at a lower rate because of the fear of some future sanction" (Paternoster & Piquero, 1995: 251). Punitive experiences make the consequences of crime more salient to offenders. Therefore, more severe punishments will create a greater perception of threat and increase the perceived costs of crime. Deterrence theory suggests, then, that harsher punishments will decrease the likelihood of future recidivism of offenders, and lower both the individual and societal crime rate. Long periods of incarceration will discourage released offenders from committing additional crimes (Blumstein, Cohen, & Nagin, 1978).

ALTERNATE THEORETICAL EXPLANATIONS ABOUT THE EFFECTS OF PRISON

Several other theoretical frameworks can also be used to explain the relationship between recidivism and sentence severity. In contrast to deterrence theory, some empirical studies hold that more severe sentences actually increase later recidivism. Researchers argue that "the prison experience may be criminogenic in itself" (Spohn & Holleran, 2002: 351), implying that greater exposure to prison would

actually increase the probability of later offending. Furthermore, interaction with the criminal justice system may reduce or limit an offender's legitimate opportunities (Bridges and Stone, 1986; Farrington Ohlin, and Wilson, 1986; Sampson and Laub, 1993), which in turn increases the likelihood of reoffending (Waring, Weisburd, and Chayet, 1994). Much research supports the idea of prison serving to amplify existing criminal behaviors; two of these theories will be discusses in brief below. While these theories are not the main focus of the study, they will be presented briefly to provide a more complete picture of recidivism.

Prison Socialization

Prisonization is one of the earliest attempts to explain increased recidivism after punishment. The idea of prison socialization argues that correctional institutions often serve as "schools of crime". Increased interaction with criminals can serve to alter an offender's view of society and further inform an offender about crime. Clemmer (1950) used the term "prisonization" to describe the process of being socialized into the norms of prison life. That is, an inmate becomes "a product of patterns of social interaction which [he] enters into day after day" (Sykes, 1958: 134). A correctional center operates as "a complex social system with its own norms, values, and methods of control" (Sykes, 1958: 134); upon entering the facility, an inmate must replace his own norms and values with those of the prison. The offender is no longer an autonomous individual, but a prisoner.

Thus, incarceration can resocialize an offender, causing them to develop a new view of themselves and society. Upon release, it can be difficult to readjust to the rules of mainstream society. Many offenders are so immersed in their criminal

identity and the criminal culture of the prison that they maintain this lifestyle upon release (Clemmer, 1950).

Labeling Theory

Incarceration may affect recidivism not only through the socialization process, but also through the challenges inmates face upon reentry. Labeling theorists (e.g. Lemert, 1951; Becker, 1963) argue that social reactions to sanctioning may limit the ability of an offender to establish a conventional post-release lifestyle. Incarcerated offenders may be negatively labeled as "deviant" upon their reentry into society. Societal labeling often leads to a change in the individual's and the formation of a "deviant self image" (Farrington, 1977). Rejection from conventional society can cause an offender to seek out others with a similar self image. Association with other criminals only amplifies negative labeling, resulting in a reduction of opportunities (Tittle, 1975). Therefore, offenders who have been officially labeled by incarceration are more likely to reoffend than offenders who have not been processed through the criminal justice system, a process commonly referred to as "secondary deviance." Labeling can also exercise a more direct effect upon the legitimate opportunities available to offenders. Individuals with a criminal history may be unable to obtain housing and gainful employment, making it difficult to conform to the demands of conventional society. Thus, offenders often revert to crime.

Since all offenders in this study have, by definition, been processed through the criminal justice system (and labeled) labeling theory may not seem as applicable as deterrence theory. However, there are also differential effects of labeling and stigma for offenders on probation, in jail, and in prison that must be considered.

Labeling theory then applies to all offenders, but to a different extent. Therefore, while the effects of labeling theory may not seem immediately prevalent in the present population, it is still important to consider this theory.

RESEARCH INVESTIGATING RECIDIVISM AND SPECIFIC DETERRENCE

The Gluecks conducted one of the earliest investigations of specific deterrence. In their 500 Criminal Careers study they followed a sample of 510 juvenile offenders for five to 15 years after their release from reformatory school, and collected information on post-release behavior as well as a number of other social characteristics (Glueck and Glueck, 1930). They found that 80 percent of the boys recidivated at some point after leaving the school, leading them to conclude that the reformatory neither reformed nor deterred the offenders. Though the methods and statistics used in the Glueck study have been criticized[2], it serves as an important step in deterrence research. Further research in the area of specific deterrence did not begin in earnest until the 1960's, when researchers determined a way to analyze it empirically.

Still, much of the recent deterrence literature focuses on the mechanisms of general deterrence (see Nagin, 1998 and Von Hirsch et al., 1999 for a review), with only a few studies examining the impact of punishment on the individual offenders. Furthermore, the research available on specific deterrence does not often focus explicitly on the impact of sentence length on recidivism. Rather, researchers examine how the implementation of sanctions will affect an offender's *perception* of

[2] Though the Glueck study offers invaluable information about recidivism and the effects of criminal offending, they do not use a control, or comparison, group.

future punishment (Pogarsky and Piquero, 2003; Bridges and Stone, 1986) or, similarly, how these changes may affect personal *predictions* and *intentions* of committing further crime (Piquero and Paternoster, 1998; Piquero and Pogarsky, 2002). Those studies that do focus on criminal offending usually compare recidivism by type of offender (Dejong, 1997), or, more commonly, compare rates of reoffense between offenders receiving different types of sanctions (Spohn and Holleran, 2002; Babst and Mannering, 1965; Gray, 1994). Still other studies compare the rates of offenders sentenced to alternative sanctions to those of incarcerees (McCorkle, Elias, and Bixby, 1957; Gottfredson and Barton, 1993; Stevenson and Scarpitti, 1967; Smith and Gartin, 1989; Smith and Paternoster, 1990). Very few studies look specifically at the effect of differential amounts of time spent in prison. Further, regardless of the method used, almost no empirical studies to date have presented convincing evidence of a deterrence effect of sanction severity.

Many of these studies and results have been criticized for weak research designs. The main problem with most of the specific deterrence research on the effects of incarceration is the use of research designs that allow for the presence of selection bias. Much of the early research in the area of deterrence was simply a comparison of groups who received different types of treatment in the criminal justice system (see Wilkens, 1969 and Lipton, Martinson, and Wilks, 1975 for early reviews). These studies did not take into account that assignment to groups is a nonrandom process in which the worst offenders receive the harshest penalties. Therefore, these groups would be more likely to reoffend even in the absence of a relationship between sentence severity and recidivism. As a result of this bias, early

studies frequently indicate a positive relationship between sentence severity and offending. They find that more severe sanctions were often associated with higher rates of recidivism, a result frequently interpreted as support for labeling theories.

More recent research attempts to reduce the effect of selection by using more sophisticated methods. Research designs in this area can be separated into two main types: comparing matched groups receiving different treatments, or comparing two separate groups while statistically controlling for, or holding constant, other variables that may influence recidivism. Despite the different approaches, these studies have produced similar results. This section will briefly review the results garnered by these studies, as well as the strengths and weaknesses of past analyses.

Matched Designs

Matched design studies require that researchers compare two separate groups of offenders while "matching" individuals across these groups on important characteristics. This method attempts to make the groups as equitable as possible to add validity to the findings. It is, essentially, a simplistic method of controlling for individual differences. Much of the early work in deterrence research utilizes matched designs. These studies have been greatly criticized for matching on too few characteristics, or on irrelevant characteristics. Though limitations in matching greatly restrict the validity and generalizability of the results, no studies of this type indicate a strong deterrent effect of punishment.

For example, studies comparing imprisonment to alternative sanctions find no evidence of deterrence. In one of the pioneer studies in specific deterrence Babst and Mannering (1951) compared matched offenders sentenced to either probation or

incarceration. They matched offenders on crime type, county of commitment, number of prior felonies, and marital status. They find that probationers were less likely to recidivate than comparable parolees. However, their results must be interpreted with caution; they are only able to analyze a small number of their original sample due to problems with the matching process.

Similarly, Bartell and Winfree, Jr. (1977) evaluated prison sentences and probation and find that, after controlling for differences in age, criminal history, and type of offense, offenders who received sentences of probation were significantly less likely to recidivate than those who were sentenced to terms of imprisonment. Though the study has been criticized for poorly matching its subjects, it clearly indicates the positive effect of increased penalties on recidivism.

Similar results for deterrence are also found when researchers compare the effects of different sentence lengths. Gottfredson, Gottfredson, and Garofalo's 1977 study separated over 5000 offenders into "risk categories" by age, offense, prior history, and substance use. Though the results are mixed, researchers conclude that, overall, increased length of sentence did not reduce recidivism. Recidivism rates were either increased or unaffected by longer sentences (Gottfredson et al., 1977).

Gottfredson, Neithercutt, Nuffield, and O'Leary (1973) are among only a few researchers who find any evidence of a deterrent effect of imprisonment using a matched design. Their results indicate varied effects on recidivism. Gottfredson et al. compared the effects of sentence length for over 100,000 male prisoners in the United States between 1965 and 1970. Their one year follow-up indicated that, while on parole, offenders with the longest sentence lengths were less likely to recidivate

than those with shorter time served (Gottfredson et al., 1973). However, these results varied significantly across the sample of offenders. In fact, the researchers suggest a curvilinear relationship between sentence length and recidivism. They found that an increase in time served was positively related to recidivism for a sentence of up to about 50 months; above 50 months, the recidivism rate began to decrease. Further, when the authors separated the offenders into two groups by a "Predicted Attribute Analysis", their results indicate a very complex relationship. The different groups of offenders responded differently to time served. Some groups showed increased recidivism with time served, while others demonstrated a negative relationship (Gottfredson, et al., 1977). While the results reveal a complicated sentence length-deterrence relationship, it is important to note that they have not been successfully replicated by other studies.

For the most part, matched design studies indicate a positive or nonexistent relationship between sentence severity and recidivism. However, a number of concerns about the validity of this design require that the results be interpreted cautiously. In order to create a strong design, researchers must match on relevant variables and incorporate as many variables as possible. As Smith and Paternoster (1990) explain, frequently "matching techniques control for too few relevant variables" (1112), which allows for the influence of omitted variable bias. It is often difficult to match several pairs on a number of variables. As a result, many matched studies include only a small sample of offenders or match participants on variables that are not entirely relevant to the investigation. Therefore, while matching designs are an improvement over uncontrolled studies, they do not eliminate selection bias.

Use of Statistical Controls

With the development of more sophisticated techniques, recent studies have been able to statistically control for a number of variables at one time. In recent years, researchers have utilized multivariate statistical models to reduce bias. These models allow for estimation of the direct effects of sentence length while controlling for other variables that are thought to influence recidivism. This approach allows researchers to hold several variables constant so the effects of the individual variable of interest can be isolated.

Weisburd, Waring, and Chayet (1995) conducted one such study. The researchers directed a sophisticated examination of the effect of prison versus alternative sanctions. They studied a group of white collar offenders convicted in seven districts across the United States in the late seventies. Approximately one-half of the entire sample received a prison sentence. The authors used a 10.5 year follow-up period to examine the differences in recidivism between the prison and non-prison groups. Their research again indicates a positive relationship; harsher sentences (prison) do not exercise a deterrent effect on offenders.

Spohn and Holleran (2002) make perhaps the best attempt to control for selection bias. The researchers examined recidivism rates of offenders placed on probation and offenders sentenced to incarceration. Specifically, they followed 1,077 Jackson County, Missouri offenders who were convicted in 1993, and compared probation and imprisonment for drug offenders, drug-involved offenders, and non-drug offenders. In an attempt to correct for earlier limitations the authors controlled for a number of additional background and legal characteristics, including criminal history, gender, race, age, and employment status.

Furthermore, the researchers used three different definitions of recidivism -- whether the offender was charged with a new offense, was convicted of a new offense, or was sentenced to prison for a new offense. Their results demonstrate that offenders who are sentenced to prison not only have higher rates of recidivism across all three measures, but recidivate more quickly than offenders who receive probation (Spohn and Holleran, 2002).

Orsagh and Chen (1988) use similar statistical techniques to examine the effect of different lengths of incarceration. Their study attempts to determine an optimum sentence length for minimizing recidivism. They followed 1,425 offenders for two years after release, finding a nonlinear relationship between the two variables. They concluded that time served does, in fact, have a direct effect on recidivism; however, the direction of this effect varies across offense type. Therefore, some offenses would benefit from longer terms of incarceration, but others require shorter terms. They caution that "the recidivistic effect of longer sentences is complex and is likely to be offender specific" (Orsagh and Chen, 1988: 165). Their results indicate a very complex relationship that neither fully supports nor refutes deterrence theory.

The use of statistical controls in the above studies limits the influence of selection bias in the results. However, while these results may consistently present compelling evidence that there is not a deterrent effect of incarceration, they also must be interpreted with limitations in mind. Even though the authors make a strong attempt to control for as much variation as possible, some of the variables that correlate with both sentence length and recidivism may be omitted from the analyses. Statistical equations control only for those variables specified by the researchers.

Thus, equations are limited by available information and researcher discretion; while researchers are able to control for some variation, they often fail to include other important variables. In fact, in many cases, researchers do not have access to all relevant variables. Omitting key variables can produce a biased and inconsistent estimation of the equation. Therefore, even carefully controlled studies may still allow for incomplete or incorrect estimations (Smith and Paternoster, 1990). Thus, the effect of sentence length on recidivism may still be influenced by variables not included in the analysis. The presence of selection bias or omitted variable bias can have extreme consequences for the results and conclusions of deterrence research.

Conclusions from Specific Deterrence Research

Overall, the results of deterrence research are varied and inconsistent. Most studies find positive effects or no effects at all, suggesting that sentence severity does not have a deterrent effect on offenders. A meta-analysis by Gendreau, Goggin, and Cullen (1999) determined that, on average, recidivism rates of individuals sentenced to prison were seven percent higher than those of individuals sentenced to other methods of punishment. Similarly, when analyzing the differential impact of time spent in prison, the authors claim that spending more time in prison resulted in increased recidivism rates for twenty of the twenty-seven studies included (Gendreau et al., 1999). These results show that the majority of empirical data indicates that sentence severity is positively related to later recidivism.

WEAKNESSES IN PAST ANALYSIS

For the most part, results of specific deterrence studies have been inconclusive. One possible reason for these inconsistent results is that the link

between sentence severity and recidivism is extremely difficult to study. Most studies suffer from at least one methodological flaw; most commonly, selection bias or omitted variable bias.

As a whole, specific deterrence studies inherently suffer from some amount of selection bias. The probability of receiving a severe sentence is not entirely random (Smith and Paternoster, 1990). Rather, offenders who receive the most severe sentences are likely inherently different in some way from offenders who receive less severe sentences. Incarceration and sentence length decisions are determined, in part, through an evaluation of risk. As Spohn and Holleran (2002) point out, "judges' determination of appropriate punishment reflect in part their assessments of the offender's dangerousness and risk of recidivism" (p. 340), implying that offenders who pose the greatest threat to society will generally receive the most severe sentences (Spohn and Holleran, 2004). Thus, the factors that are most strongly related to recidivism are the same as those correlated with sentence length (Spohn & Holleran, 2002). These offenders possess qualities found to influence both incarceration length and risk of recidivism, making it difficult to separate the true effects of sentence severity.

Further, many variables not included in the study can also correlate with future offending. Omitting relevant variables can lead to biased and inconsistent results. Smith and Paternoster (1990) demonstrate from a statistical view the impact that selection bias can impose on results. They argue that failure to recognize or control for selection bias "can lead to substantial bias in the estimated effect" (Smith and Paternoster, 1990: 1129).

Though, ideally, random assignment of subjects to treatment can ensure that the groups will be without uncontrolled variation, obvious ethical issues prevent controlling for these factors through random assignment of sentence (Zimring & Hawkins, 1973; see also Farrington, 1983). These constraints prevent the random allocation of prison sentences, resulting in the inability of researchers to make clear and unambiguous conclusions regarding prison sanctions and recidivism (Farrington, 1983).

Furthermore, as Smith and Paternoster (1990) demonstrate, the use of inappropriate methods of control can produce very different results. Smith and Paternoster run several regressions on their data that do not account for selection. From these results they determine, similar to much other research, a positive relationship between referral to court and recidivism. However, using an instrumental variable estimation (IVE) on the same dataset, results in a completely different relationship. When utilizing an IVE, the authors find a negative and significant relationship (Smith and Paternoster, 1990). Therefore, it is imperative that selection bias be taken into account in criminological research.

ADDRESSING THE PROBLEMS OF PAST RESEARCH

The current body of research presents inconsistent and controversial conclusions about the deterrent effect of harsh punishment. While researchers have limited the influence of selection bias, few studies have used random variation or experimental conditions to examine sentence severity and recidivism.

One way to estimate the relationship between incarceration and recidivism without much of the selection bias which weakens past research is through the use of

instrumental variable estimation (IVE). IVE capitalizes upon exogenous variation in an independent variable produced by a third (instrumental) variable that is related to the dependent variable of interest only through its effect on the independent variable. The variation can be used to correct for selection bias and omitted variable bias, and to isolate the direct effect of the independent variable on the dependent variable.

Unfortunately, good instrumental variables can be extremely difficult to find. In this case, I have found instrumental variation that exists due to a specific treatment ruling in the Maryland guidelines. These guidelines, described in detail below, consist of a sentencing grid to determine punishment based on two elements: severity of offense and prior criminal history. At the age of 26, an offender's juvenile record is excluded from his criminal history, reducing his score in this category by one or two points. Thus, offenders who are otherwise very similar are assigned to different sentence length by age. I propose that this variation can be used to simulate quasi-experimental conditions by comparing similar individuals who randomly fall on either side of this age cut-off. In the following sections I examine the treatment rule in depth and determine the appropriateness of this variable as it applies to IVE. I argue that it is a fitting instrumental variable that could be very useful in future research. The methodology and variables used to achieve this design will be discussed in detail below.

PART II. DATA AND METHODOLOGY

BACKGROUND OF THE MARYLAND SENTENCING GUIDELINES

The Maryland sentencing guidelines have been in place since 1983 (www.msccsp.org). Originally developed as a method for decreasing unwanted racial disparity in sentencing (personal communication, Charles Wellford, 26 Oct. 2004), the guidelines are voluntary and intended only to guide sentencing decisions. Judges follow the guidelines fairly closely, however; almost 70 percent of sentences fall within the prescribed range[3] (www.msccsp.org/publications/ar2005.pdf).

Designed to take characteristics of both the offender and the instant offense into account, the recommended sentence range is determined through the intersection of the offender's past criminal history ("offender score") on the x-axis and the severity of their current crime ("offense score") on the y-axis (www.msccsp.org). Currently, the guidelines contain three separate sentencing matrices for personal, drug, and property offenses[4]. These are examined in more detail below.

[3] Compliance rates are measured slightly differently by Maryland compared to other states. In 2001, the Maryland Sentencing Commission voted to treat sentences that departed from the guidelines in favor of other correctional alternative (e.g. home detention, substance abuse treatment, etc.), or those pursuant to American Bar Association plea agreements as compliant. Since these rules were put in place in 2001, the compliance rate has, on average, been over 65 percent. Before this time compliance rates were approximately 45 percent, on average (http://msccsp.org/publications/ar2005.pdf).

[4] All guideline matrices and relevant information are included in Appendices 1-3. Further information- including instruction on the calculations and the worksheets- can be found on the Maryland State Commission in Criminal Sentencing Policies website.

Offense Score

An offense score is computed to evaluate the severity of each punishable offense. The calculation of the offense score begins with a numerical "base" score -- a predetermined seriousness category of the current crime. For drug and property crimes, this "base" score serves as the entire offense calculation. Depending on the type of offense, individuals receive points that place them in severity categories ranging from II (most serious)[5] to VII (least serious). Scores for person crimes, however, can be enhanced through a number of factors about the victim.

Calculating Person Crimes. Similar to drug and property crimes, each person offense receives a numerical base score of severity determined by the seriousness of the offense. For person crimes, these range from Category I (most serious) to Category VII (least serious). Each of these categories corresponds to a specific amount of points (See Appendix 1). These points serve as the "base" severity score. This numerical base score can be increased through the existence of three aggravating factors: victim injury, use of a weapon, and special victim characteristics. Harm inflicted on the victim applies to psychological and physical -- but not monetary -- consequences that can be directly linked to the offender. A score for victim injury is either zero (no notable harm), one (non-permanent injury), or two (permanent harm or death). Weapon usage includes "any article or device that reasonably appears capable of causing injury" (www.msccsp.org). Weapons other than firearms increase the score by one point; the use of firearms or explosives increases the offense score by two points. The offense severity score also takes into account characteristics of the

[5] Currently, the Maryland guidelines do not designate any drug or property offenses as Category I. Additionally, the drug grid does not specify any category VI offenses. Thus, these categories will be excluded from the analyses that follow.

victim which make him or her especially vulnerable. A victim who is younger than eleven, older than sixty-five, or physically or mentally handicapped in any way is considered vulnerable. An offender receives one additional point for a vulnerable victim. The overall offense score for person crimes, then, can range from one to fifteen points (www.mscccsp.org).

Offender Score

The offender score evaluates the offender's entire criminal history (see Appendix 2). Their overall prior record of offenses is condensed into a scale ranging from one to five. Offenders receive one point for a "minor" record, three for a "moderate" record, and five for a "major" record of prior adult convictions. These records are determined by the number and severity of prior offenses. The offender receives one additional point if they were under the supervision of the criminal justice system at the time of their instant offense. Similarly, they can receive an additional point if they have a past record of violating parole or probation. A record of juvenile delinquency is also included in the calculations. According to the state guidelines, "two or more findings of a delinquent act, or one commitment" as a juvenile (17 or younger in Maryland) results in one additional point on the offender score. An offender who was "committed two or more times" receives a two-point increase. At the age of 26, however, an offender's juvenile record is no longer included in the offense score; they receive no additional points for juvenile history, regardless of the number of offenses they may have committed during this time (www.msccsp.org).

Unique Feature of the Maryland Guidelines

Juvenile offenses have a strong impact on the sentencing range calculation, increasing the offender score by as much as two points depending on the severity of the juvenile record. Until recently, the guidelines dictated that juvenile offenses be dropped from an individual's offender score at the age of 26[6]. Consequently, offenders over the age of 26 with a juvenile record experience a significant decrease in offender score, and a corresponding drop in sentence length, due simply to age. Removing delinquency points from an offender's record can have a strong impact on sentence length. Dropping one or two delinquency points from an offender's record will decrease their sentence anywhere from three months to several years. Thus, the guidelines set up a quasi-experiment by assigning sentence length by age, allowing a unique approach the problem.

This study capitalizes upon the exogenous variation introduced by the discontinuity around the age cut-off. The overall goal of the study is prove the effectiveness of the instrumental variable and to then use the quasi-experimental conditions produced to revisit the severity-recidivism issue. In doing so, it will address several main points. First, the study will address the requirements that an instrumental variable must meet to establish if the treatment rule could serve as an instrument in future studies. Second, the study will determine if, in fact, the treatment rule results in significantly longer sentence lengths for those offenders under 26. I will then use this information to compare the recidivism rates of those who fall below the cut-off to those who fall above, controlling for type of offense and a number of

[6] In 2002 the sentencing commission changed this age to 23.

legal and extralegal variables. This study will serve as an initial investigation into the use of the instrument, and the use of IVE in this area of research.

SAMPLE

My sample is drawn from data collected by the Maryland Sentencing Commission from 1999-2005. In 1983 the state legislature mandated that judges complete and submit a separate sentencing guidelines worksheet for each criminal case originating in the circuit court. The worksheet is intended to collect information on judicial departure rates for the sentencing guidelines; judges provide information on both sentence length and applicable departures. In addition to these items, the commission also gathers information on offender demographics identifying details of the case, guidelines scores, and victim information. My data, a compilation of these worksheets, contains information for all cases originating in the circuit court from 1999 through 2005 in which incarceration was an option.

Of the total number of offenders (N=95,897), I will use only a small subset for my study. The subset includes only those individuals whose initial recorded offense[7] occurred before 2002. Cases sentenced after this date are excluded for two reasons: first, the guidelines in Maryland were changed in 2002, lowering the cut-off age to 23; also, limiting the data to individuals first sentenced before 2002 allows for a sufficient follow-up period for tracking recidivism.

Additionally, as the short follow-up period introduces problems with right hand censoring, I truncate the sentence length at 36 months. Offenders sentenced to

[7] First sentence in this study refers to the first offense that is contained in the dataset; in other words, the first offense occurring after January 1, 1999. Data recorded by the commission before this date is very difficult to synthesize, and is not included in this study. This is not meant to imply that it is, in fact, the first offense ever committed by these offenders.

more than 36 months will not be included in the study. By using this cut-off I hope to include a follow-up on as many people as possible[8]. Looking at the sentence length distribution, this does not seem to be a huge limitation. The median sentence length of the entire sample is 36 months. Therefore, the majority of the offenders included in the original dataset were sentenced for fewer than three years for their first offense; truncating sentence length at this point reduces the overall dataset by about 50 percent. Truncating sentence length, however, also results in the exclusion of more serious offenders. Offenders who fall at the upper ends of the sentencing grids will receive sentences over 36 months. Serious offenders are not dropped systematically by offense severity; rather, their exclusion is a byproduct of truncating sentence length.

The data is further limited because I want to examine only the most immediate effects of the age cut-off. Therefore, the study focuses only on offenders who were 24-27 at the time of the initial recorded offense. I will look at only those offenders who are directly above and below the cut-off age, comparing 24 and 25 year olds to 26 and 27 year olds, to reduce dissimilarities due to age. For example, Stefensmeier et al. (1995) caution that age is curvilinearly related to sentence length. They claim that offenders between 21 and 29 receive harsher punishments than those on either side of this age group. However, they point out that sentence lengths within this age group do not differ significantly by age from each other (Stefensmeier et al., 1995). Including ages much below 24 or much above 27 could bias the estimations.

[8] Using a three year cut-off also ensures that I will get some follow-up on almost all offenders included in the study. Assuming offenders serve about 50 percent of their sentence, and using only initial offenses occurring before 2002, truncating sentence length at 36 months provides sufficient time for most of these offenders to serve their sentence and still have follow-up.

The final subset, then, is composed of 24-27 year-old offenders with an initial recorded offense occurring between 1999 and 2001 and an initial sentence of less than 36 months (N=4963). Three hundred eighty-eight of the 24-25 age group included in this subset are directly affected by the guideline (i.e. have a juvenile record)[9]. Of the final sample, 35.18 percent were convicted of a person crime for their initial offense; 52.18 for a drug crime; and 12.64 for a property crime. The overall recidivism rate for the sample is approximately 18 percent.

Characteristics of the Sample

This study is based on the idea that the sample of individuals within a very small interval around the age cut-off point is very similar to a randomized experiment at the cut-off point; the offenders share essentially the same characteristics and assignment scores. Table 1 (below) indicates that the two groups contain individuals who are very similar in a number of background characteristics, including race, ethnicity, gender, and current type of crime. The offenders are not significantly different on any background characteristics. In fact, the two groups are alike on almost all background measures; the test statistics vary very little from zero. The racial composure of both groups is almost identical, with blacks comprising the vast majority (close to 75 percent) of both groups. The distribution of crimes committed is also equivalent across groups. Just over half of the offenders in each group were initially arrested for drug charges, with about 36 percent charged with violent person

[9] As 26-27 year old offenders automatically receive a zero for juvenile history, I cannot determine the total number of offenders in the study who possess a juvenile record. I posit that approximately the same number of 26-27 year old offender with juvenile records would be approximately equal to the 24-25 year-olds. If I had access to the juvenile records of 26-27 year old offenders, about 776 offenders would have a record, or close to 16 percent of the final sample.

crimes and only a small number (about 13 percent) committing a property crime. Characteristics of the current crime are also equivalent. The groups differ significantly only on the measure of victim vulnerability. The older group of offenders has a slightly higher proportion of crimes with vulnerable victims, compared to 24-25 year-olds (.06 versus .09, respectively). Aside from this one measure, the two groups show no significant differences.

Similarly, the groups are significantly different on only a few measures of criminal history (offender score). Table 2 displays the background and current offense characteristics of each group. The table contains the proportion of offenders in each group that fall into each category. The two age groups are comparable on all measures of criminal history. Both groups have approximately the same distribution of prior offense record. The younger group has marginally more offenders with minor criminal records; however, 26-27 year olds have a significantly higher proportion of major criminal records. Only about 30 percent of each group was currently involved with the criminal justice system (i.e. on parole or probation, or involved in a pending case). It is possible that this result is a simple artifact of their age. The older group of offenders has had more time to commit crimes. Thus, their criminal records are, on average, more serious than those of younger offenders. The older group of offenders is also significantly more likely to be under parole/ probation than the younger group. This is not surprising given their more serious prior records. Having more serious records makes them more likely to be serving a sentence at the time of the initial offense. Also, just over one quarter of each sample has previously violated terms of probation or parole.

This provides support for the basic principle of the study. Twenty-four and 25 year olds and 26-27 year olds are essentially the same on almost all background characteristics. Using only a small subset of offenders immediately affected by the guidelines limits the differences between groups and, thus, closer approximates experimental conditions. Thus, variation in sentence length is more likely to be due to differences introduced by the treatment rule.

VARIABLES OF INTEREST

Dependent Variables

This study contains two main dependent variables of interest. The *sentence length* variable represents the total number of months an offender received during sentencing. It is coded to include offenders sentenced to a term of incarceration in jail or prison as well as those sentenced to probation; offenders who receive home confinement or suspended sentences will not be included in the analysis. The mean sentence length is 12.21 months.

Recidivism will be measured simply as whether an offender is reconvicted in the Maryland circuit court system within three years after release. In order to be counted as a recidivist, an offender must have been reconvicted in the circuit court of Maryland for a crime in which incarceration was a sanctioning option[10]. If offenders reenter the system within three years of release, they are tagged as a recidivist (*recidivism*=1). For the purposes of this study, offenders who are not reconvicted in

[10] Using conviction, rather than arrest, is a more conservative measure of recidivism. It minimizes Type I errors produced by using arrest as an indicator. However, it introduces more Type II errors – some offenders that do recidivate will not be excluded from the data if they are not officially charged or convicted. See Spohn and Holleran (2002) for a review of recidivism measures.

Maryland are not considered recidivists (*recidivism*=0). This variable is treated as a dummy variable.

Offenders sentenced to probation will be considered recidivists if they reoffend within three years after their initial sentencing date. For offenders sentenced to jail or prison, however, recidivism will be measured within three years of their estimated release date (calculated using an estimation of time served). Release date is determined by adding the time served estimate to the original disposition date (release = sentence date + number of days served).

Since actual time served by the offenders is not available, I will create an estimate of this measure to be used in the calculations. According to the Maryland Sentencing Commission, offenders, on average, serve about 50 percent of their assigned sentence, though it varies by type of offense and length of sentence (see Table 3) (Wellford and Souryal, 1998). Thus, given this information, the expected time served will approximate the time each individual would remain in prison. In other words, the assigned sentence length can be multiplied by the percent of the sentence the offender would be expected to serve. This allows me to calculate an offender specific estimate and an approximate release date and, thus, control for exposure time and calculate time until next offense. A sensitivity analysis is performed to determine the resulting impact of using this estimate and is discussed at length below.

Of the 4963 offenders included in the subset, the overall recidivism rate (for both age groups) of incarcerated individuals was approximately 18 percent (665 offenders). While this may seem low, it is important to note that I am using

reconviction as my indicator of recidivism. While approximately 66 percent of offenders are rearrested within three years, the Bureau of Justice Statistics reports that only 25.6 percent of offenders are *reconvicted* within three years of release (Bureau of Justice Statistics, 1994). Furthermore, I am looking at a very small subset of offenders – only 24-27 year olds with a sentence of less than three years. It is not surprising, therefore, that my estimate of recidivism is lower than estimates produced by studies incorporating a larger population.

Independent and Control Variables

An *age* variable represents an offender's age at the time of their initial recorded offense. This is calculated on the guidelines worksheet and provided in the dataset. Age determines whether an offender falls into the 24-25 group (*over 26*=0) or the 26-27 group (*over 26*=1).

Extralegal Variables: As discussed above, a number of factors have consistently been connected to recidivism rates. I will control for both the legal and extralegal variables most commonly associated with increased recidivism. A set of dummy variables will be included for race: a *Black* variable (*Black*=1; not *Black*=0) and a variable for *other* (*other*=1; *not other*=0) will both be included, using the White group as a comparison. Approximately 2714 offenders are classified as Black, 767 as White, and only 91 as other[11]. I will also include a dummy variable for sex (*male*=1; *not male*=0). However, a very small proportion of the overall sample (about five percent). Of those, only a few (less than three percent) have recidivated. Thus, while

[11] The data collected by the sentencing commission is delineated into nine separate race categories. The majority of observations fell into the Black or White categories, with only 70 falling into "Hispanic/ Latin origin", and fewer than 20 observations falling into any one of the other categories. Thus, the remaining categories were combined to include all races other than White and Black.

sex is included as a control variable in the equations, it will probably be dropped from the equations, due to the lack of variation.

Legal Variables: In addition, I will incorporate all of the variables used to calculate sentence range in the equation, in order to control for as much variation as possible. Criminal history, or offender score, frequently predicts recidivism, with more serious prior records leading to a higher probability of recidivism (Hepburn & Albonetti, 1994; Kruttschnitt et al., 2000). Criminal history is composed of a scale including current relationship to the system, violations of parole and probation, and the total number of previous convictions. A dummy variable (*relationship*) will be included to mark if an offender has other pending cases (*relationship*=1) or not (*relationship*=0). Similarly, a dummy variable for *violation* of parole/ probation will indicate if the offender has ever violated these conditions (*prior violations*=1; *no violations*=0). Finally, a variable for number of *priors* will be included, using a dummy variable for each level of prior record -- none, minor, moderate, and major. The equations will exclude those with no prior criminal record; these will serve as the comparison group. Thus, any conclusions regarding these effects should be interpreted relative to offenders with no priors.

Similarly, the study controls for offense severity levels. For person crimes, I treat the base severity score for each offense, ranging from I-XV, as a separate dummy variable, using a severity level of I as the comparison group.

Drug crimes are treated similarly. I use the least sever category to serve as the comparison for drug offenses. However, Category III drug crimes are further separated by the type and amount of drug trafficking (See Appendix 4). Because

these subdivisions are associated with different cells on the sentencing grid, and the effect of a juvenile record will be different at each level, it is important to examine them individually and separately. Therefore, I recode the drug offenses to account for these subcategories, and give each subcategory a separate severity[12]. Thus, the drug severity score ranges from II (most serious) to VII, with category VIII as a comparison. I use a dummy variable for each level of severity.

For Property crimes, I incorporate a dummy variable for each level of severity II- VI. Level VII (the least serious level) will be excluded from the equation, and will be the comparison group for offense severity.

Since Maryland operates separate sentencing tables for person, drug, and property offenses, and the effect of the guideline will thus be different, the analyses will be run separately for each type of offense. Age, extralegal variables, and offender score variables will be included in all equations. Offense severity, however, will be treated differently in the equations.

STATISTICAL MODEL

Similar to Angrist and Krueger (2001), I will utilize an instrumental variable estimation (IVE) to conduct an initial review of sentence severity and recidivism using this method. I will use a two step instrumental variable regression to

[12] The recoded drug severity is as follows:

Original Category	**Recoded**
II	II
III-C	III
III-B	IV
III-A	V
IV	VI
V	VII
VII	VIII

investigate the recidivism of offenders who were 24-25 and 26-27 at the time of their earliest recorded offense. This model is appropriate because IVE capitalizes upon endogenous variation in a predicting variable. This occurs if a predictor variable (in this case, sentence length) is correlated with the residuals in the model that describe its relationship with the outcome of interest (recidivism). In addition, many factors contribute to whether an individual recidivates or not, making it difficult to separate the direct effect of sentence length on recidivism; it is likely that not all of the relevant variables are controlled for in my equation. IVE, however, can help limit bias produced by omitted variables.

As discussed by Angrist and Kreuger (2001) and Smith and Paternoster (1990), IVE uses an additional variable, the instrument, to separate out endogenous variation in the predicting variable. This requires finding a variable that is correlated with the predicting variable, but uncorrelated with the error term of the dependent variable. In this case, age group is used to induce variation in sentence length, the predictor variable. As discussed above, the instrumental variable of age (a proxy for the treatment rule) meets the requirements of a good IV.

Generally, IVE involves two stages of equations. The first equation determines the effect of the instrumental variable on the predictor variable, or the effect of age on sentence length. This equation will produce a predicted value of sentence length for each offender that will reflect variation that can be attributed to the instrumental variable. This predicted sentence length will then be used to estimate a second equation, with recidivism as the dependent variable. This equation will indicate the relationship between sentence severity and recidivism.

Following, I first predict sentence lengths for the sample of offenders and then regress the predicted sentence length and the control variables on recidivism. In the first equation, with sentence length as the dependent variable, I include all relevant control variables as well as the instrumental variable (age group). This equation will predict a value for sentence length for each offender. The first stage of the regression is conceptualized in equation 1:

$$1) Y = \alpha + \beta_1 age + \beta_2 race + \beta_3 offender + \beta_4 offense + \varepsilon$$

In this equation, *Y* is the sentencing outcome with age coded as a dummy variable. The coefficient for age will demonstrate the impact of the age cut-off implemented in the guidelines. According to the guideline regulations, I would expect this value to be negative. A negative beta would indicate that "aging up", or receiving a reduction in juvenile record, results in a shorter sentence length, all else being equal. In other words, offenders who are 26 or older would be expected to receive a significantly shorter sentence than those who are 25 or younger.

The second step in the equation will use the predicted value of sentence length produced in the first equation to generate an estimate of the effect of sentence length on recidivism. This will separate the variation in recidivism due to the use of the age cut-off and estimate the effect of sentence length on future rates of recidivism. The second part of the regression is reflected in equation 2,

$$2) Y = \alpha + \beta_1 sentence + \beta_2 race + \beta_3 offender + \beta_4 offense + \varepsilon$$

where Y is equal to the likelihood of recidivism, while controlling for other variables. The sentence length coefficient (β_1) indicates the effect of the predicted sentence length on the likelihood of recidivism. The equation also includes a number of legal

and extralegal control variables, including race, criminal history, and current offense characteristics.

Therefore, using a two step regression, I can investigate both the overall effect of the age cut-off on total incarceration time received, as well as the effect of time served on future criminal offending, while controlling for offender characteristics. Given the nature of the analysis, I expect the results of the instrumental variable regression to be noticeably different from those provided by a simple regression examining the impact of sentence length on recidivism and will motivate closer examination of the issue.

Use of the Treatment Rule as an Instrumental Variable

Finding an appropriate instrumental variable can be very difficult. Frequently, as in this study, an instrument is produced through policy changes which allow for disruptions in the predicting value (Angrist and Kreuger, 2001). In following, I use the age cut-off provided by the guidelines as an instrumental variable for this study. Since I do not have information on exactly who is affected by the sentencing rule, I use age as a proxy. As I shall illustrate in the following section, age at time of offense affects recidivism through its influence on sentence length.

The age cut-off introduces random variation into sentence length. Essentially, the two age groups are sentenced to different amounts of time depending on their birthdays. In order to examine the variation produced by the age cut-off I run a reduced form regression for each crime type using 24-27 year old offenders (Model 1). The reduced form equation regresses all of the exogenous variables on the main dependent variable. In other words, the equation shows the impact of all variables

other than sentence length on recidivism. In this equation I use the age dummy variable to specify if offenders are over or under the age of 26. The results are presented by crime type in Tables 4, 5, and 6. This regression demonstrates that there is a *significant positive* relationship between age and recidivism, across all three types of crime. Therefore, it implies that aging from under 25 to 26 will result in an increase in recidivism. I can think of no explanation for the difference between these groups other than the presence of the guideline cut-off. There is no other reason to expect an inherent difference in recidivism.

To further ensure that variation exists only because of the age cut-off, I run the same reduced form equation across all crime types for only 24 and 25 year-old offenders (Model 2). In this regression, I create a dummy variable to indicate if an offender is under 25 (Over 25=0) or over 25 (Over 25=1). I use this regression to determine if variation in recidivism exists between these two ages. The results (See Model 2 in Tables 4, 5, and 6) indicate an *insignificant* relationship across all three types of crime. That is, there is no difference in recidivism between 24 and 25 year-olds I also run the reduced form regression a third time (Model 3), including only 26 and 27 year-old offenders and using a dummy variables to designate if they are 26 (Over 27=0) or 27 (Over 27=1). Again, the results for all crime types (See Model 3 in Tables 4, 5, and 6) indicate *insignificant* variation between age and recidivism for these offenders. Only property crimes show a significant relationship.

The results from these regressions ensure that age does not significantly affect recidivism at any other cut-off. I can think of no other reason why there would be a difference only around the 25-26 cut-off point. There are no obvious inherent

differences between 24 and 25 year old offenders, or 26 and 27 year olds. Therefore, since a relationship exists only between the ages of 25 and 26, it is likely due to the guideline imposed cut-off. No other characteristic can explain variation that does not exist between any other ages, but appears only around the age cut-off. This suggests that the change in treatment rules produces the required variation.

In addition, as outlined by Angrist and Krueger (2001), a good instrumental variable must meet three main requirements. First, the instrument (age) must have some effect on the model's predicting variable (sentence length). In a simple multiple regression, age cut-off does significantly predict sentence length[13]. Second, the instrument cannot be correlated with the error term in the second stage model. This assumption holds automatically because the treatment rule was not designed out of concerns for recidivism, but rather because of fundamental concerns about the fairness of holding juvenile records against adults. Third, the instrument must act on the outcome variable (recidivism) only through its effect on the predicting variable. Though the reduced from regression indicates a significant relationship between recidivism and age, once sentence length is included as a control variable in the equation, the effect disappears (See Table 7). With the incorporation of sentence length, age no longer has any significant impact on recidivism.

Therefore, I argue that the use of age, as a proxy for the Maryland treatment rule for juvenile record, is an appropriate instrumental variable for this study. It produces randomized variation in sentence length, which then can approximate a "natural experiment".

[13] The first stage of the instrumental variable regressions (See Tables 9, 10, 11) indicates a significant relationship between age and sentence length for all three crime types.

ESTIMATING EFFECTS USING INSTRUMENTAL VARIABLES

It is important to note, however, "that not every observation's behavior is affected by the instrument" (Angrist and Krueger, 2001: 77). Instrumental variables only change the behavior of *some* people (Imbens and Angrist, 1994). In this case, the age cut-off only affects those offenders aged 26-27 who had recorded juvenile offenses. These are the only offenders directly affected by the implementation of the age cut-off; if an offender has no juvenile record, aging to 26 will not impact their overall offender score. Therefore, the instrumental variable of age provides an estimate of the effect on only a small group of people in my subset. Unfortunately, I have no record of how many or which offenders over 26 had a record (as all delinquent scores are recorded as zero at this age). I can estimate the percentage of offenders with a juvenile record in this age group by using the available information on 24-25 year old offenders. Approximately 388 of the 2631 offenders between 24 and 25 have a juvenile record. Therefore, it can be estimated that about 15% of my sample is directly affected by my instrumental variable[14]. Imbens and Angrist (1994) explain that the effect, then, can only be estimated for those who are affected by the instrument. They refer to this estimate as a Local Average Treatment Effect, or LATE (Imbens and Angrist, 1994; Angrist and Kreuger, 2001). LATE can be calculated as the ratio of the overall effect to the proportion of the sample which the

[14]As demonstrated in Tables 1 and 2 (above), the two age groups do not significantly differ from each other in most measures. Therefore, I assume that the two groups do not have a significantly different proportion of individuals with a juvenile history. Thus, I estimate that approximately the same amount of offenders in each group will have a juvenile record.

instrument affects, or $\frac{\beta}{p}$. While it is possible that this effect will hold constant across the entire group, one must be cautious of generalizing outside of the affected group.

PART III. RESULTS

The Maryland guidelines contain three different matrices, separated by crime type -- person, drug, and property. Each of these crimes operates a unique set of rules and guidelines, resulting in vastly different methods of calculation and punishment. It follows that a change in offender score will affect offenders on different matrices in different ways. Further, researchers, (e.g. Spohn and Holleran, 2003), suggest that overall recidivism rates may vary by crime type. Therefore, I will run the OLS separately by crime type and separately examine the results for each type of crime. The results are discussed below.

MULTIVARIATE ORDINARY LEAST SQUARES REGRESSION ANALYSIS

In order to fully understand the effect of sentence length on recidivism I want to first examine the relationship between the two variables without the presence of an instrumental variable. To do this, I initially run a simple multivariate regression equation, with recidivism as the binary dependent variable. This equation uses sentence severity as the main independent variable and controls for all relevant variables – offense severity, criminal history, and race –except for age group. The results of these calculations are presented by crime type in Table 8. These equations serve as a starting point for this study. They indicate the relationship between sentence severity and recidivism when selection bias is not completely controlled.

According to the output in Table 8 there is, overall, no significant relationship between sentence and recidivism. Looking at all three crime types, none of the sentence length coefficients is significant.

Looking specifically at violent person crimes, the results show no significant relationship between the dependent variable and the main independent. Surprisingly, only a few legal variables show a significant impact on recidivism. Offenders with a major prior record are more likely to reoffend than offenders with no criminal history. Also, a severity level of II or IV indicates a significant effect. Offenders in these categories are more likely to recidivate than comparable offenders in severity category I. Though all severity levels indicate a positive relationship, no other levels are significant. Further, Black violent offenders are significantly more likely to recidivate than comparable White offenders; however, other minorities are not significantly different from White offenders. This follows the results of previous studies in the field.

The drug crime equation presents slightly different results. In this equation, several independent variables influence recidivism. Again the main independent variable of interest, sentence severity, has a negative but insignificant effect on the dependent variable. Criminal history also appears to be a strong predictor of recidivism for drug offenders. Both minor and major prior criminal histories indicate a positive relationship; a moderate record, however, is not a significant predictor. Further, an offender with a major prior history or a prior violation of probation or parole has a greater probability of recidivating. Many of the offense categories are also significantly related to recidivism. Compared to offenders with a severity category VIII (the least serious), offenders in category VII, V, and III are significantly more likely to recidivate. Therefore, as we would expect, more serious offenders are more likely to recidivate upon release. Race is also a large factor in drug crime

recidivism. Both Black and other minorities have a higher probability of recidivating than comparable white offenders. For the most part, my results for the drug crime OLS are in agreement with previous studies.

Similarly, the property crime regression indicates that only a few of the independent variables predict recidivism. Property crimes, like drug crimes, seem to be most influenced by criminal history; a moderate criminal record increases the likelihood of recidivism. Offense severity, on the other hand, does not seem to have an effect. Also, while Black offenders are not significantly different from White offenders, other minorities are more likely to recidivate than comparable White offenders. More importantly, however, sentence severity is not a significant predictor of recidivism in this equation either. Though the relationship is negative, it is not significant.

As none of the sentence length coefficients is significant, we cannot make any strong conclusions. The results could be produced simply by chance or random error. This implies that, in a simple OLS for all types of crime, sentence length does not significantly change the probability of recidivism in either direction. Overall, these results support some of the prior research in the field, finding no relationship between the two variables. While much of the prior research finds a strong positive relationship, most studies looking at the effect of incarceration length find mixed or null effects. It is important to note, however, that the results are imperfect, due to the limited range of offenders included in the study. Those offenders with the most serious records are not included in the results. In fact, offenders with a person severity level above VIII, and a property or drug severity level above III, are dropped

completely from the equation. Few, if any, of these offenders have been released from prison with sufficient follow-up time. Therefore, these results are difficult to generalize to the greater population of offenders. They may hold true only for less serious offenders. Though this limitation will be addressed to a greater extent at a later point, it is important to note that these results must be interpreted carefully. They cannot be easily generalized to the entire population of offenders.

If we were to look only at these equations, it would strongly imply that sentence severity does not affect recidivism in any way. That is, sentence severity neither increases nor decreases the probability of reoffending. This conclusion would support much of the prior research in the field. However, simple multivariate estimations are greatly limited by their inability to control for selection bias and for omitted variable bias. The presence of omitted variable bias can cause inconsistent and inaccurate estimations of the slope. It is important, then, to evaluate the effects of sentence severity using instrumental variable estimation to correct for omitted variable bias.

INSTRUMENTAL VARIABLE ESTIMATION

As discussed in detail above, two-stage instrumental variable estimation consists of two separate regression equations. The first stage of the regression examines the effect of age on sentence length. This equation estimates the direct impact of falling above or below the age cut-off on sentence length, while controlling for relevant variables. The results of this equation are used to model a prediction of sentence length for offenders. Then a second regression, using the predicted value of sentence length from the first equation, determines the overall effect of sentence on

recidivism. Thus, the age cut-off is incorporated as part of the sentence length variable. The introduction of the instrumental variable noticeably changes the effect of sentence severity on recidivism.

Similar to the equations above, I run the results for the two-stage least squares separately by crime type, so the effect of the age cut-off can be fully examined and interpreted. The results will be presented separately for each type of crime.

Person Crimes

Table 9 presents the results from the first and second stage calculations of person crimes. Some of the findings require explanation. Specifically, a number of offense severity variables are dropped from the equation. Presumably, severe offenders receive longer sentences than less serious offenders. Offenders who fall in the highest categories, then, are not likely to have been released during the limited time frame of this study. It follows that the *N* for these categories will be extremely small, perhaps even zero, which would then produce very little variation in the dependent variable of interest.

The first stage equation indicates a *significant negative* relationship between age and sentence length (p=.088). Offenders who are 26 or 27 at the time of the initially recorded offense receive, on average, 5.4 months less than offenders below the age cut-off. These results are expected, given the nature of the guideline policy.

Furthermore, in the first stage regression, almost all other variables (with the exception of the variable for other minorities) significantly predict sentence length. Similar to the results from the full model, White offenders, on average, get shorter sentences than Black offenders; Black offenders receive almost two additional

months on average. Other minorities, however, do not significantly differ from White offenders. Also, as expected, the guideline variables all predict a systematic increase in sentence length. Again, compared to offenders with no prior criminal history, those with any level of prior history will receive a longer sentence. These results hold true for offense severity as well.

The results of the second stage of the equation are slightly more surprising. Several estimators have changed from the simple regression of person crimes (See Table 8). Two points are worth examining in depth. First, very few of the independent variables in the current IVE are significant. Race, prior history and offense severity do not influence recidivism at a significant level. Second, the results from the simple OLS outlined above (without an instrumental variable) indicate that no relationship exists between recidivism and sentence severity. Even with the age cut-off included in this equation, the results are not significant (p=.111). I expected that the use of IVE would result in a significant relationship, so the results are slightly unexpected. The lack of significance is not completely surprising, however. The standard errors in IVE's are generally much larger than those of simple OLS – usually about 10 times larger. The size of the standard error may explain, at least in part, the insignificance of the coefficient. The fact that there is no significant relationship may also be an artifact of the crime type. Overall, the independent variables in the equation significantly influence sentence length, but they do not seem to have any real effect on recidivism. This is true for legal and extralegal variables as well as sentence length, suggesting that recidivism for person offenders is very difficult to predict. This is understandable given the nature of these crimes. Unlike many drug

and property crimes, person crimes are associated more strongly with emotions; thus, they are frequently referred to as "crimes of passion". Thus, violent crimes are more likely to be situation specific, rather than habitual, making them less likely to repeat these offenses upon release. It is important, then, to examine what happens to the size of the coefficient. The magnitude of the coefficient changes (from -.00 in the OLS to -.04 in the IVE), indicating that when IVE is used sentence severity demonstrates a stronger negative impact on recidivism. So, even though the results of the equation are not significant, they still show that the use of IVE can impact the results.

Drug Crimes

Drug crimes constitute the largest proportion of the sample of offenders (about 50 percent). The results for the drug model (See Table 10) are very different from both the single stage drug model, and the two-stage person model.

In the first stage of the equation, age is negatively related to sentence length, as expected. In this case, the results indicate that the exclusion of juvenile record will decrease an offender's sentence by about 6.3 months. This is a significant effect (p=.001). For drug crimes, as with person crimes, the guideline cut-off does result in a difference in sentence length.

Almost all of the control variables included in the first stage have a significant impact on sentence length. All levels of criminal history and offense severity (with the exception of severity category V) demonstrate a positive relationship with sentence length. As expected from the guidelines, prior criminal record systematically increases the sentence length an offender receives – as the prior record increases, so does the sentence length associated with it. Offense severity operates in

a similar fashion. For each increase in offense category, sentence length increases as well.

The results in the second stage are very interesting. Similar to the simple OLS regression, these results indicate that almost all of the independent variables are significant. Again, as expected, prior history is a significant predictor of recidivism. The presence of any criminal record significantly increases the probability of recidivism. Most levels of current offense severity are also positively related to recidivism. The exception to this is severity levels V and VI, which are not found to be significantly different from severity category VIII. In addition, *all* minority offenders are more likely to recidivate than white offenders. These results are consistent with other studies.

However, unlike most previous research, the coefficient for sentence length in the second equation is *negative* and significant, indicating that increasing severity will actually *decrease* future recidivism. In fact, for each additional month an offender is sentenced, the probability of recidivism decreases by 5.3 percentage points. The OLS regression for drug crimes presented earlier indicated no relationship between sentence and recidivism. However, after controlling for selection bias, I find that there is a significant relationship between the two variables (p=.003). These results are different from what we would expect from previous research and the equations discussed above. Similar to Smith and Paternoster (1990), I find that controlling for omitted variables and the selection artifact can provide results that are very different from those equations that do not fully address selection. Thus, by using instrumental variable estimation, I find limited support for the deterrence theory.

Property Crimes

The results from the property crime analysis are presented in Table 11. In the two-stage property crime regression model, the effect of age in the first stage is significant (p=.002). The relationship remains negative, which indicates that falling above the age cut-off will decrease sentence length by about 6.7 months.

In the first stage the guideline variables operate in the expected direction. Overall, with the exception of the variables for minor criminal history and severity category IV, an increase in offender criminal history characteristics or in severity level increases sentence length. It is important to remember that the severity coefficients should be interpreted relative to severity category VII (the least severe category). The variables indicate a positive relationship with sentence length; more serious offenders will receive longer sentences. Again, the most serious offenders are not included in this equation; those with a severity category of II are dropped from the equation. Offenders of this caliber are not likely to have been release from prison long enough to be included in this study.

When compared to the simple OLS regression, the second stage of this two-stage regression produces results similar to the drug model. Like the above equation, most of the control variables are positively related to recidivism. Though only prior history is significant in the OLS calculations, almost all legal variables are now shown to be significant influences. Race is a factor only for Black offenders, who have a higher probability of reoffending than Whites. Other minority property offenders do not differ significantly from Whites. As expected, criminal history demonstrates a positive relationship with recidivism. These results concur with those presented in previous research.

In addition, sentence length for property offenders demonstrates a significant *negative* relationship with recidivism. Increasing an offender's sentence length will decrease their overall likelihood of recidivism. In fact, the results suggest that an increase of one month in sentence length will result in decreasing the probability of recidivating by about five percentage points. Similar to drug crimes, the two-stage regression of property crime indicates a significant relationship that was not found in the single stage regression. The simple OLS indicated a null relationship between the two variables. The results from this equation imply that the use of instrumental variable estimation to control for bias in recidivism research can present vastly different results.

SENSITIVITY ANALYSIS

As explained above, since the actual amount of time served is unknown, I perform a sensitivity analysis to determine the impact of using an estimated release date on the results. As the estimate used to calculate time served directly affects the window of time included to track recidivism, and thus determines which offenders are included in the follow-up period, it can change the overall results considerably.

Time served is determined by a separate body (the parole board), whose criteria are often subjective and offender-specific, so the amount of time-served can vary greatly between individuals. The overall average time served for offender in Maryland is approximately 50 percent of their sentence (see Table 3), though it varies by crime type and sentence length (Wellford and Souryal, 1998). I use the averages provided by Wellford and Souryal (1998) in Table 3 to estimate the amount of time each individual will spend incarcerated. Though this is a relatively conservative

estimate it is still important to examine the effects of different levels of time served in order to determine the accuracy and generalizability of the results. The sensitivity analysis looks at a range of possible time served and determines the effects of different amounts on the recidivism results. I include a broad range of times, using 10 percent increments from 30 to 120 percent. I selected this range because I believe that most, if not all, offenders will be included somewhere in this range of time served. It is unlikely, especially given recent truth-in-sentencing rules, that offenders serve any less than 30 percent of their sentence; similarly, even those whose sentences are extended generally serve only an extra year or two; few, if any, offenders would serve more than 120 percent of their original sentence. Most likely, then, the amount of time served by a given offender falls between 30 and 120 percent. I calculate different exposure times using these estimates of time served at intervals of 10 percent (i.e. 30%, 40%, 50%, etc.) and then run the equations again, using these differential exposure times to designate the follow-up period. Thus, I can determine if an individual recidivated within three years of release given different levels of time served and exposure.

Using different measures of exposure changes the proportion of offenders who have been released for the full three year follow-up period. Using the measure I use for the models included in the study (approximately 50% of time served), more than 95 percent of the included offenders will have been released for three full years. However, when I increase the estimated amount of time served, this proportion decreases slightly, which may affect the results of my sensitivity analysis. For example, if I assume that offenders serve 100 percent of their sentence, the percent

that have been released for at least three years decreases to 84; using an estimate of 120 percent of their sentence, 78 percent have completed the full follow-up period. Therefore, the results of the sensitivity analysis may be slightly skewed. The greater the amount of sentence that they serve, the more offenders I track for recidivism before they are actually released. As a result, these estimates may be slightly more conservative.

If the results vary greatly in magnitude or significance across the different levels of exposure, it will lessen the validity of my results, implying that they occurred only because of the specific estimate I use, and would not hold constant if the actual amount of time served is different from this estimate. If, however, the results of the sensitivity analysis are the same as, or similar to, my original results, then my conclusions will be supported.

The results of my analyses are presented for person crimes in Table 12, for drug crimes in Table 13, and for property crimes in Table 14[15]. In all crime types, the results remain similar across the entire range of time served. Looking specifically at person crimes, the coefficient changes slightly in magnitude -- the model coefficient is slightly smaller than most of the other measures, and the coefficient of 30 percent time served is much smaller. However, these estimations vary very little as a whole and remain negative and insignificant at all levels of time served. The drug crime sensitivity analysis, similarly, shows a slightly smaller model coefficient. With the exception of the 30 percent measure, the coefficients of all the other regressions are greater in magnitude. Furthermore, across the entire range of time served, though the

[15] For the sake of simplicity, these results include only the most relevant variables. Though the control variables are incorporated into the equations, I show only the effect of sentence length on recidivism in the second stage equation.

size of the effect changes slightly at different levels, the coefficient for sentence length remains negative and very significant at all levels. The same can be said for property crimes. The sentence length coefficient produced by the study model for property crimes is slightly smaller than the other measures. But, again, the coefficient changes only slightly across the different percents, and remains significantly negative throughout.

Therefore, it is reasonable to say that the use of an estimation of time served -- rather than an official record -- is not an extreme limitation of the conclusions. The results remain consistent across the broad range of time served, suggesting that I would obtain the same results even if different measures of time served were used. Estimated exposure time does not significantly affect my resulting conclusions about recidivism rates. If anything, my results may offer slightly conservative estimate, as the coefficients from the models I use in my analysis are smaller in magnitude than those produced at other levels of time served.

OVERALL CONCLUSIONS

This section determines that the Maryland treatment rule does, in fact, change sentencing behavior. In all three types of crime, offenders above the age cut-off received significantly shorter sentences than those below the cut-off. In addition, the second stage of the regression indicates that the differential sentencing severity may change the behavior of the individual offender. Therefore, my results offer some limited support for deterrence theory.

Unlike much previous research, I conclude that, when examined using an instrumental variable estimation to correct for selection bias, sentence severity is

negatively related to recidivism for drug and property offenders. In other words, for these crimes, a more severe sentence actually results in a decreased rate of criminal offending. In both regressions, when sentence length was used as an instrumental variable for age, the results indicate that longer sentence length will lead to a significantly greater chance of future offending, all else being equal. The overall size of the effect is relatively large; the probability of recidivism for drug and property offenders is decreased by about five percentage points for every month increase in sentence length[16]. Given the base recidivism rate (18%), a decrease of five percentage points is a very impressive result. Each additional month, then, will reduce the overall probability of recidivism by a sizeable amount. Extending an offender's sentence length by just a few months could, on average, make the probability of recidivism very small.

I can further conclude that the use of instrumental variable estimation is an effective approach to the study of recidivism. Without the inclusion of the instrumental variable, the results indicate that there is no significant relationship between sentence severity and future crime. Like Smith and Paternoster (1990) I find that the exogenous variation produced by the instrumental variable generates different and unexpected results. I argue that this approach corrects for many of the weaknesses in previous research, which produces a significant negative relationship not often found in the literature. When selection bias is properly controlled, my results show rare support for deterrence theory.

[16] Sentence length does not significantly influence the probability of recidivism for person offenders.

PART IV. DISCUSSION

The large increase in state and federal prisons occurring in the past three decades reflects a punishment theory based in deterrence. Despite an impressive number of empirical studies concluding that deterrence theory does not work, many researchers and policy makers maintain that increasing sentence length will deter an offender from later crime, and eventually result in an overall reduction in crime. In other words, deterrence theorists and supporters maintain that incarceration is a more effective punishment than probation or other alternatives, and that long sentences of incarceration are more effective than short terms.

The purpose of this study is to investigate the utility of using IVE to revisit this highly debated issue from a unique angle. More specifically, this study aims to determine the appropriateness of using variation created by an offender treatment rule in the Maryland state sentencing guidelines to better examine the effect of sentence length on later offending. The study presents evidence to support the utility of this rule as an instrument. I then use the variation produced by this treatment in a two-stage instrumental variable estimation. This equation will determine if the number of months an offender spends in prison increases or decreases the likelihood of a later return to the system.

I examine the overall effect of sentence length on recidivism in a three year follow-up period. The study addresses the effect of sentence length on different types of crime, looking separately at person, drug, and property. For those crimes I *can* make conclusions about, an increase in sentence severity will result in a significantly lower probability of committing future crime. The results indicate that for drug and

property crimes recidivism is negatively related to sentence length. In other words, an increase in sentence length will result in a decrease in the probability of recidivism. This finding, however, is not constant over all crime types. While drug and property offense results both indicate a negative and very significant relationship, person offenses find no significant influence. Therefore, my results suggest that increasing sentence severity will lower recidivism rates for property and drug offenses, but will not alter the rates of violent person offenders. The results of this study, then, find mild support for deterrence theory.

I do not go so far as to suggest that there is a direct causal relationship between sentence severity and recidivism. I argue, however, that the experience of spending time in prison does in some way decrease later offending. My results support the principles behind deterrence theory, implying that the negative experience of incarceration increases the salience of punishment, making offenders less likely to reoffend upon release.

It is important to note that the relatively short follow-up period may influence these results slightly. Since the study includes information only from 1999-2005, not all Maryland offenders are included in the follow up. Specifically, offenders with a sentence length of more than three years were excluded from the study[17]. Therefore, it is possible that, if more serious offenders were included in my subset, the results would not be as strong. As discussed earlier, both offense severity and criminal

[17] A maximum of three years was used in order to ensure that most offenders would have some follow up time. In other words, to ensure that I was not tracking the recidivism of offenders who were still in prison. Assuming the offenders serve about 50% of their sentence, this allows for a short follow-up period for some offenders. This will produce a more accurate estimate of recidivism. However, it also greatly limits the severity of offenses at which I can look. Offenders who fall in a sentencing grid with greater than a 36 month sentence will, most likely, not be included in my study. Therefore, it will be difficult to generalize these findings to more serious offenders.

record have been cited as predictors of recidivism (e.g. Wooldredge, 1988; Hepburn & Albonetti, 1994; Visher et al., 1991). It follows that more serious offenders, then, are more likely to recidivate after release. Excluding these offenders from the models could impact my overall results.

LIMITATIONS AND RELEVANT THREATS TO VALIDITY

Though the results of this study are provocative, they are limited. For one thing, I use a linear estimation (OLS). A binary dependent variable (recidivism) necessitates the use of a nonlinear logit or probit estimator. An instrumental variable probit has only recently become available and is extremely difficult to use. Because of the exploratory nature of this research, I chose to use a linear OLS model for the sake of simplicity. This model, however, has several limitations when applied to a dichotomous dependent variable. Using an OLS regression to estimate a non-linear binary variable is inefficient and can result in a number of weaknesses – specifically, heteroskedasticity and non-normality. OLS regression can also produce nonsensical predicted probability estimates (e.g. probabilities that are less than zero or greater than one).

Also, since I do not have a full count of the number of 26-27 year old offenders with a juvenile record, I must use age as a proxy for my real instrument (treatment record) rather than record, as my instrument. This is problematic because age, as it applies to juvenile record, only affects about 15 percent of my sample. Furthermore, I cannot know which offenders have a juvenile record or what type of record they have. Therefore, my results are not as clear cut as they could be.

The lack of data recording actual time served is the main limitation of the study. Without an exact amount of time spent incarcerated, it is difficult to accurately calculate a release date, or time till failure. In following, the validity of my conclusions could be called into question. For one thing, not having a release date allows for differential exposure. Offenders sentenced to probation are tracked for recidivism from the date of their sentence, while offenders who receive incarceration can only be followed from an estimated release date. Therefore, individuals sentenced to probation may be tracked for a longer, or shorter, period of time than incarcerated offenders.

Further, using an estimate of time served makes it difficult to ensure that all eligible people are included in the follow-up period. If the estimate of time served is too short, it is possible that people are excluded from the three year follow-up who should have been included. That is, since the follow-up window begins with the first day of their estimated release, if this estimate is too conservative and the window is too short, many offenders may spend the first several months of the recidivism follow-up period still serving their sentence in jail. Conversely, if I overestimated the amount of time served by a given offender, then too many people will be marked as recidivists within three years. That is, offenders who did not actually commit another crime for over three years will be included because the window will be extended. In other words, offender may be out of jail for several months before their follow-up period even begins. This is especially problematic because some may even be recidivate and reenter the system before I even tag them as released.

I argue that, if anything, this study errs on the side of conservative estimates. My time served approximation of 50 percent of assigned sentence length corresponds to the research of Wellford and Souryal (1998) (See Table 3). Considering the recent increases in truth-in-sentencing, it is likely that offenders actually serve more than half of their sentence. In fact, the Maryland Sentencing Commission aims to have offender serve closer to 85 percent of their given sentence (www.msccsp.org). Therefore, my study offers a more conservative estimate of time served, and thus recidivism, than is necessary. It is more likely that I begin my three year follow-up too early after their sentence date, and do not, in fact, track the offenders for a full three years post-release. In other words, there is a greater chance that I start tracking offenders while they are still imprisoned. Thus, I risk underestimating the real rate of recidivism.

Furthermore, my sensitivity analysis to a great extent validates the measures used (See Tables 12, 13, 14); in spite of the broad range used, the overall results change negligibly when I manipulate the amount of time served and the follow-up period. Therefore, while the lack of time served data is a noticeable shortcoming of the study, I argue that it affects my results very little.

There remains, however, the issue of differential time served. I use the average amount of time served to estimate each offender's incarceration period. Obviously, as it is an average rate, some people will serve more than this time, and some will serve less. Regardless of what estimate of time served I use, some people will still be in prison when I start tracking and some will have already been released. Therefore, each offender has a slightly different rate of exposure, and either more or

less time to commit offenses. This could cause me to either over or under estimate individual recidivism rates. So, while I can conclude that the average recidivism rate is not strongly affected by my use of an estimate (as demonstrated by the sensitivity analysis), it is important to note that it represents only an average; there is still a fair amount of individual variation that, if captured, could alter the results of the study.

In addition, the limited follow-up period makes it difficult to capture the full effects of sentence length. Since the data were only collected after 1999, it is difficult to achieve a long follow-up period for all offenders in the sample; not all offenders could be followed for the full three year period. In order to adjust for this I truncate the sentence length at 36 months; any offenders who received a sentence greater than three years are excluded from this study. Approximately half of the total available cases were excluded because of sentence length. I do this in order to ensure more complete tracking of offender recidivism. Using a short observation period will produce an underestimation of recidivism; offenders who would potentially reoffend within three years are not always tracked for the full time period. Almost the entire current sample (approximately 99 percent) had been released by January 1, 2006. Of those, most had been released for the full duration of the follow-up period -- about 95 percent of the total sample had been released for at least three years.

More important is the fact that truncating sentence length directly excludes those offenders who are the most likely to recidivate. Since the study includes only seven total years of data, many of the most serious offenders could not be included in the study at all. Unfortunately, these offenders are also the most likely to recidivate. If all offenders were included in my study the results may no longer indicate a

negative relationship with sentence severity. Furthermore, cutting out serious offenders greatly limits the generalizability of my results. My results cannot be generalized beyond the population to which they apply. I can therefore not speak to the effect of prison sentence on *all* offenders, but only those with a relatively short sentence. Thus, while I have been able to control for some selection bias, my estimations are still influenced by the incomplete set of information. I recommend that additional studies be conducted at a later date to include a greater number of serious offenders.

Finally, it is important to note that judges still exercise a large amount of discretion in their decisions. Despite the inclusion of relevant control variables and the use of age as a quasi-experimental condition, the discretion of the judges can never be discerned completely. Therefore, while my study implements the use of instrumental variable estimation to control for omitted variable bias, there will always be unexplained variation in sentence length.

IMPLICATIONS FOR EXISTING EMPIRICAL LITERATURE

To date, the empirical literature has not been able to satisfactorily control for selection bias in studies of deterrence. Failing to account for this fact can greatly bias results. Variables associated with increased sentence severity are often confounded with the dependent variable. Many variables included in these equations influence both sentence severity and probability of recidivism. Therefore, it has been difficult for researchers to isolate the direct effect of sentence severity. My study uses IVE as a method for separating these effects. I preset findings that support the use of a unique Maryland rule as an instrumental variable. I find that the treatment rule meets

the requirements for a good instrument – it is related to the predictor variable (sentence length), is unrelated to the dependent variable (recidivism), and produces the necessary variation -- and I suggest it could be used to conduct similar research in the future. Further research could examine in depth the behavior of offenders affected by the ruling in Maryland, or could investigate similar policies in other states.

Like much previous research, when I use an equation which assumes that selection bias does not exist my results indicate no relationship between sentence and recidivism (See Table 11). However, examining recidivism without the problem of selection bias leads me to very different results (See Tables 12, 13, 14). Similar to Smith and Paternoster (1990), once I control selection bias I find that a negative relationship exists between sentence severity and recidivism. This suggests that the labeling and prison socialization effects apparent in much of the research may instead be a result of selection bias. This suggests that further research should capitalize on this and other existing variations, as the results could reduce selection bias and further clarify the severity-recidivism relationship.

My study should serve to bring renewed attention to selection bias in studies. In order to make strong and consistent conclusions about the severity-recidivism relationship, researchers in the field must fully consider and control for the effect of selection. It is essential that researcher separate the effect of sanctioning and selection in future research.

POLICY IMPLICATIONS

The results of this study suggest that increasing the severity of an individual's sentence does, in fact, decrease the probability of future offending. These finding lend limited support to deterrence theory. The evidence presented above suggests that, at least for some less serious offenders, longer periods of incarceration will be effective in reducing the overall crime rate.

In addition, though the overall generalizability of the study is limited to the group immediately affected by the guideline, the results do speak directly to a policy that is currently in place in the state of Maryland. Since the guidelines presently enforce a similar policy, it is important to determine its ramifications. Currently, offenders at or above the age of 23 receive a juvenile record reduction. However, my results may question the validity of this policy, as it may produce some unintended consequences. In fact, my results indicate that granting offenders of this age a reduction in sentence could increase their probability of recidivating. Decreasing sentence length by even one month could increase recidivism by five percentage points. Therefore, as the average difference in sentence length due to the treatment rule is about six months, the use of the treatment rule could increase the recidivism rates of offenders over 26 by almost 30 percentage point, which would almost double the base recidivism rate. Considering the sizeable implications of these findings, the Maryland commission members should consider these repercussions when examining the utility of the age cut-off.

The rule is meant to address the fundamental fairness involved in punishing adult offenders for juvenile crimes. My findings imply that perhaps these crimes

should be taken into account, as deleting them at this point could increase recidivism. I do not go so far as to suggest that Maryland law makers completely eradicate the treatment rule; rather, I recommend that they reevaluate the ruling in light of its effect on recidivism behavior. As decreased sentence length could lead to an undesired increase in recidivism for offender affected by the ruling, I suggest that the sentencing commission either suspend the rule or use an older age as the cut-off until further research can be conducted.

CONCLUSIONS

Much of the research investigating deterrence theory to date suffers from methodological flaws -- most commonly selection bias -- due to the difficulty in achieving random assignment of offenders to different prison conditions. This study attempts to utilize a unique feature of the Maryland sentencing guidelines that creates quasi-experimental conditions for sentence length. Although this does not completely eliminate potential methodological challenges, it greatly does away with the bias from selection and omitted variables. I examine the usefulness of an instrumental variable and use it to conduct a two-stage least squares regression analysis of the severity-recidivism relationship. This design maximizes the exogenous variation in recidivism due to an age-based guideline regulation. Sentence length can then be used as a predictive measure of the effect of age on recidivism using two different age groups of comparable offenders who are similar in most other demographics.

The overall conclusions from this research, in contrast to much other empirical evidence in the field, find mild, but mixed, support for specific deterrence theory and the assumptions underlying recent increases in strict sentencing policy. In fact, my findings imply that, in this sample, increases in sentence severity have achieved the goal of reducing recidivism.

The findings of this study are limited greatly by a lack of data tracking the exact amount of time the offenders served in prison. Without this, I can use only an estimate of time served, and thus, can not accurately determine release dates, exposure time or time till failure. Furthermore, not all offenders could be followed

for any or all of the three year follow-up period after release. In fact, more serious offenders are not included in the study at all.

This study can be used as an example to examine an old problem from a new angle. The results of this study are very provocative and hold important methodological and empirical implications. They suggest that the treatment rule present in the Maryland guidelines makes an effective instrument, which could influence further investigation into the Maryland guidelines and the behavior of offenders affected by the rule. More importantly, the study results, though limited, support the idea that the use of IVE and close attention to bias can produce unexpected results. Future research in this area could benefit from extending the analysis to include later years of data and provide a more accurate picture of recidivism, as it would allow for a longer follow-up window and would include more offenders in the study. Researchers could also look for similar treatment rules and policies in other states. I hope that it brings renewed attention to the deterrence theory debate as well as the role of selection bias in research and motivates further investigation in this area.

TABLES

Table 1. Background Characteristics of Offenders by Group

	Proportion			
	24-25	**26-27**	**Test Statistic**	**P**
Black	.747	.744	.359	.730
Other	.045	.047	-.546	.585
Current Offense				
Person	.362	.359	.334	.739
Drug	.505	.505	.021	.983
Property	.133	.136	-.517	.606
Injury				
None	.541	.567	-1.625	.104
Non-permanent	.301	.284	1.352	.176
Permanent/ Death	.153	.155	.000	1.00
Weapon				
No weapon	.351	.380	-1.457	.145
Non-Firearm	.170	.167	.294	.769
Firearm/Explosive	.450	.431	1.216	.224
Vulnerable Victim	.063	.090	-3.671	.000***
Mean Offense Severity	3.82	3.78	1.414	.157

* Indicates marginal significance (p<.10).
** Indicates significance at p<.05.
*** Indicates significance at p<.01 or below.

Table 2. Criminal History Background Characteristics of Offenders by Group

	Proportion			
	24-25	**26-27**	**Test Statistic**	**P**
Prior History				
None	.187	.180	1.16	.246
Minor	.230	.217	1.84	.066*
Moderate	.312	.280	-.143	.886
Major	.272	.323	-6.90	.000***
Violation	.321	.372	-3.69	.000***
Current Relationship	.338	.327	1.22	.224

* Indicates marginal significance (p<.10).
** Indicates marginal significance (p<.05)
*** Indicates marginal significance (p<.01).

Table 3. Mean Time Served by Maryland Offenders

		By Assigned Sentence Length (in months)		
	Overall Mean	13-41	42-60	>60
Person	55%	58%	56%	50%
Drug	50%	52%	49%	48%
Property	55%	58%	49%	57%

Table 4. Reduced Form Regression for Person Crimes

	Effect on Recidivism					
	Model 1 24-27 (N=1671)		**Model 2** 24-25 (N=893)		**Model 3** 26-27 (N=778)	
Variables[†]	**Beta Coefficient**	**Standard Error**	**Beta Coefficient**	**Standard Error**	**Beta Coefficient**	**Standard Error**
Age	.013	.005**	.008	.008	.000	.007
Black	.012	.006**	.007	.009	.016	.008**
Other	-.010	.013	-.019	.020	.001	.016
Minor Prior	.022	.078	.010	.012	.031	.010***
Moderate Prior	.030	.008***	.029	.012**	.031	.011***
Major Prior	.041	.009***	.048	.014***	.035	.012***
Violation	.001	.000***	.001	.000***	.001	.000***
Relation	-.010	.006	-.011	.009	-.005	.009
Person Severity II	.044	.012***	.044	.018**	.047	.015***
Person Severity III	.031	.009***	.041	.014***	.024	.012**
Person Severity IV	.060	.011***	.076	.016***	.048	.014***
Person Severity V	.042	.011***	.064	.017***	.021	.014
Person Severity VI	.041	.014***	.077	.021***	.007	.018
Person Severity VII	.010	.010	.025	.016	005	.013
Person Severity VIII	.018	.014	.037	.020*	003	.019
Person Severity IX	.0007	.018	026	.026	.042	.024*
Person Severity X	.046	.024*	.071	.035**	.016	.034
Person Severity XI	.122	.118	.133	.128	---	---

[†] Severity categories XII, XIII, XIV, and XV were dropped from this equation

* Indicates marginal significance ($p<.10$).

** Indicates significance at $p<.05$ or below.

*** Indicates significance at $p<.01$ or below.

Table 5. Reduced Form Regression for Drug Crimes

	Effect on Recidivism					
	Model 1 24-27 (N=2938)		**Model 2** 24-25 (N=1566)		**Model 3** 26-27 (N=1372)	
Variables	**Beta Coefficient**	**Standard Error**	**Beta Coefficient**	**Standard Error**	**Beta Coefficient**	**Standard Error**
Age	.042	.007***	.015	.010	.011	.008
Black	.015	.010	.046	.015***	-.031	.012***
Other	-.026	.019	-.020	.029	-.046	.022**
Minor Prior	.034	.008***	.048	.013***	.017	.010*
Moderate Prior	.026	.009***	.018	.014	.032	.011***
Major Prior	.105	.015***	.102	.025***	.105	.016***
Violation	.001	.000***	.001	.000	.001	.000**
Relation	-.000	.009	.008	.014	-.015	.012
Drug Severity II	---	---	---	---	---	---
Drug Severity III	.021	.047	.029	.073	.007	.054
Drug Severity IV	.016	.013	.028	.019	.008	.016
Drug Severity V	.066	.076	.041	.124	.100	.084
Drug Severity VI	.004	.014	.001	.020	.013	.017
Drug Severity VII	.028	.015	.027	.023	.026	.018

* Indicates marginal significance ($p<.10$).

** Indicates significance at $p<.05$ or below.

*** Indicates significance at $p<.01$ or below.

Table 6. Reduced Form Regression for Property Crimes

	Effect on Recidivism					
	Model 1 24-27 (N=1671)		**Model 2** 24-25 (N=893)		**Model 3** 26-27 (N=778)	
Variables	**Beta Coefficient**	**Standard Error**	**Beta Coefficient**	**Standard Error**	**Beta Coefficient**	**Standard Error**
Age	.056	.011***	.006	.018	.025	.011**
Black	.010	.012	.011	.019	-.010	.012
Other	-.027	.024	-.083	.044*	-.015	.021
Minor Prior	.002	.016	-.013	.026	.030	.016*
Moderate Prior	.040	.017	.062	.027**	.025	.016
Major Prior	.001	.022	-.005	.039	.019	.020
Violation	.002	.000	.003	.000***	.000	.000
Relation	-.018	.013	-.016	.021	-.019	.014
Property Severity II	---	---	---	---	---	---
Property Severity III	.019	.017	.045	.027	.003	.017
Property Severity IV	.051	.017***	.110	.029***	.003	.017
Property Severity V	.016	.015	.039	.023	.014	.014
Property Severity VI	.067	.030**	.144	.049***	.009	.029

* Indicates marginal significance ($p<.10$).
** Indicates significance at $p<.05$ or below.
*** Indicates significance at $p<.01$ or below.

Table7. Single Stage Multivariate Regression by Crime Type Controlling for Age and Sentence Length

	Person Crimes (N=1671)		**Drug Crimes (N=2938)**		**Property Crimes (N=1007)**	
Variables†	Beta Coefficient	Standard Error	Beta Coefficient	Standard Error	Beta Coefficient	Standard Error
Sentence Length	-.000	.000	.000	.000	.000	.000
Over 26	-.004	.004	-.053	.066	-.010	.006
Black	.001	.006	.030	.011***	-.005	.006
Other	-.026	.012**	-.026	.020	-.014	.013
Minor Prior	.014	.007*	.028	.010***	-.002	.010
Moderate Prior	.022	.008**	.021	.011**	.014	.010
Major Prior	.046	.009***	.062	.015***	.013	.014
Violation	-.016	.006	-.014	.009	-.007	.007
Relation	.004	.006	.013	.008	.005	.007
Person Severity II	.004	.118	---	---	---	---
Person Severity III	.013	.118	---	---	---	---
Person Severity IV	-.009	.118	---	---	---	---
Person Severity V	.014	.118	---	---	---	---
Person Severity VI	.013	.118	---	---	---	---
Person Severity VII	.031	.118	---	---	---	---
Person Severity IX	.059	.118	---	---	---	---
Person Severity X	.062	.118	---	---	---	---
Drug Severity II	---	---	---	---	---	---
Drug Severity III	---	---	.025	.052	---	---
Drug Severity IV	---	---	.014	.015	---	---
Drug Severity V	---	---	.036	.084	---	---
Drug Severity VI	---	---	.012	.015	---	---
Drug Severity VII	---	---	.027	.017	---	---
Property Severity III	---	---	---	---	.004	.010
Property Severity IV	---	---	---	---	.007	.010
Property Severity V	---	---	---	---	.007	.008
Property Severity VII	---	---	---	---	.017	.016

† Person Severity Categories VIII, XI, XII, XII, IV, and XV are dropped from this equation
*Indicates marginal significance (p<.10).
**Indicates significance at p<.05 or below.
***Indicates significance at p<.01 or below.

Table 8. Single Stage Multivariate Regression by Crime Type

	Person Crimes (N=1671)		**Drug Crimes (N=2938)**		**Property Crimes (N=1007)**	
Variables[†]	Beta Coefficient	Standard Error	Beta Coefficient	Standard Error	Beta Coefficient	Standard Error
Sentence Length	-.000	.000	.001	.001	-.002	.002
Black	.006	.004*	.016	.008**	-.005	.012
Other	.002	.002	.031	.007***	.047	.009***
Minor Prior	.005	.004	.026	.009***	.009	.015
Moderate Prior	.010	.007	.009	.017	.075	.021***
Major Prior	.050	.015***	.075	.027***	.056	.029*
Violation	.000	.006	-.018	.010*	.008	.019
Relation	-.004	.005	-.001	.010	-.009	.016
Person Severity II	-.012	.007*	---	---	---	---
Person Severity III	-.002	.006	---	---	---	---
Person Severity IV	-.010	.005*	---	---	---	---
Person Severity V	.001	.008	---	---	---	---
Person Severity VI	.028	.020	---	---	---	---
Person Severity VII	-.001	.013	---	---	---	---
Person Severity IX	-.002	.008	---	---	---	---
Person Severity X	-.003	.012	---	---	---	---
Drug Severity II	---	---	---	---	---	---
Drug Severity III	---	---	-.040	.018**	---	---
Drug Severity IV	---	---	-.001	.017	---	---
Drug Severity V	---	---	-.062	.019***	---	---
Drug Severity VI			-.003	.013	---	---
Drug Severity VII	---	---	-.037	.013***	---	---
Property Severity III	---	---	---	---	.032	.022
Property Severity IV	---	---	---	---	.003	.018
Property Severity V	---	---	---	---	.015	.017
Property Severity VII	---	---	---	---	-.039	.011***
R^2		**.0227**		**.0382**		**.0386**

† Person Severity Categories VIII, XI, XII, XII, IV, and XV are dropped from this equation
*Indicates marginal significance (p<.10).
**Indicates significance at p<.05 or below.
***Indicates significance at p<.01 or below.

Table 9. Instrumental Variable Regression for Person Crimes

	First Stage		***Second Stage***	
Variables[†]	*Sentence Length*	*SE*	*Recidivism*	*SE*
Sentence Length	---	---	-.040	.025
Over 26	-5.41	.317***	---	---
Black	1.71	.354***	.072	.047
Other	.439	.685	-.005	.024
Prior_1	1.58	.426***	-.045	.043
Prior_3	10.45	.483***	.465	.261*
Prior_5	10.64	.771***	.508	.275*
Violation	5.51	.411***	.199	.038
Relation	6.39	.439***	.244	.162
Severity II	3.31	.584***	.093	.086
Severity III	8.44	.481***	.308	.213
Severity IV	13.28	.569***	.481	.333
Severity V	13.72	.593***	.522	.345
Severity VI	24.11	.978***	.968	.602
Severity VII	27.43	.652***	1.09	.690
Severity VIII	---	---	---	---
Severity IX	24.20	4.21***	1.44	.826*
Severity X	43.08	4.21***	1.68	1.09
R^2 = .6083				

[†] Severity levels above X and Other are dropped from the equation.

*indicates marginal significance ($p < .10$).

** indicates significance at the $p < .05$ level or below.

*** indicates significance at the $p < .01$ level or below.

Table 10. Instrumental Variable Regression for Drug Crimes

Variables	***First Stage***		***Second Stage***	
	Sentence Length	*(SE)*	*Recidivism*	*(SE)*
Sentence Length	---	---	-.053	.018***
Over 26	-6.28	.196***	---	---
Black	1.30	.234***	.077	.027***
Other	-.315	.429	-.046	.020**
Prior_1	1.214	.255***	.086	.026***
Prior_3	15.081	.283***	.833	.272***
Prior_5	18.809	.431***	1.068	.343***
Violation	3.583	.272***	.181	.067***
Relation	3.076	.258***	.153	.057***
Severity II	---	---	---	---
Severity III	9.664	1.94***	.493	.188***
Severity IV	9.818	.247***	.542	.178***
Severity V	4.402	3.203	.186	.134
Severity VI	.675	.321**	.028	.019
Severity VII	2.236	.407***	.139	.043***
R^2 = .5034				

*indicates marginal significance (p< .10).
** indicates significance at the p< .05 level or below.
*** indicates significance at the p< .01 level or below.

Table 11. Instrumental Variable Regression for Property Crimes

	First Stage		***Second Stage***	
Variables	*Sentence Length*	*SE*	*Recidivism*	*SE*
Sentence Length	---	---	-.050	.017***
Over 26	-6.67	.215***	---	---
Black	3.84	.254***	.207	.068***
Other	.816	.506	.015	.026
Prior_1	2.17	.280***	.130	.041***
Prior_3	15.19	.313***	.786	.265***
Prior_5	16.88	.471***	.904	.298***
Violation	2.25	.298***	.100	.043**
Relation	1.91	.281***	.081	.036**
Severity II	---	---	---	---
Severity III	1.78	.616***	.115	.039***
Severity IV	.345	.632	.005	.027
Severity V	2.16	.478***	.089	.043**
Severity VI	4.54	1.25***	.264	.093***
R^2 = .3983				

*indicates marginal significance ($p < .10$).
** indicates significance at the $p < .05$ level or below.
*** indicates significance at the $p < .01$ level or below.

Table 12. Sensitivity Analysis of Person Crimes

Percent of Time Served	Effect on Recidivism	
	Coefficient (SE)	*p*
Model	-.040	
	(.025)	.111
30	-.009	
	(.008)	.267
40	-.056	
	(.034)	.105
50	-.056	
	(.034)	.104
60	-.055	
	(.034)	.105
70	-.055	
	(.034)	.105
80	-.050	
	(.031)	.108
90	-.050	
	(.034)	.108
100	-.050	
	(.034)	.108
120	-.050	
	(.031)	.108

*indicates marginal significance ($p < .10$).
** indicates significance at the $p < .05$ level or below.
*** indicates significance at the $p < .01$ level or below.

Table 13. Sensitivity Analysis of Drug Crimes

Percent of Time Served	Effect on Recidivism	
	Coefficient (SE)	*P*
Model	-.053	
	(.018)	.003***
30	-.014	
	(.006)	.011**
40	-.070	
	(.023)	.002***
50	-.070	
	(.023)	.002***
60	-.068	
	(.022)	.002***
70	-.066	
	(.022)	.003***
80	-.064	
	(.021)	.003***
90	-.064	
	(.021)	.003***
100	-.064	
	(.021)	.003***
120	-.064	
	(.021)	.003***

*indicates marginal significance ($p < .10$).
** indicates significance at the $p < .05$ level or below.
*** indicates significance at the $p < .01$ level or below.

Table 14. Sensitivity Analysis of Property Crimes

Percent of Time Served	Effect on Recidivism	
	Coefficient (SE)	*P*
Model	-.050	
	(.017)	.004***
30	-.013	
	(.005)	.012**
40	-.066	.
	(.023)	003***
50	-.067	
	(.023)	.003***
60	-.065	
	(.022)	.003***
70	-.063	
	(.022)	.003***
80	-.061	
	(.021)	.003***
90	-.061	
	(.021)	.003***
100	-.061	
	(.021)	.003***
120	-.061	
	(.021)	.003***

*indicates marginal significance ($p < .10$).
** indicates significance at the $p < .05$ level or below.
*** indicates significance at the $p < .01$ level or below.

APPENDICES

APPENDIX 1. CALCULATION OF OFFENSE SCORE

1st Off.	2nd Off.	3rd Off.	A. Seriousness Category
01	01	01	= V – VII
03	03	03	= IV
05	05	05	= III
08	08	08	= II
10	10	10	= I
1st Off.	**2nd Off.**	**3rd Off.**	**B. Victim Injury**
0	0	0	= No Injury
1	1	1	= Injury, Non-Permanent
2	2	2	= Permanent Injury or Death
1st Off.	**2nd Off.**	**3rd Off.**	**C. Weapon Usage**
0	0	0	= No Weapon
1	1	1	= Weapon Other than Firearm
2	2	2	= Firearm or Explosive
1st Off.	**2nd Off.**	**3rd Off.**	**D. Special Vulnerability**
0	0	0	= No
1	1	1	= Yes
_____	_____	_____	**OFFENSE SCORE (S)**

APPENDIX 2. CALCULATION OF OFFENDER SCORE (CRIMINAL HISTORY)

OFFENDER SCORE
A. Relationship to CJS When Instant Count Occurred 0 = None or Pending Cases 1 = Court or Other Criminal Justice Supervision
B. Juvenile Delinquency 0 = Not More Than One Finding of Delinquency or over age 25 1 = Two or More Findings, No or One Commitment 2 = Two or More Commitments
C. Prior Adult Criminal Record 0 = None 1 = Minor 5 = Major 3 = Moderate
D. Prior Adult Parole/Probation Violations 0 = No 1 = Yes
___|___ **OFFENDER SCORE**

APPENDIX 3. SENTENCING GRID FOR PERSON CRIME

	Offender Score							
Offense Score	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7 or more**
1	P	P	P-3M	3M-1Y	3M-18M	3M-2Y	6M-2Y	1Y-3Y
2	P-6M	P-1Y	P-18M	3M-2Y	6M-3Y	1Y-5Y	18M-5Y	3Y-8Y
3	P-2Y	P-2Y	6M-3Y	1Y-5Y	2Y-5Y	3Y-7Y	4Y-8Y	5Y-10Y
4	P-3Y	6M-4Y	1Y-5Y	2Y-5Y	3Y-7Y	4Y-8Y	5Y-10Y	5Y-12Y
5	3M-4Y	6M-5Y	1Y-6Y	2Y-7Y	3Y-8Y	4Y-10Y	6Y-12Y	8Y-15Y
6	1Y-6Y	2Y-7Y	3Y-8Y	4Y-9Y	5Y-10Y	7Y-12Y	8Y-13Y	10Y-20Y
7	3Y-8Y	4Y-9Y	5Y-10Y	6Y-12Y	7Y-13Y	9Y-14Y	10Y-15Y	12Y-20Y
8	4Y-9Y	5Y-10Y	5Y-12Y	7Y-13Y	8Y-15Y	10Y-18Y	12Y-20Y	15Y-25Y
9	5Y-10Y	7Y-13Y	8Y-15Y	10Y-15Y	12Y-18Y	15-25Y	18Y-30Y	20Y-30Y
10	10Y-18Y	10Y-21Y	12Y-25Y	15Y-25Y	15Y-30Y	18Y-30Y	20Y-35Y	20Y-L
11	12Y-20Y	15Y-25Y	18Y-25Y	20Y-30Y	20Y-30Y	25Y-35Y	25Y-40Y	25Y-L
12	15Y-25Y	18Y-25Y	18Y-30Y	20Y-35Y	20Y-35Y	25Y-40Y	25Y-L	25Y-L
13	20Y-30Y	25Y-35Y	25Y-40Y	25Y-L	25Y-L	30Y-L	L	L
14	20Y-L	25Y-L	28Y-L	30Y-L	L	L	L	L
15	25Y-L	30Y-L	35Y-L	L	L	L	L	L

P=Probation, M=Months, Y=Year, L=Life

APPENDIX 4. SENTENCING GRID FOR DRUG CRIME

	Offender Score							
Offense Seriousness Category	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7 or more**
VII	P	P	P	P-1M	P-3M	P-6M	3M-6M	6M-2Y
VI	Available for future use. There are currently no seriousness category VI drug offenses.							
V	P-6M	P-12M	3M-12M	6M-18M	1Y-2Y	1.5Y-2.5Y	2Y-3Y	3Y-4Y
IV	P-12M	P-18M	6M-18M	1Y-2Y	1.5Y-2.5Y	2Y-3Y	3Y-4Y	3.5Y-10Y
III-A Marijuana import over 45 kilograms, and MDMA over 750 grams	P-18M	P-2Y	6M-2Y	1Y-4Y	2Y-6Y	3Y-8Y	4Y-12Y	10Y-20Y
III-B Non-marijuana and non-MDMA, Except Import	6M-3Y	1Y-3Y	18M-4Y	3Y-7Y	4Y-8Y	5Y-10Y	7Y-14Y	12Y-20Y
III-C Non-marijuana and non-MDMA, Import	1Y-4Y	2Y-5Y	3Y-6Y	4Y-7Y	5Y-8Y	6Y-10Y	8Y-15Y	15Y-25Y
II	20Y-24Y	22Y-26Y	24Y-28Y	26Y-30Y	28Y-32Y	30Y-36Y	32Y-37Y	35Y-40Y

P=Probation, M=Months, Y=Years

APPENDIX 5. SENTENCING GRID FOR PROPERTY CRIME

Offense Seriousness Category	*Offender Score*							
	0	**1**	**2**	**3**	**4**	**5**	**6**	**7 or more**
VII	P-1M	P-3M	3M-9M	6M-1Y	9M-18M	1Y-2Y	1Y-3Y	3Y-5Y
VI	P-3M	P-6M	3M-1Y	6M-2Y	1Y-3Y	2Y-5Y	3Y-6Y	5Y-10Y
V	P-6M	P-1Y	3M-2Y	1Y-3Y	18M-5Y	3Y-7Y	4Y-8Y	8Y-15Y
IV	P-1Y	3M-2Y	6M-3Y	1Y-4Y	18M-7Y	3Y-8Y	5Y-12Y	10Y-20Y
III	P-2Y	6M-3Y	9M-5Y	1Y-5Y	2Y-8Y	3Y-10Y	7Y-15Y	15Y-30Y
II	2Y-5Y	3Y-7Y	5Y-8Y	5Y-10Y	8Y-15Y	10Y-18Y	12Y-20Y	15Y-40Y

P=Probation, M=Months, Y=Years

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