

SENATE JUDICIARY COMMITTEE
JANUARY 27, 2025

SUPPLEMENTARY TESTIMONY OF
THE OFFICE OF ATTORNEY GENERAL

IN SUPPORT OF
SENATE BILL 2128

Senator Larson and Members of the Committee:

During the hearings on Senate Bill 2128, as amended by the Attorney General, you heard multiple opponents of the bill claim that longer sentences do not deter crime, increase recidivism, make prisons unsafe, or some combination of these notions. These claims do not withstand scrutiny.

While many agenda-driven groups have made claims like these, the studies they cite are flawed and do not apply to the bill under consideration. When the relevant research is viewed more comprehensively, a different picture emerges. This is especially true when the studies' shortcomings are not hidden.

When inmates spend more time in prison, as they would under a law that requires them to serve a larger percentage of their judge-imposed sentences, they have more time to participate in rehabilitation, treatment, and re-entry programs. They also are unable to commit crimes while in prison and are more likely to be deterred from committing future crimes after release. These are some of the reasons that – contrary to testimony offered by opponents of Senate Bill 2128 – more recent and robust studies often show a reduction in recidivism when inmates are in prison for longer periods of time.

Opponents of Senate Bill 2128 Presented a Distorted View of the Research

During the hearing on Senate Bill 2128, opponents of the bill who testified about research findings relied on flawed and inapplicable research. When they extrapolated findings from these studies and tried to project them onto Senate Bill 2128, they were comparing apples to oranges.

For example, the opponents of the bill often conflated truth-in-sentencing with mandatory minimum sentencing. These are very different practices, and studies on them are not interchangeable. Additionally, truth-in-sentencing can mean many things, and not all studies on truth-in-sentencing would be applicable to Senate Bill 2128.

Also, many studies in this area fail to control for variables (such as age, sex, etc.) that distort their results. When those variables are not controlled, it is difficult to account for their effects on the studied population, even if the studies try to do so with mathematical modeling.

Opponents of the bill who testified about research findings should have identified the limitations of that research – why it does not predict the outcomes of Senate Bill 2128 – rather than imply the research is conclusive.

Opponents of the bill also painted an extremely one-sided and inaccurate picture of the research into the effect of incarceration on recidivism and criminal behavior. They neglected to notify the committee that many studies, especially recent studies, demonstrate a reduction in recidivism from longer incarceration times. Studies are not nearly as conclusive and one-sided as opponents of the bill testified.

In June 2022, the United States Sentencing Commission issued a study report titled *Length of Incarceration and Recidivism*. As part of its study, the Commission reviewed preexisting research on this issue and found much of it was flawed. The Commission’s report stated:

Empirical research on the relationship between length of incarceration and recidivism is limited and presents mixed results. Of the studies that have been published, many are dated (*e.g.*, conducted prior to 2000), use less rigorous research designs, or present results on the relationship between incarceration and recidivism as a sub-analysis within a broader study. Further, a number of the prior studies have methodological deficiencies relating to not appropriately controlling for offender age and, therefore, are not considered valid.¹

Similarly, a detailed and extensive study of existing research by the Criminal Justice Legal Foundation in 2022 noted significant limitations to earlier research on this topic.

Overall, the effect of incarceration length on recidivism appears too heterogeneous to draw universal conclusions, and findings are inconsistent across studies due to methodological limitations. For example, many study samples are skewed toward people with shorter sentences while others include confounds that render results invalid. Of the studies reviewed, some suggested that longer sentences provide additional deterrent benefits in the aggregate, though some studies also had null effects. None suggested a strong aggregate-level criminogenic effect.²

The authors identified several specific problems with earlier research on this topic. Randomized controlled trials (the gold standard) would be unethical for prisoners. Controlling for other variables impacting recidivism or violence is almost impossible. Criminals with longer sentences may be more violent, male, and younger than those with shorter sentences, and those factors may influence recidivism rates. Moreover, there are different methods to measure recidivism, so consistency across studies and studied populations is lacking.

Researchers from Loyola University who studied Illinois’ truth-in-sentencing laws also identified some inherent problems in relying on the existing research.

Given the fact that the impact of the law appears to vary from state to state, depending on the offenses covered under [truth-in-sentencing] and the overall sentencing structure and/or courtroom culture in place, it is clear that analyses need to be done on a state-by-state basis to take into account the nuances of each state's [truth-in-sentencing] law and sentencing structure to assess impact on sentence lengths and/or lengths of time to serve.³

Conclusory study findings offered to the committee in hearings on Senate Bill 2128 simply painted an inaccurate picture.

More Time in Incarceration Has Been Shown to Reduce Recidivism and Reduce Violence in Prison

There are at least three ways that requiring an inmate to serve more of the sentence imposed by a judge can reduce the risk of recidivism.

1. Deterrence: The inmate will realize the cost of committing a crime is higher than the reward.
2. Incapacitation: The inmate cannot commit new crimes while in prison.
3. Rehabilitation: The inmate will have more time to be rehabilitated through treatment programs, education, re-entry programs, and other services available in prison.

Studies have found these impacts increase as the amount of time incarcerated increases.⁴ The reduction in recidivism from incapacitation is self-evident. And some researchers have commented on the need for increased incarceration time to have a rehabilitative effect on criminals.⁵

The 2022 Criminal Justice Legal Foundation study report included citations to many research reports that found reductions in recidivism from increases in incarceration time, although fewer studies showed no difference.

The United States Sentencing Commission's 2022 study, which controlled for many of the variables that plague earlier studies, found that "offenders serving longer sentences had a lower likelihood of recidivism and took longer to recidivate."⁶ Specifically, offenders who were incarcerated between 60 and 120 months had about an 18% reduction in recidivism compared to those with shorter sentences. Offenders who were incarcerated more than 120 months had about a 29% reduction in recidivism compared to those with shorter sentences.⁷

The Commission's findings were not an outlier. The findings "were almost identical for both the 2010 cohort studied in [the 2022] publication and the 2005 cohort studied in the Commission's previous publication."⁸

Also, in Appendix A of the Commission's report, there is a review of literature on this topic. Many of the studies listed in the appendix demonstrated a reduction in recidivism correlated with longer times in prison.

A study of Illinois' truth-in-sentencing (TIS) law on murderers and sex offenders found that longer sentences correlated to a reduction in violence in prison.

Among the overall sample of murderers included in the analyses of disciplinary incidents, the average number of disciplinary tickets was 22. When multivariate statistical analyses were performed to isolate the influence of TIS on the overall number of disciplinary incidents/tickets, the analyses revealed that murderers subject to TIS *receiving an average of almost 5 fewer tickets*, on average, than non-TIS inmates. Additional analyses revealed that TIS had no statistical relationship with whether or not the inmate received a disciplinary ticket for a *serious* incident: roughly 55 percent of both TIS and non-TIS murderers had a ticket for a serious incident. Serious incidents were defined as any offenses that carry a maximum penalty of one year of loss or restriction of privileges, grade reduction, good time revocation and/or segregation, and included offenses in [sic] such as violent assaults or participation in a security threat group. Similarly, TIS had no statistical relationship with whether or not the inmate received a disciplinary ticket for an assault, against either another inmate or staff, with roughly 19 percent of both TIS and non-TIS murderers receiving a ticket for any assault.⁹ ...

Further, the existing literature on inmate disciplinary patterns, particularly for those convicted of murder, appears to suggest that longer lengths of time to serve may actually reduce the incidence and nature of institutional violence by inmates, and that other inmate characteristics, such as age, need to be statistically controlled.¹⁰

Conclusions

Studies on the relationships among truth-in-sentencing, incarceration time, recidivism, and violence in prison have significant limitations and are not universally applicable.

The research cited by critics of Senate Bill 2128, as amended by the Attorney General, almost certainly is not applicable to the contents of this bill. The limited utility of that research – and the flaws in the methods used in such research – have been highlighted by several major studies in the past few years. The shortcomings of the studies should have been addressed in the testimony to the committee.

Recent research – that attempts to control for confounding variables and is transparent about its applicability – generally shows that longer incarceration times correlate to decreased recidivism (or does not show an impact) and has shown a reduction in violence in prison.

¹ United States Sentencing Commission, *Length of Incarceration and Recidivism*, p. 3 (June 2022).

² Berger, Elizabeth, and Scheidegger, Kent, *Sentence Length and Recidivism: A Review of the Research*, Criminal Justice Legal Foundation, p. 2 (June 2022) (emphasis added).

³ David E. Olson, Ph.D. et al., *FINAL REPORT: The Impact of Illinois' Truth-in-Sentencing Law on Sentence Lengths, Time to Serve and Disciplinary Incidents of Convicted Murderers and Sex Offenders*, Loyola University Chicago, Department of Criminal Justice, Prepared for The Illinois Criminal Justice Information Authority (June 2009) (emphasis added).

⁴ E.g., Wei Long, 2016. *Does Longer Incarceration Deter or Incapacitate Crimes? New Evidence from Truth-in-Sentencing Reform*, Working Papers 1607, Tulane University, Department of Economics. ("We observe statistically significant -7 percent deterrent effect of TIS on growth of violent crime two years after its passage. A series of placebo tests confirm the robustness of the estimates and inferences. In the long-run, additional incapacitative effect also becomes significant, making the treatment effect of TIS even greater in magnitude. Even though insignificant in the first two years after TIS was passed, growth of non-violent property crime rates decreases by 7 percent in the long-run in TIS states, indicating relative greater importance of incapacitative effect which locks up offenders who commit both types of crimes. A rough approximation shows that TIS is an economically efficient method to decrease crimes.")

⁵ Al Weswasi, E., Sivertsson, F., Bäckman, O. et al., *Does sentence length affect the risk for criminal recidivism? A quasi-experimental study of three policy reforms in Sweden*, *J Exp Criminal* 19, 971–999 (2023).

⁶ United States Sentencing Commission, at p. 19, 20.

⁷ United States Sentencing Commission, at p. 19, 20.

⁸ United States Sentencing Commission, at p. 22.

⁹ Olson, at p. 5-6.

¹⁰ Olson, at p. 17.



LENGTH OF INCARCERATION AND RECIDIVISM

UNITED STATES SENTENCING COMMISSION
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INTRODUCTION

INTRODUCTION

The United States Sentencing Commission (“the Commission”) began studying recidivism shortly after the enactment of the Sentencing Reform Act (SRA) of 1984.¹ Understanding federal offender recidivism was central to the Commission’s initial work. The Commission’s various studies on recidivism advance its mission of conducting research on sentencing issues related to the purposes of sentencing set forth in the SRA.² Exemplifying this, the criminal history provisions in the *Guidelines Manual* were developed, in part, based on information regarding federal offenders’ risk of recidivism.³ Information about recidivism is also relevant to the Commission’s obligation to formulate sentencing policy that “reflect[s], to the extent practicable, advancement[s] in knowledge of human behavior as it relates to the criminal justice process.”⁴

In 2021, the Commission began its second multi-publication recidivism series.⁵ The first publication, *Recidivism of Federal Offenders Released in 2010*, provides a broad overview of recidivism amongst 32,135 federal offenders either released from federal prison or sentenced to a term of probation in 2010.⁶ The Commission also released three reports that examined recidivism among specific groups of federal offenders: *Recidivism of Federal Firearms*

*Offenders Released in 2010, Recidivism of Federal Drug Trafficking Offenders Released in 2010, and Recidivism of Federal Violent Offenders Released in 2010.*⁷ The fifth and sixth publications in the series examine the recidivism of federal offenders receiving Federal Bureau of Prisons programming while incarcerated, *Recidivism and Federal Bureau of Prisons Programs: Drug Program Participants Released in 2010*, and *Recidivism and Federal Bureau of Prisons Programs: Vocational Program Participants Released in 2010.*⁸

This study, the seventh in the recidivism series, examines the relationship between length of incarceration and recidivism. In 2020, the Commission published its initial comprehensive study on length of incarceration and recidivism. In that study, which examined offenders released in 2005, the Commission found that federal offenders receiving sentences of more than 60 months were less likely to recidivate compared to a similar group of offenders receiving shorter sentences.⁹ This study replicates the prior analysis, however, it examines a more current cohort of federal offenders released in 2010.

This study empirically explores three potential relationships that may exist between length of incarceration and recidivism:

Relationship	Description
Incarceration is preventative.	As the length of incarceration increases the likelihood of recidivism decreases.
Incarceration is criminogenic.	As the length of incarceration increases the likelihood of recidivism increases.
No relationship between incarceration and recidivism.	No statistically significant relationship between length of incarceration and recidivism is identified. ¹⁰

Empirical research on the relationship between length of incarceration and recidivism is limited and presents mixed results.¹¹ Of the studies that have been published, many are dated (e.g., conducted prior to 2000), use less rigorous research designs, or present results on the relationship between incarceration and recidivism as a sub-analysis within a broader study.¹² Further, a number of the prior studies have methodological deficiencies relating to not appropriately controlling for offender age and, therefore, are not considered valid.¹³

Most studies examining the association between length of incarceration and recidivism examine state offenders.¹⁴ Of the thirteen relevant studies identified (Appendix A), only two focused on the federal offender population.¹⁵ The first study, Rhodes et al. (2018) found that longer prison terms modestly reduce recidivism.¹⁶ Specifically, the likelihood of recidivism was reduced by approximately one percent for every 7.5-month increase

in sentence length.¹⁷ In the second study, the Commission found that federal offenders receiving sentences of more than 60 months incarceration had lower odds of recidivism when compared to similar offenders receiving shorter sentences.¹⁸ Specifically, the odds of recidivism were approximately 30 percent lower for offenders incarcerated for more than 120 months and approximately 17 percent lower for offenders incarcerated for more than 60 months up to 120 months.¹⁹ The Commission did not find any statistically significant relationship between length of incarceration and recidivism for offenders incarcerated for less than 60 months.²⁰

The purpose of this report is to expand on the limited research examining length of incarceration and recidivism in the federal offender population and, thereby, inform policymakers.²¹

KEY FINDINGS

1 The results of this study, examining federal offenders released in 2010, are almost identical to the findings established in prior Commission research examining federal offenders released in 2005. In both studies, the odds of recidivism were lower for federal offenders sentenced to more than 60 months incarceration compared to a matched group of offenders receiving shorter sentences.

2 The odds of recidivism were approximately 29 percent lower for federal offenders sentenced to more than 120 months incarceration compared to a matched group of federal offenders receiving shorter sentences.

3 The odds of recidivism were approximately 18 percent lower for offenders sentenced to more than 60 months up to 120 months incarceration compared to a matched group of federal offenders receiving shorter sentences.

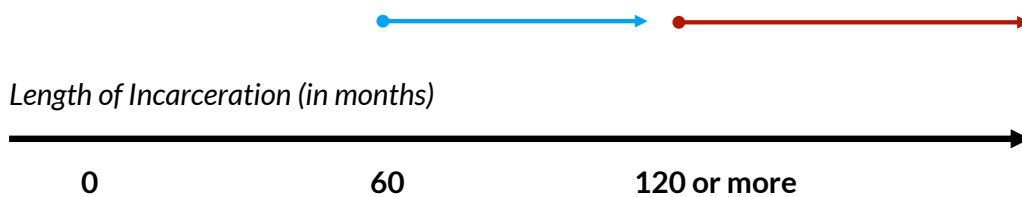
4 For federal offenders sentenced to 60 months or less incarceration, the Commission did not find any statistically significant differences in recidivism.



Incarceration lengths of more than 60 months up to 120 months had a preventative effect.



Incarceration lengths of more than 120 months had a preventative effect.



SCOPE OF ANALYSIS

This report uses data from the Commission, Federal Bureau of Investigation (FBI), Federal Bureau of Prisons (BOP), and the Administrative Office of the United States Courts (AO) to analyze the recidivism of federal offenders released from federal prison or sentenced to probation in 2010. The offenders in the study cohort were identified in cooperation with the BOP and the AO. The BOP provided identifying information, release dates, and other pertinent information for the Commission to identify offenders released from prison. The AO provided identifying information, some revocation information, and other pertinent information for offenders sentenced to probation. The Commission compiled the identifying information for these offenders to obtain criminal records in partnership with the FBI.

The data used in this report combines data regularly collected by the Commission²² with data compiled as part of a data sharing agreement with the FBI's Criminal Justice Information Services Division.²³ Through an agreement with the FBI, the Commission collected and processed criminal history records from

Study Cohort

This study examines 32,135 federal offenders who satisfied the following criteria:

- United States citizens;
- Re-entered the community during 2010 after discharging their sentence of incarceration or by commencing a term of probation;
- Not reported dead, escaped, or detained;
- Have valid FBI numbers which could be located in criminal history repositories (in at least one state, the District of Columbia, or federal records).

all state and federal agencies for the offenders in the study.²⁴ The Commission then combined the criminal history record data with offender and offense related data collected by the Commission.

Defining and Measuring Recidivism

Recidivism “refers to a person’s relapse into criminal behavior, often after the person receives sanctions or undergoes intervention for a previous crime.”²⁵ Recidivism measures can provide policymakers with information regarding the relative threat to public safety posed by various types of offenders, and the effectiveness of some public safety initiatives in deterring crime and rehabilitating offenders.²⁶ Recidivism measures are used by numerous public safety agencies to measure program performance and inform policy decisions on issues such as pretrial detention, prisoner classification and programming, and offender supervision in the community.²⁷

Two measures are foundational to recidivism research, both of which can impact the outcomes of recidivism analyses. The first measure is the *type of event* used to indicate a relapse into criminal behavior. Recidivism is typically measured by criminal acts that resulted in the rearrest, reconviction, or reincarceration of an offender.²⁸ The Commission used rearrest for this study for several reasons. Rearrest is the most common measure of recidivism used by federal agencies in recent recidivism studies.²⁹ Federal agencies are using rearrest as the primary measure because it is a more reliable measure than reconviction or reincarceration due to the incomplete nature of disposition data.³⁰ Criminal records often fail to include information pertaining to reconviction

or reincarceration because jurisdictions inconsistently report them. The records compiled for this study reflect this inconsistency. For example, records for 44.1 percent of rearrest charges had no associated disposition information.

Using rearrest does result in higher recidivism rates than reconviction or reincarceration. Not only are rearrests more consistently reported, but also the evidentiary standard for an arrest (probable cause) is less stringent than the evidentiary standard for a conviction and, therefore, incarceration (beyond a reasonable doubt). Because not all arrests result in conviction or incarceration, rearrests can overstate recidivism.³¹

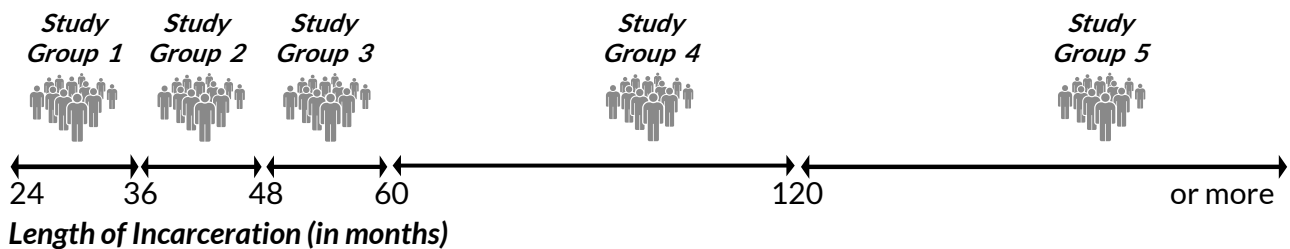
The second component of measuring recidivism is the *follow-up period*; the period of time over which events are counted following re-entry into the community. After a starting event—in this study, release from prison into the community or placement on probation—recidivism events are documented through the end of the follow-up period. The length of the follow-up period varies across recidivism studies. Due to limitations on available data, some studies follow offenders for as little as six months. Other studies follow offenders for several years. Tracking offenders for a longer duration provides a more accurate estimate of recidivism or desistance from crime.³² For this study, the Commission used an eight-year follow-up period.

METHODOLOGY

This study analyzes five ordered study groups receiving different lengths of incarceration (Figure 1). The Commission determined the length of incarceration interval for each study group based on natural timeframes and available sample size. The first three study groups are composed of offenders sentenced within one-year intervals: more than 24 months

up to 36 months; more than 36 months up to 48 months; and more than 48 months up to 60 months. The fourth and fifth groups required larger timeframes due to a smaller number of offenders in each group. This study used five-to-ten and greater than ten years as the boundaries to align with five-year clustering often seen in federal mandatory minimum sentences.³³

FIGURE 1.

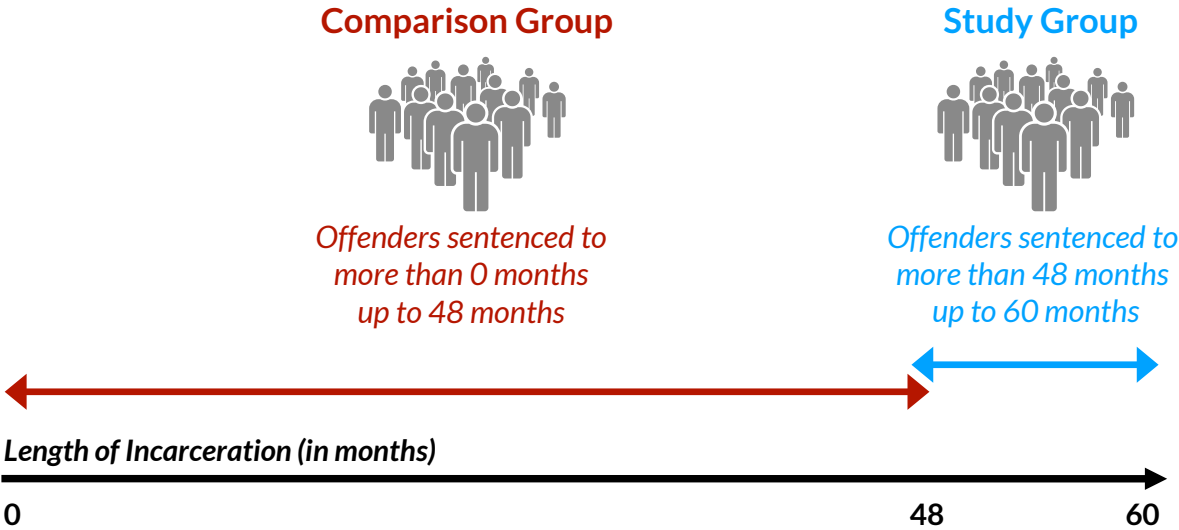


Study Group	Length of Incarceration
1	>24-36 months
2	>36-48 months
3	>48-60 months
4	>60-120 months
5	>120 months

The Commission compared the recidivism rate of offenders in each study group to the recidivism rate of a similar group of offenders receiving shorter lengths of incarceration. For example, this study compared the recidivism rate of

offenders incarcerated for more than 48 months up to 60 months to the recidivism rate of a similar group of offenders incarcerated for 48 months or less (Figure 2). This process was replicated for each of the five study groups (Figure 1).

FIGURE 2.



Doubly Robust Estimation

This study utilized a two-stage research process to analyze the relationship between length of incarceration and recidivism. In the first stage, the Commission created a comparison group for each study group through matching and weighting. In the second stage, the Commission used an outcome regression model—in this case multiple logistic regression—to estimate the effect of length of incarceration on recidivism. This two-stage process of creating comparison groups and then utilizing regression modeling results in a *doubly robust estimation*, which is particularly powerful in that only one of the two models needs to be correctly specified to obtain unbiased estimates.³⁴

Creating Comparison Groups Using Matching and Weighting

The first stage of the analysis focused on creating a similar comparison group for each study group (Figure 1). When creating a comparison group, researchers must consider two important factors: the necessary degree of similarity between study and comparison groups, and sample size. The study group and comparison group must be sufficiently similar on

select attributes to isolate the effect of the variable of interest (*i.e.*, length of incarceration) on the outcome variable (*i.e.*, recidivism). Colloquially, this is often described as comparing *apples-to-apples*. The attributes selected by the researcher, called control variables, are generally important variables that are perceived to influence the outcome. For example, if the study and comparison groups have similar proportions of males, any difference in recidivism rates observed would not be attributed to gender. Here, the researcher would have *controlled* for gender in their model. Ideally, groups being compared would be identical on all attributes except for the variable of interest.

Sample size is also important because statistical tests require sufficiently large sample sizes to detect existing relationships. This is referred to as *power* in statistics. Larger unbiased samples provide better estimates of how similar individuals not involved in the study will perform (*i.e.*, generalize results). Therefore, in addition to similarity between groups, researchers must be cognizant of ensuring sufficient sample size exists to detect a relationship between the variables of interest and the outcome variable.

In this study, the Commission used *propensity score matching* to create

comparison groups. Matching creates a comparison group by identifying individuals who are similar on key attributes determined by the researcher. The researcher specifies the level of precision for matches. As the level of precision increases, it becomes more difficult to identify matches and, therefore, the sample size shrinks. Thus, researchers must balance the level of precision in matching with the subsequent sample size. With weighting, generally, individuals in the comparison group who are similar to individuals in the study group are given more weight than individuals who are dissimilar. As the level of similarity between matches increases, the weights increase.³⁵ One advantage of weighting, as compared to matching with a high degree of precision (e.g., exact matching), is that it often results in a larger sample size because individuals with some dissimilarity will remain in the study but receive less weight.

The Commission chose to determine the length of incarceration for each study group and then create a matched comparison group for each study group. By determining the study groups' length of incarceration first and then developing comparison groups second, this study preserves the natural characteristics of offenders serving various lengths of

incarceration. For example, in general, fraud offenders receive shorter sentences than drug trafficking offenders. This means the composition of offenders serving sentences longer than 120 months will, understandably, be different than the composition of offenders serving sentences of 24 to 36 months of incarceration. While there will be some variation in the characteristics of offenders in each study group (i.e., at various lengths of incarceration), the differences between study groups and comparison groups will be minimal due to matching and weighting.

With either matching or weighting, the first step is to determine which attributes must be controlled for. Researchers have identified five principal attributes that studies on length of incarceration and recidivism should address: age, gender, race, prior criminal history, and instant offense type.³⁶ These prior research studies have principally examined the recidivism of offenders sentenced in state courts.³⁷ In prior studies, the Commission has confirmed that these factors are also associated with the recidivism of federal offenders. Specifically, as offenders' age-at-release increases recidivism decreases;³⁸ male offenders have higher recidivism rates than female offenders;³⁹ as an offender's criminal history category increases their recidivism rate increases;⁴⁰ and the type

of instant offense is associated with recidivism (e.g., drug trafficking offenders have higher recidivism rates than fraud offenders).⁴¹

Additionally, prior Commission research has identified associations between education level, violence, and weapons offenses, with recidivism. The Commission found that offenders with higher levels of education have lower recidivism rates than offenders with lower levels of education.⁴² The Commission has also found that offenders who used violence in conjunction with the instant offense or in prior offenses have higher rates of recidivism than offenders who have never used violence in

connection with an offense.⁴³ Additionally, the Commission has found that offenders who commit weapons offenses have higher recidivism rates than offenders who commit other offenses.⁴⁴ Based on the Commission’s prior research, and that conducted by other researchers, this report controls for the attributes listed in Table 1.

As noted, the research design utilized a combination of matching and weighting to create comparison groups. This study used exact matching to balance the following principal attributes: age-at-release, gender, race, criminal history category, and primary sentencing guideline. Therefore, the study group and comparison group were identical

TABLE 1.

Control Attributes and Variables	
Exact Match	Weighted
Age-at-release	High school completion
Gender	Violent offense
Race	Weapons offense
Criminal history category (CHC)	Received substantial assistance departure
Instant offense type (sentencing guideline)	Received safety valve adjustment

on these attributes. Weighting was used to balance the remaining attributes: high school completion, violent offense, weapons offense, received substantial assistance, and received safety valve adjustment.⁴⁵

After matching and weighting, the study and comparison groups were assessed to confirm the groups were sufficiently similar. To assess the similarity between study and comparison group, the Commission confirmed the Standardized Mean Difference (SMD) between each study and comparison group was less than the conventional 0.1 threshold.⁴⁶ The overall SMD between study and comparison groups for each analysis ranged from 0.02 to 0.08. Therefore, with respect to the attributes in Table 1, the study and comparison groups were extremely similar.

Regression Modeling

After establishing the comparison groups, the Commission used multiple logistic regression to estimate the relationship between length of incarceration and recidivism. Logistic regression is a modeling technique used to analyze the relationship between attributes (e.g., length of incarceration, age, gender, etc.) and a binary response variable (e.g., recidivism).⁴⁷ In this study, logistic regression was used to analyze the relationship between length of incarceration and recidivism while controlling for the attributes listed in Table 1.

Logistic regression estimates are often reported as an *odds ratio*. In this study, the odds ratio represents the odds of recidivism for the study group as compared to the odds of recidivism for the comparison group. An odds ratio of one indicates no difference in recidivism between the groups. An odds



The Commission's research design utilized a combination of matching and weighting to create comparison groups.

ratio less than one indicates the study group had lower odds of recidivism than the comparison group. An odds ratio greater than one indicates the study group had greater odds of recidivism than the comparison group.⁴⁸

In addition to producing an estimate, each estimate is tested for statistical significance. Testing estimates for statistical significance can be analogized to the burden of proof consideration in a criminal trial. The significance test begins with the premise that there is no relationship between the variables being tested, in this study length of incarceration and recidivism, similar to the premise that a defendant is innocent until proven guilty. In statistics, this presumption of innocence is referred to as the null hypothesis. The

researcher collects data, or evidence, which is then judged to determine if the results of the analysis could have happened by random chance. In statistics, the threshold of beyond a reasonable doubt is usually numerically defined with a *p-value*. The *p-value* numerically defines the degree of evidence required to reject the null hypothesis (*i.e.*, no relationship exists between length of incarceration and recidivism). In this study, the Commission used the conventional threshold of 0.05 to denote statistical significance. Thus, if the *p-value* is less than 0.05 we reject the null hypothesis and consider the results to be indirect evidence that a relationship between the variable of interest and outcome—length of incarceration and recidivism in this study—exists.

RESEARCH FINDINGS

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As noted, the Commission identified the study groups first and then created comparison groups to preserve the natural composition of offenders sentenced to various lengths of incarceration. The comparison groups were composed of offenders who were similar, based on matching and weighting, and had a shorter length of incarceration.⁴⁹

In addition to achieving extremely similar study and comparison groups through matching and weighting, this study retained a large sample size. When creating matched comparison groups, it is natural to lose some portion of the study group. Overall, a large proportion, between 75.4 and 92.2 percent, of the study group was retained after matching for each analysis.

The sample size for individual analysis ranged from 2,598 to 8,578 offenders and the total sample size for the study was 22,928 (Figure 3).

FIGURE 3.

Total Sample Size: 22,928

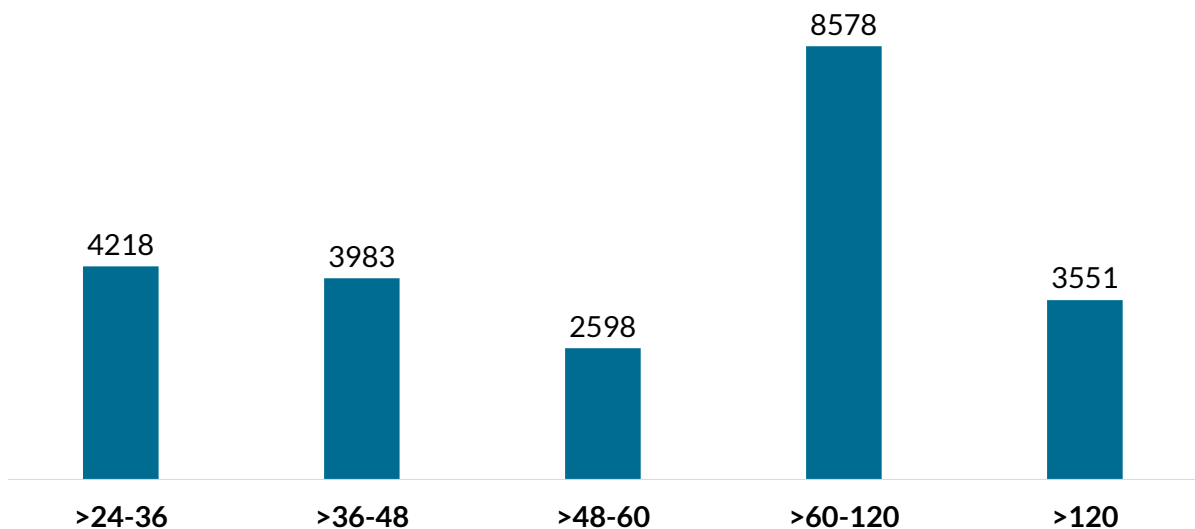


Table 2 provides information on the median length of incarceration for each study and comparison group. Differences in the median length of incarceration range from 13.0 to 76.0 months. As the length of incarceration for the study group increased, the difference in median sentences increased. For example, the differences in median sentence for the first study and comparison group (>24 to 36 months) was 13.0 months, while the difference in median sentence imposed for the last study group (>120 months) was 76.0 months.

Table 3 provides information on the five principal offender characteristics that are exactly matched between the study and comparison groups. Prior research examining sentence length and offender recidivism identified a minimum set of control variables: age, gender, race, prior

criminal history, and instant offense type.⁵⁰ Due to the importance of these attributes, the Commission used exact matching for these control variables. Consequently, the study group and comparison groups were identical on each of these attributes. For example, for the >24 to 36 months analysis, offenders in the study and comparison groups had average ages of 35.8 years, 86.7 percent of each group was male, 65.5 percent of each group was White, etc.

As expected, the composition of offenders varied between the study groups with different lengths of incarceration. As the length of incarceration increased, the proportion of males increased, the proportion of Black offenders increased, the severity of CHC increased, and the proportion of §2D1.1 offenders increased (Table 3).

TABLE 2.

Median Incarceration Length of Study and Comparison Groups		
Group	Study Group	Comparison Group
>24 to 36 Months	28.0	15.0
>36 to 48 Months	40.0	24.0
>48 to 60 Months	51.0	33.0
>60 to 120 Months	78.0	39.0
>120 Months	160.0	84.0

TABLE 3.

Attributes	Exact Matched Characteristics				
	Length Of Incarceration				
	>24-36 Months	>36-48 Months	>48-60 Months	>60-120 Months	>120 Months
Age (years)					
Mean Age	35.8	36.0	36.4	37.3	41.0
Gender (%)					
Male	86.7	88.8	90.7	92.6	96.5
Race (%)					
White	65.5	59.5	56.3	52.1	33.2
Black	30.6	36.6	40.6	45.9	66.0
Other	4.0	3.8	3.1	2.0	0.8
Criminal History Category (%)					
CHC I	53.5	47.8	37.8	29.6	16.1
CHC II	10.4	8.5	10.2	13.8	11.1
CHC III	18.4	16.6	15.9	20.4	19.0
CHC IV	10.3	13.0	12.4	11.8	14.0
CHC V	3.0	6.4	10.5	8.6	8.2
CHC VI	4.4	7.6	13.3	15.8	31.6
Principal Guidelines (%)					
§2D1.1	41.2	48.2	50.5	71.7	81.6
§2B1.1	18.8	12.1	8.3	2.7	0.0
§2B3.1	0.4	2.1	2.4	3.0	8.7
§2K2.1	20.0	25.9	31.1	15.9	6.8
§2L1.1	4.9	1.5	0.5	0.1	0.0
Other	14.6	10.2	7.3	6.6	2.8

The Commission used a multiple weighted logistic regression to estimate the effect of length of incarceration on recidivism. The results were mixed across the various study groups (Table 4). For offenders sentenced to 60 months or less, there was no statistically significant effect. For offenders sentenced to more than 60 months, there was a statistically significant preventative effect.

Offenders incarcerated for more than 60 months up to 120 months had a statistically significant preventative relationship between length of incarceration and recidivism. Specifically, the regression model estimated the odds of recidivism for offenders incarcerated for more than 60 months up to 120 months was approximately 18 percent lower than the odds of recidivism for the comparison group receiving shorter sentences (Figure 4). In addition to estimating the likelihood of recidivism, the Commission analyzed time-to-rearrest. The average time-to-

rearrest for the study group was 861 days while the average time-to-rearrest for the comparison group was 807 days.⁵¹ Thus, offenders serving longer sentences had a lower likelihood of recidivism and took longer to recidivate.

TABLE 4.

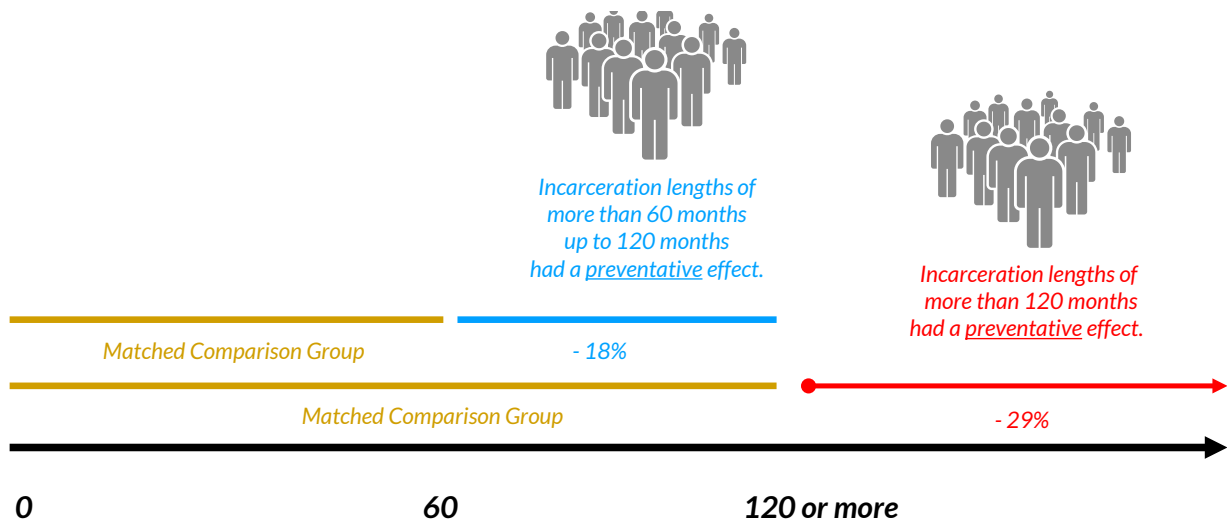
Weighted Multiple Logistic Regression Results	
	Likelihood of Recidivism
>24 to 36 Months	+ 7%
>36 to 48 Months	- 2%
>48 to 60 Months	- 1%
>60 to 120 Months	- 18% ***
>120 Months	- 29% ***

p-values: * p ≤ .05; ** p ≤ .01; *** p ≤ .001

Offenders incarcerated for more than 120 months had a statistically significant preventative relationship between length of incarceration and recidivism. Specifically, the regression model estimated the odds of recidivism for individuals incarcerated for more than 120 months was approximately 29 percent lower than the odds of recidivism for the comparison group receiving shorter sentences (Figure

4).⁵² In addition to estimating the likelihood of recidivism, the Commission analyzed time-to-rearrest. The average time-to-rearrest for the study group was 915 days while the average time-to-rearrest for the comparison group was 852 days.⁵³ Thus, offenders serving longer sentences had a lower likelihood of recidivism and took longer to recidivate.

FIGURE 4.



COMPARISON OF 2010 AND 2005 RELEASE COHORTS

COMPARISON OF 2010 AND 2005 RELEASE COHORTS

In 2020, the Commission published *Length of Incarceration and Recidivism*, which examined the relationship between length of incarceration and recidivism for offenders released in 2005.⁵⁴ The 2020 study included two research designs: (1) a design using propensity score matching and weighted multiple logistic regression, and (2) a design using non-bipartite matching and multiple logistic regression.

For this study, the Commission replicated the first research design—propensity score matching and weighted multiple logistic regression—examining offenders released in 2010. Using this research design, the findings were almost identical for both the 2010 cohort

studied in this publication and the 2005 cohort studied in the Commission’s previous publication. Specifically, findings were only statistically significant for offenders sentenced to more than 60 months incarceration, and those effect sizes were identical (Table 5). In both studies, offenders sentenced to more than 60 months up to 120 months were approximately 18 percent less likely to recidivate relative to a comparison group receiving shorter lengths of incarceration. Offenders sentenced to more than 120 months were approximately 29 percent less likely to recidivate relative to a comparison group receiving a shorter sentence of incarceration.

TABLE 5.

	Likelihood of Recidivism	
	2010 Cohort	2005 Cohort
>24 to 36 Months	+ 7%	+ 3%
>36 to 48 Months	- 2%	- 5%
>48 to 60 Months	- 1%	- 6%
>60 to 120 Months	- 18% ***	- 18% **
>120 Months	- 29% ***	- 29% ***

p-values: * p ≤ .05; ** p ≤ .01; *** p ≤ .001

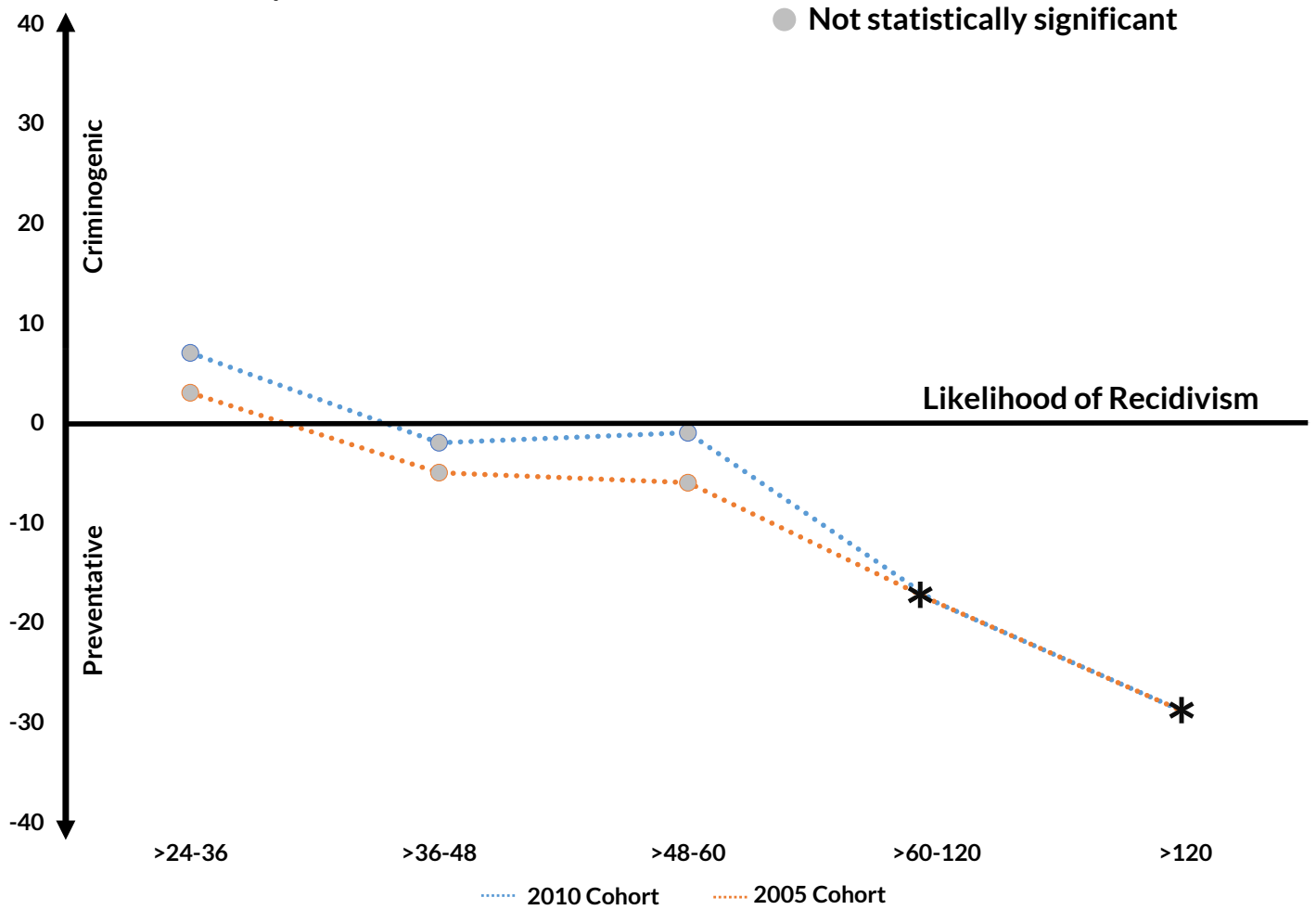
Figure 5 graphs the model estimates for federal offenders released in 2005—the original study—and 2010. The plot shows the similarity in model estimates for all study groups and denotes the statistically significant estimates, represented by asterisks, for offenders sentenced to more than 60 months incarceration. Thus, the findings remain stable between the two studies analyzing the 2005 and 2010 release cohorts.

As noted, the Commission also used an alternative research design in its previous study released in 2020. The alternative research design used non-bipartite matching to create comparison groups that exactly matched all characteristics in Table 1, except for age-at-release which could vary by one year, then used multiple logistic regression to estimate the effect of sentence length on recidivism.⁵⁵ While these alternative designs are not described in this publication, the Commission did confirm that analysis of the 2010 offender cohort using the same alternative research design yielded similar results.⁵⁶

FIGURE 5.

Recidivism Findings
2010 and 2005 Comparison

* Indicates statistically significant
● Not statistically significant



CONCLUSION

CONCLUSION

This study examined offenders released from federal prison or sentenced to probation in 2010. The study used a combination of matching, weighting, and multiple logistic regression to estimate the effect of length of incarceration on recidivism.

The Commission found a statistically significant preventative effect for offenders sentenced to more than 60 months incarceration. Specifically, offenders sentenced to more than 60 months incarceration had lower odds of recidivism as compared to similar offenders receiving shorter sentences. The odds of recidivism were approximately 18 percent lower for offenders sentenced to more than 60 months up to 120 months incarceration compared to a matched group of federal offenders receiving shorter sentences. The odds of recidivism were approximately 29 percent lower for federal offenders sentenced to more than 120 months incarceration compared to a matched group of federal offenders receiving shorter sentences. In the 2010 release cohort, approximately 41 percent of offenders received sentences of more than 60 months incarceration.

The Commission found no statistically significant effect for offenders sentenced to 60 months, or less, incarceration. Consequently, the Commission has no basis to conclude that incarceration for 60 months or less has a criminogenic or preventative effect. In the 2010 release cohort, approximately 59 percent of offenders received sentences of 60 months or less incarceration.

The current research findings were similar to the findings established in the original study, *Length of Incarceration and Recidivism*, which examined offenders released in 2005. In the original study, using a comparable research design, offenders sentenced to more than 60 months incarceration had a statistically significant preventative effect.

In conclusion, this study found that offenders confined for longer periods of incarceration had lower odds of recidivism, however, those odds were only substantively lower for offenders sentenced to lengthy incarceration terms. The findings suggest the preventative effect of length of incarceration on recidivism was only realized for offenders sentenced to more than 60 months incarceration.

APPENDICES

APPENDIX A

Appendix A provides a review of published literature on the relationship between length of incarceration and recidivism.

Literature Review

There have been numerous studies, and two comprehensive literature reviews, examining the association between length of incarceration and recidivism.

In 2009, Nagin et al. completed a thorough review of existing literature on the relationship between incarceration length and recidivism.⁵⁷ The review examined two experimental studies and 17 nonexperimental studies (three matched studies and 14 regression studies).⁵⁸ Conclusions were not drawn from the 14 regression studies due to fundamental analytical flaws in the study design, specifically, sensitivity in the regression-based studies related to specification errors in modeling the relationship between age and offending.⁵⁹ Nagin and his co-authors concluded “there [was] little convincing evidence on the dose-response relationship between time spent in confinement and reoffending rate.”⁶⁰

Following that review, only a small number of methodologically rigorous studies examining the relationship between length of incarceration and recidivism have been conducted. In 2021, Berger et al.

completed an updated literature review and concluded that the literature on length of incarceration and recidivism continues to be somewhat inconsistent, with some studies finding no effect on recidivism, while other studies indicating increased prison length reduces recidivism, albeit in some studies only slightly.⁶¹ These mixed results may be explained, in part, by the use of varying methodologies (e.g., propensity score matching, regression discontinuity design, etc.) and the examination of varying research populations (e.g., juvenile, state, or federal offender populations).

A brief synopsis of the primary studies examining the association between length of incarceration and recidivism are presented here.

Jaman et al. (1972) examined the recidivism rate of male burglars who received sentences longer than 25 months with a similar group of burglars who served 24 months or less.⁶² The study used by-variable matching to control for a number of offender attributes.⁶³ The authors followed offenders for 6, 12, and 24 months post-release.⁶⁴ They found that offenders who served longer sentences

had a higher recidivism rate, however, the difference was only statistically significant for a 24-month post-release follow-up period.⁶⁵

Kraus (1981) examined juvenile offenders serving varying incarceration lengths.⁶⁶ Juvenile offenders were separated into three distinct age groupings: 9-12, 13, and 14-15.⁶⁷ The study matched on a variety of offender attributes (e.g., age, sex, offense, and prior record).⁶⁸ Kraus found longer confinement was associated with higher recidivism for two age groups and lower recidivism for the third age group. None of the study findings were statistically significant.⁶⁹

Berecochea et al. (1981) examined a sample of felony male offenders in California who had their incarceration sentences reduced by six months.⁷⁰ The recidivism rate of the offenders receiving the six-month reduction was higher than the recidivism rate of offenders not benefiting from a sentence reduction.⁷¹ Thus, offenders receiving longer prison sentences had lower recidivism rates. However, this effect was not statistically significant.⁷²

Deschenes et al. (1995) compared the recidivism rates of incarcerated offenders serving their full sentence to offenders receiving a reduction in incarceration time due to placement on Intensive Community Supervision (ICS).⁷³ While all study offenders were originally incarcerated, the comparison group had a reduced

incarceration period due to placement on ICS. The study found “similar rates of rearrest among those who were diverted from prison and those who remained in prison . . . [the] rates were not significantly different”.⁷⁴

Loughran et al. (2009) used longitudinal data from a sample of serious juvenile offenders to explore the relationship between length of stay in institutional placement and future rearrests.⁷⁵ The study used propensity score matching to balance several offender attributes across treatment and comparison groups.⁷⁶ The study found no evidence that varying doses of length of stay affect future rearrests in either a criminogenic or preventative direction.⁷⁷

Green et al. (2010) examined more than 1,000 defendants sentenced in the District of Columbia Superior Court.⁷⁸ They exploited a pseudo-randomization strategy between nine judges and argued that variation in judicial discretion resulted in random variation in sentence lengths. Offenders were tracked for four years post-release.⁷⁹ The study found that “incarceration seems to have little net effect on the likelihood of subsequent rearrest”.⁸⁰

Snodgrass et al. (2011) examined the felony reconviction rate of more than 4,500 prisoners in the Netherlands.⁸¹ The study used propensity score matching to create balanced groups on a variety of offender attributes.⁸² The majority of

the offenders in the study, 86 percent, were sentenced to less than one year of incarceration.⁸³ Offenders were grouped into two categories, *low-dose* and *high-dose*, for comparison. Overall, *low-dose* offenders were convicted of .033 more felonies per year as compared to similar *high-dose* offenders.⁸⁴ However, the observed preventative effect was not statistically significant.⁸⁵

Kuziemko (2012) examined the impact of length of incarceration on recidivism for more than 17,000 parolees in Georgia.⁸⁶ The author used an instrumental variable to account for imbalance on several attributes (e.g., prior incarceration, offense severity, etc.).⁸⁷ Offenders were tracked for a three-year follow-up period to determine if they returned to prison for a new crime. The study found that lengthier sentences were associated with a decrease in recidivism of 1.3 percent per additional month of incarceration served.⁸⁸ Further, Kuziemko analyzed a subgroup of 519 offenders released early to curb prison overcrowding. In this subgroup, lengthier sentences were associated with a 3.2 percent decrease in return to prison for each additional month served.⁸⁹ In a later reexamination of the data, Roodman (2017) found a trivial impact of length of incarceration on recidivism.⁹⁰

Meade et al. (2012) estimated the dose-response relationship between time served in prison and odds of recidivism for 1,989 offenders released from prison in Ohio.⁹¹ The study used propensity score matching to create balanced groups of offenders serving various lengths of incarceration.⁹² The study found that offenders confined for lengthier terms of incarceration had lower odds of recidivism. The authors note, the findings suggest an inverse effect of length of incarceration on recidivism which was realized after offenders were incarcerated for at least 60 months.⁹³ Offenders serving at least 60 months had statistically significant lower odds of recidivism as compared to similar offenders serving less time.⁹⁴

Roach et al. (2015) examined between 7,700 and 8,780 felony offenders sentenced in Seattle.⁹⁵ Offenders committed lower-level felony offenses as indicated by an overall average sentence length of nine months.⁹⁶ The authors argued that variation in judicial discretion would result in random variation in sentence lengths.⁹⁷ Offenders were tracked for three years post-release and recidivism was defined as being resentenced for a new felony offense.⁹⁸ Roach et al. found that increased sentence

length was associated with a decrease in recidivism rates. Specifically, three-year felony recidivism rates decreased by approximately one percent for each additional month of incarceration imposed.⁹⁹

Mears et al. (2016) examined the felony reconviction rates of more than 90,000 inmates released from Florida prisons.¹⁰⁰ The authors used propensity scores to balance various offender attributes.¹⁰¹ The average length of incarceration in the cohort was 24 months.¹⁰² The effect of incarceration length on recidivism varied across different incarceration lengths. Specifically, incarceration lengths of less than one year were associated with increased recidivism but the effect plateaued after one year post-release; incarceration lengths of one to two years were associated with a slight decrease in recidivism rates compared to offenders serving between six and twelve months incarceration which plateaued after two years post-release; incarceration lengths of three to five years had no effect on recidivism; and incarceration lengths of six years or more were associated with a slow consistent decline in recidivism (however, this model had issues with larger standard errors).¹⁰³

Rhodes et al. (2018) used a regression discontinuity design and instrumental variable identification strategy to examine the dose-response relationship between prison length of stay and recidivism for a large sample of federal offenders.¹⁰⁴ The study found that longer prison terms were associated with a slight decrease in recidivism during a three-year follow-up period. Specifically, Rhodes and his co-authors found that a 7.5-month increase in incarceration length was associated with a one percent decrease in recidivism.¹⁰⁵

Cotter (2020) examined the dose-response relationship between sentence length and rearrest for a large cohort of offenders released from federal prison in 2005.¹⁰⁶ The study used several different matching approaches (e.g., propensity score matching) and multiple logistic regression to estimate the relationship between length of incarceration and recidivism.¹⁰⁷ The study found that incarceration lengths of more than 60 months were associated with a reduction in recidivism. Specifically, offenders sentenced to more than 60 months of incarceration had lower odds of recidivism relative to a comparable group of offenders receiving shorter sentences.¹⁰⁸

APPENDIX B

Appendix B provides information on the process utilized to create the foundational analytical data for this study.

Datafile Creation Methodology

The Commission entered into a data sharing agreement with the FBI's Criminal Justice Information Services (CJIS) Division and the Administrative Office of the United States Courts (AO) to provide the Commission with secure electronic access to criminal history records through CJIS's Interstate Identification Index (III) and International Justice and Public Safety Network (NLETS). Results received using this system provide an individual's Criminal History Record Information (CHRI) maintained by all U.S. states, the District of Columbia, U.S. territories, and federal agencies. Once the raw CHRI was obtained, the Commission organized and standardized the arrest and court disposition information into an analytical dataset. The resulting data contained CHRI for 32,135 offenders with valid identifying information who were released in 2010.

A. Identifying the Study Cohort

The study cohort included all federal offenders who were U.S. citizens and released from federal prison after serving a sentence of imprisonment or placed on probation in 2010. For offenders

released from prison, the Federal Bureau of Prisons (BOP) provided release dates and identifying information for all offenders released in 2010. The Commission identified offenders placed on probation in 2010 and, with the assistance of the AO, identified and removed offenders who died while on supervised release during the recidivism follow-up period.

B. Processing the Criminal History Record Information

The Commission entered into a data sharing agreement with the FBI's CJIS Division and the AO to acquire electronic records of offender CHRI. The AO extracted offender CHRI through its Access to Law Enforcement System (ATLAS), which provides an interface to III and NLETS. The III allows authorized agencies to determine whether any federal or state repository has CHRI on an individual. Agencies can then securely access specific state CHRI through NLETS. As a result, ATLAS collects CHRI from all state and federal agencies.

The ATLAS system returns the literal text in the RAP sheets in the format in which the original records appear: dates of criminal justice system actions

(e.g., arrests); offense categories which indicate the charges in the terminology used by that agency (e.g., text strings or numeric categories); subsequent action tied to arrest charges (e.g., charges filed by prosecutors, court findings of guilt, etc.); and sentencing and corrections information. All of these records are subject to availability from the originating source.

The ATLAS system also “parses” records from RAP sheets received from all 50 states, the District of Columbia, and federal agencies. Parsing records involves organizing key data elements into logical components, for example: arrest, court, and correctional events. Key data elements include offender identifiers, dates of key actions (e.g., arrests and convictions), the criminal charges, and outcomes such as convictions and sentencing information when provided by the courts. The parsing process collates the multi-state records into a uniform structure, regardless of the state, for all individuals with a valid FBI number who were found in one or more repositories across the country.

C. Standardizing the Criminal Records

After acquiring offender CHRI, the Commission contracted with Integrity One Partners (IOP) to consolidate records for each offender and remove duplicative or extraneous material.¹⁰⁹ Following this preliminary process, IOP utilized a crosswalk created for the Commission’s

prior recidivism research¹¹⁰ to standardize offense codes across states and federal agencies. The crosswalk was updated to standardize new offense codes not mapped in the original crosswalk. The crosswalk standardizes arrest and court codes, regardless of originating sources, into a common framework for analysis. This step was needed because criminal records repositories are primarily designed to store records in ways that accurately reflect the requirements of each state or federal repository, such as the criminal code for that jurisdiction. As a result, any two repositories are likely to use many unique text strings to indicate the nature of the criminal charges and actions taken in response to those charges. Thus, standardizing the offense information was necessary for cross-jurisdictional analysis.

Within each arrest cycle, arrest charges were categorized using standardized codes. A charge severity index was created which incorporates both criminal law classification (e.g., felony or misdemeanor) and offense severity. Offenses were first classified into one of 98 standardized subcategories. These categories were then further grouped for analytical purposes into one of 20 major crime categories in ranking order by severity.¹¹¹ For each offender, the most severe major crime category was identified in their arrest information. The rearrest categories and their underlying subcategories are provided in Table B-1.

Table B-1. Rearrest Offense Categories and Charges

MURDER	<i>Murder of public officer</i>
	<i>Murder</i>
	<i>Attempted murder</i>
	<i>Unspecified manslaughter/homicide</i>
	<i>Nonnegligent manslaughter/homicide</i>
SEXUAL ASSAULT	<i>Rape</i>
	<i>Forcible sodomy</i>
	<i>Fondling</i>
	<i>Statutory rape</i>
	<i>Luring minor by computer</i>
	<i>Other sexual assault</i>
	<i>Sexual assault unspecified</i>
	<i>Armed robbery</i>
ROBBERY	<i>Robbery unspecified</i>
	<i>Unarmed robbery</i>
	<i>Aggravated/felony assault</i>
ASSAULT	<i>Simple/misdemeanor assault</i>
	<i>Assault unspecified</i>
	<i>Assault of public officer</i>
	<i>Intimidation</i>
	<i>Hit and run driving with bodily injury</i>
	<i>Intimidating a witness</i>
	<i>Kidnapping</i>
OTHER VIOLENT	<i>Blackmail/Extortion</i>
	<i>Rioting</i>
	<i>Child abuse</i>
	<i>Other violent offense</i>
	<i>Arson</i>
DRUG TRAFFICKING	<i>Trafficking cocaine/crack</i>
	<i>Trafficking heroin</i>
	<i>Trafficking marijuana</i>
	<i>Trafficking methamphetamine</i>
	<i>Trafficking other/unspecified controlled substance</i>
BURGLARY	<i>Burglary</i>

LARCENY	<i>Motor vehicle theft</i>
	<i>Grand/felony larceny</i>
	<i>Petty/misdemeanor larceny</i>
	<i>Larceny unspecified</i>
	<i>Receiving stolen property</i>
	<i>Trafficking stolen property</i>
FRAUD	<i>Unauthorized use of vehicle</i>
	<i>Fraud/forgery</i>
	<i>Identity theft</i>
	<i>Embezzlement</i>
OTHER PROPERTY	<i>Bribery</i>
	<i>Destruction of property</i>
	<i>Hit and run with property damage</i>
	<i>Trespassing</i>
	<i>Possession of burglary tools</i>
DRUG POSSESSION	<i>Other property offense</i>
	<i>Possession of cocaine/crack</i>
	<i>Possession of heroin</i>
	<i>Possession of marijuana</i>
	<i>Possession of methamphetamine</i>
OTHER DRUG	<i>Possession of other/unspecified controlled substance</i>
	<i>Unspecified cocaine/crack offense</i>
	<i>Unspecified heroin offense</i>
	<i>Unspecified marijuana offense</i>
	<i>Unspecified methamphetamine offense</i>
WEAPON	<i>Unspecified other/unspecified drug offense</i>
OTHER SEX OFFENSE	<i>Weapon offense</i>
	<i>Morals offense</i>
	<i>Indecent exposure</i>
	<i>Commercialized vice</i>
DUI/DWI	<i>Contributing to the delinquency of a minor</i>
	<i>Driving while intoxicated/under the influence, substance unspecified</i>
	<i>Driving while intoxicated/under the influence, alcohol</i>
IMMIGRATION	<i>Driving while intoxicated/under the influence, drugs</i>
	<i>Immigration offense</i>

ADMINISTRATION OF JUSTICE OFFENSES

Escape from custody
Flight to avoid prosecution
Warrant
Contempt of court
Failure to appear
Violation of restraining order
Other court offense
Prison contraband offense
Sex offender registry offense
Obstruction of justice

**PROBATION/PAROLE/
SUPERVISED RELEASE VIOLATION**

Parole violation
Unspecified probation/parole violation
Probation violation

PUBLIC ORDER OFFENSES

Family-related offense
Drunkenness/vagrancy/disorderly conduct
Invasion of privacy
Liquor law violation
Other public order offense
Curfew violation

OTHER/UNSPECIFIED OFFENSES

Vehicle manslaughter/homicide
Negligent (involuntary) manslaughter/homicide
Habitual offender
Runaway
Truancy
Ungovernability
Status liquor law violation
Miscellaneous status offense
Other offense
Unspecified inchoate offense
Military offense
Not applicable
Unspecified offense

APPENDIX C

Regression Model Tables

Appendix C provides information on the regression models for each study group, including: estimate, standard error, odds ratio, and 95-percent confidence interval.

TABLE C-1.

Study Cohort: >24-36 Months					
Term	Estimate	S.E.	Odds Ratio Exp(B)	95% CI	
				Lower	Upper
(Intercept)	1.819	0.455			
Research Group					
Study vs. Comparison	0.064	0.076	1.07	0.92	1.24
Age-at-Release					
Age	-0.068	0.024	0.93 **	0.89	0.98
Age^2	0.000	0.000	1.00	1.00	1.00
Gender					
Female vs. Male	-0.332	0.110	0.72 **	0.58	0.89
Race					
Black vs. White	0.191	0.083	1.21 *	1.03	1.43
Other vs. White	0.245	0.197	1.28	0.87	1.88
High School Completion					
Yes vs. No	-0.578	0.078	0.56 ***	0.48	0.65
Criminal History Category					
CHC II vs. CHC I	0.902	0.128	2.47 ***	1.92	3.17
CHC III vs. CHC I	1.162	0.115	3.20 ***	2.55	4.00
CHC IV vs. CHC I	1.770	0.147	5.87 ***	4.40	7.83
CHC V vs. CHC I	1.834	0.238	6.26 ***	3.93	9.98
CHC VI vs. CHC I	2.569	0.227	13.06 ***	8.37	20.36
Guideline					
§2B1.1 vs. §2D1.1	-0.132	0.131	0.88	0.68	1.13
§2B3.1 vs. §2D1.1	-0.394	0.545	0.67	0.23	1.96
§2K2.1 vs. §2D1.1	0.248	0.122	1.28 *	1.01	1.63
§2L1.1 vs. §2D1.1	0.675	0.198	1.96 ***	1.33	2.89
Other vs. §2D1.1	0.028	0.139	1.03	0.78	1.35
Violence					
Yes vs. No	0.626	0.216	1.87 **	1.22	2.86
Weapons Adjustment					
Yes vs. No	-0.141	0.180	0.87	0.61	1.23
Substantial Assistance					
Yes vs. No	-0.269	0.095	0.76 **	0.63	0.92
Safety Valve					
Yes vs. No	0.090	0.129	1.09	0.85	1.41
-2 Log Likelihood	-2557				
McFadden Pseudo R²	0.209				
N	4,218				
Response Variable: recidivism (rearrest)					
p-values: * p ≤ .05; ** p ≤ .01; *** p ≤ .001					

TABLE C-2.

Study Cohort: >36-48 Months					
Term	Estimate	S.E.	Odds Ratio Exp(B)	95% CI	
				Lower	Upper
(Intercept)	2.631	0.500			
Research Group					
Study vs. Comparison	-0.015	0.077	0.98	0.85	1.14
Age-at-Release					
Age	-0.114	0.026	0.89 ***	0.85	0.94
Age^2	0.001	0.000	1.00 **	1.00	1.00
Gender					
Female vs. Male	-0.381	0.121	0.68 **	0.54	0.87
Race					
Black vs. White	0.240	0.083	1.27 **	1.08	1.50
Other vs. White	0.187	0.190	1.21	0.83	1.75
High School Completion					
Yes vs. No	-0.338	0.081	0.71 ***	0.61	0.83
Criminal History Category					
CHC II vs. CHC I	0.600	0.149	1.82 ***	1.36	2.44
CHC III vs. CHC I	1.166	0.132	3.21 ***	2.48	4.16
CHC IV vs. CHC I	1.518	0.159	4.56 ***	3.34	6.24
CHC V vs. CHC I	1.641	0.198	5.16 ***	3.50	7.62
CHC VI vs. CHC I	2.268	0.190	9.66 ***	6.65	14.03
Guideline					
§2B1.1 vs. §2D1.1	-0.231	0.143	0.79	0.60	1.05
§2B3.1 vs. §2D1.1	0.315	0.352	1.37	0.69	2.73
§2K2.1 vs. §2D1.1	0.357	0.129	1.43 **	1.11	1.84
§2L1.1 vs. §2D1.1	0.104	0.326	1.11	0.59	2.10
Other vs. §2D1.1	0.072	0.154	1.07	0.79	1.45
Violence					
Yes vs. No	0.349	0.249	1.42	0.87	2.31
Weapons Adjustment					
Yes vs. No	-0.212	0.169	0.81	0.58	1.13
Substantial Assistance					
Yes vs. No	-0.112	0.094	0.89	0.74	1.08
Safety Valve					
Yes vs. No	-0.140	0.135	0.87	0.67	1.13
-2 Log Likelihood	-2409				
McFadden Pseudo R²	0.197				
N	3,983				
Response Variable: recidivism (rearrest)					
p-values: * p ≤ .05; ** p ≤ .01; *** p ≤ .001					

TABLE C-3.

Study Cohort: >48-60 Months					
Term	Estimate	S.E.	Odds Ratio Exp(B)	95% CI	
				Lower	Upper
(Intercept)	3.390	0.706			
Research Group					
Study vs. Comparison	-0.009	0.095	0.99	0.82	1.19
Age-at-Release					
Age	-0.148	0.036	0.86 ***	0.80	0.93
Age^2	0.001	0.000	1.00 **	1.00	1.00
Gender					
Female vs. Male	-0.618	0.169	0.54 ***	0.39	0.75
Race					
Black vs. White	0.114	0.102	1.12	0.92	1.37
Other vs. White	0.237	0.259	1.27	0.76	2.10
High School Completion					
Yes vs. No	-0.526	0.100	0.59 ***	0.49	0.72
Criminal History Category					
CHC II vs. CHC I	0.487	0.189	1.63 **	1.12	2.35
CHC III vs. CHC I	1.142	0.178	3.13 ***	2.21	4.44
CHC IV vs. CHC I	1.788	0.213	5.98 ***	3.94	9.07
CHC V vs. CHC I	2.300	0.243	9.98 ***	6.19	16.07
CHC VI vs. CHC I	2.642	0.221	14.03 ***	9.10	21.64
Guideline					
§2B1.1 vs. §2D1.1	-0.500	0.200	0.61 *	0.41	0.90
§2B3.1 vs. §2D1.1	0.226	0.396	1.25	0.58	2.73
§2K2.1 vs. §2D1.1	0.000	0.151	1.00	0.74	1.34
§2L1.1 vs. §2D1.1	0.207	0.713	1.23	0.30	4.98
Other vs. §2D1.1	0.145	0.208	1.16	0.77	1.74
Violence					
Yes vs. No	0.188	0.269	1.21	0.71	2.04
Weapons Adjustment					
Yes vs. No	-0.076	0.192	0.93	0.64	1.35
Substantial Assistance					
Yes vs. No	-0.253	0.117	0.78 *	0.62	0.98
Safety Valve					
Yes vs. No	-0.041	0.184	0.96	0.67	1.38
-2 Log Likelihood	-1482				
McFadden Pseudo R²	0.213				
N	2,598				
Response Variable: recidivism (rearrest)					
p-values: * p ≤ .05; ** p ≤ .01; *** p ≤ .001					

TABLE C-4.

Study Cohort: >60-120 Months						
Term	Estimate	S.E.	Odds Ratio Exp(B)		95% CI	
					Lower	Upper
(Intercept)	3.100	0.403				
Research Group						
Study vs. Comparison	-0.203	0.056	0.82 ***		0.73	0.91
Age-at-Release						
Age	-0.137	0.020	0.87 ***		0.84	0.91
Age^2	0.001	0.000	1.00 ***		1.00	1.00
Gender						
Female vs. Male	-0.294	0.094	0.75 **		0.62	0.90
Race						
Black vs. White	0.154	0.052	1.17 **		1.05	1.29
Other vs. White	0.013	0.172	1.01		0.72	1.42
High School Completion						
Yes vs. No	-0.332	0.051	0.72 ***		0.65	0.79
Criminal History Category						
CHC II vs. CHC I	0.602	0.086	1.83 ***		1.54	2.16
CHC III vs. CHC I	1.021	0.080	2.78 ***		2.37	3.25
CHC IV vs. CHC I	1.396	0.096	4.04 ***		3.35	4.87
CHC V vs. CHC I	1.674	0.114	5.33 ***		4.26	6.67
CHC VI vs. CHC I	2.093	0.100	8.11 ***		6.66	9.88
Guideline						
§2B1.1 vs. §2D1.1	-0.081	0.157	0.92		0.68	1.25
§2B3.1 vs. §2D1.1	0.481	0.229	1.62 *		1.03	2.54
§2K2.1 vs. §2D1.1	0.542	0.086	1.72 ***		1.45	2.04
§2L1.1 vs. §2D1.1	1.090	0.754	2.98		0.68	13.05
Other vs. §2D1.1	0.317	0.105	1.37 **		1.12	1.69
Violence						
Yes vs. No	0.138	0.180	1.15		0.81	1.63
Weapons Adjustment						
Yes vs. No	0.061	0.070	1.06		0.93	1.22
Substantial Assistance						
Yes vs. No	-0.171	0.065	0.84 **		0.74	0.96
Safety Valve						
Yes vs. No	-0.006	0.097	0.99		0.82	1.20
-2 Log Likelihood	-4832					
McFadden Pseudo R²	0.215					
N	8,578					
Response Variable: recidivism (rearrest)						
p-values: * p ≤ .05; ** p ≤ .01; *** p ≤ .001						

TABLE C-5.

Study Cohort: > 120 Months						
Term	Estimate	S.E.	Odds Ratio Exp(B)		95% CI	
					Lower	Upper
(Intercept)	4.065	0.777				
Research Group						
Study vs. Comparison	-0.340	0.082	0.71 ***		0.61	0.84
Age-at-Release						
Age	-0.187	0.036	0.83 ***		0.77	0.89
Age^2	0.001	0.000	1.00 ***		1.00	1.00
Gender						
Female vs. Male	-0.251	0.210	0.78		0.52	1.17
Race						
Black vs. White	0.100	0.082	1.11		0.94	1.30
Other vs. White	0.183	0.440	1.20		0.51	2.85
High School Completion						
Yes vs. No	-0.055	0.075	0.95		0.82	1.10
Criminal History Category						
CHC II vs. CHC I	0.948	0.154	2.58 ***		1.91	3.49
CHC III vs. CHC I	1.156	0.138	3.18 ***		2.43	4.16
CHC IV vs. CHC I	1.358	0.147	3.89 ***		2.91	5.19
CHC V vs. CHC I	1.732	0.172	5.65 ***		4.03	7.93
CHC VI vs. CHC I	2.133	0.137	8.44 ***		6.45	11.04
Guideline						
§2B1.1 vs. §2D1.1	-0.106	0.262	0.98		0.39	1.41
§2B3.1 vs. §2D1.1	0.337	0.323	1.40		0.74	2.64
§2K2.1 vs. §2D1.1	0.758	0.170	2.13 ***		1.53	2.98
Other vs. §2D1.1	0.230	0.241	1.26		0.78	2.02
Violence						
Yes vs. No	-0.139	0.301	0.87		0.48	1.57
Weapons Adjustment						
Yes vs. No	0.165	0.087	1.18		1.00	1.40
Substantial Assistance						
Yes vs. No	0.218	0.306	1.24		0.68	2.26
Safety Valve						
Yes vs. No	0.231	0.325	1.26		0.67	2.38
-2 Log Likelihood	-2032					
McFadden Pseudo R²	0.213					
N	3,551					
Response Variable: recidivism (rearrest)						
p-values: * p ≤ .05; ** p ≤ .01; *** p ≤ .001						

ENDNOTES

ENDNOTES

1 See, e.g., U.S. SENT'G COMM'N, SUPPLEMENTARY REPORT ON THE INITIAL SENTENCING GUIDELINES AND POLICY STATEMENTS (1987); KIM STEVEN HUNT & ANDREW PETERSON, U.S. SENT'G COMM'N, RECIDIVISM AMONG OFFENDERS RECEIVING RETROACTIVE SENTENCE REDUCTIONS: THE 2007 CRACK COCAINE AMENDMENT (2014); U.S. SENT'G COMM'N, REPORT TO THE CONGRESS: FEDERAL CHILD PORNOGRAPHY OFFENSES 293–310 (2012); LINDA DRAZGA MAXFIELD, MILES HARER, TIMOTHY DRISKO, CHRISTINE KITCHENS, SARA MEACHAM & MATTHEW IACONETTI, U.S. SENT'G COMM'N, A COMPARISON OF THE FEDERAL SENTENCING GUIDELINES CRIMINAL HISTORY CATEGORY AND THE U.S. PAROLE COMMISSION SALIENT FACTOR SCORE (2005); LINDA DRAZGA MAXFIELD, MILES HARER, TIMOTHY DRISKO, CHRISTINE KITCHENS & SARA MEACHAM, U.S. SENT'G COMM'N, RECIDIVISM AND THE “FIRST OFFENDER” (2004); LINDA DRAZGA MAXFIELD, MILES HARER, TIMOTHY DRISKO, CHRISTINE KITCHENS & SARA MEACHAM, U.S. SENT'G COMM'N, MEASURING RECIDIVISM: THE CRIMINAL HISTORY COMPUTATION OF THE FEDERAL SENTENCING GUIDELINES (2004). Commission materials cited herein are available on the Commission's website at www.ussc.gov.

2 See Sentencing Reform Act of 1984, Pub. L. No. 98–473, § 212(a), 98 Stat. 1837, 1987 (codified as amended in 18 U.S.C. § 3553(a)).

3 See U.S. SENT'G COMM'N, *Guidelines Manual*, Ch.4, Pt.A, intro. comment. (Nov. 2021) [hereinafter USSG].

4 28 U.S.C. § 991(b)(1)(C).

5 In 2016, the Commission began its first multi-publication recidivism series which culminated with seven publications. See KIM STEVEN HUNT & ROBERT DUMVILLE, U.S. SENT'G COMM'N, RECIDIVISM AMONG FEDERAL OFFENDERS: A COMPREHENSIVE OVERVIEW (2016) [hereinafter 2016 RECIDIVISM OVERVIEW REPORT]; TRACEY KYCKELHAHN & TRISHIA COOPER, U.S. SENT'G COMM'N, THE PAST PREDICTS THE FUTURE: CRIMINAL HISTORY AND RECIDIVISM OF FEDERAL OFFENDERS (2017) [hereinafter 2017 RECIDIVISM CRIMINAL HISTORY REPORT]; KIM STEVEN HUNT & BILLY EASLEY II, U.S. SENT'G COMM'N, THE EFFECTS OF AGING ON RECIDIVISM AMONG FEDERAL OFFENDERS (2017) [hereinafter 2017 RECIDIVISM AGE REPORT]; LOUIS REEDT, KIM STEVEN HUNT, JAMES L. PARKER, MELISSA K. REIMER & KEVIN T. MAASS, U.S. SENT'G COMM'N, RECIDIVISM AMONG FEDERAL DRUG TRAFFICKING OFFENDERS (2017); KIM STEVEN HUNT, MATTHEW J. IACONETTI & KEVIN T. MAASS, U.S. SENT'G COMM'N, RECIDIVISM AMONG FEDERAL VIOLENT OFFENDERS (2019) [hereinafter 2019 RECIDIVISM VIOLENCE REPORT]; MATTHEW J. IACONETTI, TRACEY KYCKELHAHN & MARI MCGILTON, U.S. SENT'G COMM'N, RECIDIVISM AMONG FEDERAL FIREARMS OFFENDERS (2019) [hereinafter 2019 RECIDIVISM FIREARMS REPORT]; RYAN COTTER, U.S. SENT'G COMM'N, LENGTH OF INCARCERATION AND RECIDIVISM (2020) [hereinafter 2020 RECIDIVISM INCARCERATION REPORT].

6 RYAN COTTER, COURTNEY SEMISCH & DAVID RUTTER, U.S. SENT'G COMM'N, RECIDIVISM OF FEDERAL OFFENDERS RELEASED IN 2010 (2021).

7 TRACEY KYCKELHAHN, KRISTEN SHARPE & AMANDA KERBEL, U.S. SENT'G COMM'N, RECIDIVISM OF FEDERAL FIREARMS OFFENDERS RELEASED IN 2010 (2021); VERA M. KACHNOWSKI, MELISSA K. REIMER, KEVIN T. MAASS, CHRISTINE KITCHENS & KEVIN BLACKWELL, U.S. SENT'G COMM'N, RECIDIVISM OF FEDERAL DRUG TRAFFICKING OFFENDERS RELEASED IN 2010 (2022); COURTNEY R. SEMISCH, CASSANDRA SYCKES & LANDYN ROOKARD, U.S. SENT'G COMM'N, RECIDIVISM OF FEDERAL VIOLENT OFFENDERS RELEASED IN 2010 (2022).

8 KRISTIN M. TENNYSON, ROSS THOMAS, TESSA GUITON & ALYSSA PURDY, U.S. SENT'G COMM'N, RECIDIVISM AND FEDERAL BUREAU OF PRISONS PROGRAMS: DRUG PROGRAM PARTICIPANTS RELEASED IN 2010 (2022); KRISTIN M. TENNYSON, ROSS THOMAS, TESSA GUITON & ALYSSA PURDY, U.S. SENT'G COMM'N, RECIDIVISM AND FEDERAL BUREAU OF PRISONS PROGRAMS: VOCATIONAL PROGRAM PARTICIPANTS RELEASED IN 2010 (2022).

9 2020 RECIDIVISM INCARCERATION REPORT, *supra* note 5, at 30.

10 While various statistical models may predict either a preventative or criminogenic relationship between length of incarceration and recidivism, those relationships may not be statistically significant.

Statistical significance indicates that the relationship observed, preventative or criminogenic, is unlikely to be a false positive (i.e., indicating there is a relationship when, in fact, there is not). Statistical significance is important in that it provides a degree of certainty that an observed relationship is, in fact, not a false positive. Thus, it is possible that no statistically significant relationship between length of incarceration and recidivism is identified.

11 See Appendix A for a review of the literature on length of incarceration and recidivism.

12 See, e.g., Daniel S. Nagin, Francis T. Cullen & Cheryl Lero Jonson, *Imprisonment and Reoffending*, 38 CRIME & JUST. 115, 169 (2009). A number of the regression studies examining length of incarceration and recidivism were designed to answer a different research question; time served was included only as a control variable, rather than explanatory variable. *Id.* at 168–69.

13 *Imprisonment and Reoffending* notes that conclusions regarding the relationship between length of incarceration and recidivism cannot be drawn from a number of the regression studies because of fundamental analytical flaws. Specifically, the regression studies are flawed in their model specification of the relationship between age and reoffending. *Id.* at 175.

14 See Appendix A for a review of the literature on length of incarceration and recidivism. Only two studies focusing on federal offenders were identified.

15 See *supra* note 14.

16 See William Rhodes, Gerald G. Gaes, Ryan Kling & Christopher Cutler, *Relationship Between Prison Length of Stay and Recidivism: A Study Using Regression Discontinuity and Instrumental Variables with Multiple Break Points*, 17 CRIMINOLOGY & PUB. POL'Y 731, 758 (2018).

17 *Id.*

18 See 2020 RECIDIVISM INCARCERATION REPORT, *supra* note 5, at 30.

19 *Id.*

20 *Id.*

21 See Nagin et al., *supra* note 12, at 121 (concluding that “existing research [on the impact of imprisonment on subsequent recidivism] is not nearly sufficient for making firm evidence-based conclusions for either science or public policy”).

22 The Commission collects and analyzes data on federal sentences to carry out its various statutory responsibilities. As authorized by Congress, the Commission’s numerous research responsibilities include: (1) the establishment of a research and development program to serve as a clearinghouse and information center for the collection, preparation, and dissemination of information on federal sentencing practices; (2) the publication of data concerning the sentencing process; (3) the systematic collection and dissemination of information concerning sentences actually imposed and the relationship of such sentences to the sentencing factors in 18 U.S.C. § 3553(a); and (4) the systematic collection and dissemination of information regarding the effectiveness of sentences imposed. See 28 U.S.C. § 995(a)(12), (14)–(16). The Commission collects information for every federal felony and Class A misdemeanor offense sentenced each year. Sentencing courts are statutorily required to submit five sentencing documents to the Commission within 30 days of entry of judgment in a criminal case, including: (1) the charging document; (2) the plea agreement; (3) the Presentence Report; (4) the Judgment and Commitment Order; and (5) the Statement of Reasons form. See 28 U.S.C. § 994(w)(1). For each case in its Individual Offender Datafile, the Commission routinely collects case identifiers, sentencing data, demographic variables, statutory information, the complete range of court guideline application decisions, and departure and variance information from these documents.

23 The data used to conduct the analyses in this report includes information obtained pursuant to an interagency agreement with the FBI, which prohibits the Commission from releasing the dataset.

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- 24 Appendix B provides a detailed description of the data collection methodology.
- 25 Nat'l Inst. of Just., U.S. Dep't of Just., *Recidivism*, <https://nij.ojp.gov/topics/corrections/recidivism> (last visited Mar. 3, 2022); *see also* MICHAEL D. MALTZ, *RECIDIVISM* 1, 54 (2001) [hereinafter MALTZ].
- 26 *See* MALTZ, *supra* note 25, at 7–20; *see also* RYAN KING & BRIAN ELDERBROOM, *URB. INST., IMPROVING RECIDIVISM AS A PERFORMANCE MEASURE* (2014).
- 27 *See, e.g.*, CHRISTOPHER T. LOWENKAMP, MARIE VANNOSTRAND & ALEXANDER HOLSINGER, *INVESTIGATING THE IMPACT OF PRETRIAL DETENTION ON SENTENCING OUTCOMES* (2013).
- 28 *See* MALTZ, *supra* note 25, at 61–64; *see also* Nat'l Inst. of Just., U.S. Dep't of Just., *Measuring Recidivism* (Feb. 20, 2008), <https://nij.ojp.gov/topics/articles/measuring-recidivism>.
- 29 *See, e.g.*, MARIEL ALPER, MATTHEW R. DUROSE & JOSHUA MARKMAN, BUREAU OF JUST. STAT., U.S. DEP'T OF JUST., *UPDATE ON PRISONER RECIDIVISM: A 9-YEAR FOLLOW-UP PERIOD (2005–2014)* (2018) [hereinafter ALPER]; Admin. Off. of the U.S. Cts., *Just the Facts: Post-Conviction Supervision and Recidivism* (Oct. 22, 2018), <https://www.uscourts.gov/news/2018/10/22/just-facts-post-conviction-supervision-and-recidivism#chart1>; WILLIAM RHODES, CHRISTINA DYOUS, RYAN KLING, DANA HUNT & JEREMY LUAllen, *ABT ASSOCS., RECIDIVISM OF OFFENDERS ON FEDERAL COMMUNITY SUPERVISION* (2012).
- 30 *See* MALTZ, *supra* note 25, at 55–60.
- 31 *See id.* at 56–58.
- 32 *See* ALPER, *supra* note 29, at 14.
- 33 *See, e.g.*, 18 U.S.C. § 924(c)(1)(A)(i) (providing a five-year mandatory minimum for offenders convicted of using or carrying a firearm during and in relation to, or possessing a firearm in furtherance of, a crime of violence or drug trafficking crime); 18 U.S.C. § 2252(b)(1) (providing a five-year mandatory minimum for offenders convicted of receipt, distribution, and possession with the intent to distribute or sell child pornography); *see also* U.S. SENT'G COMM'N, *AN OVERVIEW OF MANDATORY MINIMUM PENALTIES IN THE FEDERAL CRIMINAL JUSTICE SYSTEM* (2017).
- 34 *See* Michele Jonsson Funk, Daniel Westreich, Chris Wiesen, Til Sturmer, M. Alan Brookhart & Marie Davidian, *Doubly Robust Estimation of Causal Effects*, 173 *AM. J. EPIDEMIOLOGY* 761, 761–67 (2011).
- 35 Weights were created using a logistic link function with linear propensity score.
- 36 *See* Nagin et al., *supra* note 12, at 136, 142 (identifying five principal attributes that studies examining length of incarceration and recidivism should control for: age, sex, race, conviction offense, and prior record).
- 37 *See supra* note 14.
- 38 *See* 2017 *RECIDIVISM AGE REPORT*, *supra* note 5, at 3; 2016 *RECIDIVISM OVERVIEW REPORT*, *supra* note 5, at A-1.
- 39 *See* 2016 *RECIDIVISM OVERVIEW REPORT*, *supra* note 5, at A-1.
- 40 *See* 2017 *RECIDIVISM CRIMINAL HISTORY REPORT*, *supra* note 5, at 7; 2016 *RECIDIVISM OVERVIEW REPORT*, *supra* note 5, at A-1.
- 41 *See* 2016 *RECIDIVISM OVERVIEW REPORT*, *supra* note 5, at A-1.
- 42 *See id.*
- 43 *See* 2019 *RECIDIVISM VIOLENCE REPORT*, *supra* note 5, at 3.
- 46

- 44 See 2019 RECIDIVISM FIREARMS REPORT, *supra* note 5, at 4; 2016 RECIDIVISM OVERVIEW REPORT, *supra* note 5, at A-1.
- 45 See USSG §§5C1.2, 5K1.1 (guidelines defining both safety valve and substantial assistance).
- 46 See Peter C. Austin, *An Introduction to Propensity Score Methods for Reducing the Effects of Confounding in Observational Studies*, 46 MULTIVARIATE BEHAV. RSCH. 399, 412 (2011). The threshold for weighting balance is 0.1 standardized mean differences. *Id.* This means the study and comparison groups should not have a standardized mean difference greater than 0.1 after matching and weighting.
- 47 See Daniel L. Rubinfeld, *Reference Guide on Multiple Regression*, in REFERENCE MANUAL ON SCIENTIFIC EVIDENCE 303 (3d ed. 2011) (providing an overview of regression modeling).
- 48 Appendix C provides the Odds Ratios for each research model.
- 49 For each of the study cohorts, balance was achieved between the study and comparison groups. The standardized mean differences between study and comparison groups ranged from 0.02 to 0.08; all below the conventional threshold of 0.1. See *supra* note 46 and accompanying text for discussion on the conventional 0.1 threshold.
- 50 See *supra* note 36 and accompanying text.
- 51 The difference in average time to rearrest was statistically significant. Welch's two sample t-test was used to test for statistical significance: $t = -2.00$, $df = 2091$, $p = 0.03$.
- 52 For the full regression models, including estimates for all study cohorts, see Appendix C.
- 53 The difference in average time to rearrest was not statistically significant. Welch's two sample t-test was used to test for statistical significance: $t = -2.00$, $df = 1139$, $p = 0.09$.
- 54 2020 RECIDIVISM INCARCERATION REPORT, *supra* note 5.
- 55 See *id.* at 16-23
- 56 Employing the first variation of the alternative research design on the 2010 cohort, the Commission found a preventative effect for offenders sentenced to more than 120 months, however, that effect was not statistically significant. For offenders sentenced to more than 120 months, the unstandardized Beta weight for the study group was: $B = -0.245$, $SE = 0.202$, $p = 0.225$, [$\text{Exp}(B) = 0.78$, 95% CI (0.53, 1.16)]. In the second variation, the Commission found a statistically significant preventative effect for offenders sentenced to more than 60 months incarceration. For offenders sentenced to more than 60 up to 120 months, the unstandardized Beta weight for the study group was: $B = -0.151$, $SE = 0.065$, $p < 0.05$, [$\text{Exp}(B) = 0.86$, 95% CI (0.76, 0.98)]. For offenders sentenced to more than 120 months, the unstandardized Beta weight for the study group was: $B = -0.294$, $SE = 0.081$, $p < 0.001$, [$\text{Exp}(B) = 0.75$, 95% CI (0.64, 0.87)].
- 57 See Nagin et al., *supra* note 12.
- 58 *Id.* at 169.
- 59 *Id.* at 175.
- 60 *Id.* at 183.
- 61 ELIZABETH BERGER & KENT S. SCHEIDEGGER, CRIM. JUST. LEGAL FOUND., SENTENCE LENGTH AND RECIDIVISM: A REVIEW OF THE RESEARCH 27 (2021).
- 62 Dorothy R. Jaman, Robert M. Dickover & Lawrence A. Bennett, *Parole Outcome as a Function of Time Served*, 12 BRIT. J. CRIMINOLOGY 5 (1972).

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63 *Id.* at 6.

64 *Id.* at 29.

65 *Id.* at 19.

66 J. Kraus, *The Effects of Committal to a Special School for Truants*, 25 INT'L J. OFFENDER THERAPY & COMPAR. CRIMINOLOGY 130 (1981).

67 *Id.* at 133 tbl.2.

68 *Id.* at 131–32.

69 *Id.* at 133–34.

70 JOHN E. BERECOCHEA & DOROTHY R. JAMAN, RSCH. DIV., CAL. DEP'T OF CORR., *TIME SERVED IN PRISON AND PAROLE OUTCOME: AN EXPERIMENTAL STUDY* (1981).

71 *See* Nagin et al., *supra* note 12, at 167–69.

72 *Id.* at 169.

73 Elizabeth Piper Deschenes, Susan Turner & Joan Petersilia, *A Dual Experiment in Intensive Community Supervision: Minnesota's Prison Diversion and Enhanced Supervised Release Programs*, 75 PRISON J. 330 (1995).

74 *Id.* at 352.

75 Thomas A. Loughran, Edward P. Mulvey, Carol A. Schubert, Jeffrey Fagan, Alex R. Piquero & Sandra H. Losoya, *Estimating a Dose-Response Relationship Between Length of Stay and Future Recidivism in Serious Juvenile Offenders*, 47 CRIMINOLOGY 699 (2009).

76 *Id.* at 716.

77 *Id.* at 726–29.

78 Donald P. Green & Daniel Winik, *Using Random Judge Assignment to Estimate the Effects of Incarceration and Probation on Recidivism Among Drug Offenders*, 48 CRIMINOLOGY 357 (2010).

79 *Id.* at 363.

80 *Id.* at 381.

81 G. Matthew Snodgrass, Arjan A. J. Blokland, Amelia Haviland, Paul Nieuwbeerta & Daniel S. Nagin, *Does the Time Cause the Crime? An Examination of the Relationship Between Time Served and Reoffending in the Netherlands*, 49 CRIMINOLOGY 1149 (2011).

82 *Id.* at 1162–67.

83 *Id.* at 1179.

84 *Id.* at 1166, 1173.

85 *Id.* at 1178–79.

86 Ilyana Kuziemko, *How Should Inmates Be Released From Prison? An Assessment of Parole Versus Fixed-Sentence Regimes*, 128 Q.J. ECON. 371 (2013).

87 *Id.* at 391 tbl.II, 392.

- 88 *Id.* at 393.
- 89 *Id.* at 394–95.
- 90 DAVID ROODMAN, OPEN PHILANTHROPY PROJECT, *THE IMPACTS OF INCARCERATION ON CRIME* 101–15 (2017).
- 91 Benjamin Meade, Benjamin Steiner, Matthew Makarios & Lawrence Travis, *Estimating a Dose-Response Relationship Between Time Served in Prison and Recidivism*, 50 J. RSCH. CRIME & DELINQ. 525 (2012).
- 92 *Id.* at 534–35.
- 93 *Id.* at 539–42.
- 94 *Id.*
- 95 MICHAEL ROACH & MAX SCHANZENBACH, *THE EFFECT OF PRISON SENTENCE LENGTH ON RECIDIVISM: EVIDENCE FROM RANDOM JUDICIAL ASSIGNMENT* (2015).
- 96 *Id.* at 2.
- 97 *Id.* at 1.
- 98 *Id.* at 6.
- 99 *Id.* at 11–12.
- 100 Daniel P. Mears, Joshua C. Cochran, William D. Bales & Avinash S. Bhati, *Recidivism and Time Served in Prison*, 106 J. CRIM. L. & CRIMINOLOGY 83 (2016).
- 101 *Id.* at 102.
- 102 *Id.* at 101.
- 103 *Id.* at 118–23.
- 104 Rhodes et al., *supra* note 16, at 758.
- 105 *Id.*
- 106 2020 RECIDIVISM INCARCERATION REPORT, *supra* note 5.
- 107 *Id.* at 8–9.
- 108 *Id.* at 30.
- 109 Instances of arrest or sentencing that appeared to be duplicates of existing events were removed by IOP. Minor offenses (e.g., speeding) and arrest entries occurring outside of the eight-year follow-up period were removed and, therefore, not used to ascertain recidivism.
- 110 See 2016 RECIDIVISM OVERVIEW REPORT, *supra* note 5, Appendix B.
- 111 The major crime categories, as ranked by the Commission, beginning with the most serious were: murder, violent sexual assault/rape, robbery, assault, other violent offense, drug trafficking, burglary, larceny, fraud, other property offense, drug possession, other drug offense, weapons offense, other sex offense, driving under the influence, immigration, administration of justice offense, probation/parole/supervised release violation, other public order offense, and other unspecified offense. See *infra* Table B-1.



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**Sentence Length and Recidivism:
A Review of the Research**

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Abstract

In response to prison overcrowding concerns in recent years, many U.S. officials have undertaken efforts to reduce sentence lengths for certain crimes. However, it is unclear how these changes affect recidivism rates. Among the research on incarceration and recidivism, the majority of studies compare custodial with noncustodial sentences, while fewer examine the impact of varying incarceration lengths. This article reviews the research on the latter. Overall, the effect of incarceration length on recidivism appears too heterogeneous to draw universal conclusions, and findings are inconsistent across studies due to methodological limitations. For example, many study samples are skewed toward people with shorter sentences while others include confounds that render results invalid. Of the studies reviewed, some suggested that longer sentences provide additional deterrent benefits in the aggregate, though some studies also had null effects. None suggested a strong aggregate-level criminogenic effect. We argue that a conclusion that longer sentences have a substantial criminogenic effect, large enough to offset incapacitative effects, cannot be justified by the existing literature.

Keywords: sentencing, incarceration, prison, recidivism, sentencing policy, deterrence, custodial sentence

INTRODUCTION

There is currently a high degree of public interest regarding how length of incarceration affects recidivism rates of released offenders. The interest is particularly strong in Los Angeles, where the recently-elected District Attorney has adopted policies that sharply reduce sentence lengths by omitting allegations that would otherwise increase sentences beyond the base sentence for the crime.¹ The policies require that allegations be omitted from charging documents altogether, seemingly so that judges can no longer use them to justify lengthier sentences. The policies are supported with a statement regarding empirical research in the field: “While initial incarceration prevents crime through incapacitation, studies show that each additional sentence year causes a 4 to 7 percent increase in recidivism that eventually outweighs the incapacitation benefit.”² Despite the plural “studies” alluded to, only one unpublished manuscript is actually cited.³ The manuscript’s findings are in contrast with other research, and the methodology is not fully comparable to past literature. It is concerning that such a drastic policy change is based on only one study without full consideration of the evidence base.

Michael Mueller-Smith’s findings have not been published in academic research, yet his claim that longer periods of incarceration disproportionately increase risk for recidivism has attracted prominent support from people within the academic community. For example, the dean of the U.C. Berkeley Law School co-authored a newspaper opinion piece where he asserted that sentence enhancement “approaches have exacerbated recidivism, creating more victims of crime.”⁴ A hyperlink in the online version of the article links to Mueller-Smith as authority for the assertion. A “friend of the court” brief filed in litigation over the policies, by one of the same co-authors, makes a similar assertion also citing the 2015 article.⁵

Although Gascón's stated objective is to improve criminal justice policy based on empirical research, it is unclear whether Mueller-Smith's findings⁶ hold when considering the wider body of research. Specifically, there is not much consistency supporting these claims throughout the entire breadth of research.⁷ In fact, the assertion that lengthier sentences result in greater likelihood of reoffending contrasts sharply with findings from the last thorough review of the literature on the subject.⁸ When considering the consistency and strength of findings across numerous studies, Daniel Nagin, Francis Cullen, and Cheryl Jonson⁹ found "little convincing evidence on the dose-response relationship between time spent in confinement and reoffending rate." This article is 12 years old, though.

In 2022, a new review was published by Charles Loeffler and Daniel Nagin¹⁰ that examined the relationship between incarceration experiences and recidivism. However, this review did not focus on the impact of varying sentence lengths. Rather, they focused on exposure to incarceration in general, and many of the studies reviewed compared custodial with non-custodial sentences. While they did find some recidivism reduction effects¹¹, the review did not tell us much about how varying lengths of incarceration affect recidivism.

An updated review of the literature of the last 12 years focused on the "dose-response relationship between time spent in confinement and the reoffending rate" is therefore needed.

I. DIFFICULTIES IN MEASUREMENT

Nagin, Cullen, and Jonson appear to have been writing for a larger audience than just researchers in their field, as they described the problems of measuring effects in criminal justice that need explanation for that broader audience. This article will follow a similar path, with frequent references to Nagin, Cullen, and Jonson for the "long form" explanation.

In the physical sciences, it is generally possible to test a hypothesis with a tightly controlled experiment that eliminates all variables except those of interest. As we move to living things, individual variation in the test subjects becomes inevitable. As we move up the evolutionary ladder, ethical constraints become more restrictive. For studies actively involving humans, informed consent of the participants is required. Past studies with unwilling or uninformed subjects, such as the infamous Tuskegee Experiment,¹² are regarded with horror today.¹³

In medicine and social sciences, the randomized controlled trial (RCT) is often considered the “gold standard” for assessing the effectiveness of a policy, program, or intervention. In an RCT, researchers assign participants at random to either a treatment group or a control group. The treatment group receives the intervention or “treatment” to be evaluated. The control group receives a comparison intervention – typically either through the form of an alternative treatment, a “business-as-usual” treatment, or a placebo treatment (i.e., a “dummy treatment”) that does nothing. The element of randomization is the key strength of this design that sets it apart from others, making it a true experiment rather than a quasi-experiment.¹⁴ In contrast, an observational study design would simply compare those receiving a treatment with those who did not, based on data from participant records. In the latter, assignment is not random nor is it controlled by the researcher, which introduces a higher probability of bias known as “selection bias” that can compromise the generalizability of results.

These seemingly minute differences in study procedures have important ramifications for policy, because studies on similar topics with different methodological approaches often find contrasting results. This is also typical of the research assessing the relationship between

imprisonment and recidivism, with different methodological procedures often resulting in mixed findings. There are several challenges in studying the relationship between imprisonment and recidivism. First, in circumstances related to incarceration, it is not ethical to assign people at random, so an RCT is not possible. Thus, any research on this topic needs to be quasi-experimental at best, with the resulting challenges in being able to ascertain causality. Though there are many types of quasi-experiments, and some designs resemble an RCT but lack random assignment. Without randomization, it is challenging to sufficiently control for the impact of outside factors, making it more difficult to ascertain a clear causal relationship between two things.¹⁵ This is because a quasi-experiment can only minimize differences that are *observable*, while unobservable differences remain unknown.¹⁶ In contrast, a well-executed RCT will ensure that both unobservable and observable characteristics are randomly balanced out between groups. Thus, quasi-experimental designs have lower causal validity than RCTs. That is, they are less able to demonstrate cause-and-effect relationships rather than mere correlations. There is also considerable variation in the approaches used to methodologically control for the impact of important characteristics (e.g., offending history), which can result in mixed findings. Finally, there are many ways of measuring recidivism, making it more complicated to compare results across studies.

Regarding quasi-experiments, studies are stronger when researchers have discretion to assign participants to groups and decide data collection and measurement procedures.¹⁷ Nagin, Cullen, and Jonson encourage researchers to seek such opportunities. Researchers can take steps to strengthen their quasi-experiments and increase their causal validity, namely by: 1) strategically assigning subjects to groups in a way that minimizes observable differences

between groups, or, when this is not possible, 2) using statistical controls to account for any observed differences between groups.¹⁸

One common approach to strategic assignment is a “matched-pairs” design, where participants who differ on an outcome of interest are paired based on other shared factors, and one person from each pair is randomly assigned to each group. While matched-pairs designs are not random, they tend to generate treatment and control groups that are statistically similar¹⁹ that is more akin to that of an RCT. Some studies of imprisonment have used variations of this process where a courthouse randomly assigns cases to judges based on a random drawing.²⁰ Other times, authors may statistically account for observed differences between groups. This is often done by balancing groups using propensity score modeling (PSM), instrumental variables, or through incorporation of statistical controls for relevant variables. When done well, these methods have greater causal validity that are more akin to that of an RCT.

Observational studies are those where a researcher looks at the effect of an intervention (e.g., imprisonment) but does not interfere or try to influence outcomes; that is, they have no control over assignment to groups.²¹ These studies can be retrospective (where past records are examined) or prospective (where data is collected in real-time during the study).²² The differences between an observational study and an RCT were dramatically demonstrated to the general public during the Covid-19 pandemic.²³ Early observational studies raised hopes that a widely available and inexpensive drug, hydroxychloroquine, might be a promising treatment for Covid-19,²⁴ though multiple RCTs have since demonstrated otherwise.²⁵ Nonetheless, preliminary findings were touted in an unpublished manuscript²⁶ and circulated to millions on social media,²⁷ eventually leading to a rapid increase in off-label use of the drug.²⁸ This example

is one of many where policy has outpaced empirical research, only to be met with lackluster results.²⁹

A “natural experiment” refers to a design where researchers take advantage of some outside event (e.g., natural disaster, policy change, economic change) that produces measurable impacts, though it is not a “true” experiment. In natural experiments, researchers have no way of assigning people to groups -- thus, the groups are likely to differ from each other. However, when groups do differ from each other, researchers can partially compensate for this problem by including statistical adjustments to account for differences between groups. One example of a natural experiment is the 2009 revision to New York’s Rockefeller Drug Laws and the impact on recidivism rates for drug offenders.³⁰ A more relevant example is a study by Francesco Drago, Roberto Galbiati, and Pietro Vertova³¹ that examined recidivism patterns among offenders released from prison as a result of a bill passed by the Italian Parliament. We discuss this study in more detail in section II.

A key benefit of the RCT is higher causal validity, i.e., the ability to infer that one thing caused another, which is stronger than just detecting a correlation.³² To argue causality between two things, such as incarceration and recidivism, one must eliminate every alternative explanation for that relationship. Randomization limits the possibility that alternative explanations exist, because the design ensures that unmeasured factors will be randomly distributed and therefore not affect results. In nonrandomized studies, the treatment group and the control group may be different, and efforts to control for those differences are hampered by the reality that the differing factors may be numerous, unmeasurable, or even unknown. With truly random assignment and a large sample, variation among participants is less critical because

any differences will average out between groups and therefore not impact results.³³ If there are differences between groups, then these differences should be “controlled for” using statistical techniques.

The same is true in the study of crime and punishment.³⁴ A well-conducted randomized experiment with a large enough sample provides an assurance that the treatment group and the control group differ in no way other than the treatment, an assurance that observational studies cannot provide. However, in circumstances such as incarceration, it is typically not ethical to consider RCTs.³⁵ For example, there would be an obvious ethical problem in assigning people to arbitrary sentence lengths at random, particularly for serious crimes deserving severe punishment. How many people would give informed consent to being sentenced to five or ten years in prison, at random? Even if consenting defendants could be found, the individuals consenting would likely be so atypical that it would introduce additional bias to the design.³⁶ Further, one must consider how the public, and especially the victims, would react. With this in consideration, it is not surprising that Nagin, Cullen, and Jonson³⁷ found only two actual experiments doing so, mostly using old data, and only one of which involved serious adult offenders. They found the evidence from this group of studies weak due to the data and sampling constraints, coupled with the fact that many of the findings were not statistically significant.³⁸ Additionally, the methodologies were not consistent, and many of the studies reviewed by Nagin, Cullen, and Jonson³⁹ do not examine incarceration length as its own variable. It is often confounded with offense type, criminal history, or other factors. The Nagin et al. review is discussed in further detail below.

Selection bias is a major challenge when assessing the causal impact of incarceration on

reoffending. This is because judges might be more likely to apply lengthier sentences for more serious criminals, so people who are selected for longer sentences will naturally differ from those with shorter sentences. Many studies attempt to account for selection bias using matching techniques or through applying statistical controls for things like offense type and prior records. However, these approaches are still susceptible to unmeasured factors.

Some recent studies exploit naturally occurring variability in the use of incarceration, reducing selection bias. This is often in the form of random assignment of cases to judges, which helps ensure that both unmeasured and measured case characteristics (e.g., criminal history, offense seriousness) are the same across judges. Judges with identical caseloads but differential use of incarceration can then be compared to see if recidivism differences are related to differences in sentence length. However, in this method the cases are not assigned at random to different “treatments” (i.e., length of incarceration) but rather to a factor that is correlated with but one step removed from the variable of interest.

Another way to exploit naturally occurring variability in sentence length is by relying on pre-existing sentencing grids that guide judges’ sentencing decisions. This type of design compares cases above and below the relevant guideline thresholds, assuming that offenders on each side of the cutoff are similar to each other in most ways except sentence length.⁴⁰

Understanding the degree to which research designs can vary provides insight into how studies on similar topics can come to different conclusions. The research assessing the relationship between imprisonment and recidivism follows a similar pattern, with different methodologies often resulting in varying findings.⁴¹ Admittedly, there are several challenges in the research. First, many of the designs include “confounds,” or variables that are directly

aligned with the variable of interest. Even in the best research study, confounds can render results invalid. Second, most of the studies are skewed toward samples of lower-level offenders and those with shorter sentence lengths, which may not be generalizable to serious/violent offenders with longer sentence lengths. Third, there are many different ways of measuring recidivism, which can affect findings. For example, re-arrest rates tend to be higher than re-incarceration rates, so studies relying on the former will naturally have higher recidivism outcomes. In contrast, if re-incarceration rates include returns to prison for parole violations (not new crimes), then re-incarceration rates may trend higher than re-arrest rates. Follow-up timeframes also vary across studies, with lengthier follow-up periods leaning toward higher recidivism rates. These inconsistencies make it complicated to compare outcomes across studies.⁴² We explain this in more detail below followed by examples from research.

When thinking about evidence-based policy, we have to consider the quality of the research evaluations being used to determine effectiveness, how effectiveness is being defined and measured, and how consistent the results are across a variety of methodologies, geographies, and contexts.⁴³ Recall how difficult causality is to prove. It is easier to demonstrate that two things are *correlated*; it is much more difficult to demonstrate that one *caused* the other.⁴⁴ Considering the impact of incarceration and recidivism, for example, it is unlikely that an empirical straightforward explanation exists. It is more likely that people respond to policy changes in a variety of ways that may or may not be directly or indirectly related to recidivism risk.⁴⁵ This is much different from claiming a causal relationship between two factors.

Even as evidence-based policy has gained some acceptance in the field, some policies such as Gascón's⁴⁶ are based on selectively cited research rather than the full breadth of research

as a whole. When this happens, the policy relevance of research findings declines rapidly.⁴⁷ The U.S. criminal justice system has a lengthy history of rapid policy change without comprehensively assessing the empirical evidence, often resulting in damaging consequences that are difficult if not impossible to reverse. While reliance on empirical research is critical for effective policymaking, it depends on the quality of the studies, the consistency of the results, and other contextual factors across time and place.⁴⁸ Further, no matter how sound a study methodology is, no one study on its own provides a basis for policy transformation. Though, a series of consistent findings across studies with sound methodologies may do so.⁴⁹

II. EFFECTS OF PUNISHMENT ON CRIME

Punishment is thought to affect crime in various ways, which are often referred to as purposes of punishment.

Deterrence. One key purpose that underlies many penal policies and crime control efforts is deterrence.⁵⁰ This can be in the form of specific deterrence or general deterrence. Specific deterrence is when the painful experience of being punished convinces an individual to refrain from crime in the future to avoid repeating the experience.⁵¹ General deterrence, in contrast, is when the knowledge of others' punishments deters would-be offenders from committing crime due to fear of receiving a similar punishment.⁵²

Incapacitation. Incapacitation is another key purpose of punishment that involves removing an individual from society, typically via long-term confinement or death. This physically prevents a person from committing crimes, or at least limits his targets to those inside the prison.⁵³

Rehabilitation. Rehabilitative efforts refer to any experiences (e.g., drug treatment programs, vocational training) that attempt to positively alter one's behavior, ideally transforming the offender into a person who will refrain from crime for reasons other than incapacitation or fear of punishment.⁵⁴ The inverse of rehabilitation is a criminogenic effect, or the notion that prison is a school for crime and people come out more crime-prone than when they went in.⁵⁵ Many of these theories argue that interactions and socialization within prisons can lead to the learning of criminal behavior from fellow inmates.⁵⁶ However, this effect tends to be more relevant to lower-level offenders who are more similar to offenders sentenced to probation.⁵⁷ All of these effects are possible, and sorting them out is one of the major challenges of research in this area.⁵⁸

As mentioned, some of the explanation for inconsistency of findings is 1) heterogeneity in response to punishment for different types of offenders; 2) differences in study design, measurement, geography, and other methodological factors; and 3) variation in the degree to which policies are actually enforced/implemented.⁵⁹ Not surprisingly, crime reduction impacts tend to be stronger when programs are successfully implemented, something that is not always easy to control or monitor.

The next section will briefly describe what is known about general deterrence and incapacitation, followed by a review of the literature on the post-release effects specific to the individual.

A. General Deterrence.

General deterrence is based on the basic principle of human behavior that if the cost of doing something outweighs the reward, then fewer people will do it.⁶⁰ The principle is so basic

that the debatable question is not whether a deterrent effect exists, but only how strong it is.⁶¹

There is also little doubt that deterrent effects include components of both how severe the punishment is and how likely it is to be imposed.⁶² Obviously, a punishment would have no significant deterrent effect if it was so mild as to be inconsequential or if it was never imposed. Considering the empirical evidence from a wide range of studies on deterrence theory, research has consistently shown presence of a deterrent effect of punishment in at least some contexts.⁶³ For example, a study by Eric Helland and Alexander Tabarrok⁶⁴ examined California's widely-known "three strikes" law by comparing post-sentencing criminal activity for people convicted of a strikable offense with a group of similar offenders who were *tried* for their third strikable offense but were instead convicted of a non-strikable offense. In addition to California, the study examined people within New York, Illinois (both which did not have three strikes laws), and Texas (which had a similar three-strikes law that preceded California). Regression models found that the legislation significantly reduced three-year felony arrest rates by 17-20% among criminals with two strikes in California and Texas – the states with three-strikes laws – but not in Illinois or New York.

Other studies have explored the effect of sentence enhancements on recidivism. Daniel Kessler and Steven Levitt⁶⁵ examined the changes in California crime rates following the passage of a voter initiative in 1982 that provided enhanced sentences for repeat offenders of certain crimes.⁶⁶ Kessler and Levitt⁶⁷ determined that enhancement-eligible crimes in California dropped four percent in the first year after enactment, compared to the overall national trend. This drop could not be an incapacitative effect because the persons sentenced for these crimes would not have begun the enhanced part of the sentence; this suggested that the drop was a

deterrent effect. Similar legislation on sentencing enhancements for gun crimes was studied by David Abrams,⁶⁸ who found that gun use enhancements reduced gun-related robberies by an average of 6.6%, 14.8%, and 17.9% when examined at one, two, and three years after enactment, respectively. Abrams⁶⁹ also found small reductions in gun-related assaults, an average drop of 1.81% and 0.82% after two and three years, respectively.

Francesco Drago, Roberto Galbiati, and Pietro Vertova⁷⁰ studied an unusual type of natural experiment in which sentence enhancements for recidivism were applied irrespective of criminal history and current offense, meaning that the impact of sentence enhancements could be examined independently of criminal history and offense characteristics. Under an Italian clemency law passed in 2006, a cohort of inmates were released from prison early on the condition that if they offended again within five years, the time subtracted from their sentence would be added on to a new sentence for any subsequent crime. People in this cohort had varying lengths of time remaining on their sentence (ranging from one month to three years), so they were subject to sentence enhancements of various lengths, should they reoffend. Drago and colleagues⁷¹ examined database records for 25,800 of these individuals to study the deterrent effect on future offending. They found that those threatened with lengthier enhancements were somewhat less likely to reoffend within the seven months initially following release.⁷² Specifically, a one-month increase in anticipated punishment lowered the probability of re-imprisonment by about 0.16%.⁷³

There is considerable room for disagreement about deterrence,⁷⁴ but the legitimate disagreement is about the magnitude and conditioning of the effect, not the existence of an effect.⁷⁵ Arguments that punishments always deter and never deter are equally and oppositely

wrong. Given that sanctions do have some deterrent effects, eliminating them altogether would produce some increase in crime. A policy argument for eliminating sanctions would require justification that the elimination would produce benefits sufficient to offset the additional crimes. Policymakers often fail to consider the potential unintended effects of expeditious policy change. This is an important oversight that can undercut the effectiveness of any policy. For example, mass release of prisoners is one approach to reducing the prison population. However, this could negatively impact public safety if done too rapidly without adequate consideration of recidivism risk.

B. Incapacitation.

Incapacitation is the most obvious effect of punishment on crime. In most cases, everyone outside of the prison walls will be safe from any further crimes by a given criminal who has been removed from society.⁷⁶ The existence of an incapacitative effect is not debatable. Estimating the magnitude is not a simple task, and requires estimating the crimes that would be committed by the prisoners if they were either released or never incarcerated for their crimes. Not surprisingly, this is hard to do without error. Due to the difficulties involved in formulating such a prediction, it is difficult to ensure that treatment and comparison groups are comparable in this regard. This presents a significant methodological challenge in constructing an adequate comparison group.

Alex Piquero and Alfred Blumstein⁷⁷ note that estimates of the incapacitative effect “vary markedly from study to study.” This may be an understatement. The primary factor in dispute is the estimated number of crimes per year committed by a criminal who would have otherwise been imprisoned. However, the overall estimate is largely irrelevant to questions of sentence

enhancement policy because individual rates of crime commission vary widely. Research shows that a small percentage of habitual offenders are likely responsible for a large portion of crime,⁷⁸ and their offending trajectory may differ from nonhabitual offenders.⁷⁹

High-rate chronic offenders appear to be a small percentage of offenders whose offending trajectory generally follows an age-crime curve,⁸⁰ but with an overall higher likelihood to recidivate than lower-rate offenders.⁸¹ Other research suggests that this effect may be pronounced for violent offenders (such as those using weapons), who have been found to be re-arrested at higher rates and for more serious crimes than nonviolent offenders.⁸² This difference was the most pronounced for those identified as “career offenders,”⁸³ suggesting that a small number of offenders are likely responsible for a large portion of crime. Data from the U.S. Sentencing Commission also shows that seriousness of offense is linked to increased recidivism rates.⁸⁴

One implication for future research would be to learn how to better identify these high-rate chronic offenders. In theory, substantial crime reduction could be achieved by incarcerating a relatively small number of prolific offenders, a phenomenon known as “selective incapacitation.” In practice though, identifying those most likely to reoffend is not a precise exercise.⁸⁵ Further, if the factors used to identify prolific offenders are not related to culpability, longer sentences for those identified may be unjust.⁸⁶

Despite these issues, there is no doubt that incapacitation plays an important role in public safety, as even the foremost opponents of “mass incarceration” agree. According to Alfred Blumstein,⁸⁷ “Incapacitation through imprisonment is probably the only effective means of restraining the violent crimes committed by some individuals otherwise out of social control.”

The key question, then, is whether the incapacitative benefit for “individuals otherwise out of social control” is outweighed by a criminogenic effect – i.e., whether longer sentences will actually increase recidivism rates. In the next section of this paper, we review the research to determine whether it is rigorous enough to answer this question. The research reviewed includes studies published prior to March 2022.

III. INCARCERATION AND POST-RELEASE RECIDIVISM

The effects of incarceration on a prisoner’s post-release recidivism includes at least four conceptually different mechanisms: rehabilitation, general deterrence, specific deterrence, and criminogenic effects, as described in section II. To understand the big-picture relationship between incarceration and recidivism, though, it is not necessary to separate these mechanisms. Thus, the research often attempts to examine the overall correlation between incarceration and subsequent offending while statistically controlling for other factors.⁸⁸

Aggregate trends in recidivism rates are highlighted in a Bureau of Justice Statistics (BJS) report⁸⁹ that examined recidivism patterns of 73,600 federal prisoners from 24 states, with a lengthy follow-up period of ten years. Criminal history records, prison admissions/release data, and arrest records were used to provide information about offending patterns and recidivism. Among the sample, 80.5% were arrested at least once for a new crime (i.e., not a parole violation) during the ten years following release, and 40% were re-arrested for a violent crime. The report did not focus on incarceration length of stay specifically, though length of stay was examined in a prior report by Ryan Cotter⁹⁰ that is discussed in more detail below.

A. The Nagin, Cullen, and Jonson 2009 Review.

For research before 2009, we already have the benefit of a thorough review.⁹¹ Nagin, Cullen, and Jonson⁹² reviewed 5 studies that used random assignment, 11 quasi-experiments,⁹³ and 31 regression-based studies. This review highlights a key distinction between two sentencing decisions: (1) whether to sentence the defendant to prison or jail at all, rather than a noncustodial sentence such as a fine or probation; and (2) for those sentenced to incarceration, how long the sentence will be. The two are not the same, each with a different causal mechanism,⁹⁴ and studies of these different effects should be considered separately.⁹⁵ Only the second of these is relevant to the current article.

The first topic of inquiry involves offenders who are on the “policy margin between prison and probation sentences.”⁹⁶ These offenders tend to have less serious current convictions and fewer if any prior convictions. Going to jail for any time at all disrupts family, social, and employment relationships followed by social stigma, all of which is more likely to interfere with resumption of lawful employment for a first-time or less serious offender than it is for repeat violent offenders.⁹⁷ In comparison, there are more serious offenders who do not fit within this “policy margin,” for whom probation is clearly not an appropriate sentence. Offenders who might be better candidates for probation are those who are first-time offenders or those convicted of non-violent offenses.

In contrast, offenders who are not candidates for probation typically have committed especially grave crimes or are already repeat offenders.⁹⁸ Sentence enhancements, which can increase the penalty for certain crimes, typically are applied for offenders with prior felony convictions or those who are particularly culpable, wanton, or cruel.⁹⁹ For example, a robbery with a gun presents a greater threat to safety than a robbery committed without a weapon (or

with a less lethal weapon), and thus is eligible for an enhanced sentence.¹⁰⁰ In this case, the use of a gun increases an offender's culpability and therefore justifies a harsher punishment. Prior felony convictions are also often used as a justification for an enhanced sentence, because criminal history tends to generally indicate a higher recidivism risk.¹⁰¹ For example, in California, one who commits a crime on the state's "serious felony" list after one or more previous convictions for crimes on the same list may be eligible to receive an enhanced sentence.¹⁰²

As the purpose of this article is to discuss the relationship between sentence length and recidivism, the studies discussing "the effect of custodial versus noncustodial sanctions"¹⁰³ are pertinent only to the extent they have other implications. The studies examining "the effect of sentence length on reoffending" are more directly relevant, though there are fewer of them.¹⁰⁴

Among the studies reviewed by Nagin, Cullen, and Jonson¹⁰⁵ that specifically examined the impact of sentence length on recidivism, there were three total experimental designs across two articles (one study combined both experiments into one paper).¹⁰⁶ One showed non-statistically significant deterrent effects.¹⁰⁷ The other article published results from two experiments in the same paper, and showed increased recidivism among inmates randomly selected for a shorter sentence.¹⁰⁸ However, results from the latter are invalid due to various methodological issues discussed below, and do not add value to our current review.

John Berecochea and Dorothy Jaman¹⁰⁹ used an experimental design to examine recidivism rates among a sample of inmates convicted of various violent and non-violent offenses, all of whom had received a set parole date but still had six months or more remaining on their sentence. The jurisdiction used a random number table to allocate inmates to two groups,

one of which would have their parole dates advanced by six months (thereby reducing their length of stay by six months). Randomization was successful, generating two groups that were comparable with each other. Average time served was about 35 months, with a difference of 6.6 months between groups (reflective of treatment group status). Recidivism was measured at one-, two-, and three-year follow-ups by a binary indicator of returning to prison (for either a court conviction, new felony, or for a parole violation) vs. not returning to prison. At the end of the first year, slightly more of the early release group returned to prison (34%) than the control group (28%). At the end of two years, 47% of the early release group had returned to prison, compared to 40% of the control group. These effects showed a slight deterrent effect of lengthier sentences on recidivism, however, neither of these differences were statistically significant and there was no sizable deterrent nor criminogenic effect either way.

Elizabeth Deschenes, Susan Turner, and Joan Petersilia¹¹⁰ conducted two randomized experiments to evaluate the effectiveness of two intensive supervision programs (ISPs) in Minnesota. The two programs were intensive community supervision (ICS) and intensive supervised release (ISR), both of which diverted people to community supervision in lieu of prison time. The former was a true diversion program that diverted people from incarceration prior to their prison sentence, while the latter was more akin to an early release program. The offenders were male, and the majority were arrested for theft, burglary, or probation violations, all of whom were facing or serving prison time of 27 months or less. The authors compared the number of days in confinement for: 1) ICS (the true diversion program) vs. ICS control, and 2) ISR (the early release program) vs. ISR control.

Recidivism was quantified by proportions of people arrested or re-incarcerated for a new

arrest or technical violation. Data on arrests, technical violations, and criminal histories was obtained at six- and 12-month follow-ups, as well as 24-month follow-ups for the ICS groups only. Randomization was successful, and groups in both experiments were statistically equivalent on criminal history at baseline. Unfortunately, though, prison length of stay is only a minor consideration in this study. On its face, the study by Deschenes et al.¹¹¹ actually appears to be a study of community supervision versus confinement, where length of incarceration was only considered as a minor point. In addition, all of the sentence lengths are very short and measured in days, so this is certainly a limited measure of “length of stay.”

The ICS program was a true diversion program in that it diverted people from prison to the community as part of their sentence, while people in the ISR program were already incarcerated and diverted to the community during the last six months of their sentence (by being released early). In the ICS comparisons, offenders (n~248) experienced significantly different lengths of incarceration (an average of 108 days and 220 days for the ICS and control group, respectively) prior to being randomly assigned into groups. At the two-year follow-up (only available for ICS groups), there were no significant differences in terms of re-incarceration rates (about 50% in each group) nor re-arrests for new crimes (about 60% in each group). However, the only reason why groups differed in terms of time served (a difference of about four months) is because one group was purposefully diverted. Given these considerations, it seems that differences regarding time served is confounded with the treatment group.

The ISR program was an early release group that released people early from prison if they had six months or less remaining on their sentence. Individuals who had six months or less remaining on their prison sentence (n~350) were randomly assigned to either finish their

sentence or be released early. Both groups served an average of 44 days in confinement.

Unfortunately, because groups served similar amounts of time in confinement, it is not possible to ascertain a causal relationship between length of stay and recidivism. In addition, there were no statistically significant differences between groups regarding one-year re-arrest rates (15% and 21% for treatment and control, respectively) or the likelihood of returning to prison (about 50% for both groups).

In the Nagin et al.¹¹² review, there were three quasi-experimental designs,¹¹³ though the overall “results of these studies are quite varied.”¹¹⁴ Indeed.

Dorothy Jaman, Robert Dickover, and Lawrence Bennett¹¹⁵ conducted a quasi-experimental matching study using a sample of 390 parolees from California prisons. All participants had been incarcerated for first-degree robbery or second-degree burglary, and data were collected from four periods in parolees’ lives: pre-institutional, admission, release, and first two years on parole. Participants were matched on a number of factors related to parole outcome (e.g., offense category, age), generating 75 matched pairs for the first-degree robbery group and 120 matched pairs for the second-degree burglary group. For each pair, one person served more than the median time of 45 months, considered the “high dose” group, while the other person in the pair served less than the median, considered the “low dose” group. For both offense types, treatment and comparison groups were similar to each other on all observed factors, except for time spent in confinement. Unfortunately though, the groups likely differed on some unobserved factors. Specifically, this study examined a time and place where parole board discretion would have heavily impacted release decisions. Parole boards’ release decisions are made based on a deliberate effort to predict whether someone would reoffend, and there are likely myriad factors

examined by the parole board that are not controlled for in this study. This increases the risk that groups might differ in an important way.

For first-degree robbery, the men in the low dose group served 36 months (3 years) on average and those in the high dose group served 65 months (5 years) on average. The authors were then able to compare the two categories of 'time served' and the relationship with recidivism at six months, one year, and two years post-release. Recidivism was measured by unsuccessful parole outcome, meaning that the person returned to prison. The authors found small criminogenic effects on re-incarceration rates for the high dose group for all follow-ups. The difference between groups at the six-month follow-up was non-significant (4% and 8% for low dose and high dose groups, respectively). However, at the one-year follow-up, re-incarceration rates were 6.7% for the low dose group and 16.1% for the high dose group (a statistically significant difference). At the two-year follow up, the difference was still statistically significant, with 37.4% those in the high dose group being re-incarcerated, compared with 18% of those in the low dose group.

In the second-degree burglary group, the average time spent in confinement was 24 months (2 years). The average time served was 16 months for the low dose group and 36 months for the high dose group. The low dose group was less likely to return to prison than the high dose group; however, this was not significant until the two-year follow-up. At six months, the low dose group had a lower average percentage of returning to prison (14.2%) than the high dose group (11.7%). At the one-year follow-up, the low dose group had a slightly lower rate of returning to prison (24.2%) than those in the high dose group (25.8%). While the findings from the first two follow-ups were not statistically significant, a statistically significant criminogenic

effect emerged at the two-year follow-up, with the low dose group having a significantly lower rate of returning to prison (42%) than those in the high dose group (46.7%).

In other words, this study suggested a criminogenic effect of longer sentences. However, the sample was limited to robbery and burglary offenders with an average time served of 27 months, so it is hard to know whether these results would extend to more serious criminals or those serving longer sentences. Further, these results could have been impacted by unobserved factors related to parole board discretion, as discussed above.

J. Kraus¹¹⁶ conducted a quasi-experimental matched-pairs study using a sample of 446 juvenile offenders convicted mostly of theft and burglary. This study primarily compared probationers with non-probationers regarding recidivism rates, and a ‘length of stay’ variable was included as part of the analysis. Kraus first used several demographic variables and data from criminal records on offense type and the number and length of stays in institutions to create 233 matched pairs of offenders. Then, he continued to collect data on recidivism for five years post-release. Due to the different severity levels of the offenses, the sentence lengths varied by offense type, which was used as a proxy for time served. Though, as measured, “time served” was actually indicative of offense type rather than measuring the actual impact of length of confinement.

Relatedly, this study seemed to compare differences in recidivism across different offenses, rather than actually examining differences in incarceration length. Findings suggested that some juveniles might experience higher numbers of criminal offenses after committal to an institution, but this effect was based on offense type. For example, offenders convicted of “stealing and burglarizing” were less likely to recidivate after spending time in prison, but there

were no differences regarding recidivism rates for motor vehicle theft, assault, and sexual offenses. However, offense type is highly correlated with length of confinement, so the impact of time served is not clearly isolated in this particular study. Due to this confound, there are no valid results that can be drawn from this study regarding the impact of time served on recidivism. Additionally, most of the offenders were juveniles serving sentences of less than two years, limiting generalizability of the sample.

A longitudinal quasi-experiment by Loughran and colleagues¹¹⁷ examined recidivism rates among 921 juvenile offenders from two large cities. Most of them had been convicted of a felony, though they all had relatively short sentences (the maximum sentence was 15 months). Recidivism was measured by post-confinement re-arrest rates and self-reported re-offending, with data collected at six-month intervals for the first three years and annually for the fourth year. The main analyses compared probationers with non-probationers, but a portion of the analysis examined the marginal benefits for longer length of stay once the institutional placement decision had been made. The authors tested two iterations of dosage categories; the first iteration included four dosage categories (i.e., 0-6 months, 6-10 months, 10-13 months, and >13 months) and the second iteration included five dosage categories (i.e., 0-3 months, 3-6 months, 6-9 months, 9-12 months, and >12 months). For both iterations, dosage categories were compared to each other to examine whether different lengths of stay impacted future offending.

Overall, they found little impacts of longer length of stay and future rates of re-arrest or self-reported offending. Re-arrest rates were lower for people serving more than three months and for those serving 13 months or more (for the in-between categories, rates appeared more similar). However, the samples for each of these categories were so small that the statistical

power was quite low. In addition, 28 out of 66 important covariates (e.g., offending history, exposure to violence, legal cynicism, and association with antisocial peers) were statistically different between dosage categories, hampering the validity of these comparisons. Due to the low statistical power and the lack of statistical equivalence between categories, the study did not show strong support for either a deterrent effect or a criminogenic effect resulting from longer lengths of confinement.

Based on the Nagin et al.¹¹⁸ review, there is little evidence to support criminogenic effects related to longer periods of incarceration. First, these studies tended to compare confinement vs. non-confinement and typically only included length of stay as a minor point. Secondly, the studies are compounded by a number of methodological issues, such that only three of the six high-quality studies reviewed by Nagin et al.¹¹⁹ are actually helpful for understanding how differences in time served affect recidivism. The bottom line is that as of 2009, “there [was] little convincing evidence on the dose-response relationship between time spent in confinement and reoffending rate.”¹²⁰ That is, studies did not clearly demonstrate that longer prison sentences increased recidivism.¹²¹

B. Subsequent Research.

As discussed above, estimating the causal relationship between length of incarceration and recidivism is difficult for a variety of reasons,¹²² and only a handful of methodologically rigorous studies have attempted to do so since the 2009 review.¹²³ The findings are still mixed, providing little conclusive evidence for or against the specific deterrent effects of imprisonment. Of the studies published since, three employed judge-assignment pseudo-randomization strategies and collected data of key variables after the randomization.¹²⁴ Eight studies exploited

natural experiments and relied on pre-existing data while attempting to control for other factors using methods such as propensity score matching (PSM), instrumental variables, or controlling for observed factors in statistical models.¹²⁵ Finally, one study re-examined data from two of the prior studies but with different analytical strategies.¹²⁶

Sarah Walker and Jerald Herting¹²⁷ examined 22,276 matched pairs of juvenile cases that were filed in 32 court jurisdictions in a northwest state from January 2002 through December 2015. One-year recidivism was measured by two binary indicators: whether the youth had a court filing for a misdemeanor within 12 months, or whether they had a court filing for a felony within 12 months. The main analysis compared those who were detained pretrial with those who were not, while controlling for factors such as prior record, offense severity, and demographics. When number of days in jail was included as a predictor variable, the authors found a small, statistically significant increase of 1% in felony recidivism per day of incarceration. However, sentence length is confounded with pretrial detainment in this case, as any differences in length of stay would be attributable to differences in pretrial detainment. Because the types of offenders being detained pretrial are likely different from those that are released, it is very unlikely that the effects of days in jail could be untangled from the effects of pretrial detainment itself. In addition, the number of days spent in jail was heavily skewed, with a range of .03 to 362 days, a mean of 8 days, and a mode of 2 days, which prohibits any valid comparisons across varying sentence lengths. Thus, the results do not add value to the current paper.

Randi Hjalmarsson and Matthew Lindquist¹²⁸ examined the impact of two Swedish early release reforms in 1993 and 1999 that held prison sentences constant but increased the share of time inmates were required to serve from one-half to two-thirds. This created natural variation in

days served, which allowed the authors to compare individuals with the same sentence length who served different amounts of time incarcerated. The sample was comprised of 46,800 individuals who began their sentences between 1992 and 2001. Original sentence lengths ranged from 4-48 months, and the average sentence length was 11.7 months. Those exposed to the reform served an additional 46 days in jail, on average. Recidivism was measured in three ways: any conviction, more than one conviction, and any return to prison. This was measured at 12, 24, and 36 months.

The authors employed a regression model that examined the impact of sentence length on recidivism while controlling for various related factors (e.g., offense history, offense type). Results showed that people who were affected by the legislation had lower rates of recidivism across all three follow-ups. Specifically, reform exposure significantly decreased the rate of people with one new conviction at 12 months by $-.015$ (though this was not statistically significant at 24 or 36 months). Reform exposure was also associated with significant declines in return to prison rates when measured at 12 months ($-.029$) and 24 months ($-.020$), but not at 36 months. This reduction was driven by property offenders, older offenders, those with prior incarcerations, and those with no recent history of employment. The authors also found beneficial labor effects and improvements in long-term health outcomes for people affected by the reform. These effects were driven by specific at-risk populations (e.g., people with pre-incarceration mental health problems). The authors argued that these people benefitted from in-prison health and treatment services, which could explain why increased length of stay was associated with better outcomes.

Benjamin Meade, Benjamin Steiner, Matthew Makarios, and Lawrence Travis¹²⁹

examined one-year felony re-arrest rates using a quasi-experimental design with a sample of nearly 2,000 parolees in Ohio. Offenders were all released at the same time following statewide changes in parole statutes, but differentiated in terms of time served. The goal of the study was to isolate the impact of time served (measured by the categories: <1 year, 1-2 years, 2-3 years, 3-5 years, and >5 years) on felony re-arrest rates. Because groups naturally occurred and likely differed from each other, the authors used pre-existing data to generate propensity scores to balance groups on important factors (e.g., prior convictions). The average effect for time served on recidivism suggested that individuals who served longer sentences in prison had lower rates of recidivism. The results showed that the odds of felony re-arrest were highest among those serving between one and two years. In comparison, those serving less than one year had slightly decreased odds of re-arrest. Similarly, those serving two years or more also had decreased odds of re-arrest. Aside from the people serving less than one year, the odds of re-arrest decreased as time served increased. This deterrent effect was statistically significant for those serving more than five years.

Meade and colleagues¹³⁰ opine that potentially longer prison terms may deter people from committing more serious crimes rather than being deterred from committing crime altogether. Regardless, it is possible that offense history and seriousness of the initial crime may confound with recidivism rates, making it difficult to isolate the impact of length of stay on its own. Importantly though, the observation of initial deterrent effects (for those serving less than one year), followed by criminogenic effects (for those serving one to two years), followed again by deterrent effects (for those serving more than two years) suggests that the relationship may be curvilinear rather than linear. This has important implications for research, particularly for

studies that rely on samples with short sentences. If time served increases criminogenic effects up until the point of two years, this means that studies examining offenders serving two years or less will be prone to finding criminogenic effects. Moreover, if a curvilinear relationship does exist, it will not be fully captured in studies that rely on offenders with overall short sentences. Another important consideration is that offenders with short sentences are typically less serious offenders, and these findings are may not be applicable to more serious offenders who typically serve longer sentences.

Daniel Mears, Joshua Cochran, William Bales, and Avinash Bhati¹³¹ studied the recidivism patterns among a cohort of more than 90,000 violent and non-violent inmates released from Florida prisons. The study was a quasi-experiment where authors examined the impact of time served (in months) on one-, two-, and three-year felony re-conviction rates. Groups were naturally occurring and were not similar to each other at the outset. The authors sought to remedy this by generating propensity scores with pre-existing data that balanced groups on variables related to time served. Among the whole sample, the average time served was two years, and 47% of inmates were re-convicted of a new felony offense within three years post-release. Similar to Meade et al.,¹³² Mears et al.¹³³ were interested in how linear models and curvilinear models can yield different results. The authors estimated multiple time series models that hypothesized various positive, negative, and curvilinear offending trajectories. Each model included specifications regarding the direction of the hypothesized relationship between variables and included various covariates in the model to control for pre-incarceration differences between groups. The authors compared results across the three iterations to look for consistencies.

They found similar results across all trajectory iterations, but there were a few interesting caveats. For people serving less than one year, an initial criminogenic effect emerged with significant increases in felony re-conviction rates at the one-year follow-up. However, this was not significant in subsequent follow-ups. For people serving one to two years, a deterrent effect emerged, with significant decreases in recidivism rates at one- and two-year follow-ups. Deterrent benefits tapered off once terms exceeded two years, resulting in no criminogenic or deterrent effect after this point. Results were consistent across all models, though actual estimates varied. Similar to Meade et al.,¹³⁴ Mears et al.¹³⁵ found initial criminogenic effects followed by deterrent effects, suggesting that the relationship between time served and recidivism may be curvilinear (rather than linear). Mears et al.¹³⁶ described the curvilinear relationship as an “inverse U-shape.”

The finding of a curvilinear relationship has important implications for research and may explain a lot of the mixed findings. If length of stay initially increases recidivism *before* decreasing it, studies with samples that are skewed toward shorter sentence lengths may not capture the curvilinear relationship, particularly those relying only on linear models. Thus, they are unable to observe the subsequent deterrent effect that occurs as sentence lengths increase and are prone to finding criminogenic effects. Further, some research that has used samples with wider variations in sentence length¹³⁷ have found similar results suggesting a curvilinear relationship. While more research is certainly warranted, Mears and colleagues¹³⁸ argue that lengthier sentences may be justified in some circumstances to achieve retributive goals or a stronger deterrent benefit.

William Rhodes, Gerald Gaes, Ryan Kling, and Christopher Cutler¹³⁹ examined the dose-

response relationship between prison length of stay and re-incarceration rates among a large sample of federal offenders. All offenders were convicted of various violent and non-violent felonies or misdemeanors and sentenced under the U.S. Sentencing Guidelines. The federal guidelines consist of a grid system of pre-determined sentence ranges that are based on offense seriousness and criminal history, which the authors leveraged as a proxy for criminal history and offense seriousness. Then, the authors generated an instrumental variable that balanced groups on criminal history and other baseline factors. Average time served for the whole sample ranged from 2 weeks to 18.6 years, and the wide range in time served allowed for observation of a wide range of offenders, a key benefit to this study. Rhodes et al.¹⁴⁰ measured recidivism using re-incarceration rates. At the three-year follow-up, they found small but significant deterrent benefits. Recidivism decreased about 1% for every additional 7.5 month increase in sentence length. Regression results with and without instrumental variable specification were consistent.

Three studies employed judge-randomization strategies, where defendants were randomly assigned to judges based on a random drawing. In these studies, defendants assigned to one judge are considered a group. In theory, judges differ on certain characteristics (e.g., some judges are more lenient than others) that could lead to disparate sentences across similar offenders. As a result, the defendants are similar enough to each other to make comparisons, but would differ in terms of sentence length depending on which judge they are randomly assigned to. Then, defendants assigned to a lenient judge can be compared to similar defendants assigned to a more punitive judge. In this way, similar defendants with different incarceration lengths can be compared to each other.

Michael Roach and Max Schanzenbach¹⁴¹ employed a judge-randomization strategy with

a cohort of nearly 8,000 lower-level felony offenders and 25 judges within a Seattle courthouse. All offenders in the sample pled guilty under one judge, and then were randomly assigned to a new judge for sentencing.¹⁴² Under state sentencing guidelines, judges have a fair amount of discretion to depart from recommended sentencing ranges for crimes of low severity and for first-time offenders.¹⁴³ Among offenders in the sample, the average offense seriousness level was fairly low (2 out of a possible 16), meaning that judges would have ample opportunity to depart from recommended sentencing ranges, if desire. The authors contended that this wide variation in judicial discretion resulted in “random” variation in prison sentences. The average sentence was nine months, and the 74% of imposed sentences were less than 12 months. Recidivism was measured based on whether an offender was sentenced for any new felony at one, two, and three years post-release. When controlling for differences between groups, the authors found a deterrent effect of lengthier sentences on re-sentencing rates, with an average decrease of about one percentage point per each additional month of incarceration.¹⁴⁴ While deterrent benefits were evident across all three follow-ups, the majority of effects occurred within the first year.

Manudeep Bhuller, Gordon Dahl, Kartine Løken, and Magne Mogstad¹⁴⁵ conducted a study that examined 33,500 criminal cases that were randomly assigned to 500 judges in Norway. Judges differed in their stringency when sentencing defendants, which generated natural variation in sentence length. Judge incarceration stringency was based on the judge’s average incarceration rate, which reflected their overall propensity toward custodial sentences. Judge sentence length severity was defined as the average sentence length across a judge’s caseload. The average length of time spent incarcerated was six months, and over 90% of people were serving less than one year. Importantly, this is much shorter than the average prison time in

the United States, which is approximately three years.

The authors examined the impact on recidivism using judge incarceration stringency as an instrumental variable, along with various statistical controls for demographics and type of crime. Based on the models, imprisonment exposure significantly decreased the chances that someone would receive new charges; the average reduction was 11 charges per person. This was statistically significant at two years and five years post-release. The decline was driven by individuals who were not working prior to incarceration but then participated in programs aimed at improving employability and earnings. When sentence length stringency was included in the model, results did not change much. This model revealed that increasing a sentence by 250 days resulted in small but statistically significant increases (ranging from .02 to .05) in future charges when measured at two years and five years post-release. Further analysis revealed large correlations between judge incarceration and sentence length stringency, which could explain why the latter only minimally impacted results.

Donald Green and Daniel Winik¹⁴⁶ also used a judge-randomization strategy with 1,000 offenders and nine judges/courtrooms from the District of Columbia superior courts. All of the offenders were convicted of drug-related felonies, and most had prior offenses. The average sentence length for the sample was one year, with a maximum of four years. Defendants were randomly assigned to judges. The researchers examined whether defendants assigned to punitive judges were more likely to be re-arrested than those assigned to lenient judges (presumably because judges impose different sentence lengths). The nine judges did tend to vary in sentencing tendencies as expected; some judges were more lenient than others, resulting in a range of administered sentences from five to 12 months across judges.

While the authors refer to the process as random, it is not technically random because there is some discretion within the court regarding judicial calendar assignment. The authors mitigate this by comparing the groups at the outset to assess their similarities on several factors (e.g. criminal history, offense type, demographics). The groups were statistically similar, and the researchers also controlled for the impact of these factors using an instrumental variable. Green and Winik¹⁴⁷ measured recidivism using four-year felony and misdemeanor re-arrest rates. They initially found a criminogenic effect of lengthier sentences, suggesting that on average, each additional month of incarceration increased the four-year re-arrest rate by about two percentage points. This effect was not statistically significant, though, and it disappeared when an instrumental variable was added to the model. Instead, statistically significant (yet small) deterrent effects emerged, suggesting that each additional month of incarceration lowered the probability of recidivism by .006.

In a 2017 study, David Roodman re-analyzed Green and Winik's¹⁴⁸ data. Roodman¹⁴⁹ applied a slightly different design upon questioning whether the two groups in Green and Winik's¹⁵⁰ study were actually fully comparable. Roodman¹⁵¹ used the same courtroom/judge assignment as the prior study, but when running regression models, he included additional specifications, measures of recidivism, and multiple follow-up periods. He measured recidivism using re-conviction rates for felony crimes and re-arrest rates for felonies and misdemeanors.

Results were less robust than the original study and varied based on recidivism measure. Roodman¹⁵² found criminogenic effects when looking at four-year re-arrest rates, but found deterrent effects when looking at four-year re-conviction rates. Specifically, he found that each additional month of incarceration was associated with a 1.3% *increase* in re-arrest rates and a

.04% *reduction* in felony re-conviction rates. It is not entirely surprising that the results varied by outcome measure; re-arrest rates are often higher than re-conviction rates due to the lower burden of proof required to arrest. The deterrent effect also seemed to go hand-in-hand with certain variables, such as participation in therapeutic programs or the ability to maintain employment. For example, people who received job training after being incarcerated experienced decreased recidivism rates. The author also examined whether results changed over time by examining different follow-up times (up to four years), though this did not impact results.

Considering all variations of analyses of Green and Winik's¹⁵³ data, some of the findings showed associations between deterrent effects and longer incarceration time. Though, effect sizes were minimal in both Green and Winik's¹⁵⁴ initial study and Roodman's¹⁵⁵ replication study. It is not uncommon to see smaller effect sizes and nonsignificant effects when the research design is strengthened, as Roodman¹⁵⁶ did here. Unfortunately, the sentences for all offenders were relatively short (approximately one year), so the study would not be able to sufficiently capture any "inverse U-shaped" effects, nor would it capture deterrent benefits associated with lengthier periods of incarceration. The significant deterrent effect is also independently related to participation in rehabilitative programs, such as job training, which may explain some of the mixed findings.

Ilyana Kuziemko¹⁵⁷ examined the relationship between time served and re-incarceration rates among state parolees released in Georgia. She used four different quasi-experiments, three of which found correlations between time served and recidivism rates. Based on the results, she concluded that spending more time in prison significantly reduced recidivism, with each additional month of time served reducing the probability of return to prison by one to three

percentage points at the three-year follow-up. The first part of the study included a large cohort of parolees while the second part of the study focused on a subgroup that was released following a specific statute.

For the first part of the study, Kuziemko¹⁵⁸ exploited state parole guidelines that release prisoners based on a “recidivism risk” calculation, i.e., the sentencing grid; this is typically indicative of time served. Georgia’s pre-existing risk classification system served as a proxy of time served to make strategic comparisons within a large sample of prisoners (n=17,000) who were released over a period of nearly 30 years (1981-2007). Kuziemko¹⁵⁹ compared similar nonviolent convicts entering prison on either side of the calculated “high-risk” cutoff. Among the 17,000 convicts, time served ranged from seven months to ten years, with an average of approximately 33 months (2.75 years). On average, offenders had about 0.8 prior incarcerations, and their offense severity levels ranged from one through four on a scale of 20. When assessed at the three-year follow-up, there was a non-significant deterrent effect of lengthier sentences, with a 1.3% decrease in re-incarceration rates (for a new crime) per additional month served. Prior incarcerations also positively impacted recidivism rates. For every additional prior incarceration, three-year re-incarceration rates increased significantly by .039%. She examined models with and without control variables, and not surprisingly, the models with control variables were methodologically stronger but resulted in findings that were less robust.

Second, Kuziemko¹⁶⁰ examined a subgroup of these nonviolent offenders (n=519) who were released as a result of 1981 state statutes. She tested whether those with different sentence lengths (recommended per the grid system) varied in regard to three-year re-incarceration rates. The recidivism risk calculation was a proxy to roughly account for time served, and the author

constructed an instrumental variable to balance factors between groups. On average, the 519 offenders had about 0.53 prior incarcerations, had served 13 months in confinement (ranging from one month to six years), and were released about five months early. At the three-year follow-up, 36% had returned to prison for a new crime. Using two regression models (with and without control variables), the author found that lengthier sentences were associated with a significant decrease in re-incarceration rates, a reduction of approximately 3.2% per additional month served.

Roodman¹⁶¹ also examined Kuziemko's¹⁶² data using similar methods, but with alternative model specifications and different measures of recidivism. Recidivism was measured using two measures at the three-year follow-up: 1) return to prison for a new crime (not a parole violation), and 2) re-conviction rates for new, serious crimes (excluding parole violations). Further, Roodman introduced an important predictor variable – the sentence *commute* time, i.e., the recommended sentence minus the actual sentence served. Roodman's¹⁶³ re-analysis of the data found similar results to Kuziemko,¹⁶⁴ but results were less robust. Overall, Roodman¹⁶⁵ found evidence of a deterrent effect related to lengthier sentences; they were associated with an average decrease of 1.3% in re-incarceration rates (for a new crime) per every additional month served. This effect was not statistically significant, though.

Roodman¹⁶⁶ also re-analyzed Kuziemko's¹⁶⁷ data on the subgroup of offenders released after 1981 changes in statutes. He employed regressions that compared the five years before the policy change with four years after the policy change. Roodman¹⁶⁸ included another measure of recidivism (i.e., total recidivism, measured by felony re-conviction or re-incarceration). When accounting for control variables, Roodman¹⁶⁹ found both criminogenic and deterrent effects. He

found that every additional month served statistically significantly decreased return-to-prison rates by .0031% on average, but this changed to a criminogenic effect when considering *total* recidivism rates (i.e., re-incarceration and re-conviction rates combined). In other words, a criminogenic effect emerged when recidivism was measured using re-incarceration, but flipped to a deterrent effect when *total* recidivism rates were considered. It isn't surprising that total recidivism rates trend higher than re-incarceration rates though, due to the higher burden of proof required for incarceration. While the effect sizes in this particular study were small, different measures of recidivism may be one reason why results vary.

Matthew Snodgrass, Arjan Blokland, Amelia Haviland, Paul Nieuwbeerta, and Daniel Nagin¹⁷⁰ employed a quasi-experimental study to examine the relationship between time served and recidivism for 4,683 prisoners in the Netherlands. All prisoners had been convicted of felony violent, property, or drug offenses. Data were collected from inmate records and recidivism was measured using three-year felony re-conviction rates. The authors used interquartile ranges of sentence length to create incarceration dosage categories, and they balanced categories on important factors (e.g., criminal history, offense type) using propensity scores. The authors were unable to balance groups across four important variables, which were subsequently included as statistical controls: age at first felony conviction, number of violent offenses, maximum possible punishment, and proportion convicted of a violent offense.

Dosage categories were compared with each other (i.e., <1 month, 1-2 months, 2-3 months, 3-6 months, 6-12 months, and >12 months). Offenders were classified as low dose if their sentence was on the lower end of the interquartile range and classified as high dose if their sentence was on the higher end of the range. The average length of incarceration was relatively

short at 6.7 months, with 86% of sentences being less than one year. When measured at the three-year follow-up, a deterrent effect emerged, though it was not statistically significant. High dose offenders faced .033 fewer felony re-convictions per year than comparable low dose offenders (re-conviction rates were .384 and .416, respectively). Based on these results, incarceration length seemed to have no real deterrent or criminogenic effect. However, this sample was limited to people with short sentence lengths, and results may not generalize to people with lengthier sentences.

Although not peer-reviewed, one of the most recent studies examining sentence length and recidivism was conducted by Ryan Cotter¹⁷¹ of the U.S. Sentencing Commission. The author used two different designs and four modeling approaches to estimate the impact of different sentence lengths (i.e., 2-3 years, 3-4 years, 4-5 years, 5-10 years, and >10 years) on eight-year re-arrest rates. The sample was 25,400 offenders released from federal prison, and the author used pre-existing data from FBI records to collect information on recidivism rates and criminal history. In the first design, Cotter¹⁷² matched approximately 13,000 offenders into pairs to generate comparable groups. In the second design, the author used matching *and* weighting (in an attempt to retain a larger sample size, a total of 16,800) to generate comparable groups. Both procedures were successful in generating groups that were fairly similar to each other with the exception of sentence length. The first design compared people sentenced from 12-36 months with comparable offenders who were sentenced 48-60 months. The second design compared people sentenced to 48-60 months with comparable offenders who were serving 12-36 months less time relative to their matched counterpart. The author then used a series of regression models to study the impact on re-arrest rates for new crimes and technical violations. The models

included statistical controls for important variables (e.g., age at release, race, criminal history, high school completion, violent offense, weapons offense), strengthening the design.

Cotter¹⁷³ found evidence of deterrent effects for certain length of stay categories. For example, offenders who were incarcerated for more than five years had significantly lower recidivism rates when compared with similar offenders with shorter incarceration terms. Consistently across all four models, incarceration terms that exceeded ten years were significantly associated with lower eight-year re-arrest rates (the decrease ranged from 30-45% depending on the model). In two of the four models (those with larger sample sizes), incarceration terms that exceeded five years were significantly associated with a 17% reduction in eight-year re-arrest rates relative to the offenders serving sentences that were 12-36 months shorter. When examining incarceration terms between two and five years, none of the models revealed significant criminogenic or deterrent effects. Terms that lasted one to two years were sometimes consistent with reductions in recidivism, but this effect was not statistically significant and varied across the designs.¹⁷⁴

Cotter¹⁷⁵ found that people with sentences of less than six months had the lowest average re-arrest rates (42%), followed by those serving ten years or more (50%), those serving two to five years (55%), and finally those serving five to ten years (56%). Overall, Cotter¹⁷⁶ found a non-significant criminogenic effect for terms of two to three years, followed by a null effect for terms of three to five years, and a significant deterrent effect for terms of five years or more. The initial criminogenic effects followed by deterrent effects as sentence length increased suggests again (similar to Meade et al., 2013 and Mears et al., 2016)¹⁷⁷ that the relationship between time served and recidivism is curvilinear.

The table in Appendix A summarizes information on the studies reviewed, with information on their adequacy in being able to determine cause and effect. Regarding study design, randomization is ideal for the purpose of generating comparable groups. If this is not possible, the next best option is to strategically create statistically comparable groups (e.g., by using a matched-pairs design or purposive assignment). When the groups are not statistically similar, authors must remedy this problem as best they can by controlling for factors that differ between groups as well as variables that are theoretically related to the primary outcome. If authors fail to do this, or are unable to do so adequately, it significantly decreases the causal validity of the design. Types of statistical adjustments (e.g., propensity score matching, instrumental variable identification, or regression-based statistical adjustments) can control for many but not all differences between groups. This paper only discusses studies that meet the above criteria, and results are summarized in Table 1 (see Appendix A).

VI. CONCLUSION

A total of 19 high-quality studies were included in the current review, though four did not add value to our conclusions due to methodological limitations.¹⁷⁸ Of the 15 applicable studies, there was one experimental design¹⁷⁹ and 14 quasi-experimental designs. Of the quasi-experimental designs, four used judge-assignment pseudo-randomization strategies.¹⁸⁰ All of the designs either met the criteria for statistical equivalence between groups, or they applied statistical controls to account for differences between groups.¹⁸¹ Eight studies suggested an aggregate deterrent effect in their results,¹⁸² five of which were statistically significant, but effect sizes were small.¹⁸³ Two studies suggested a significant aggregate criminogenic effect,¹⁸⁴ but one

of these studies suffered from a confound that rendered results meaningless.¹⁸⁵ Five studies had mixed results, suggesting both criminogenic and deterrent effects of lengthier sentences,¹⁸⁶ with one study¹⁸⁷ finding mixed effects based on the recidivism measure used.

Of the five studies with mixed findings, four of the studies found similar trends in the trajectory of re-offending. These studies showed initial criminogenic effects for those serving shorter-than-average sentences (generally, less than two years). After a certain threshold of time served, deterrent effects emerged. This suggests that the relationship between time served and recidivism may follow a curvilinear or “inverse U-shape” pattern (i.e., a pattern that increases and then decreases) rather than a linear pattern. The suggestion of a curvilinear pattern of recidivism has important implications for both research and policy. Many studies rely on linear models and samples of offenders with short sentence lengths (typically less than two years), and thus are unable to capture the additional deterrent benefits that may occur once incarceration length exceeds a certain threshold. The more recent research suggesting a curvilinear relationship also might explain why research has found so many mixed findings regarding the impact of time served on recidivism.

In summary, considering both the Nagin, Cullen, and Jonson¹⁸⁸ review as well as subsequent literature regarding time served on recidivism, findings are still inconclusive. Many studies do find a deterrent effect of lengthier sentences, but these studies have small effect sizes and are not always statistically significant. Many other studies have found mixed effects, suggesting that deterrent effects may be more associated with sentences exceeding two years. However, the study methodologies vary in terms of their approaches and limitations (see Table 1; Appendix A), which could explain some of the mixed results. In addition, studies span

different geographical jurisdictions and use inconsistent measures of recidivism (e.g., re-arrest vs. re-incarceration), which can condition findings. Third, most people examined in these studies had short sentence lengths, so the results may not extend to people with lengthier sentences. In addition, for studies examining people with short sentence lengths, results are unable to capture whether potential curvilinear relationships.

Research has not fully unpacked the complex relationship between length of incarceration and recidivism. Overall, some important considerations remain unclear. First, it remains unknown whether shorter prison sentences would result in the same sense of retribution for the victim as well as society. Second, it is unclear whether shorter prison sentences would counterbalance public safety gains achieved from incapacitation or deterrence. Third, identifying recidivism risk remains a daunting and complex task,¹⁸⁹ mostly because many factors independent of sentence length may influence one's likelihood to recidivate. Some of these factors include age,¹⁹⁰ offense history,¹⁹¹ post-release social supports,¹⁹² experiences in prison,¹⁹³ and factors associated with post-release supervision.¹⁹⁴

At present there is no substantial evidence that a criminogenic effect exists in the aggregate. Indeed, in the peer-reviewed literature, there is somewhat more evidence of a deterrent effect from longer sentences.

The literature on the impact of incarceration on recidivism is admittedly limited by important methodological considerations and inconsistencies across studies. Perhaps the most important implication from the research is best summarized by Mears, Cochran, and Cullen:¹⁹⁵

“We argue that a better understanding of the heterogeneity of incarceration—
including the types and sequences of sanctions and experiences that occur before,

during, and after imprisonment—and of incarceration effects among different groups is important for two reasons. First, it can assist with assessing the salience of prior research on the effects of incarceration on recidivism. Second, it serves to identify conceptual and methodological challenges that must be addressed to provide credible assessments of incarceration effects.... [I]ncarceration likely exerts a variable effect depending on the nature of the prison experience...including prior sanction history, and the specific populations subject to imprisonment.”

Considering the research on incarceration and recidivism, there is evidence suggesting that certain punishments may effectively deter crime, though the methodologies used to evaluate these effects vary. So far, the research appears mixed, with no studies finding a large aggregate-level criminogenic effect associated with longer sentences. This review demonstrates why true evidence-based practice should involve a critical examination of the breadth and depth of the existing empirical research rather than “cherry-picking” results from a study or two. The policy relevance of a study varies widely based on context, and policymakers would benefit from considering the totality of findings across studies and the various contexts to which they apply before enacting rapid policy change.

Appendix A. Table 1.

Citation	Design	Sample Characteristics	Recidivism Measure	Results	Limitations
Berecochea et al. (1981)	Random assignment to two groups; those who had their sentence lengths reduced by 6 months and those who did not Groups were statistically similar; no statistical adjustments needed	Parolees convicted of various violent and non-violent offenses Average time served = 34.6 months	Re-incarceration rates Follow-up: 1, 2, and 3 years	Non-significant deterrent effect of lengthier sentences at all follow-ups	May not be generalizable to people serving >35 months
Deschenes, Turner, and Petersilia (1995a)	Random assignment to a diversion program (ICS) vs. prison. Groups were statistically similar; no statistical adjustments needed	Theft/burglary offenders and probation violators facing sentences ≤20 months Average time served = 108 days for ICS and 220 days for incarceration	Re-arrest rates Proportion of people re-incarcerated Follow-ups: 6 months, 1 year, and 2 years	Significant deterrent effect of lengthier sentence at all follow-ups, though the finding is confounded with treatment group status The prison control group had significantly fewer technical violations than the ICS group. However, this is likely a byproduct of custodial sentences vs. non-custodial	The findings are irrelevant for our conclusions. While the prison control group had fewer technical violations than the ICS group, this finding is confounded with the fact that people cannot receive technical violations while in prison.
Deschenes, Turner, and Petersilia (1995b)	Random assignment to an early release diversion program (ISR) vs. prison. Groups were statistically similar; no statistical adjustments needed	Theft/burglary offenders and probation violators facing sentences ≤20 months Average time served = 44 days for both groups	Re-arrest rates Proportion of people re-incarcerated Follow-ups: 6 months and 1 year	Null effects at all follow-ups Groups did not differ on average re: 'time served' variable and authors did not break down comparisons across adjacent sentence length categories	The findings are irrelevant for our conclusions There is no variation in average time served, and authors do not break down information on adjacent categories

<p>Jaman, Dickover, & Bennett (1972)</p>	<p>Quasi experiment matched-pairs design comparing recidivists with non-recidivists</p> <p>Groups were statistically similar; no statistical adjustments needed</p>	<p>Robbery and burglary offenders</p> <p>Average time served = 27 months</p>	<p>Re-incarceration rates</p> <p>Follow-ups: 6 months, 1 year, and 2 years</p>	<p>Non-significant criminogenic effect at 6 months among robbers</p> <p>Significant criminogenic effect at years 1 and 2 among robbers</p> <p>Non-significant criminogenic effect at 6 months and 1 year among burglars</p> <p>Significant criminogenic effect at 2 years among burglars</p>	<p>May not be generalizable beyond burglary/robbery offenders or those serving sentences >3 years</p> <p>Possibility that naturally occurring groups differ on unobserved or unmeasured factors</p>
<p>Kraus (1974)</p>	<p>Quasi-experiment matched-pairs design comparing probationers with non-probationers</p> <p>Groups were not statistically similar; remedied with statistical adjustments</p>	<p>Juvenile offenders convicted mostly of theft or burglary</p> <p>Most had previously served <2 years in confinement</p>	<p>Re-conviction/re-institutionalization rates</p> <p>Follow-up: 5 years</p>	<p>Mixed effects conditional on offense type</p> <p>Null effects for motor vehicle theft, assault, and sexual offenses.</p> <p>Significant criminogenic effects for stealing and burglary offenses</p> <p>Significant criminogenic effects for first-time offenders</p>	<p>An identified confound weakens the study design, rendering findings irrelevant. With the identified confound, the study is akin to comparing offense types.</p> <p>The only variation in 'time served' results from different offenses receiving different penalties. In other words, 'time served' is confounded with offense type, so the effects of each cannot be disentangled.</p>

Loughran et al. (2009)	<p>Quasi-experiment comparing recidivists with non-recidivists</p> <p>Groups were not statistically similar; remedied slightly but not completely with propensity score matching</p>	<p>Serious felony-level juvenile offenders</p> <p>All were serving sentences ≤ 15 months</p>	<p>Re-arrest rates</p> <p>Self-reported re-offending</p> <p>Follow-ups: every 6 months for 3 years, then one follow-up in the 4th year</p>	<p>Mixed effects at all follow-ups</p> <p>Significant criminogenic effect for people serving < 3 months</p> <p>Null effect for people serving 3-13 months</p> <p>Significant deterrent effect for people serving ≥ 13 months</p>	<p>May not be generalizable to adults or those serving > 15 months</p> <p>Criminogenic and deterrent effects should be interpreted with caution, as they contained large degrees of error due to small sample sizes</p> <p>Propensity score matching was not entirely successful and groups were not entirely similar</p>
Meade et al. (2013)	<p>Quasi-experiment comparing recidivists with non-recidivists</p> <p>Groups were not statistically similar; remedied with propensity score matching and statistical adjustments</p>	<p>Felony-level drug and property offenders</p> <p>Average time served = 2 years</p>	<p>Felony re-arrest rates</p> <p>Follow-up: 1 year</p>	<p>Mixed effects</p> <p>Significant criminogenic effect associated with shorter sentences, up until the point of 2 years</p> <p>Significant deterrent effect emerged once time served exceeded 2 years</p> <p>Deterrent effects were strongest for those serving five years or more</p>	<p>May not be generalizable beyond property and drug offenders or those serving > 2 years</p> <p>Initial criminogenic effects followed by deterrent effects suggests that the relationship may be curvilinear (rather than linear). This has important implications for research.</p>

Mears et al. (2016)	<p>Quasi-experiment comparing recidivists with non-recidivists</p> <p>Groups were not statistically similar; remedied with propensity score matching</p>	<p>Various violent and nonviolent offenders</p> <p>Average time served = 2 years</p>	<p>Felony re-conviction rates</p> <p>Follow-ups: 1 year, 2 years, and 3 years</p>	<p>Mixed effects at 1- and 2-year follow-ups</p> <p>Significant criminogenic effect for people serving less than one year at one-year follow-up</p> <p>Significant deterrent effect for people serving 1-2 years at 1 and 2-year follow-ups</p> <p>Null effect for people serving more than 2 years</p>	<p>May not be generalizable to those serving >2 years</p> <p>Initial criminogenic effects followed by deterrent effects suggests that the relationship may be curvilinear (rather than linear). This has important implications for research.</p>
Green & Winik (2010)	<p>Random assignment to harsh vs. lenient judges</p> <p>Groups were statistically similar; statistical adjustments also included</p>	<p>Felony-level drug offenders</p> <p>Average sentence = 1 year</p>	<p>Re-arrest rates for any misdemeanor or felony</p> <p>Follow-up: 4 years</p>	<p>Significant deterrent effect</p> <p>On average, each additional month of incarceration significantly lowered the probability of recidivism</p> <p>There was an initial non-significant criminogenic effect, but it disappeared when statistical adjustments were included</p>	<p>May not be generalizable to non-drug offenders or those serving >1 year</p>

<p>Roodman (2017a)</p> <p><i>Note: Roodman re-examined Green and Winik's (2010) earlier data</i></p>	<p>Random assignment to harsh vs. lenient judges</p> <p>Groups were not statistically similar; remedied with statistical adjustments</p>	<p>Felony-level drug offenders</p> <p>Average sentence = 1 year</p>	<p>General re-arrest rates (misdemeanor or felony arrest or re-conviction)</p> <p>Felony re-arrest rates</p> <p>Felony re-conviction rates</p> <p>Follow-up: 4 years</p>	<p>Mixed effects conditional on recidivism measure</p> <p>Significant deterrent effect on felony re-conviction rates</p> <p>Significant criminogenic effect on general re-arrest rates (i.e., re-arrest or re-conviction)</p> <p>Significant deterrent effect related to participation in rehabilitative programs, such as job training.</p>	<p>May not be generalizable to non-drug offenders or those serving >1 year</p>
<p>Snodgrass et al. (2011)</p>	<p>Quasi experiment comparing low dose incarceration with high dose incarceration categories</p> <p>Groups were not statistically similar; remedied with propensity scores and statistical adjustments</p>	<p>Felony-level violent, drug, or property offenders</p> <p>Average sentence = 6.7 months</p>	<p>Felony re-conviction rates</p> <p>Probability of re-conviction</p> <p>Follow-up: 3 years</p>	<p>Non-significant deterrent effect</p> <p>People serving longer periods of incarceration faced fewer felony re-convictions per year after release (non-significant)</p>	<p>May not be generalizable to people serving >6.7 months</p>
<p>Cotter (2020a)</p>	<p>Quasi-experiment matched-pairs design comparing recidivists with non-recidivists</p> <p>Groups were statistically similar to each other; no statistical adjustments needed</p>	<p>Various violent and nonviolent offenders</p> <p>Time served ranged from 6 months to 10 years</p>	<p>Re-arrest rates (including technical violations)</p> <p>Follow-up: 8 years</p>	<p>Mixed effects</p> <p>Non-significant criminogenic effect for terms of 2-3 years</p> <p>Null effect for people serving 3-5 years</p>	<p>May not be generalizable to people serving >10 years</p> <p>Initial criminogenic effects followed by deterrent effects suggests that the relationship may be curvilinear (rather than linear). This has important implications for research.</p>

				Significant deterrent effect for terms of 5 years or more	
Cotter (2020b)	<p>Quasi-experiment matched-pairs design (with weighting) comparing recidivists with non-recidivists</p> <p>Groups were not statistically similar to each other; remedied with statistical adjustments</p>	<p>Various violent and nonviolent offenders</p> <p>Time served ranged from 6 months to 10 years</p>	<p>Re-arrest rates (including technical violations)</p> <p>Follow-up: 8 years</p>	<p>Mixed effects</p> <p>Non-significant criminogenic effect for terms of 2-3 years</p> <p>Null effect for people serving 5 years or less</p> <p>Significant deterrent effect for terms of 5-10 years and terms of more than 10 years</p>	<p>May not be generalizable to people serving >10 years</p> <p>Initial criminogenic effects followed by deterrent effects suggests that the relationship may be curvilinear (rather than linear). This has important implications for research.</p>
Rhodes et al. (2018)	<p>Quasi-experiment comparing recidivists with non-recidivists</p> <p>Groups were not statistically similar; remedied with statistical adjustments</p>	<p>Various violent and non-violent offenders convicted of serious felonies or misdemeanors</p> <p>Average time served across criminal history and offense seriousness categories ranged from 2 weeks to 18.6 years</p>	<p>Re-incarceration rates</p> <p>Follow-up: 3 years</p>	<p>Significant deterrent effect</p> <p>On average, recidivism decreased significantly for every 7.5 month increase in sentence length</p>	<p>Incarceration length varies widely among the sample and is also examined in the aggregate only. Thus, it is difficult to compare adjacent 'time served' categories</p>

<p>Roach & Schanzenbach (2015)</p>	<p>Random assignment to harsh vs. lenient judges</p> <p>Groups were not statistically similar; remedied with statistical adjustments</p>	<p>Lower-level felony offenders</p> <p>Average sentence = 9 months</p>	<p>Re-sentence for any new felony offense</p> <p>Follow-ups: 1 year, 2 years, and 3 years</p>	<p>Significant deterrent effect at all follow-ups</p> <p>On average, re-sentencing rates decreased significantly for each additional month served</p> <p>Most recidivism effects occurred within the first year</p>	<p>May not be generalizable to high-level felony offenders or those serving >9 months</p>
<p>Kuziemko (2012)</p>	<p>Quasi-experiment comparing recidivists with non-recidivists for a full sample and one subgroup</p> <p>Groups were not statistically similar; remedied with statistical adjustments</p>	<p>Non-violent prisoners</p> <p>Average time served (full sample) = 33 months</p> <p>Average time served (subgroup) = 13 months</p>	<p>Return to prison for a new crime (i.e., not a parole violation)</p> <p>Follow-up: 3 years</p>	<p>Non-significant deterrent effect for the full sample</p> <p>Significant deterrent effect for subgroup</p>	<p>The sample may not be generalizable to violent offenders</p> <p>The full sample may not be generalizable to offenders serving >33 months.</p> <p>The subgroup may not be generalizable to offenders serving >13 months.</p>
<p>Roodman (2017b)</p> <p><i>Note: Roodman re-examined of Kuziemko's (2012) data</i></p>	<p>Quasi-experiment comparing recidivists with non-recidivists for a full sample and one subgroup</p> <p>Groups were not statistically similar; remedied with statistical adjustments</p> <p>Roodman (2017) added additional model specifications, outcome measures, and predictor variables</p>	<p>Non-violent prisoners</p> <p>Average time served (full sample) = 33 months</p> <p>Average time served (subgroup) = 13 months</p>	<p>Re-incarceration rates (excluding parole violations)</p> <p>Total recidivism rates, measured by re-conviction and re-incarceration for serious crimes (excluding parole violations)</p> <p>Follow-up: 3 years</p>	<p>Non-significant deterrent effect of lengthier sentences for the full sample</p> <p>Mixed effects for the subgroup, depending on recidivism measure</p> <p>Significant but small deterrent effect of lengthier sentences on re-incarceration rates among the subgroup</p> <p>Significant but small criminogenic effect of lengthier sentences on</p>	<p>The sample may not be generalizable to violent offenders</p> <p>The full sample may not be generalizable to offenders serving >33 months.</p> <p>The subgroup may not be generalizable to offenders serving >13 months.</p>

				total recidivism rates among the subgroup	
Bhuller et al. (2016)	Random assignment to harsh vs. lenient judges Offenders were not statistically similar; remedied with statistical adjustments	Various violent and non-violent offenders in Norway Average time served = 6 months	New criminal charges (rates) Follow-ups: 2 years and 5 years	Significant but small deterrent effect of lengthier sentences on new criminal charges at 2- and 5-year follow-ups	The sample may not be generalizable to the United States or those serving >6 months Binary incarceration exposure was highly correlated with time served, so the true effect of time served is less clear
Walker & Herting (2020)	Quasi-experiment matched-pairs design comparing people who were detained pretrial vs. those who were not Offenders were not statistically similar; remedied with statistical adjustments	Juvenile offenders Average time served = 2 days	Court filing for a new misdemeanor (rate) Court filing for a new felony (rates) Follow-up: 1 year	Significant but small criminogenic effect of lengthier sentences (due to being detained pretrial) on felony court filings Every additional day of incarceration was associated with an approximate 1% increase in felony recidivism	An identified confound weakens the study design, rendering findings irrelevant. With the identified confound, the study is akin to comparing pretrial detainment vs. not detained. The sample may not be generalizable to adult offenders or those serving >1 year The 'time served' variable is heavily skewed, such that the majority of the sample served one week or less
Hjalmarsson & Lindquist (2020)	Quasi-experiment comparing people serving one-half of their sentence vs. people serving two-thirds of their sentence	Various violent and non-violent offenders in Sweden	One new conviction (rates) Re-incarceration rates	Significant but small deterrent effect of lengthier sentences on one new conviction at 1-year follow-up	The sample may not be generalizable to the United States or to those serving >1 year

	Offenders were not statistically similar; remedied with statistical adjustments	Average time served = 11.7 months	Follow-ups: 1 year, 2 years, and 3 years	Significant but small deterrent effect of lengthier sentences on re-incarceration at 1- and 2- year follow-ups	
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Endnotes

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⁷¹ *Id.*

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Does sentence length affect the risk for criminal recidivism? A quasi-experimental study of three policy reforms in Sweden

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Abstract

Objectives This study examines the relationship between incarceration time and post-release recidivism among first-time incarcerated adult offenders.

Methods A quasi-experimental design was adopted consisting of three policy reforms that were treated as separate natural experiments. While holding imposed sentence length constant, these policy reforms either decreased or increased the required share of a sentence inmates needed to be incarcerated before being eligible for parole. Data consisted of large-scale administrative records containing all convictions for the Swedish cohorts born in 1958 and later.

Results Results indicate that neither increased nor decreased incarceration time had a statistically significant effect on post-release recidivism, irrespective of how recidivism was measured.

Conclusions Findings reveal little evidence for incarceration time having a criminogenic or specific preventive effect on post-release recidivism.

Keywords Incarceration length · Recidivism · Parole · Quasi-experiment

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Introduction

Sweden has long been perceived as being moderate in terms of penal attitudes, but over recent decades “tough on crime” policies have challenged this “Scandinavian exceptionalism” (Pratt, 2008; von Hofer & Tham, 2013). The themes that are currently prominent on the political agenda in Sweden indicate that a Swedish version of the “punitive turn” has emerged, which also has been expressed in multiple Swedish Government Official Reports (SOU), with examples including proposals for longer sentences for serious offenses and recidivism (SOU, 2021a; SOU, 2021b), harsher penalties for young adults (SOU, 2018), and the abolition or severe restriction of early release from prison (SOU, 2017). Consequently, the Swedish Prison and Probation Service predict a 40% expansion in prison capacity by 2030 (Kriminalvården, 2021b). This increasing trust among policymakers in the ability of incarceration and longer prison sentences to reduce crime has refocused the spotlight on the longstanding theoretical discussion on the criminogenic versus deterrent effects of prison.

Although the literature on the consequences of imprisonment is extensive, most research on the effects of incarceration on recidivism has analyzed the dichotomy of incarceration versus non-custodial sanctions (for systematic reviews on the effects of incarceration, see, e.g., Villettaz et al., 2015; Petrich et al. 2021). Research that explicitly addresses the effects of the length of incarceration is more limited, and those studies that do exist show inconsistent findings, making it difficult to draw any overall conclusions (Berger & Scheidegger, 2021; Nagin et al., 2009). A considerable amount of the research on the effects of incarceration times has been conducted in a US context, where sentencing lengths are at the higher end of the spectrum, which has subsequently led scholars to stress the importance of research based on European data (Durlauf & Nagin, 2011).

In this article, we analyze how the length of imprisonment affects recidivism for individuals who are incarcerated for the first time. We utilize three Swedish legislation reforms that may be treated as natural experiments. The reforms in question are the 1983, 1993, and 1999 parole reforms, which all either decreased or increased the amount of time an inmate was required to serve in prison prior to release on parole. The analyses are based on large-scale administrative data containing all convictions for the Swedish cohorts born in 1958 and later.

Theoretical background: from deterrent to criminogenic effects

Theories of punishment in general, and of imprisonment in particular, can be separated into two very different categories, from which two opposing hypotheses can be generated: one maintains that prison has crime deterrent effects, while the other predicts that prison has criminogenic effects.

Deterrence theory focuses on two main types of deterrence: *general* and *specific* deterrence, with incarceration playing a crucial role in relation to both

(Nagin, 1978). General deterrence can be defined as the crime preventive effect in the general public from the threat of a criminal sanction. Specific deterrence, on the other hand, focuses on the deterrent effect on the individual who experiences a sanction. Since the present study analyzes the expected deterrent effect of having experienced varying increases or decreases in sentence lengths, our focus is solely on specific deterrence. Within specific deterrence, three mechanisms can explain how incarceration may prevent recidivism. First, isolating an individual interrupts a criminal career, creating an *incapacitation effect* (Zimring & Hawkins, 1995). For an individual, however, the size of this effect decreases with time as a result of the well-known curvilinear relationship between age and crime (Hirschi & Gottfredson, 1983). Second, the experience of enduring a prison sanction might itself have a deterrent effect, preventing post-release recidivism due to concerns about being reincarcerated (Nagin et al., 2009; von Hirsch et al., 1999). Third, various correctional intervention programs may *rehabilitate* inmates and serve to inhibit an individual's criminal tendencies (Lipsey & Cullen, 2007). Many of these interventions require sentence lengths to be sufficiently long, and increasing sentence lengths may enable more inmates to participate in rehabilitative interventions (see for example Bhuller et al., 2020; Hjalmarsson & Lindquist, 2020). Since this study does not aim to estimate incapacitation effects, the mechanisms that may be present are rehabilitation and deterrence (for empirical evidence regarding incapacitation effects, see Miles & Ludwig, 2007; Piquero & Blumstein, 2007; Wermink et al., 2013). It should be noted, however, that we do not have access to data that would allow us to distinguish rehabilitative from deterrent effects.

Another set of theories suggests that a prison sentence may have (unintended) criminogenic effects. One strand of these theories describes how prisons are “schools of crime” where criminal skills are exchanged and learned within close-knit groups of individuals (see Bayer et al., 2009; Nygaard Andersen, 2019; Roxell, 2016). Adapting to prison conditions may therefore involve a normative and collective process among inmates who become socialized into embracing deviant attitudes. Inmates who are imprisoned for longer periods may potentially become even more entrenched in their criminogenic attitudes and exposed to wider anti-social networks.

Being subjected to prison may also result in a societal reaction in the form of labeling and stigmatization (Becker, 1963; Braithwaite, 1989). The mechanisms behind possible criminogenic effects of this type of labeling are twofold: First, treating and labeling an individual as a “criminal” has consequences for the self-image of the offender, who risks internalizing the criminal identity and subsequently acting in ways that are in line with this identity. Increasing sentence lengths could, accordingly, result in an even stronger internalization of the criminal identity, thus affecting post-release recidivism risks (see Harris, 1975). Furthermore, it has been suggested that an individual is more susceptible to labeling and stigmatization at the beginning of a criminal career and that the first experience of incarceration is more likely to result in such consequences than reincarceration (Motz et al., 2020; Walters, 2003). Second, society's collective discomfort with offenders may limit work opportunities for former inmates (e.g., Apel & Sweeten, 2010; Bäckman et al., 2018). Extensive

periods of incarceration could exacerbate this effect, as ties to conventional society are further diminished, which may result in weaker social bonds both to individuals (e.g., family, friends, co-workers) and institutions (e.g., workplaces and organizations) that could otherwise potentially prevent the individual from recidivating (Sampson & Laub, 1997).

Lastly, there is a third strand of literature that argues that incarceration length has a minimal effect on offenders' post-release recidivism risks (Gendreau et al., 1999). In this line of research, recidivism risks are instead explained by various background characteristics and pre-incarceration risk factors. Because inmates are a highly selected group characterized by addiction problems and resource deficiencies in areas such as education, employment, and health (Nilsson, 2003), deterrent interventions may therefore not have the desired effect on recidivism (Bäckman et al., 2018).

The relationship between incarceration length and recidivism

In this section, we review the literature on the effects of incarceration length and pay particular attention to more recent quasi-experimental studies. Broadly, these studies utilize natural experiments or various matching designs to identify the relationship between incarceration length on recidivism.

A systematic review by Nagin et al. (2009) has played an important role regarding the approaches employed in more recent studies estimating the effects of incarceration length. In this review, the authors concluded that the bulk of the pre-existing literature suffered from serious methodological shortcomings. Large parts of this literature, which had been dominated by regression-based studies, suffered from issues regarding selection bias and limitations regarding the interpretation of causality. Moreover, the outcomes from these studies were remarkably heterogeneous and the authors, therefore, refrained from drawing any overall conclusions regarding the then available research. Following this review, and in line with a general discussion regarding the "causal revolution" (Sampson et al., 2013), increasing focus has been directed at the use of quasi-experimental evidence in discussions of the causal impact of incarceration length.

The first of more recent quasi-experimental studies on the effects of incarceration length was conducted by Loughran et al. (2009), who used propensity score matching to enable comparisons between juvenile offenders from two US counties who had been sentenced to varying prison terms. The results indicated no effect of incarceration length on either re-arrest rates or self-reported offending during a 2-year follow-up. Snodgrass et al. (2011) used propensity score matching with data on Dutch offenders between 12 and 40 years of age at sentencing. The authors found little evidence of a relationship between the length of prison stays and 3-year reconviction rates. Also utilizing the propensity score methodology on Dutch data, but only for adult offenders, Wermink et al. (2018) studied the short-term effects (6-month follow-up period) of sentences that were on average 4.1 months in length, and found no effects on reoffending, reconviction, or reincarceration. Using data on individuals released from Florida prisons, and using matching techniques, Mears et al. (2016)

found that longer periods in prison were initially associated with a greater risk for reconviction, but that these effects disappeared approximately 2 years after release, underscoring the importance of longer follow-up periods, since effects may dissipate over time.

Meade et al. (2013) utilized propensity score matching with data on individuals released under post-release supervision in Ohio and compared offenders that were differentiated in terms of sentence length and found no effect on the odds for rearrest during a 1-year follow-up period for offenders who had served less than 5 years. Although the authors found a small effect for offenders who had been sentenced to 5 years or more, the mechanism behind this effect was unclear, and the authors discuss that it might be due to maturation. Using parametric survival models on a dataset comprising inmates from several US states, Rydberg and Clark (2016) replicated parts of the study by Meade and colleagues. For those serving long sentences, exceeding 4 years, they found that increasing incarceration lengths reduced reconviction risks. Increased incarceration length was, however, associated with an increased risk for reincarceration due to technical violations. Since their results were heterogeneous with respect to the type of recidivism measure and crime type, the authors refrained from drawing any firm conclusions. Roach and Schanzenbach (2015) made use of the randomization of offenders to judges that occurs within a courthouse in Seattle. A two-stage least square regression analysis revealed that for each additional month incarcerated, reconviction rates decreased by 1%. This estimate was robust to the length of follow-up periods (1, 2, or 3 years). On average, the sentence lengths among inmates were relatively short (median 3 months), however, and the study was limited to offenders who had entered a guilty plea, which may have introduced some selection bias. Rhodes et al. (2018) exploited the quasi-experimental setting created by the US Sentencing Guidelines and employed an instrumental variable approach, finding that an average increase of 7.5 months in the length of incarceration reduced the 3-year reincarceration rate from 20% to approximately 19%. Because of this small impact, the authors concluded that small reductions in average incarceration lengths are possible with only minimal effects on recidivism.

Tollenaar et al. (2014) analyzed a Dutch policy reform that increased the length of incarceration for high-frequency offenders. Utilizing propensity score matching, the authors found that increasing sentence lengths for highly active offenders reduced 2-year reconviction rates by between 12 and 16%. Because offenders who had been subjected to the reform were a very problematic group characterized by addiction, unemployment, and mental health problems, the authors argued that correctional rehabilitation interventions might be a potential mechanism underlying the observed effects.

In the Scandinavian context, we are only aware of one study on the effect of incarceration length on crime (Hjalmarsson & Lindquist, 2020). The study's primary focus was not, however, directed at recidivism, but rather at health outcomes. Hjalmarsson and Lindquist's paper is of particular relevance for the present study since they too exploited the 1993 and 1999 parole reforms in Sweden. Their analyses revealed health-promotive effects of an increase in incarceration length. Regarding recidivism, the authors found that the increase in the length of incarceration produced by the reforms on average decreased recidivism. These effects were strongest

for reincarceration within 12 months, which decreased by 2.9 percentage points, and for the prevalence of two or more reconvictions within 36 months, which decreased by 2.5 percentage points.¹

A striking conclusion with regard to the more recent quasi-experimental studies is that although research designs have improved, the overall evidence remains somewhat unclear, with some studies yielding null effects and others pointing to the existence of a minor deterrent effect. Further, there are also issues regarding the generalizability in recent studies. Not only have few studies been conducted outside the USA, but the findings are also often based on specific populations of offenders, such as juveniles or inmates sentenced within a specific court system. Furthermore, quasi-experimental approaches often estimate local treatment effects at the threshold. Utilizing local estimates does increase the possibility of identifying a causal relationship, but it may also result in generalization difficulties because offenders at the threshold constitute a specific offender population. These limitations highlight the need for further studies on the effects of incarceration length across a range of contexts and groups of offenders.

The Swedish parole institution and the background to the natural experiments

In this study, we utilize three distinct reforms from 1983, 1993, and 1999, which changed the legislation concerning the required share of a sentence inmates needed to be incarcerated before being eligible for parole. Prior to 1983, inmates were eligible for discretionary parole, with the law requiring an inmate to have served two-thirds of the sentence prior to parole, although under special circumstances parole could be granted after half the sentence. In practice, discretionary parole was based on sentence length. Long-term inmates who had been sentenced to more than 24 months were eligible for parole after half their sentence. Although inmates serving between 2 and 24 months could be eligible for parole after half their prison sentence, hardly anyone serving a sentence of 2 to 12 months was released after half the sentence, but rather after two-thirds (SOU, 1981).² Parole was granted at some point between half and two-thirds of the sentence for inmates serving 13 to 24 months.

On July 1, 1983, the “half-time reform” was enacted, which replaced discretionary parole and introduced mandatory release on parole after serving half the prison sentence for inmates who had been sentenced to 4 months or more (proposition 1982/83:85). This affected all individuals who were either already incarcerated on

¹ Although both our own and Hjalmarsson’s and Lindqvist’s study employ Swedish parole reforms to study the effects of variations in the length of incarceration, the studies differ in several respects. Besides the fact that our study focuses on recidivism, we (i) utilize natural experiments that both decreased and increased incarceration time, (ii) have access to individuals’ full criminal record histories, (iii) evaluate the long-term effect of incarceration length, and (iv) direct particular attention at individuals receiving a prison sentence for the first time. See the section on data, sample, and measures for further information.

² Inmates were required to spend at least 2 months in prison, which means that inmates sentenced to less than 3 months often spent more than two-thirds of their sentence incarcerated.

the date of the reform's introduction, or who were incarcerated thereafter. In addition to a decrease in the required incarceration time prior to parole, the length of the period of post-release supervision was also reduced, from 1–3 years to 1 year. One important rationale for the implementation of mandatory parole was the legal uncertainty created by the discretionary parole system that had been in place prior to 1983.

The reform became the subject of substantial criticism and was repealed on July 1, 1993 (proposition 1992/93:4). All inmates sentenced to between 4 and 23 months were subsequently required to serve two-thirds of their prison sentence prior to release on parole. For long-term inmates serving 24 months or more, parole was still granted after half the prison sentence. In order to avoid threshold effects in the implementation of a system involving parole after both half and two-thirds of the sentence, a graduation scale was implemented for sentences of 13 to 23 months (proposition 1992/93:4).³

On January 1, 1999, the parole legislation was changed once again (proposition 1997/98:96). Long-term inmates serving 24 months or more were subsequently subject to the same parole rules as short-term inmates. All those serving sentences of more than 1 month were now eligible for parole after two-thirds of their sentence. Unlike the 1983 reform, the 1993 and 1999 parole legislations were applied to offenders based on whether the date of their conviction came before or after the date of the reforms' introduction, and the extent of post-release supervision was, likewise, not changed by either the 1993 or 1999 reform and was set at 12 months regardless of the sentence length.

Figure 1 illustrates the expected change in required incarceration time before parole. For example, individuals sentenced to 12 months in prison for crimes committed after the 1993 reform are required to spend 2 additional months in prison (8 months in total) in comparison to individuals who are sentenced to 12 months during the half-time reform (6 months in total). Note that their imposed sentence lengths of 12 months are identical. The only difference is the amount of required incarceration time before parole.

The fact that parole has sometimes been discretionary and sometimes mandatory introduces some uncertainty with respect to how the reforms actually played out in terms of required prison time before parole. Using prison data, Hjalmarsson and Lindquist (2020) were, however, able to show that the 1993 and 1999 reforms both were implemented as intended and that required incarceration time before parole was increased from half-time to two-thirds.

The parole reforms of 1983, 1993, and 1999 are here treated as natural experiments in which the post-reform group has been “treated” with an either increase or decrease in the length of incarceration, while the pre-reform group acts as a control group. The study design is presented in Fig. 2 and is described further in our data, sample, and measures section.

³ The scale employed was 8 months plus one-third of the sentence length that exceeded 12 months. For example, an inmate sentenced to 15 months was released on parole after 8 months plus 1 month.

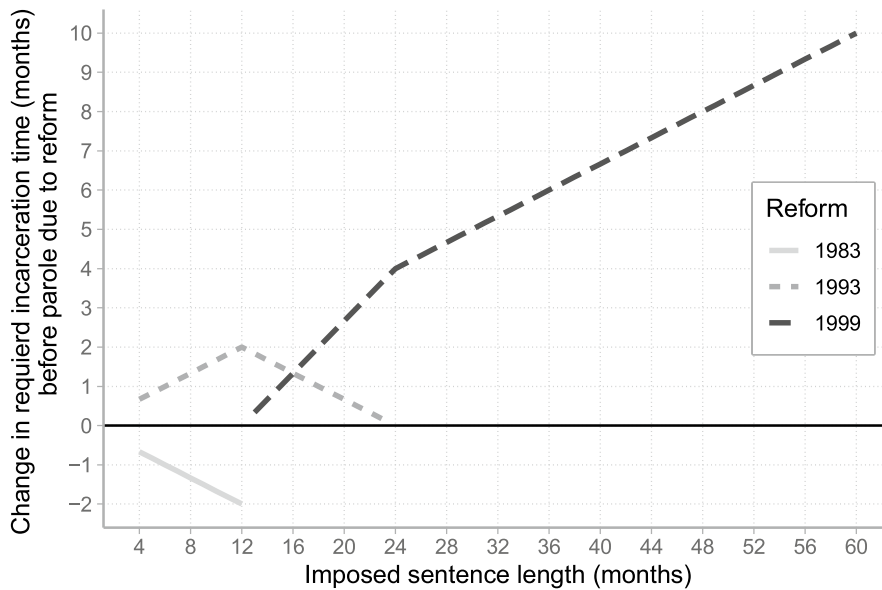


Fig. 1 The 1983, 1993, and 1999 parole reforms' effect on required time served before being eligible for parole. Notes: each reform effect should be read as the change (in months) when going from prior parole law. On July 1, 1983, parole laws were changed so that offenders sentenced to 2–24 months in prison were required to serve at least half the sentence before being eligible for parole, from previously two-thirds of the sentence. Parole laws were then again changed on July 1, 1993, requiring offenders sentenced to 4–23 months in prison to serve two-thirds of the sentence. On January 1, 1999, a reform was enacted that targeted inmates sentenced to more than 24 months in prison that required them to serve two-thirds of the sentence before being eligible for parole, from previously half of the sentence

Data, samples, and measures

Data

The study data were drawn from the convictions register maintained by the Swedish National Council for Crime Prevention (BRÅ), and cover the period 1973 to 2017.⁴ This means that we have the entire conviction histories of all cohorts born after 1957 (the age of criminal responsibility is 15 in Sweden). From this register, we have extracted data on the date of the offense, the date of conviction, offense type, sanction type, and sentence length (in days). In order to censor the dataset, we have also collected data on dates of death and emigration

⁴ This study was preregistered at Open Science. See <https://osf.io/br875>. The linking of the various registers and the anonymization of the dataset have been carried out by Statistics Sweden (SCB). The dataset is stored on, and has been analyzed via, the system used by Statistics Sweden to make microdata available for online research (MONA). Due to the Swedish Public Access to Information and Secrecy Act, microdata cannot be made publicly available.

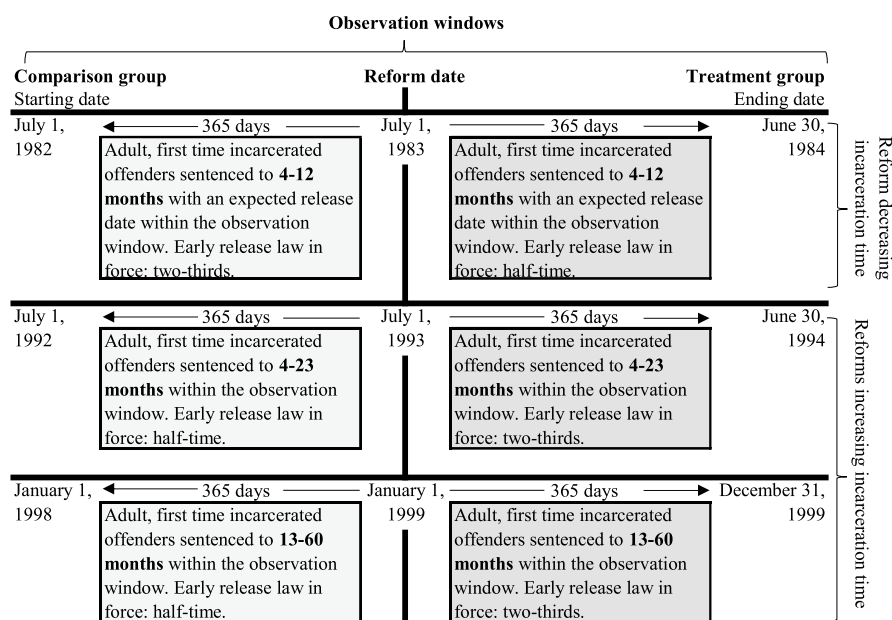


Fig. 2 Inclusion criteria and study design relative to the 1983, 1993, and 1999 parole reforms

from the Total Population Register (RTB) maintained by Statistics Sweden (SCB).⁵

Our attention is directed at offenders who have been imprisoned for the first time. For these individuals, who tend to be younger, incarceration may be more consequential than it is for individuals with multiple experiences of imprisonment (Bačák et al., 2019; Nieuwbeerta et al., 2009). Focusing on first-time incarcerated individuals also enables us to circumvent the feedback effect between imprisonment and crime. Because we are interested in isolating the effect of incarceration length, excluding the potential influence of previous exposure to incarceration is beneficial.

Analytic samples

Three different reform samples were created (see Fig. 2), one for each reform year. The reform samples were selected on the basis of five inclusion criteria. First, for the 1993 and 1999 reforms, individuals *convicted* within 12 months after the reform (July 1, 1993, and January 1, 1999) were assigned to the treatment group, while individuals sentenced during the 12 months prior to the reform were assigned to the control group. Because the 1983 reform affected all individuals

⁵ We are able to link data from these registers by using the Swedish personal identification number, which is based on the individual's date of birth and an additional four-digit identification number. Statistics Sweden produces an anonymized pseudo-key to avoid the identification of specific individuals.

who were incarcerated on the day the reform was introduced — which was not the case for the subsequent reforms — the inclusion criteria are slightly different for the 1983 reform sample. Here the treatment group consists of individuals who had an expected *release date* within 12 months after the reform (July 1, 1983), while the control group comprises individuals who had an expected release date during the 12 months prior to the reform. Since our data consist of offenders in the cohorts born after 1957, our design entails certain age restrictions. This is most apparent for the 1983 reform, where the maximum age is restricted to 25 years old (i.e., those born in 1958). Second, because of the way in which the 1983 reform was implemented, offenders who were convicted prior to the reform but who had a release date subsequent to the reform were released after serving somewhere between half and two-thirds of their sentence. We exclude these offenders due to the risk for measurement error in their exposure to treatment. Third, each reform impacted specific sentence lengths in a way that varied from reform to reform, and we have therefore only included the following sentence lengths: The 1983 reform allows for estimates of the effect of a decrease in incarceration time (on average, an expected 36-day decrease) for sentences of 4 to 12 months ($n = 654$); the 1993 reform allows for estimates of the effect of an increase in incarceration time (on average, an expected 36-day increase) for sentences of 4 to 23 months ($n = 1,688$); the 1999 reform allows for estimates of the effect of an increase in incarceration time (on average, an expected 115-day increase) for sentences of between 12 and 60 months ($n = 637$). The upper limit of 60 months has been imposed because there are too few observations above this threshold. Fourth, the offender had no prior prison sentences (but could have a prior conviction history involving non-custodial sanctions). Fifth, the offender was not below the age of 18 at the time of the offense. In Sweden, offenders under the age of 18 are in general not sentenced to prison but rather to institutional care outside the prison system.

Outcome variables

We measure three outcome variables: *reconviction*, *reincarceration*, and *recidivism frequency*, with time at risk starting from the *expected* release date and capped at 10 years. We calculate the expected release date by taking the date on which a conviction comes into force, and adding the length of the imposed prison sentence with parole subtracted. As a result of time spent in pre-trial detention, this will not yield perfect estimates, but there is no reason to expect the potential errors, due to this factor, to vary between treatment and control groups. *Reconviction* measures whether an individual has been reconvicted for a post-release offense, *reincarceration* measures whether an individual has been convicted of an offense that led to a new prison sentence, and *recidivism frequency* is a count variable that measures the total number of offenses included in post-release convictions. Descriptive statistics for all variables are included in Table 1.

Table 1 Comparison between treatment and control group on background characteristics in the three reform samples. *N* = 3002

	1983 reform			1993 reform			1999 reform		
	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.
First prison sanction									
Age at crime	21.19	21.55	0.36**	24.28	24.87	0.6**	25.82	25.80	-0.01
Imposed sentence lengths (days)	214.47	210.28	-4.19	260.38	264.82	4.44	740.05	786.48	46.43
Criminal history up to and including first prison sanction									
Conviction frequency	5.52	5.48	-0.04	4.27	4.20	-0.07	3.68	3.56	-0.12
Crime frequency	23.05	22.32	-0.73	14.98	14.68	-0.30	12.42	12.74	0.33
Age of onset	17.18	17.52	0.34	18.67	18.92	0.25	19.34	19.04	-0.31
Crime mix (frequency)									
Violent	1.35	1.23	-0.12	1.87	2.13	0.27*	1.94	2.58	0.64*
Sex	0.03	0.03	0.00	0.10	0.11	0.01	0.19	0.23	0.04
Property	12.70	11.60	-1.10	6.26	5.76	-0.50	4.41	4.01	-0.40
Fraud	1.18	1.26	0.09	1.35	1.23	-0.12	1.03	0.89	-0.14
Vandalism	1.06	0.78	-0.28	0.63	0.71	0.08	0.53	0.47	-0.06
Traffic	3.93	4.61	0.68	2.31	2.48	0.17	1.69	1.64	-0.05
Narcotics	1.20	1.29	0.09	0.70	0.68	-0.03	1.01	1.48	0.46*
Demographic background									
Female	0.05	0.06	0.01	0.07	0.08	0.01	0.08	0.07	-0.01
Born in Sweden	0.89	0.91	0.02	0.86	0.87	0.01	0.82	0.82	0.00
Censoring									

Table 1 (continued)

	1983 reform			1993 reform			1999 reform		
	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.	Pre-reform (control group) Mean	Post-reform (treatment group) Mean	Diff.
Age at follow-up	49.80	50.51	0.71	46.36	46.32	-0.04	43.33	42.34	-0.99
Deceased in 2017	0.30	0.28	-0.02	0.13	0.11	-0.02	0.09	0.12	0.03
Age at decease	41.97	40.50	-1.47	38.51	37.48	-1.02	34.94	37.82	2.88
N	322	334		884	815		313	334	

Notes: * $p < .05$, ** $p < .01$

Independent variables

A dummy variable has been constructed indicating whether the offender was exposed to a reform or was subject to the parole legislation in force prior to the reform. This variable is used to estimate the reduced-form effect of being “treated” by a parole reform.

The continuous variable measuring imposed sentence length was transformed into a categorized ordinal variable, with sentence lengths being categorized differently for each of the three reforms. The following categories were created to include a sufficient number of offenders in each: for the 1983 reform, 4–5 months ($n = 375$), and 6–12 months ($n = 279$); for the 1993 reform, 4–5 months ($n = 785$), 6–12 months ($n = 645$), and 13–23 months ($n = 258$); for the 1999 reform, 13–23 months ($n = 314$), 24–36 months ($n = 197$), and 37–60 months ($n = 126$).

In the adjusted models, we control for the following criminal justice variables: age and age squared at first conviction, prior conviction frequency, prior crime frequency by offense type (violent, sex, property, fraud, vandalism, traffic, narcotics), number of prison days imposed for the first prison sanction, conviction month, and age at the time of the offense that resulted in the first prison sentence. In addition, we include the following demographic controls: sex and whether or not the offender was born in Sweden.

Analytical strategy

We employ event history analysis to analyze recidivism measured in terms of reconviction or reincarceration, and each reform is treated separately. Event history analysis allows for an estimation of the time it takes for a criminal event to occur measured from a given “at-risk” starting point (see, e.g., DeJong, 1997; Sivertsson, 2016). In essence, the length of time between two criminal events is used to estimate the hazard for recidivism, which is assumed to measure the strength of recidivism tendencies (Allison, 2014). This approach handles right-censored data with ease, which is particularly useful when using long follow-up periods. In the current study, we analyze recidivism over a 10-year period. The data are right-censored at the time of emigration, death, or the end of the period of time at risk. We utilize the precision provided by daily information on convictions, and analyze reconviction in a continuous time, where the time to reconviction is calculated as the number of days between the date of expected release from prison and a new conviction.

We estimate non-parametric Kaplan-Meier cumulative probability functions to illustrate “the speed” of recidivism over the follow-up period between the treatment and control groups in a bivariate fashion, and we employ Cox proportional hazard regression to model the association between reform exposure and recidivism. We furthermore employ Cox proportional hazard regression in relation to

different subsets of categorized sentence lengths. This stratification by imposed sentence length enables us both to analyze the groups that were most impacted by the reforms in terms of increases/decreases in incarceration length prior to parole, and also whether incremental adjustments in incarceration length accelerate or decelerate recidivism timing.

Further, we use negative binomial regression to estimate the recidivism frequency.⁶ Negative binomial regression is preferred over Poisson regression for our data structure, since a likelihood-ratio chi-square test indicated that the dependent variable is over-dispersed (Osgood, 2000). In contrast to the Cox-regression analyses, the negative binomial model requires that the offender had been alive and had not emigrated throughout the follow-up period.

Reform implementation and methodological considerations

Because the conviction date and not the date of the offense determined whether an individual was affected by the 1993 and 1999 reforms, the possibility of offender “self-selection” is not a particular concern in relation to the effects of these reforms. This is even less of a concern in relation to the 1983 reform, since in this case the reform was not implemented on the basis of individuals’ offense or conviction dates (see the earlier section on the Swedish parole institution). However, judges might hypothetically hasten or delay a conviction in order to ensure that an offender was convicted on one side of the reform date or the other. Appendix Fig. 5b and e do indeed show that fewer conviction decisions were made during the days following the 1993 and 1999 reforms, which might indicate a preference for allowing offenders to receive half-time parole rather than two-thirds parole. However, when comparing the reform years with the surrounding years, we do not find any deviating patterns (see Appendix Fig. 5). Instead, the sorting that does seem to occur is not because of preference but more likely an annual period effect with fewer convictions during the summer months. One concern regarding our identification strategy is its assumption that the reforms only create variation in the length of incarceration and not in the imposed sentence length. One possible way the reforms might indirectly affect imposed sentence length would be if judges wished to counter the effects of the reforms. For example, in order to minimize the effect of the 1993 or 1999 reforms, judges might use their discretion and sentence offenders convicted after these reforms to shorter sentences. We have plotted the distribution of the length of imposed prison sentences separately for the treatment and control groups for all 3 reform years and see no evidence of differences in sentencing patterns between these groups (see Appendix Fig. 6).

Our quasi-experimental design may be susceptible to period effects that might produce differences between treatment and control groups that are not related to the reforms. While there is, most certainly, a multitude of developments going on in each observation window, we are unaware of any particular phenomenon that could

⁶ As a result of the presence of extreme outliers, our count variable has been winsorized at the 95th percentile.

produce systematically different effects for treatment and control groups. As a way of checking for period effects, we have nonetheless compared groups who were sentenced to prison for the first time prior to and after the reform date, but who were sentenced to prison terms that were not affected by a reform (either because the sentence was too short or too long). We have also performed “placebo tests,” specifying the parole reform dates as instead having occurred 1 year prior to and 1 year after the actual reform years. A further test for robustness is done by expanding the time window from 1 to 2 years prior to and after the reforms, thus increasing the total time window to 4 years. The outcomes from these sensitivity tests will be presented in “Results” and accessed in the supplementary material.

As was noted earlier in the section on the Swedish parole institution, the length of post-release supervision decreased with the 1983 reform. This might affect the comparison between the treatment and control groups in two ways. The longer parole supervision prior to the reform might have made individuals less prone to crime following their release, but it might also have resulted in recidivism being detected more frequently and more easily in this group by comparison with the treatment group.⁷ For these reasons, the outcomes from the 1983 reform analyses should be interpreted with some caution.⁸

Results

Table 1 presents a comparison of demographic characteristics and criminal justice contacts between the treatment and control groups for the three reform samples. The groups are overall similar, but with some exceptions. The treatment groups for the 1983 and 1993 reforms are 4–7 months older at their age of onset than their control groups. This is expected, however, since our data consist of the cohorts born after 1957 and as a result of the temporal order of the design, the treatment groups are allowed to be 1 year older. Because of this, we control for age at crime in the adjusted model but also perform a robustness check that minimizes age differences between the treatment and control group. There is virtually no difference between treatment and control groups in the age of criminal onset in any of the reform years. The only statistically significant difference in prior crimes is observed in the 1993 reform sample where the treatment group has on average been convicted of 0.3 more violent crimes and in the 1999 reform sample, where the control group has on average been convicted of 0.6 fewer violent crimes, and 0.5 fewer narcotics crimes. Despite the overall similarity between treatment and control groups in the three reform samples, there are nonetheless slight differences, and to ensure that these do

⁷ See the discussion by Roodman (2017) concerning what he refers to as “parole bias,” and who argues that potential differences in recidivism rates among those serving different sentence lengths may be due to differences in the duration of post-release supervision.

⁸ In an effort to control away potential bias produced by the variation in the length of supervision periods, we performed a sensitivity analysis with respect to the 1983 reform, with a shorter follow-up period. The results from an analysis where the follow-up period was reduced to 1 year did not deviate from the results from the 10-year follow-up analysis (Supplementary Table 5).

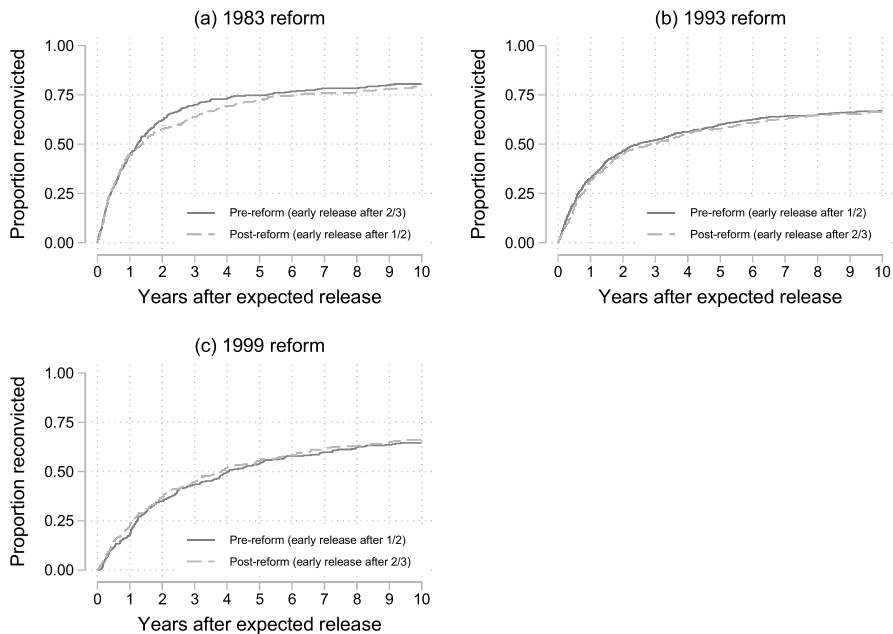


Fig. 3 a–c Cumulative reconviction probabilities for 1983 parole reform sample (a), 1993 parole reform sample (b), and 1999 parole reform sample (c) for treatment (post-reform) and control group (pre-reform)

not bias our estimates, we have controlled for these characteristics when estimating the average effect of the respective reforms on recidivism.

Before turning to our regression analysis, we first explore the extent to which our treatment and control groups have been reconvicted over a 10-year follow-up period. Figure 3 presents estimated Kaplan-Meier cumulative probability functions by treatment and control group in the three reform samples. It may first be noted that the reconviction risk in these first-time imprisonment groups is generally high, irrespective of group membership. As indicated by the steep increase over the first years following expected release, the hazard for recidivism is highest during the years immediately following release, after which it declines. This pattern replicates the conventional wisdom in recidivism research (e.g., Nygaard Andersen & Skardhamar, 2017). It is also noteworthy that although there are differences in the reconviction risk between treatment and control groups, the differences are relatively small with the largest difference between the cumulative curves is found for the 1999 sample, where the treatment group's reconviction risk is 1.5 percentage points higher than that of the control group. A similar small difference between treatment and control group is observed when recidivism is measured as reincarceration (see Appendix Fig. 7).⁹

⁹ One interesting deviation is, however, that the temporary differences between the reconviction curves for the treatment and control groups that occur 1–5 years after the expected release in the 1983 reform (see Appendix Fig. 7a) are not observable when recidivism is measured as reincarceration.

Table 2 Cox regression models predicting the risk for recidivism in three reform samples. Hazard ratios (HR) and confidence intervals (CI)

	Reconviction						Reincarceration					
	Model 1			Model 2			Model 1			Model 2		
	HR	CI 95%		HR	CI 95%		HR	CI 95%		HR	CI 95%	
1983 reform												
Pre-reform	1			1			1			1		
Post-reform	0.940	0.792	1.117	1.045	0.875	1.248	0.957	0.781	1.172	0.990	0.801	1.223
1993 reform												
Pre-reform	1			1			1			1		
Post-reform	0.959	0.852	1.078	0.961	0.853	1.082	0.948	0.818	1.098	0.935	0.805	1.086
1999 reform												
Pre-reform	1			1			1			1		
Post-reform	1.053	0.868	1.278	1.052	0.859	1.289	0.989	0.774	1.262	1.050	0.813	1.356

Model 1: unadjusted. Model 2: adjusted for age at crime, squared age at crime, imposed prison days, conviction month, prior conviction frequency, prior crime frequency by crime type (violent, sex, property, fraud, vandalism, traffic, narcotic), age of first convicted crime, and whether the offender was born in Sweden, and sex. Each estimate represents results from a separate regression

* $p < .05$

Moving on to the regression models, Table 2 presents unadjusted and adjusted estimates from Cox regression models by reform sample on the hazards for reconviction and reincarceration. The estimates from the unadjusted models are parameterizations of the Kaplan-Meier curves in terms of hazard ratios (HR). For example, the difference of 1.2 percentage points in the absolute reconviction risk that we noted between the treatment and control group in the 1983 sample (Fig. 3) is equivalent to a 6% lower (unadjusted) hazard for the treatment group. For the adjusted model, the 1983 treatment group has a 4.5% increase in reconviction risk. In the adjusted models for the 1993 and 1999 reforms — where incarceration time prior to parole release was increased — we see contradictory outcomes with a 3.9% decrease in reconviction risk for the 1993 treatment group and a 5.2% increase in reconviction risk for the 1999 treatment group. In general, the patterns are repeated when we instead look at the hazard of reincarceration in the right-hand side panel. Common to all estimates in Table 2 is, furthermore, that they are small and far from reaching statistical significance at the $p < .05$ alpha level.

Moving on to Fig. 4, we examine the relationship between the different categories of incarceration lengths and post-release reconviction. The figure should be read as reflecting a stratification of sentence lengths that were all impacted in various ways by a parole reform in terms of either a decrease or an increase in incarceration time, with the control groups used as a reference (see Fig. 1 for the size of the changes for specific sentence lengths). For example, the 4–5-month group in Fig. 4a were subject to an imposed sentence of 4–5 months, and as a result of the 1983 reform experienced a decrease of approximately 3 weeks in their period of incarceration. The adjusted HR for this group is 1.041, which means that a decrease of approximately

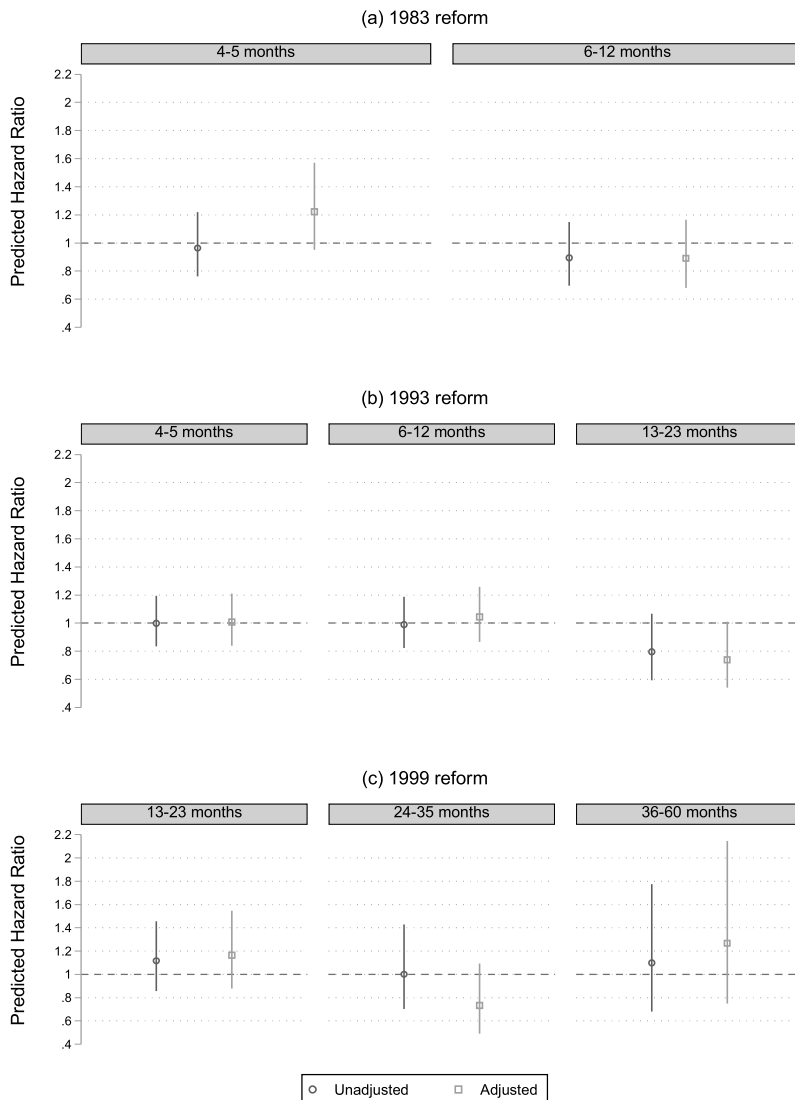


Fig. 4 a–c Cox regression models predicting the risk for reconviction in 1983 parole reform sample (a), 1993 parole reform sample (b), and 1999 parole reform sample (c). Adjusted hazard ratios accompanied by 95% confidence intervals

3 weeks in incarceration time for individuals sentenced to 4–5 months resulted in a 4.1% higher reconviction risk. All three reforms display inconsistent patterns, with estimates going in both directions and none being statistically significant. We nonetheless see a 16.4% decrease in the risk for reconviction in Fig. 4b when incarceration time was increased for inmates with the longest sentence lengths in the 1993

reform, of 13–23 months. For the group who were subject to the longest imposed sentence lengths in the 1999 reform (Fig. 4c), and who were accordingly also subject to the largest increase in incarceration time, we see an increase in the reconviction risk of 26.1%. Overall, it is difficult to draw any general conclusions regarding the relationship between categories of incarceration length and recidivism, in part as a result of the fluctuating pattern in the hazard for recidivism, but also because of the large standard errors.

For recidivism frequency, Table 3 presents estimates in terms of average marginal effects and incidence rate ratios (IRR). Over the course of the 10-year follow-up period, we see that ~4 more offenses (IRR: 1.187) were committed when incarceration time was decreased in the 1983 treatment group. When incarceration time was increased, we see an increase of 0.2 offenses during the 10-follow-up period for the 1999 treatment group (IRR: 1.025) and practically no effect on recidivism frequency for the 1993 treatment group. Again, none of the coefficients are statistically significant at the $p < 0.05$ alpha level.

Sensitivity analyses

To obtain a better understanding of the robustness of our results, we performed a series of sensitivity analyses. First, we looked at recidivism among offenders who are within the observation window (1 year on either side of the reform date) but who were not affected by the reforms because their sentences were either too short or too long (Supplementary Fig. 1). Second, we investigated if any notable changes could be obtained in our estimates by extending the observation window and thus increasing the number of offenders in our data (Supplementary Table 1). Third, we subjected our sample to various restrictions (such as age restrictions) to maximize comparability between the treatment and control groups (Supplementary Table 2). Fourth, we constructed placebo reforms 1 year prior to and 1 year after each actual reform (Supplementary Table 3). None of these sensitivity tests revealed any notable changes in the results, and all estimates remained non-significant.

Discussion

In this study, we have exploited three separate natural experiments in order to measure the effect of incarceration time on recidivism (measured as reconviction, reincarceration, and recidivism frequency) among offenders incarcerated for the first time, using an extensive follow-up period of 10 years. The use of prison and sentence lengths varies widely between cultural contexts. In Sweden, the average sentence in 2020 was 13.5 months (Kriminalvården, 2021a), which can be contrasted with USA, where the average time served in federal prisons in 2012 was 37.5 months (Motivans, 2015). Only a small number of studies utilizing quasi-experimental designs have been conducted outside USA, which means that the scholarly knowledge concerning more moderate penal contexts is limited. Our results contribute to the discussion on the individual preventive effect of incarceration time, particularly

Table 3 Negative binomial regression models predicting recidivism frequency in three reform samples and with 3-year and 10-year follow-up time. Incidence rate ratios (IRR), marginal effects, and confidence intervals (CI)

	Follow-up: 3 years				Follow-up: 10 years						
	IRR	CI 95%	Marginal effect	CI 95%	IRR	CI 95%	Marginal effect	CI 95%			
	Reform										
1983	1.074	0.875	1.317	0.55	2.12	1.187	0.962	1.466	4.11	-1.02	9.23
1993	0.986	0.837	1.162	-0.09	0.97	1.003	0.856	1.175	0.06	-2.95	3.07
1999	0.896	0.663	1.212	-0.29	0.51	1.025	0.785	1.338	0.20	-1.92	2.31

Coefficients adjusted for age at crime, squared age at crime, imposed prison days, conviction month, prior conviction frequency, prior crime frequency by crime type (violent, sex, property, fraud, vandalism, traffic, narcotic), age of first convicted crime, whether the offender was born in Sweden, and sex. Each estimate represents results from a separate regression

* $p < .05$

with regard to the relatively unexplored effect of changes in mid-to-lower range sentences.

Utilizing large-scale administrative data containing all convictions for Swedish cohorts born after 1957, we find little evidence that increasing or decreasing the length of incarceration has a specific preventive effect on post-release offending. We were unable to detect any statistically significant effects of incarceration time, irrespective of how recidivism was measured or whether there was an increase or decrease in the time spent incarcerated. A less restrictive interpretation of the results would suggest that increasing incarceration time for short-term sentences (of less than 2 years) did not *increase* post-release recidivism, regardless of how it was measured, and instead showed a tendency towards minor decreases in recidivism. For long-term sentences of 2 years and more, the effects were the opposite, with tendencies towards an increase in recidivism when incarceration time was increased. The effects of a decrease in incarceration time were too heterogeneous (depending on how recidivism was operationalized) to draw any conclusions regarding tendencies. With regard to the relationship between categorizations of sentence length and recidivism, we found no clear relationship, with non-significant effects in both directions. It could be argued that for some sentence lengths observed in this study, the reforms only had a relatively minor impact (see Fig. 1) and that such small changes in incarceration time may not be sufficient to produce post-release effects. While we do acknowledge that this could be the case for some inmates, increases as low as 1 additional month of incarceration have still been proved to produce post-release effects on, for example, labor market attachment (Landersø, 2015), health outcomes (Hjalmarsson & Lindquist, 2020), and recidivism (Kuziemko, 2012). This suggests that the mechanisms that impact post-release behavior could be active even at minor changes in incarceration time.

As has been noted, European research on the effect of incarceration length is scarce, but previous studies have found no effect when analyzing Dutch offenders (Snodgrass et al., 2011; Wermink et al., 2018), and instances where there was a decrease in recidivism following increased incarceration time among Swedish offenders (Hjalmarsson & Lindquist, 2020). Although our observed null effect echoes the results of Wermink et al. (2018), since their data were limited to offenders sentenced to between 1 week and 15 months, there are difficulties when comparing the results. At the same time, we do observe a similar null effect for the sentence lengths of 4–5 and 6–12 months, which are partly comparable to the sentence lengths studied by Wermink et al. (2018). Further, our findings are in line with those of other second-generation studies that have examined the effects of incarceration time, and that have also found a null effect (Loughran et al., 2009; Meade et al., 2013; Mears et al., 2016; Rhodes et al., 2018; Rydberg & Clark, 2016).

As described by Mears et al. (2015), heterogeneity can be found not only in post-release effects but also in terms of the heterogeneity of in-prison experiences (i.e., treatment heterogeneity), which might explain both why some recent quasi-experimental studies have found recidivism-reducing effects (Kuziemko, 2012; Roach & Schanzenbach, 2015), but also the instances of recidivism-preventive tendencies noted in our study. Hjalmarsson and Lindquist (2020), for example, show that when incarcerated, participation in health programs could

help to reduce recidivism (see also Bhuller et al., 2020; Lipsey & Cullen, 2007). Variation in participation and inmate programs may thus act as confounders and contribute to outcome heterogeneity between penal contexts, which suggests a need for further analyses to pinpoint why the effects of increased incarceration time vary. As noted by Hjalmarsson and Lindquist (2020), for some incarceration lengths, it may be that an increase in incarceration time enables participation in effective rehabilitating programs which would otherwise not be possible. The Swedish context is particular in the sense that the Scandinavian penal institutions are known for being rehabilitation-oriented with comprehensive in-prison health care and programs for education and vocational training (see Pratt & Eriksson, 2014; Ugelvik & Dullum, 2012; von Hofer & Tham, 2013). From an international point of view, Swedish prisons have one of the highest per inmate expenditures and this is in part due to the small-scale prisons, a low staff-to-inmate ratio, and the extensive rehabilitating programs. Alongside the relatively small prison population, these are features that may affect the extent to which the results reported above are generalizable to countries outside the Scandinavian context. On a similar note, because of national differences in the criminal sanction system inmate composition and thus recidivism risks differ between countries. For example, traffic offenses in Norway are punished by incarceration far more often than in Sweden (Kristoffersen, 2013), and drug possession and drug use are criminalized in Sweden as opposed to the Netherlands (Chatwin, 2003).

Before discussing the policy implications of this study, a number of limitations need to be addressed. First, because of the negative relationship between age and criminal participation (i.e., the age-crime curve), age may have a confounding effect when analyzing recidivism. In our study, this is primarily an issue for those individuals who experienced the largest increase in incarceration time (see Fig. 1), producing an age gap between the treatment and control groups at the time when the offenders were released. Second, with regard to a more general discussion concerning internal validity and unobserved confounding, it should be mentioned that although we have utilized natural experiments to minimize the influence of confounders and selection effects, we cannot rule out the existence of such biases.¹⁰ This issue is the most prominent concern in relation to the design employed in this study, since the treatment group is observed on average 1 year after the comparison group. Because our study design limits us from controlling for period effects, we cannot with certainty rule the effect from the general crime decline in convictions witnessed in Sweden during the study period (Bäckman et al., 2020). Third, our reliance on natural experiments has meant that we have been limited to those few occasions on which these have occurred in Sweden. It would be methodological preferable if these natural experiments had been more recent in order to minimize the limitations to generalizability associated with possible differences in how the correctional services operate, but also general societal, economic, and legal changes. Examples of the latter

¹⁰ As described in the section on the Swedish parole institution, Hjalmarsson and Lindquist (2020) evaluated whether the 1993 and 1999 reforms could be used as a natural experiments, and found that they could.

are the criminalization of the purchase of sex (Levy, 2014) and zero-tolerance drug policies (Lenke & Olsson, 2002), both part of a general trend toward a more punitive crime policy (Tham, 2001). To some extent, these changes may limit the comparability between reforms since the time distance between them have potentially resulted in variation with respect to the composition of the inmate population over the three reform periods. Fourth, although the results regarding different categories of incarceration lengths provide us with important knowledge with regard to nonlinearity, stratifying incarceration lengths in this way involves a loss of precision in our estimates, which can be seen in the large confidence intervals. Fifth, because we do not have exact dates on prison entry and release, we are limited to approximations of the release date (see outcome variables section). The estimation of release dates could produce bias if there were a reason to believe that the period between receiving a conviction and starting one's sentence differed between the periods before and after a given reform. However, we have no reason to expect any systematic differences between the treatment and control groups in any of the reform samples. Sixth, because treatment heterogeneity may be critical to the understanding of confounders that might impact treatment effects, differences in quality or intensity of prison programs (and other in-prison experiences) may be an issue when generalizing our results to penal contexts that vary from the Swedish correctional system, in terms of both sanctioning policies and also the emphasis on rehabilitation. At the same time, other parts of Western and Northern Europe have similar policies and conditions, and a similar focus on rehabilitation, and thus generalizability should be possible to a broader context than just Sweden.

Limitations aside, the policy implications of this study are not clear. In general, the issue of the effectiveness and crime-reducing potential of custodial sanctions and longer incarceration times is complex with heterogeneous effects depending on offender characteristics and the environment in which the inmate is held. An increase in incarceration time could allow for further rehabilitative interventions for some at-risk individuals serving shorter sentence lengths, but this presupposes that the environment in which they are incarcerated has the resources to correctly identify at-risk individuals, and sufficient social and health programs. In contrast to non-custodial sanctions, incarcerating offenders with no prior prison record is, however, associated with increased post-release recidivism risks (Nieuwbeerta et al., 2009; Toman et al., 2015; Walters, 2003). In addition, there is evidence from Scandinavia showing crime-reducing benefits of electronic monitoring among individuals who have not previously been incarcerated (Andersen & Telle, 2019). From a policy perspective, redirecting individuals without prison records to alternative sanctions may therefore be a more effective means of reducing crime, as well as being more cost-effective. As suggested by this study, the overall crime-control benefits of increasing incarceration time for first-time incarcerated offenders may be questioned.

Appendix

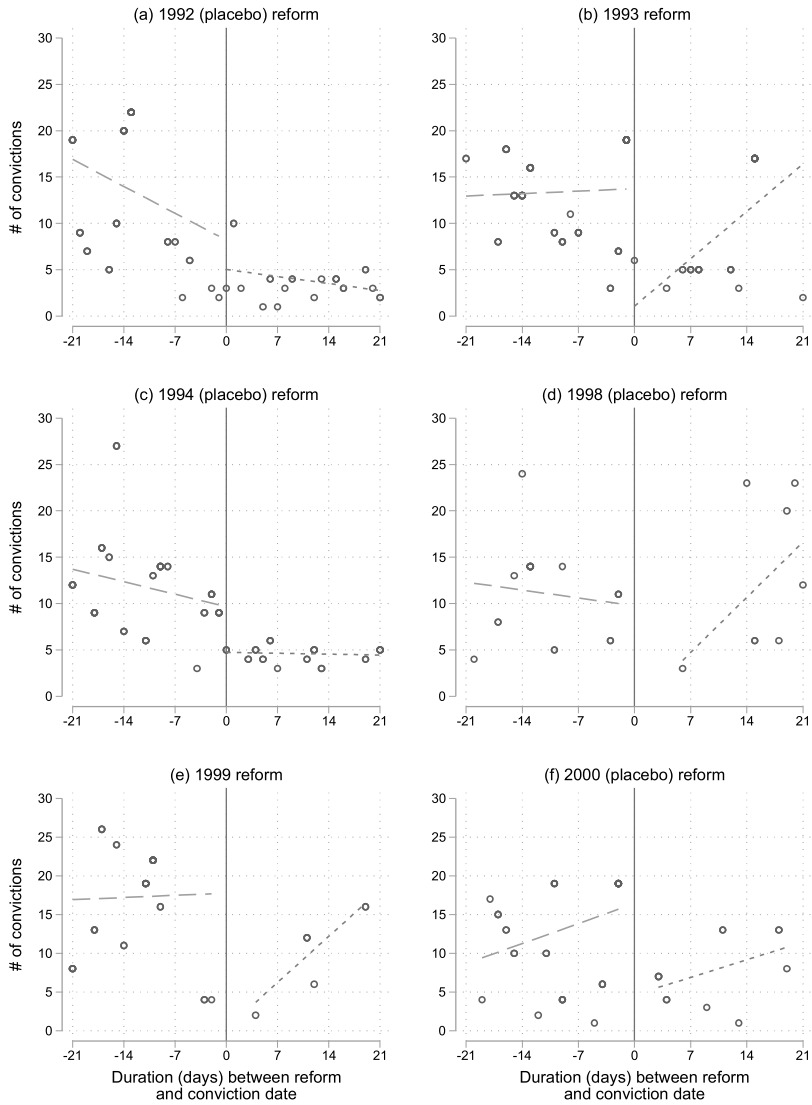


Fig. 5 a–f Number of convictions (per day) prior and after July 1, 1992, placebo reform (a), July 1, 1993, parole reform (b), July 1, 1994, placebo reform (c), January 1, 1998, placebo reform (d) January 1, 1999, parole reform (e), and January 1, 2000, placebo reform (f)

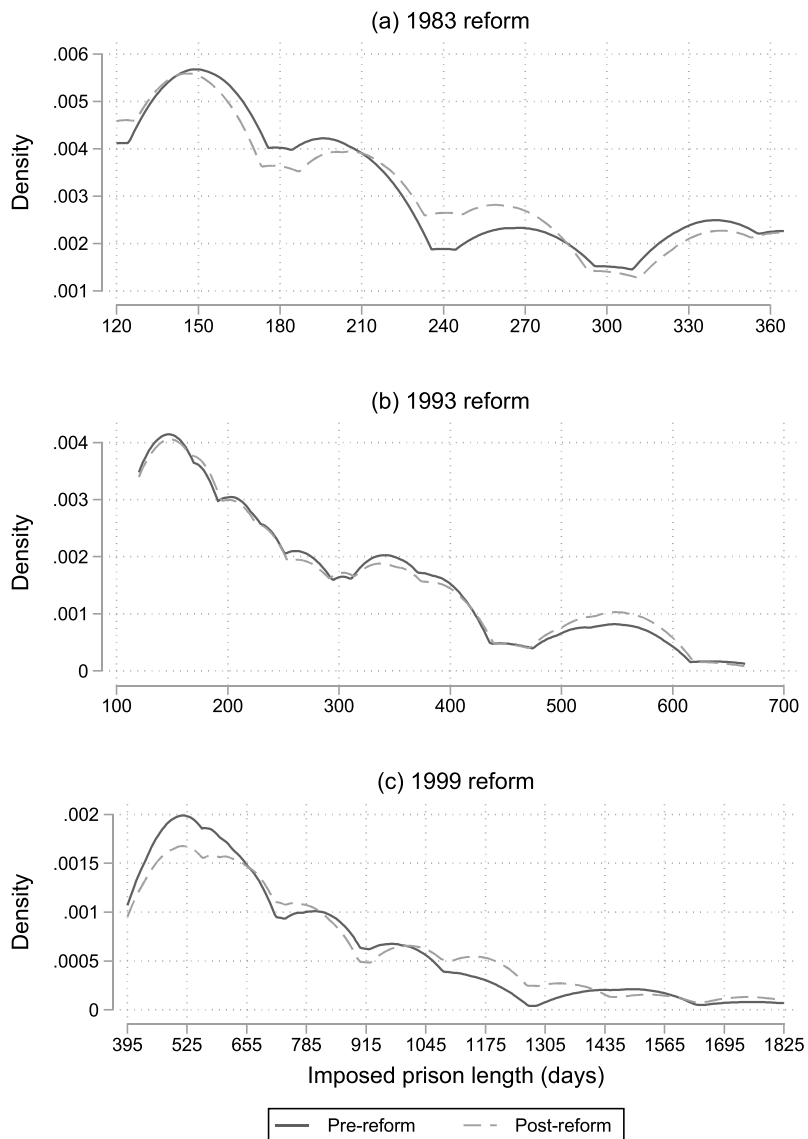


Fig. 6 a–c Densities of imposed sentence length in 1983 parole reform (a), 1993 parole reform (b), and 1999 parole reform (c) for treatment (post-reform) and control group (pre-reform). Only sentence lengths that are affected by the reforms are presented

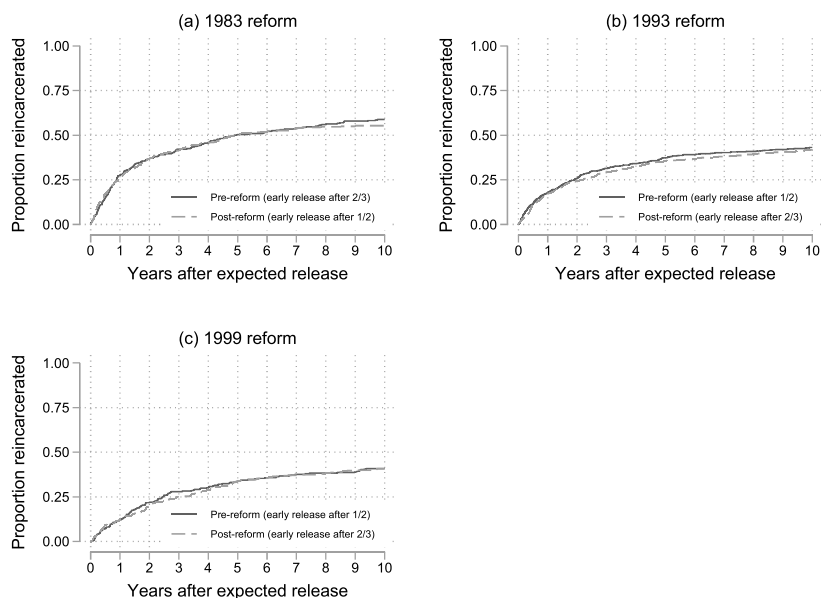


Fig. 7 a–c Cumulative reincarceration probabilities for 1983 parole reform sample (a), 1993 parole reform sample (b), and 1999 parole reform sample (c) for treatment (post-reform) and control group (pre-reform)

Supplementary Information The online version contains supplementary material available at <https://doi.org/10.1007/s11292-022-09513-1>.

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TRUTH IN SENTENCING



FLORIDA SHERIFFS RESEARCH INSTITUTE
JANUARY 2020

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INTRODUCTION

Nearly 50 years ago, Florida’s sheriffs were talking about the importance of criminals serving their full court-imposed prison sentences. It was an important issue then and it remains a big deal today because sheriffs have seen the benefits of a system where there is “Truth in Sentencing.”

As you will read on the following page, former Leon County Sheriff Raymond Hamlin described in great detail in a 1975 issue of the Sheriff’s Star about the need for “truth in sentencing” to ensure criminals serve their full time. Almost 20 years later, sheriffs saw their persistent work realized when Florida passed its version of Truth in Sentencing – the Stop Turning Out Prisoners (S.T.O.P.) Act, which now ensures all criminals sentenced to the Department of Corrections must serve at least 85% of their time behind bars before being eligible for release.

Lately, the S.T.O.P Act has been criticized by opponents who claim that because of that Act, inmates have no incentive to behave well while incarcerated, and that the

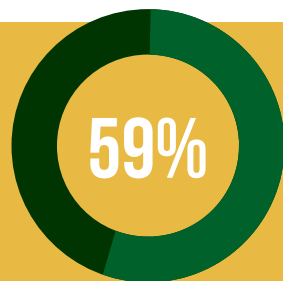
law should be changed to 65%. We disagree with this proposition because rewarding inmate misbehavior with early release is illogical.

On one hand, advocates for reduction in the law requiring that inmates serve at least 85% of the judge-imposed sentence also advocate for the elimination of minimum mandatory sentences. These advocates opine that judges should be allowed to sentence, and that judges should not be hamstrung by minimum mandatory sentences because minimum mandatory sentences lead to unfair results for criminals. These advocates further argue that judges are best poised to make the right decision about the length of sentence that should be imposed on a criminal defendant based upon the judge’s analysis of the facts and law, and that sentencing should not be dictated by statute.

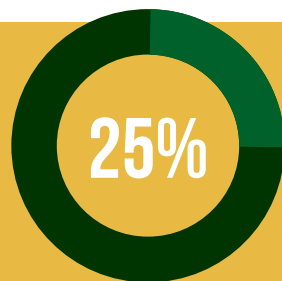
They cannot have it both ways by arguing that a judge’s well-informed sentence is paramount to fairness and correct sentencing, while at the same time advocating

to gut the judge’s sentence. Reducing a judge’s sentence to 65% of the imposed time flies in the face of the well-informed sanction for criminal activity imposed by the court and results in “false sentencing.” If a judge imposes a sentence, it should be served (with certain gain time considerations) and not gutted to put a convicted felon back out on the street early. Over 95% of the inmates in the Florida Department of Corrections are repeat offenders with histories of wreaking havoc on the law-abiding citizens. It is unfair to their victims and future victims by releasing these criminals earlier than the judge determined appropriate.

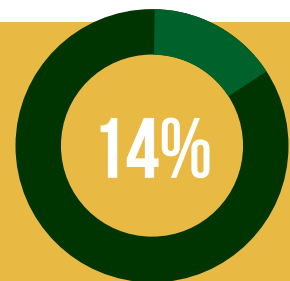
The S.T.O.P. Act, and the investments made in building more prison bed space, has not turned out to be as draconian as opponents claim. The end result of the S.T.O.P. Act, and criminals serving their full-time, has been safer neighborhoods and as the research shows - offenders less likely to commit new crimes.



Total crime index per 100,000 residents decreased **59%** from 1997 to 2017 (Florida Statistical Analysis Center, 2017)



Incarceration rate has decreased by **25%** since 2014 (Florida Policy Institute, 2019)



Florida’s jail population dropped **14%** from 2012 to 2017 (Florida Department of Corrections, n.d.)

HISTORY

Truth in Sentencing (TIS) refers to the practice of requiring a prisoner to serve a pre-determined amount of the court-imposed sentence in prison. Starting in the 1970s, critics of the then-used indeterminate sentencing scheme saw disparities in sentencing, leniency by judges, and the failure of the rehabilitation model as unfair and inefficient (Sabol, Rosich, Kane, Kirk, & Dubin, 2002). In response, sentencing reform legislation in the 1980s and early 1990s included:

- 1. A shift to determinate sentencing (a process where the court assigns a set prison term to a convicted offender);**
- 2. Restrictions or abolition of parole and good time credits;**
- 3. Reduction of judicial discretion;**
- 4. Emphasis on diversion programs and intermediate sanction for nonviolent offenders;**
- 5. Greater emphasis on incapacitation and deterrence; and**
- 6. The Crime Control Model of criminal justice (Sabol, Rosich, Kane, Kirk, & Dubin, 2002).**

Federal legislation passed in 1994 as part of the Violent Crime Control and Law Enforcement Act aimed to support sentencing reform by providing states with grants to expand their prison capacity if they imposed TIS requirements on violent offenders. The Federal TIS Incentive Grant Program was based on the 85% rule, meaning that states were to have or pass laws requiring serious violent offenders to serve at least 85% of their imposed sentences in prison (Rosich & Kane, 2005). Since October 1, 1995, Florida Statute 944.275 has mandated that all state prisoners must serve no less than 85% of their sentence.

Eighteen states and the District of Columbia now require prisoners serve 85% of their sentence. An additional 22 states require prisoners serve a mandatory sentence of more or less than 85%. Just twelve states have no truth in sentencing law. The sentencing requirements for each state are listed in Table 1.

ON TRUTH IN SENTENCING (REPRINTED FROM THE MARCH-APRIL 1975 EDITION OF THE SHERIFF'S STAR)

TAMPA – Leon County Sheriff Raymond Hamlin told a criminal justice conference he was concerned about a lack of “truth in sentencing” and frustrated with a system that convicts too few and paroles too many.

He said judges, juries and attorneys do not get the full impact of serious and violent crimes. “The judge sits in a clean courtroom and everybody is clean and the prisoner’s got religion; but law enforcement officers see the criminal and victim where the crime has taken place. We see the criminal nearest the crime, where he portrays himself as he really is.”

Hamlin added that sentences handed down by judges usually bear little resemblance to the time actually served. He said if an individual is given a 10-year sentence, he rarely ever serves the full 10 years and is likely to be paroled in three and half years.



DETERMINANT SENTENCING REQUIREMENTS BY STATE (TABLE 1)

STATE	85%	DETERMINATE	INDETERMINATE	RELEASE REQUIREMENT
Alabama	X		X	Some offenses eligible for parole after 1/3 of sentence or 10 years, whichever is less.
Arizona	X	X		
Arkansas		X		100% for violent felonies. 70% for other crimes.
California	X	X		
Connecticut	X		X	
Deleware		X		75%
Florida	X	X		
Georgia	X	X		85% for violent crimes. Certain other crimes serve 50%, 75%, and 100% based on the offense.
Idaho		X		100%
Illinois		X		Inmates serve 50%, 75%, 85%, or 100% of their sentence based on the offense.
Indiana	X	X		85% for violent crimes. 80% for nonviolent and some drug crimes.
Iowa	X		X	70% of maximum sentence for violent crimes. sexual predator or forcible felonies which carry an 85% or 70% mandatory sentence.
Kansas	X	X		
Kentucky	X		X	
Louisiana	X		X	
Maine		X		Time served is at the discretion of the judge.
Minnesota	X	X		
Mississippi		X		50% for violent offenses. 25% for other offenses.
Montana		X		Eligible for parole after serving 25% of the sentence.
New Mexico		X		
North Carolina	X	X		
Ohio		X		
Oklahoma	X	X		
Oregon		X		80%
South Carolina	X		X	
Virginia	X	X		
Washington	X	X		
Wisconsin		X		100% with no parole.

* Georgia and Indiana cannot be verified.

* The following states have indeterminate sentencing: Alaska, Colorado, Hawaii, Maryland, Massachusetts, Michigan, Missouri, Nebraska, Nevada, New Hampshire, New Jersey, New York, North Dakota, Pennsylvania, Rhode Island, South Dakota, Tennessee, Texas, Utah, Vermont, West Virginia and Wyoming.

* Full chart located at the link: http://flsheriffs.org/uploads/docs/TIS_by_state.xlsb_xlsx

ARRESTS, SENTENCING, INCARCERATION AND RELEASE

Sentencing in Florida is based on a points system that scores the primary offense, any additional offenses, any prior record, victim injury and any enhancements. Each crime is assigned a point value, and any additional points are added to achieve a final sentencing score. The total score required for a non-prison sanction is 44 points or less. A score of more than 44 points may result in a prison sentence. According to an Office of Program Policy Analysis and Government Accountability (2019) report, “96% of felony sentences...are the result of offenders’ pleas” and “it is common for sentences to be below the recommended minimum sentence” (p. 3). A list of crimes and the points assessed for those crimes is displayed in Table 2. The chart illustrates that many felonies and first-time offenses (not including murder) do not result in a prison sentence.

Nationally and in Florida, the confined population has been steadily declining for some time. Kaeble and Glaze (2016) reported the national prison population dropped for the ninth straight year in 2016 and fell 18% from 2007 to 2016. These figures represent the lowest prisoner population levels since 1993 (Kaeble & Glaze, 2016).

These figures follow national and state crime trends. Nationally, violent crime and property crime have decreased 19% (Federal Bureau of Investigation, n.d.). Florida experienced a decrease in violent and property crime between 2012 to 2017 of 9.5% and 13% respectively (Florida Department of Law Enforcement, n.d.). The total number of arrests in Florida decreased 17.4% between 2014 and 2018 despite a population increase of 6.4% during the same time period (Florida Department of Law Enforcement, 2019a). Florida’s incarceration and crime rates compared with other states is included in Table 3.

CRIME AND SCORESHEET POINTS (TABLE 2)

CRIME	FELONY LEVEL	SCORESHEET POINTS	SANCTION
1st degree murder	1st degree	116	Prison
Trafficking in cocaine	1st degree	56	Prison
Aggravated battery	2nd degree	56	Prison
Burglary to an occupied dwelling	2nd degree	36	Non-prison
Burglary to an unoccupied dwelling	3rd degree	22	Non-prison
Possession of cocaine	3rd degree	16	Non-prison
Robbery, no weapon	2nd degree	36	Non-prison
Possession of cannabis, > 20 grams	3rd degree	4	Non-prison
Battery on a 1st responder	3rd degree	22	Non-prison
Grand Theft, less than \$5,000	3rd degree	10	Non-prison

According to the Florida Department of Corrections (2018a), 56% of Florida's inmate population was incarcerated for a violent crime in the fiscal year 2017-2018. Violent burglaries accounted for 73% of all burglaries. The top 20 crimes for which prisoners are incarcerated account for 59.2% of the prison population, with the largest offense being burglary to an occupied dwelling (8% of the total prison population). These top 20 crimes are listed in Table 4.

Prisoners convicted of violent crimes comprise the majority (32%) of all Florida prisoners released annually. Property crimes and drug crimes account for 31% and 22.5% of annual releases in Florida. From 2015-2018, more state inmates were released for property (3.8%) and drug crimes (1.5%) than were admitted (Florida Department of Corrections, 2018).

STATE INCARCERATION FIGURES PER 100,000 FOR 2017 (TABLE 3)

STATE	INCARCERATION RATE**	CRIME RATE***		NUMBER OF PRISONERS**	ADMISSIONS**	RELEASES**
		VIOLENT	PROPERTY			
TX	553	410	2,367	162,523	76,877	77,196
GA	506	326	2,573	53,667	16,699	15,210
AL	486	519	2,817	27,608	12,170	13,624
FL	466	384	2,281	94,722*	28,189	30,467
CA	328	447	2,350	131,039	37,077	36,203
NY	249	350	1,440	49,461	20,421	21,667

*as of October, 2019.

**Bureau of Justice Statistics, Prisoners in 2017, <https://www.bjs.gov/content/pub/pdf/p17.pdf>

*** FBI Uniform Crime Report, 2018. Table 4, Crime in the U.S.

TOP OFFENSES RESULTING IN PRISON INCARCERATION (TABLE 4)

OFFENSE	PRISONERS*	% OF TOTAL POPULATION
Burglary to an occupied dwelling	7,562	8.0
Murder, 1st degree	7,257	7.6
Murder, 2nd degree	5,782	6.1
Aggravated Assault with a weapon	4,234	4.4
Cocaine possession	4,044	4.2
Molesting a juvenile less than 12 years old, offender 18 or older	3,554	3.7
Burglary to an occupied structure	3,413	3.6
Committing a felony while armed with a gun	3,219	3.3
Aggravated battery with a deadly weapon	3,126	3.3
Robbery with a gun	2,501	2.6
Armed Burglary	1,859	2.0
Sale/Manufacture/Delivery of cocaine	1,858	2.0
Burglary with assault on a person	1,705	1.8
Battery on a 1st responder (LEO, firefighter, EMT)	1,459	1.5
Grand Theft, less than \$5,000	1,331	1.4
Aggravated battery	1,320	1.4
Robbery without a gun	1,272	1.3
Murder, during the commission of a felony	1,038	1.0%
Total prisoners/% of total population	56,534	59.2%

*as of October, 2019.

WHY TRUTH IN SENTENCING MATTERS: PERSPECTIVE OF A FLORIDA SHERIFF WHO WAS ALSO A VICTIM

By Flagler County Sheriff Rick Staly



On July 31, 1978 as a young Orange County Deputy Sheriff I was shot 3 times saving the life of a fellow deputy during a disturbance call. The suspect, Jackson LeGree, Jr. was also shot and taken into custody. I am alive only because I was wearing my personally purchased protective armor. The emergency room doctor told me I would not have survived the shot to my chest had the armor not

stopped the bullet. I still have the scars on my chest and right arm from my assailant's bullets. The suspect also survived.

Almost two years later the suspect went to trial. On May 27, 1980, after a full week of trial the jury found LeGree guilty of: Count 1 - Attempted First Degree Murder; Count 2 - Aggravated Assault; and Count 3 - Resisting Arrest with Violence. LeGree was sentenced to 20 years in Florida Department of Corrections (DOC)—or so I was told. However, LeGree was released on September 1, 1988 after serving just 8.5 years of a 20-year sentence for almost killing a law enforcement officer. In reality, he served only about 40% of his sentence because Florida had yet to enact a "Truth in Sentencing" law.

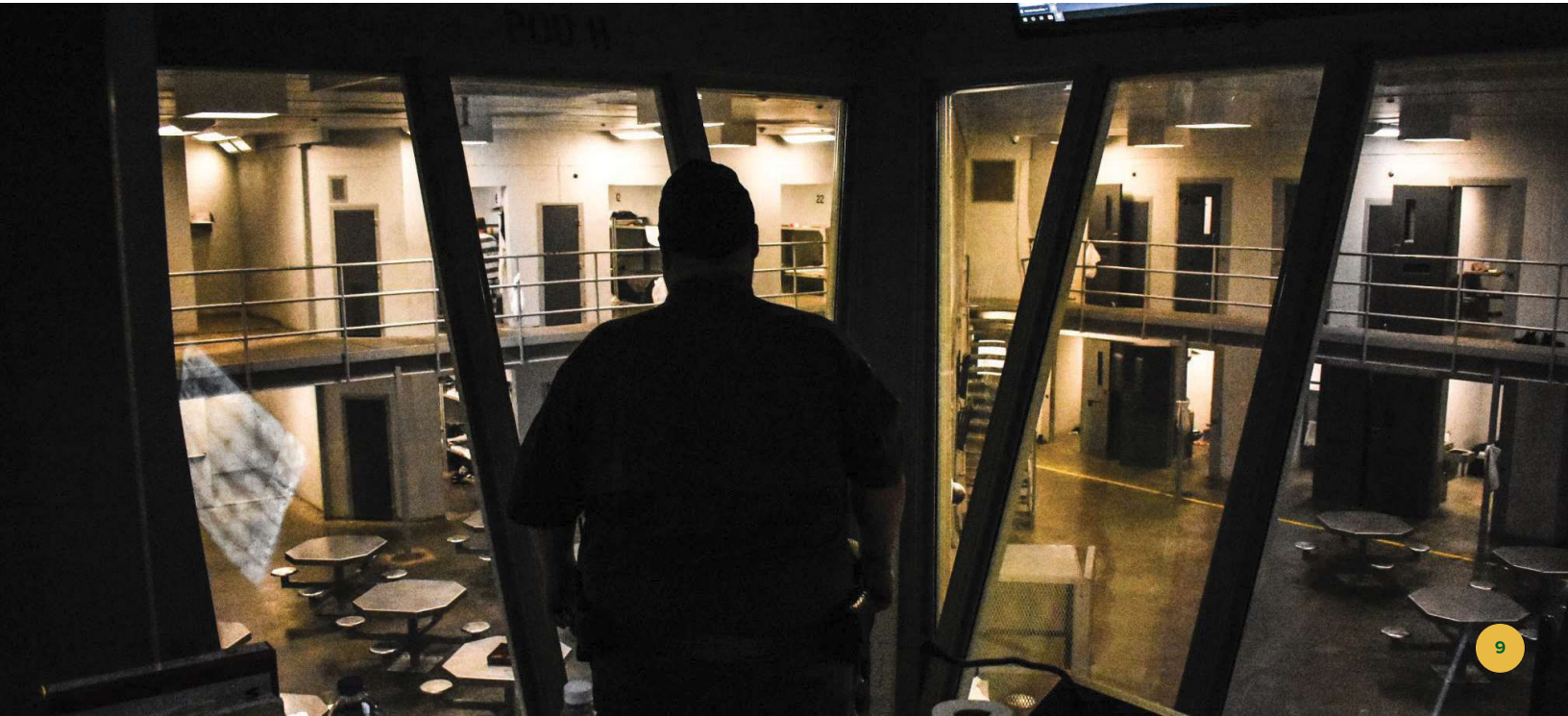
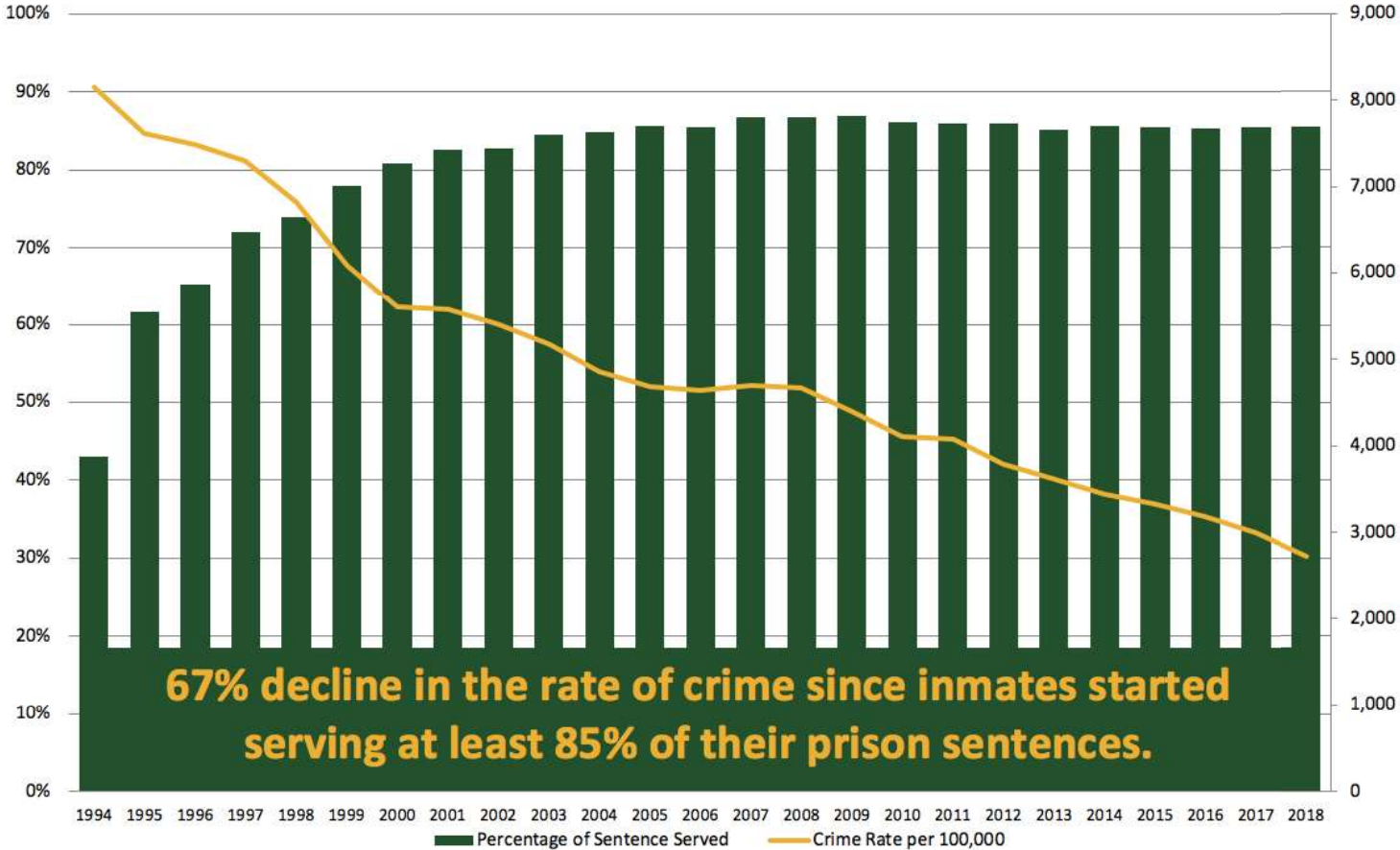
Since LeGree's release he has been arrested for selling cocaine. He pled guilty and was sentenced to 3 months in the Orange County Jail with credit for

98 days. Less than 2 years later he was arrested for misdemeanor possession of marijuana. He pled Nolo Contendere. Sentencing records show no jail sentence. Then less than 2 years later he was arrested for not having a driver's license. Adjudication was withheld and he paid a total of \$434 in court costs and fines. Then in 2011 he was arrested for burglary, plea bargained down to Trespassing, pled Nolo Contendere and was sentenced to 2 days in jail, which was actually time served, and 12 months' probation.

As you can see this is a career criminal, maybe not by statute definition, but from the eyes of his victim he fit the definition. Since his release he continued committing crimes (and the crimes listed are only the ones he was caught on), but the court system treated him like a minor offender with no significant prior criminal history.

Florida's 85% law (Truth in Sentencing) was passed because of cases like mine. Florida has a 50-year low in crime partly because more criminals are having to serve their full amount of their court-ordered sentence. As a victim of crime, a 45-year Florida Law Enforcement Officer and current Sheriff, lessening the terms of incarceration for offenders will be disingenuous to the duty we all share for public safety. It will make Florida more dangerous for our citizens, our visitors and for law enforcement officers protecting our cities and counties.

Percentage of Sentence Served in Prison Compared to Crime Rate Florida's 85% Law



RECIDIVISM IN FLORIDA AND EFFECT OF TIS ON RECIDIVISM

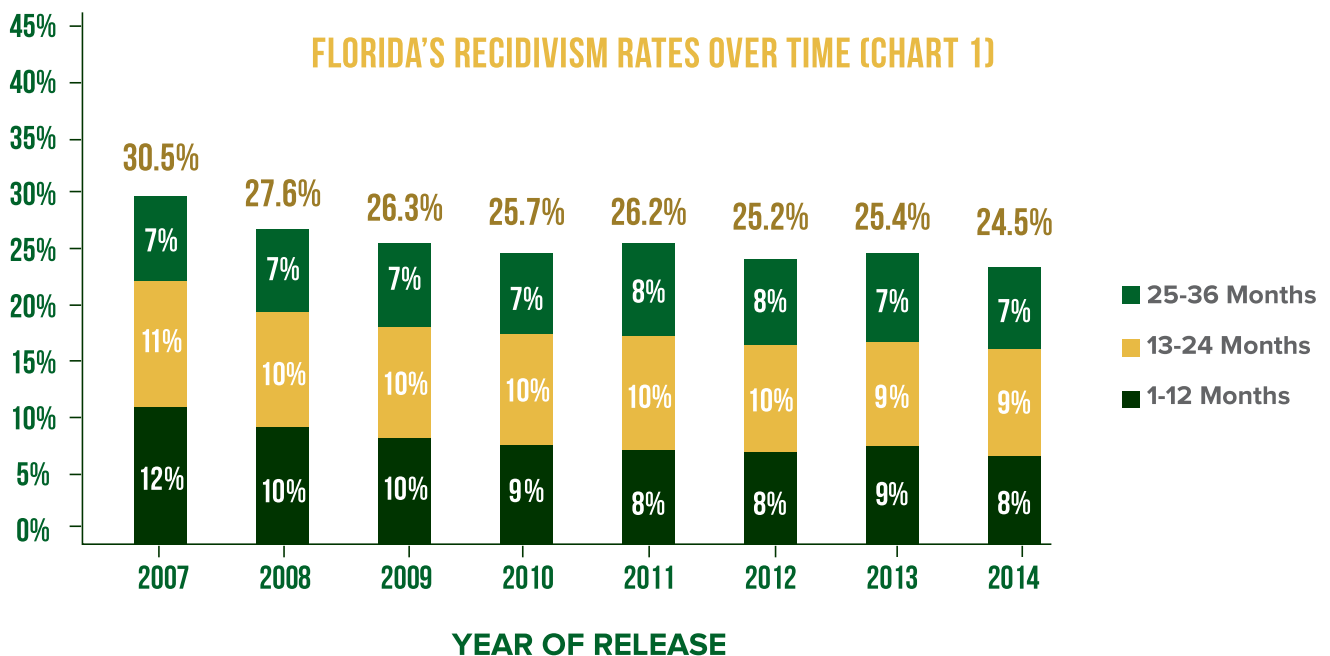
The Florida Department of Corrections defines recidivism as a return to prison, either upon conviction for a new crime or administrative action, within 3 years of an inmate’s prison release (Florida Department of Corrections, 2018b). It’s important to note that defining recidivism varies state by state, and even nationally. In Florida, this definition does not include any rearrests or time sentenced to the county jail - only if the offender is convicted and sentenced to felony state probation or state prison. According to a Florida Department of Corrections report (2018b), recidivism is influenced by many factors including:

- Post-release supervision
- Number of prior prison commitments
- Age upon release
- Number of disciplinary reports while in prison
- Number of other nonviolent offenses within five years prior to admission
- Number of burglary offenses before release
- Number of theft offenses before release

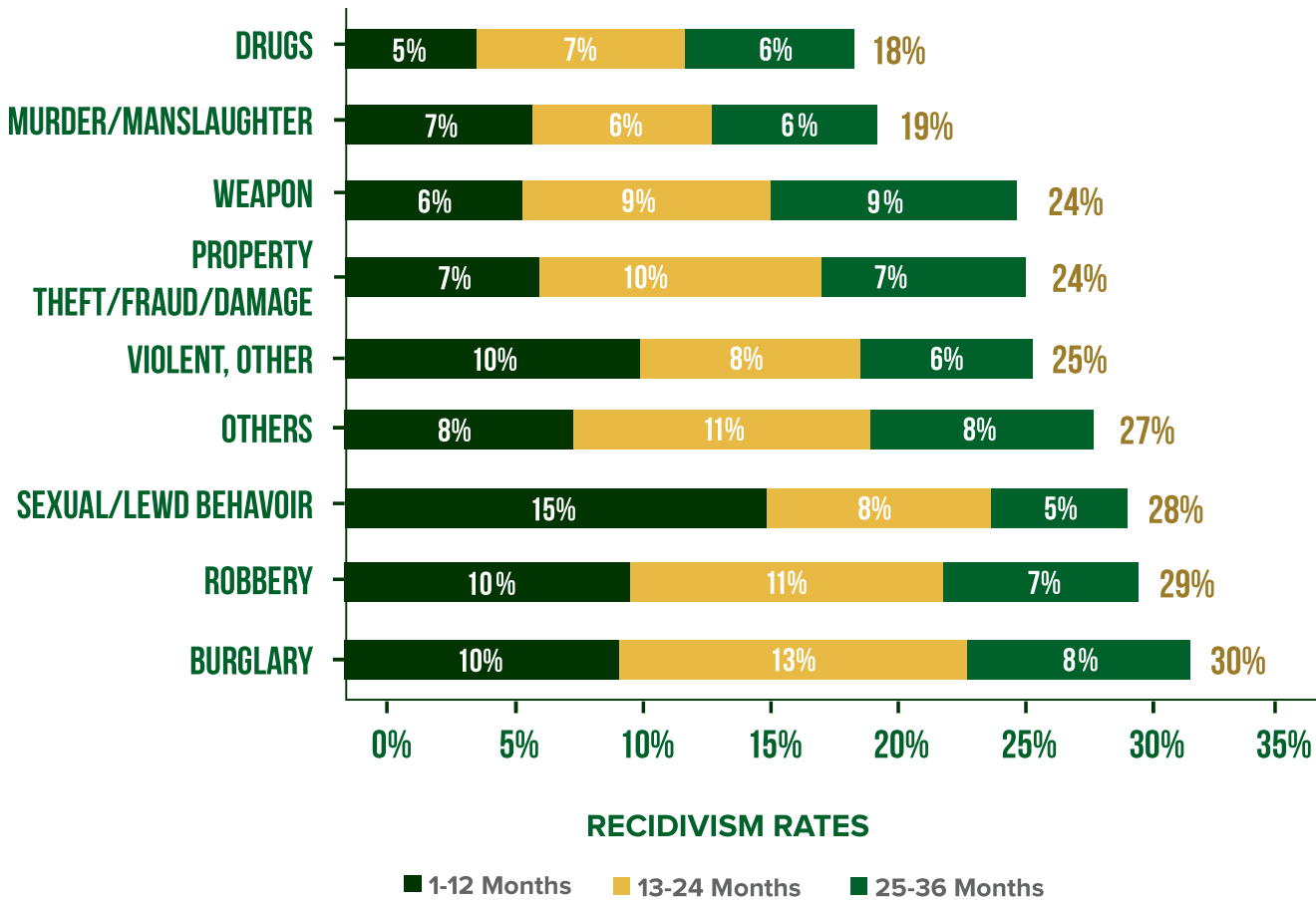
• Nationally, **68%** of prisoners were arrested for a new crime within three years of their release (Alper, Durose, & Markman, 2018)



• In Florida, the three-year recidivism rate has dropped from **30.5%** in 2007 to **24.5%** in 2014 (Florida Department of Corrections, 2018b)



RECIDIVISM RATES BY PRIMARY OFFENSE - 2014 RELEASES (CHART 2)



Inmates who served time for burglary have the highest recidivism rate (30%) (Florida Department of Corrections, 2018b). Robbery (29%), sexual/lewd behavior (28%), and other violent crimes (25%) not including murder/ manslaughter have higher recidivism rates than the three-year recidivism rate for 2014 (24.5%) (Florida Department of Corrections, 2018b). Chart 2 illustrates Florida’s recidivism rate by primary offense.

Recently, there has been a push for taking inmates out of prison and placing them on community supervision. The problem with this scenario is that these inmates will probably fare no better than if they were incarcerated because probation officers already have large caseloads with little worthwhile rehabilitative interaction with offenders. In an examination of post-release community supervision and recidivism, Clark et. al. (2015) reported that supervised offenders were as much as 20% more likely to be arrested for a felony and 360% more likely to be reincarcerated than those with no community supervision. This may be due, in part, to probation officers spending little time with offenders and prioritizing enforcement over rehabilitation (Bonta, et al, 2008).

Technical violations related to post-release supervision are a significant contributor to Florida’s prison population. Fewer prisoners are incarcerated for drug crimes (12.7%) in Florida’s prisons than those imprisoned for supervision violations (17%). A report published by the Council of State Governments Justice Center (2019) also noted that:

On any given day in Florida, 16,558 people are incarcerated as a result of a supervision violation at an annual cost to the state of \$330 million. Technical supervision violations account for \$145 million of this total amount, and new offense supervision violations make up \$185 million. These figures do not account for the substantial local costs of keeping people in jail for supervision violations.

THE EFFECT OF TIS ON RECIDIVISM

Two studies exist using a sample of inmates from the Florida Department of Corrections that conclude TIS laws have had a positive impact on reducing crime and recidivism. These studies are most appropriate for this report due to their focus on Florida’s prison population.

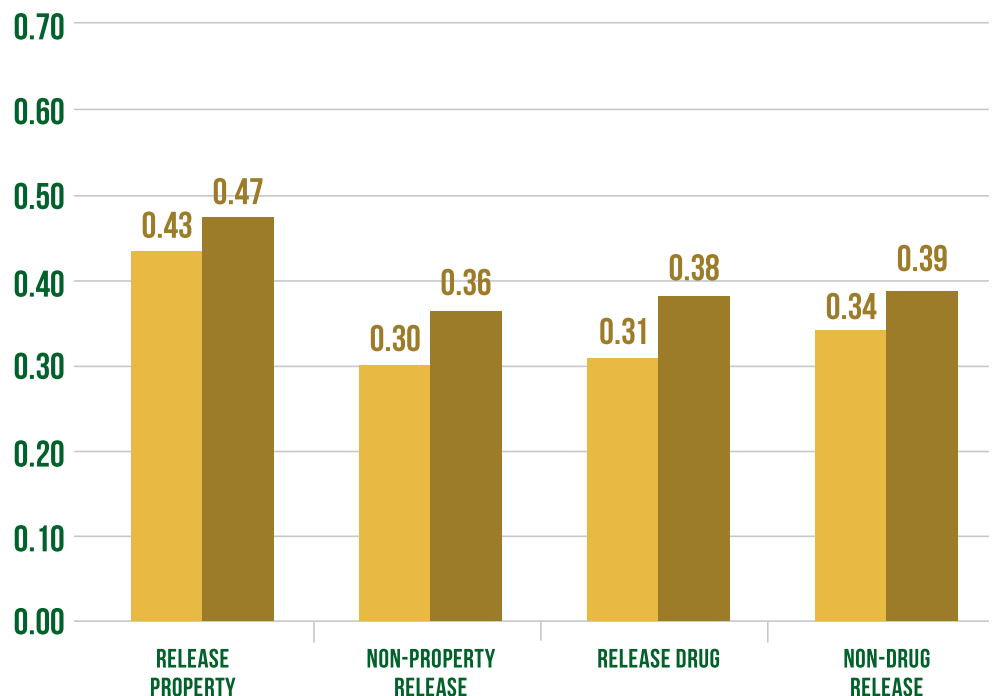
Pate (2010) analyzed the effectiveness of Florida’s TIS law on recidivism by examining a cohort of 182,929 cases obtained from the Florida Department of Corrections. These inmates were released from the Florida prison system between January 1, 1995 and December 31, 2005. Pate (2010) used binary logistic regression, which measures the effect of a variable (85% sentencing) on other variables (likelihood of recidivism). Pate (2010) could not be more clear in her findings: “Sentencing offenders to serve at least 85% of the court-imposed sentence does reduce the probability of recidivism” (Pate, 2010, p. 71). Specific findings suggest TIS reduces reoffending among drug offenders, violent offenders, young offenders, and others. (Pate, 2010, p.72). These findings are illustrated in Chart 3.

A similar study (Bales, Gaes, Blomberg, & Pate, 2010) supports Pate’s (2010) findings. Bales, Gaes, Blomberg, and Pate (2010) analyzed Florida arrest records and recidivism data to determine the impact of Florida’s 85% TIS laws on recidivism and if these laws are more effective at reducing recidivism than the state’s previous sentencing policies. The analysis shows, on average, a 13% reduction in recidivism and a 20% reduction in reimprisonment. The effect increases with the length of the sentence. For a prisoner serving a 10-year sentence, the reductions for recidivism and reimprisonment are 52% and 60% respectively. At the time, the authors concluded that 85% TIS had not resulted in longer prison sentences or prison overcrowding but rather an increase in felony convictions. The authors suggest pessimistic conclusions in the literature related to determinant sentencing could be premature (Bales, Gaes, Blomberg, & Pate, 2010).

EFFECT OF THE MINIMUM 85% SENTENCE SERVED LAW ON RECONVICTION WITHIN 3 YEARS FOR A NEW FELONY CONVICTION BY RELEASE OFFENSE TYPE (CHART 3)

Predicted Probabilities

- 85% Law
- Non-85% Law



Source: Pate, 2010.

HB 189/SB 394/SB 572 & THE MYTH OF THE “NONVIOLENT” OFFENDER

Senator Bracy (SB 394), Senator Brandes (SB 572), and Representative Hart (HB 189) have filed bills for the 2020 legislative session that will shorten the amount of time served by a convicted felon from 85% of the full sentence to just 65%. Their goal is to release 10,000 convicted felons, which represents about 11% of the state’s 94,722 inmates. Their bills use loose definitions of what constitutes a “nonviolent” offender. Just 1% of Florida’s total prison population are first-time, nonviolent offenders. The overwhelming majority of all prisoners—over 95%—are violent, repeat offenders with a history of criminal behavior.

The table below displays the number of inmates currently incarcerated who would be affected by the passage of these bills and the number of prisoners who are true first-time offenders. The table clearly illustrates that nonviolent offenders are rarely incarcerated in the state’s prisons for a first offense. Even when accounting for violent crimes (murder, aggravated battery, robbery, kidnapping, sexual battery, and burglary), first-time offenders represent just 4.8% of the total prison population.

TOP 10 CRIMES ELIGIBLE FOR EARLY RELEASE UNDER HB 189/SB 394/SB 572	# OF INMATES (AS OF NOVEMBER 30, 2019)	# OF 1ST-TIME DOC INMATES (AS OF NOVEMBER 30, 2019)
Sale of illegal drugs while armed	3,881	0
Trafficking in stolen property	2,250	79
Sale/manufacturing/delivery of cocaine	2,170	110
Burglary to an occupied structure	1,871	400
Grand theft of up to \$5,000	1,023	27
Drug trafficking (4-14 grams)	924	69
Fleeing from a law enforcement officer with no regard	865	46
Possession of cocaine	864	15
Possession of a control substance/other*	836	30
Trafficking of cocaine up to 200 grams	835	61
ADDITIONAL “NONVIOLENT” CRIMES ELIGIBLE FOR EARLY RELEASE		
Sex offender failing to comply PSIA	701	51
Selling/manufacturing/delivering a controlled substance within 1,000 feet of a school	700	105
Possession of child pornography	635	118
Leaving a crash scene involving a death	164	38
4th or subsequent felony DUI conviction	146	18
Promoting a sexual performance by a child	107	16

*Includes any controlled substance that is not GHB, MDMA, Cocaine, Heroin, Opium or Marijuana

¹ Analysis of Florida Department of Corrections, Nov, 2019 data.

FLORIDA'S PRISONS ARE NOT FULL OF FIRST-TIME DRUG OFFENDERS

Drug crime is not what's driving Florida's prison population. It's violent crime and repeat offenders. For all the attention we pay to people convicted of drug crimes, they make up only 13 percent of the prison population. Over half (60%) the people convicted for the first time have been convicted of a violent crime; more than half of those convicted of violence—or 40 percent of all people convicted for the first-time—have been convicted of the most serious violent crimes: murder/ manslaughter, sexual battery, robbery, kidnapping, and aggravated assault/battery¹. Repeat offenders account for 95% of the total population of Florida's prisons.

FIRST-TIME OFFENDERS WITH NO PREVIOUS CONVICTIONS

	PRISONERS	% TOTAL POPULATION ²
1. All drug crimes	793	0.83%
a. Trafficking	408	0.43
b. Manufacture/sale/delivery	320	0.33
c. Possession	65	0.0628
2. Marijuana crimes	41	0.042%
a. Trafficking	29	0.030
b. Manufacture/sale/delivery	12	0.012
c. Possession	0	0
3. Cocaine crimes	234	0.24%
a. Trafficking	109	0.43
b. Manufacture/sale/delivery	110	0.33
c. Possession	15	0.0628
4. Heroin crimes	76	0.08%
a. Trafficking	47	0.049
b. Manufacture/sale/delivery	26	0.027
c. Possession	3	0.0031

“REPEAT OFFENDERS ACCOUNT FOR 95% OF THE TOTAL POPULATION OF FLORIDA'S PRISONS.”

¹Adapted from Pfaff, John (Aug. 14, 2019). What Democrats Get Wrong About Prison Reform. Politico. Retrieved from <https://www.politico.com/magazine/story/2019/08/14/what-democrats-get-wrong-about-prison-reform-227623>.

² Percentages based on an analysis of Florida Department of Corrections data of first-time offenders, based on a total prison population of 94,700 as of October, 2019



**“THE 85% TIME-SERVED LAW PASSED
IN 1995...HAS BEEN ASSOCIATED WITH
SIGNIFICANT REDUCTIONS IN THE
LIKELIHOOD OF RECIDIVISM.”**

(BALES, GAES, BLOMBERG, & PATE, 2010).

**“BALES, GAES, BLOMBERG, AND
PATE (2010) FOUND THAT 85%
TIS HAS NOT RESULTED IN LONGER
PRISON SENTENCES OR PRISON
OVERCROWDING IN FLORIDA.”**

EFFECT OF TIS ON CRIME

Following the national trend, Florida has experienced a 60% drop in Total Index Crimes between 1998 and 2018 despite an almost 40% increase in population during the same period (Florida Department of Law Enforcement, 2019b). Violent crime, which accounted for 14.4% of all crime, dropped 39.7% and property crime, which accounted for 85.6% of all crime, decreased 45.2% (Florida Department of Law Enforcement, 2019b). Chart 4 depicts the change in total index crimes over time.

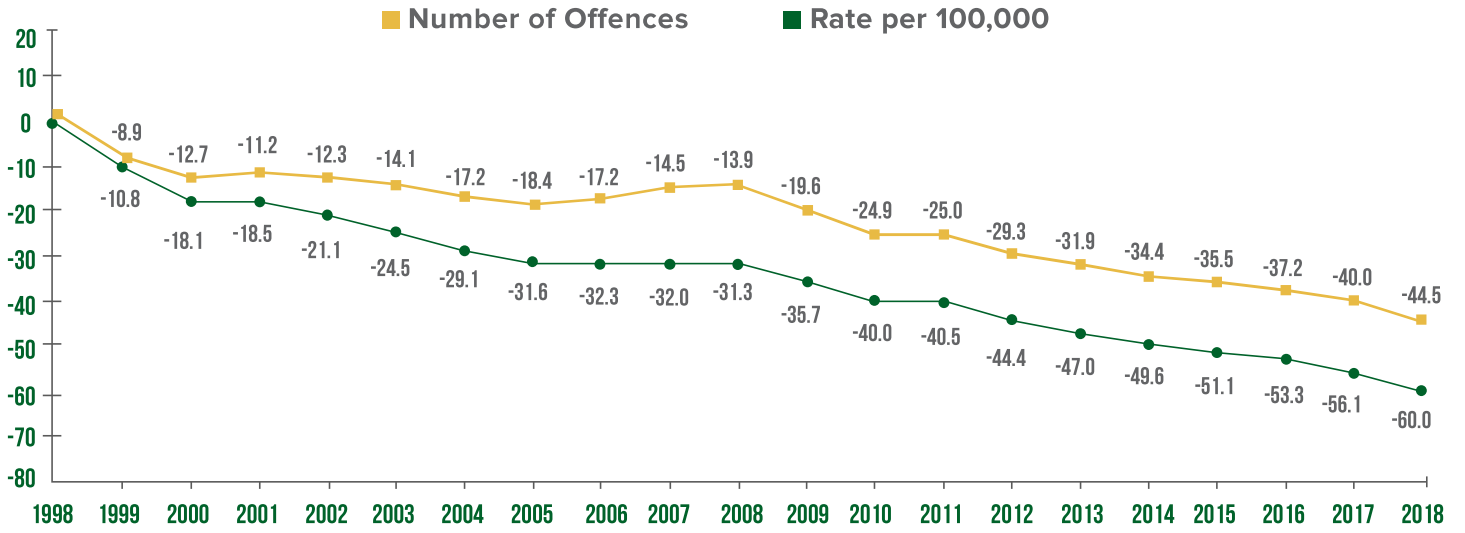
Shepherd (2002) found that TIS laws deter violent offenders and increase the probability of arrest. He concluded the laws decreased murders by 16%, aggravated assaults by 12%, robberies by 24% and rapes by 12%. In an analysis of large, urban areas, Ross (2012) found a decrease in violent crime of 8.7% and a reduction of property crime by 7.1%. Similarly, Long's (2018) analysis of states with TIS laws revealed statistically significant reductions in both violent and property crimes. Murder and robbery rates declined almost immediately, while property crimes were slower to exhibit a significant decline (Long, 2018).

In an analysis of 19 years of state data, Marvell and Moody (1994) found that prison population growth leads to lower crime rates. Steven Levitt (2004), the most prominent scholar in this area of study and the William B. Ogden Distinguished Service Professor of Economics at the University of Chicago, credits the rising prison population with reducing crime through incapacitation and deterrence stating, "the evidence linking increased punishment to lower crime rates is very strong" (p.178). Levitt (2004) found increased incarceration accounted for a reduction in homicide and violent crime of 12% and a reduction in property crime of 8%. In an earlier study that included data from Florida prisons, Levitt (1996) similarly reported an increase in prison population substantially reduces violent crime by 10% and property crime by 7%. Additionally, Levitt (1996) concluded "increased prison populations appear to substantially reduce crime" (p. 348) citing the addition of one prisoner eliminates approximately 15 crimes per year. Marvell and Moody (1994) observed that incarceration averts 17 index crimes per prisoner. Spelman (2000) observed doubling the prison population would reduce crime between 20 and 40 percent.

Between 1998 and 2018

- Florida has experienced a **60%** drop in Total Index Crime despite an almost **40%** population increase
- Violent crime decreased **39.7%**
- Property crime decreased **45.2%**
- Truth in Sentencing laws decreased murders by **16%**, aggravated assaults by **12%**, robberies by **24%**, and rapes by **12%**

CHANGE IN TOTAL INDEX CRIME IN FLORIDA OVER TIME (CHART 4)



CONCLUSION

Determinant sentencing and 85% Truth in Sentencing are used in most states. The extant literature on the effects of Truth in Sentencing is mixed. Several empirical studies suggest Truth in Sentencing reduces recidivism (Pate, 2010; Bales, Gaes, Blomberg, & Pate, 2010) and crime (Shepherd, 2002; Ross, 2012), while others find questionable (Mears, Cochran, Bales, & Bhati, 2016) or opposite effects (Cochran, Mears, & Bales, 2014). Still others suggest longer prison sentences reduce crime (Marvell & Moody, 1994; Levitt, 1996; Spelman, 2000; Levitt, 2004). Finally, Florida prisons house mostly violent offenders (58%). From 2015 to 2018, more nonviolent offenders were released from Florida prisons than were committed. While many factors influence recidivism and crime, Florida's crime rate has dropped for two decades, and both jail and prison populations have declined.

Despite being third-largest state in the U.S., with an additional annual tourist population of almost 125 million visitors (VisitFlorida, 2019), Florida's crime rate and incarceration rate are significantly lower than many smaller states. The notion that Florida's prisons are overcrowded with first-time, nonviolent offenders is not reflected in the data. Many nonviolent, felony offenders are diverted from prison through the statutorily created Pre-Trial Intervention program and other treatment-based diversion programs. Additionally, while some offenders are incarcerated for nonviolent crimes, aggravating factors, such as previous offenses, likely have resulted in a prison sentence. The data could not be clearer: Florida's 85% truth in sentencing laws coupled with the state's incarceration model has resulted in a lower crime rate and lower incarceration rate than many other states.

“THE DATA COULD NOT BE CLEARER: FLORIDA’S 85% TRUTH IN SENTENCING LAWS COUPLED WITH THE STATE’S INCARCERATION MODEL HAS RESULTED IN A LOWER CRIME RATE AND LOWER INCARCERATION RATE THAN MANY OTHER STATES.”

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FINAL REPORT

The Impact of Illinois' Truth-in-Sentencing Law on Sentence Lengths, Time to Serve and Disciplinary Incidents of Convicted Murderers and Sex Offenders

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Executive Summary

Truth-in-sentencing (TIS) policies require those convicted and sentenced to prison to serve at least 85 percent of their court-imposed sentence, and often results in inmates serving longer periods of incarceration. Although the move to increase sentence lengths and time served for violent offenders through TIS types of laws in the United States began in the late 1980s, the federal TIS legislation passed in 1994 and the federal TIS Incentive Grant Program initiated in 1996 were associated with many states adopting laws that required those sentenced to prison for various violent crimes to serve at least 85 percent of their sentence (Rosich & Kane, 2005). Although most criminal justice practitioners, and convicted offenders, were well aware that most inmates only served a fraction—35 to 50 percent—of their sentence, the increasingly punitive sentiment during the early 1990s, associated with a relatively high rate of violent crime, brought the issue of this disparity between sentences imposed and time served to light and prompted the increased passage of TIS laws. Following the passage of the federal TIS grant program, Illinois formed a Truth-in-Sentencing Commission to examine Illinois' current sentencing policies and determined that the state should adopt its own version of TIS. Illinois adopted its version of TIS in August 1995 (State Fiscal Year 1996), which requires those convicted of murder to serve 100 percent of their sentence, those convicted of criminal sexual assault to serve at least 85 percent of their sentence, and those sentenced to prison for other violent crimes involving great bodily harm to also serve at least 85 percent of their sentence. Prior to the implementation of TIS in Illinois, those sentenced to prison for murder and criminal sexual assault served, on average, less than 40 percent of their sentences as a result of the various Good Conduct Credit (GCC), Meritorious Good Time (MGT), and Supplemental Meritorious Good Time (SMGT) reductions (Illinois Criminal Justice Information Authority, 1994).

However, the proposed implementation of TIS in Illinois was also met with some concern and criticism, including: 1) the potential for increased inmate assaults and rule violations due to fewer incentives to behave, 2) an overly burdensome financial impact if the law actually resulted in inmates spending more time in prison than before TIS, or 3) no change whatsoever in the amount of time served in prison due to criminal justice practitioners (judges, prosecutors and defense attorneys) merely reducing sentences proportionally, thereby resulting in those sentenced to prison still serving the same time behind bars (Illinois Criminal Justice Information Authority, 1994:2). Concerns raised by the Illinois Department of Corrections (IDOC) included the fiscal impact if the law resulted in inmates actually serving longer sentences, as well as concerns regarding the behavior of inmates and the safety of staff if those sentenced to prison for serious crimes had no incentive (i.e., good conduct credit) to follow institutional rules. On the other hand, many argued that the fiscal impact projections by the IDOC were exaggerated and the logic of longer lengths of stay in prison flawed. TIS opponents argued that the actual length of time inmates spent in prison would remain the same if judges adjusted their sentences to be consistent with the amount of time offenders served before TIS was implemented. After 15 years of actual experience with TIS in Illinois, and thousands of offenders being sentenced under the law, the current study sought to answer two of the key questions regarding the implementation and impact of Illinois' TIS law as it pertains to convicted murderers and sex offenders: 1) has TIS changed the sentence lengths and lengths of time to serve in prison for murderers and sex offenders, and if so, to what degree, and 2) has TIS had an influence on the extent and nature of disciplinary infractions of inmates admitted to prison for murder and sex offenses subject to the law, and if so, to what degree.

Impact of TIS on Sentences & Lengths of Time to Serve for Murderers

The first set of analyses sought to merely examine the overall pattern of sentences imposed on those convicted of murder in Illinois between SFY 1989 and 2008, including analyses of the mean and median sentences imposed (for determinate sentences), and the proportion of offenders that received sentences beyond the statutory maximum of 60 years. From these analyses, a number of patterns were evident that have implications for understanding the potential impact of TIS on murder sentences. First, using multivariate analyses that statistically controlled for the influence of age, race, gender, marital status, education level, if the inmate has children, gang membership, prior criminal history, and jurisdiction of sentencing (independent variables) on the sentence lengths (dependent variable), we found that TIS was associated with, on average, a 3.9 year *reduction* in the mean *sentence* length of those that received determinate sentences (i.e., excluding natural life or death sentences). In other words, once you statistically take into account the effects of the independent variables and adjust for the pre-TIS trend in sentencing, TIS resulted in a decrease of 3.9 years on the average *sentence* imposed on murderers, or about a 10 percent reduction in sentence lengths. However, a more practical way to examine the impact of TIS is by considering the *actual amount of time* that will be required to be served by those convicted of murder. Substituting the time to serve for the sentence imposed reveals that those subject to TIS are expected, on average, to serve 17 years *more* in prison than those not subject to TIS after statistically controlling for the other variables in the analyses. Thus, while TIS did reduce the length of *sentences* imposed on convicted murderers *to some degree*, the decrease was nowhere near what some believed it would be (i.e., that sentences would be cut nearly in half to account for the fact that 100 percent will be served under TIS as opposed to the 50 percent served under the old law).

The next set of analyses sought to determine if the TIS law was associated with any change in the likelihood that a convicted murderer would receive a sentence beyond the statutory maximum of 60 years. Under Illinois law, a sentence beyond the statutory maximum, or a sentence of natural life or death in the case of murder, can be sought when specific, aggravating circumstances are present. Again using multivariate analyses to statistically control for other factors, a generally consistent pattern emerged: those subject to TIS were *less likely* to receive a sentence beyond 60 years (relative to the pre-TIS group as well as relative to those convicted during the same time period but not subject to the law). Specifically, those subject to TIS were 57 percent *less likely* to receive a sentence of more than 60 years (including natural life or death sentences) than those sentenced prior to TIS. Looked at another way, roughly 25 percent of convicted murderers not subject to TIS received a sentence beyond the statutory maximum of 60 years, compared to 17 percent of those subject to TIS.

Taking these findings into account, our analyses regarding the impact of TIS on murder sentences revealed two substantial findings, which have considerable implications: 1) the average determinate sentence imposed on convicted murderers was reduced *only slightly* as a result of TIS, resulting in offenders serving much longer periods of time in prison, and 2) TIS appears to have reduced the use (or need) to impose sentences beyond the statutory maximum of 60 years. Thus, the passage of TIS has dramatically increased the *actual amount* of time those convicted of murder will spend in prison, and as a result, the cost per murder sentence imposed in Illinois dramatically increased as a result of TIS. In addition to longer periods of incarceration, and therefore higher costs, a much larger proportion of convicted murderers in Illinois will now serve the rest of their life in prison, despite the fact that the actual imposition of natural life sentences has been reduced due to TIS. Because the

lengths of time to serve in prison increased so much as a result of TIS, it is projected that 30 percent (886 of the 3,000 murderers sentenced under TIS) of all inmates convicted of murder and subject to TIS will not be eligible for release until after their 75th birthday--the average life expectancy of males in the United States. However, while 30 percent of murderers sentenced under TIS received sentences that will result in them most likely spending the rest of their life in prison, only a small proportion of these were explicit “natural life” or “death” sentences. Of all the murderers sentenced under TIS and projected to be in prison beyond their 75th birthday, only 13 percent had a “natural life” or “death” sentence imposed by the court. By comparison, pre-TIS, only 15 percent of all convicted murderers received a sentence that would keep them in prison beyond their 75th birthday, and almost all of these (90 percent) were court-imposed sentences of “natural life” or “death.”

Impact of TIS on Sentences & Lengths of Time to Serve for Class X Sex Offenders

Analyses similar to those performed to examine the impact of TIS on murder sentences were performed to examine the impact of TIS on the sentence lengths of those convicted of aggravated criminal sexual assault. The average sentence imposed on Class X sex offenders pre-TIS was 13.5 years. Using multivariate statistical analyses similar to those in the murder analyses, we found that TIS was associated with a slight *reduction* in the mean sentence length by approximately 6 months. Thus, the impact of TIS on the sentence lengths and lengths of time to serve for sex offenders is somewhat similar to that seen with convicted murderers, although to a lesser degree due to the sentence lengths involved. Still, as a result of TIS, convicted Class X sex offenders are now serving substantially longer periods of incarceration than they did pre-TIS. On average, those sex offenders subject to TIS will serve an average of 9.7 years in prison, compared to the roughly 6.2 years those sentenced prior to TIS served.

Impact of TIS on Disciplinary Incidents and Sanctions for Murderers

To determine if TIS had an impact on the extent and nature of disciplinary incidents among murderers in Illinois, data were obtained that allowed for the tracking of disciplinary incidents for a cohort of murderers admitted to prison in Illinois between July 1999 and June 2001. Disciplinary data included those recorded through March 2008, so the average amount of time inmates were at risk of having a disciplinary incident was 8 years. Within the sample of murderers tracked were 300 inmates subject to TIS and 550 that were eligible for day-for-day good conduct credit. The specific disciplinary outcomes examined included: total number of disciplinary tickets, any serious incident (yes or no), any assaults (yes or no), assaults of staff (yes or no), and assaults of other inmates (yes or no).

Among the overall sample of murderers included in the analyses of disciplinary incidents, the average number of disciplinary tickets was 22. When multivariate statistical analyses were performed to isolate the influence of TIS on the overall number of disciplinary incidents/tickets, the analyses revealed that murderers subject to TIS *receiving an average of almost 5 fewer tickets*, on average, than non-TIS inmates. Additional analyses revealed that TIS had no statistical relationship with whether or not the inmate received a disciplinary ticket for a *serious* incident: roughly 55 percent of both TIS and non-TIS murderers had a ticket for a serious incident. Serious incidents were defined as any offenses that carry a maximum penalty of one year of loss or restriction of privileges, grade reduction, good time revocation and/or segregation, and included offenses in such

as violent assaults or participation in a security threat group. Similarly, TIS had no statistical relationship with whether or not the inmate received a disciplinary ticket for an assault, against either another inmate or staff, with roughly 19 percent of both TIS and non-TIS murderers receiving a ticket for any assault. Analyses were also performed to determine if an inmate's TIS status had any influence on the types of sanctions imposed by IDOC for disciplinary incidents. Among the sample of murderers, the most prevalent form of sanction imposed on those receiving disciplinary tickets was loss of commissary privileges, and no statistical difference was noted between those murderers subject to TIS and those not: roughly 90 percent of both groups lost their commissary privileges for a period of time as a result of a disciplinary incident. For the other forms of punishment, including placement in segregation, loss of gym/yard privileges, and loss of good conduct credit, no statistical differences were identified based on the inmates' TIS status.

Thus, based on these analyses, there were few differences between those murderers subject to TIS and those not subject to TIS in terms of their disciplinary records, particularly in terms of assaults on staff and other inmates.

Impact of TIS on Disciplinary Incidents and Sanctions for Class X Sex Offenders

Similar analyses were performed to determine if TIS had an impact on the extent and nature of disciplinary incidents among Class X sex offenders in Illinois, with data obtained that allowed for the tracking of disciplinary incidents for a cohort of sex offenders admitted to prison in Illinois between July 1999 and June 2001. Disciplinary data included that recorded through March 2008, so the average amount of time inmates were at risk of having a disciplinary incident was 8 years. Within the sample of sex offenders tracked were 806 inmates subject to TIS and 599 that were eligible for day-for-day good conduct credit. Overall, the analyses showed no statistical difference in the average number of disciplinary tickets between Class X sex offenders subject to TIS and those not subject to TIS (an average of roughly 22 incidents for each group). As with the analyses of convicted murderers, among the Class X sex offenders there was no statistical relationship between whether they were subject to TIS whether or not the inmate received a disciplinary ticket for a serious offense, including assaults against staff or other inmates, and most forms of sanction imposed on disciplinary incidents.

Conclusions

As a result of the examination of sentences imposed, time to serve, and disciplinary incidents, the following general conclusions were reached. First, the length of court-imposed sentences changed very little as a result of Illinois' Truth-in-Sentencing law, and as a result, the length of time *to be served* by those convicted of murder and aggravated criminal sexual assault in Illinois has increased dramatically. For those convicted of murder, these increased lengths of time to serve has resulted in a much larger proportion of these offenders that will not be eligible for release until after their 75th birthday. Although the length of time to serve among sex offenders has also increased, because the sentence lengths are not as long as those imposed on murderers, the impact of TIS on the projected age of offenders at release did not change as substantially. Thus, while many believed that sentence lengths under TIS would change (be reduced) dramatically to take into account the fact that a larger proportion of the sentence would be served, this has not occurred, and those sentenced under TIS are serving up to twice as long in prison as they did prior to TIS. Second, TIS does not appear to have

had any influence over the extent and nature of disciplinary incidents of murderers and sex offenders in Illinois' prisons. Prior to TIS there were concerns that inmates with little opportunity to earn good conduct credit would engage in more, and more serious, disciplinary incidents. Our analyses revealed that this has not occurred, with inmates subject to TIS having similar patterns of disciplinary infractions as those not subject to TIS.

INTRODUCTION

Truth-in-sentencing, or TIS, reflects the public policy designed to address what many see as a misleading disparity between the sentences imposed on convicted felons and the actual amount of time they serve in prison. As a result of prison inmates being eligible to receive good-conduct credit and other sentence reductions, those sentenced to prison in the United States and released during 1990 were found to have served, on average, only 38 percent of their court-imposed sentence, although the proportion of time served was slightly higher for those convicted of murder (43 percent) and rape (45 percent) (Ditton & Wilson, 1999: 8). Part of this disparity between sentence length and amount of time actually served is the result of statutory provisions that allow prison officials to give inmates good conduct credit, usually one day off of their sentence for each day they follow prison rules (i.e., day-for-day good conduct credit). Many point to this provision as an important behavior management tool correctional administrators use to provide inmates with an incentive to follow rules.

In addition, some states, including Illinois, have allowed correctional officials to further reduce lengths of stay in prison through additional types of good conduct credits, such as Meritorious Good Time (MGT) and Supplemental Meritorious Good Time (SMGT). In Illinois, MGT and SMGT were primarily put in place in response to prison crowding conditions and the need to reduce prison populations (Illinois Task Force on Crime and Corrections, 1993), and allowed most inmates to receive an additional 180 days off of their sentence. In addition, Illinois also created a provision designed as an incentive for prison inmates to participate in rehabilitative programming, known as Earned Good Conduct Credit (EGCC), whereby inmates could earn an additional one-half day off their sentence for each day they participated in drug treatment and vocational training. Thus, the ability of correctional administrators to reduce the actual amount of time served for court imposed prison sentences was seen not only as an effective tool for increasing inmate compliance with rules and allowing for early release to control prison populations, but also, in the case of EGCC, providing inmates with an incentive to rehabilitate themselves through program participation. Evidence that inmates are motivated by these types of incentives, such as EGCC, have been documented in the ongoing evaluation of the Illinois Department of Corrections' Sheridan Correctional Center (Olson, 2005), which found inmates earning EGCC were more likely to comply with program requirements

and complete the program successfully than those not eligible for EGCC.

Despite the fact that most criminal justice practitioners, and convicted offenders, were well aware that most inmates only served a fraction of their sentence, the increasingly punitive sentiment during the early 1990s, associated with a relatively high rate of violent crime, brought the issue of this disparity between sentences imposed and time served to light. Further, although the move to increase sentence lengths and time served for violent offenders through Truth-in-Sentencing (TIS) types of laws in the United States began in the late 1980s, the federal TIS legislation passed in 1994 and the federal TIS Incentive Grant Program initiated in 1996 were associated with many states adopting laws that required those sentenced to prison for various violent crimes to serve at least 85 percent of their sentence (Rosich & Kane, 2005). Illinois adopted its version of TIS in August 1995 (State Fiscal Year 1996), which requires those convicted of murder to serve 100 percent of their sentence, those convicted of criminal sexual assault to serve at least 85 percent of their sentence, and those sentenced to prison for other violent crimes involving great bodily harm to also serve at least 85 percent of their sentence. Prior to the implementation of TIS in Illinois, those sentenced to prison for murder and criminal sexual assault served, on average, less than 40 percent of their sentences as a result of the various GCC, MGT, and SMGT reductions (Illinois Criminal Justice Information Authority, 1994).

The support for TIS in Illinois and elsewhere in the country came from the belief that the law would achieve crime reduction and increase public satisfaction with the criminal justice system. In Illinois, supporters believed crime rates would be reduced by inmates being kept off the streets for longer periods of time, and thereby being released at an older age, which was correlated with lower recidivism rates (Illinois Criminal Justice Information Authority, 1994). This long-term reduction in crime, it was argued, would result in lower expenditures by the components of the criminal justice system (Illinois Criminal Justice Information Authority, 1994:2). This logic of reduced crime as a result of longer sentences and deterrence was also argued in other states, and in Virginia these assumptions were examined by researchers and found to be “conceptually sound and conservative” (Ostrom, Cheesman, Jones, Peterson & Kauder, 2001:2). In addition, proponents felt that the “integrity of the criminal justice system would be strengthened” (Illinois Criminal Justice Information Authority, 1994:2) since large differences between the imposed sentences and actual

time served “breed disrespect for the system among offenders, victims and the general public” (Illinois Criminal Justice Information Authority, 1994:2). Through TIS, it was believed, this disparity would be eliminated and public satisfaction with the justice system would be enhanced.

However, the proposed implementation of TIS was also met with some concern and criticism, including: 1) the potential for increased inmate assaults and rule violations due to fewer incentives to behave, 2) an overly burdensome financial impact if the result actually resulted in inmates spending more time in prison than before TIS, or 3) no change whatsoever in the amount of time served in prison due to criminal justice practitioners (judges, prosecutors and defense attorneys) merely reducing sentences proportionally, thereby resulting in those sentenced to prison still serving the same time behind bars (Illinois Criminal Justice Information Authority, 1994:2). Concerns raised by the Illinois Department of Corrections (IDOC) included the fiscal impact if the law resulted in inmates actually serving longer sentences, as well as concerns regarding the behavior of inmates and the safety of staff if those sentenced to prison for serious crimes had no incentive (i.e., good conduct credit) to follow institutional rules. In terms of cost, IDOC projections in 1998 on the cost to expand TIS to all non-probationable violent felonies exceeded \$500 million over ten years (Illinois Truth-in-Sentencing Commission, 1998:25). On the other hand, many argued that the fiscal impact projections by the IDOC were exaggerated and the logic of longer lengths of stay in prison was flawed. TIS opponents argued that the actual length of time inmates spent in prison would remain the same if judges adjusted their sentences to be consistent with the amount of time offenders served before TIS was implemented. Despite these concerns, TIS in Illinois was implemented in 1995.¹

As described earlier, it was projected by policy makers and practitioners that this law could result in either longer lengths of incarceration, if sentencing practices did not change, or similar lengths of time served, but higher proportions of the court-imposed sentences being served, if sentence lengths

¹ Although passed and signed into law in 1995 (with an effective date of August 1, 1995), a legal challenge to the law was quickly filed challenging the legislative procedure used to pass the TIS law. Concern over this challenge led the Illinois legislature to re-pass a new version of the law, ensuring procedural processes were followed, and the new version of TIS was signed into law with an effective date of June 18, 1998 (State Fiscal Year 1998). During this period between the first version being passed and in effect and the new version being passed (August 1995 to June 1998), offenders were being sentenced under the original version of TIS. The Illinois Supreme Court ultimately ruled the original TIS law unconstitutional (the one effective August 1995) due to it violating the single subject rule for legislation. As a result of the original law being overturned, all those sentenced under the old law had their sentences automatically changed to allow them to earn good conduct credit similar to that pre-TIS.

were adjusted downward to take into account the effect of TIS. For those sentenced to prison under TIS, they must serve between 85 and 100 percent of their court-imposed sentence, and thus, it was argued by some that the law would reduce crime by incarcerating serious offenders for a longer period of time. The two most serious types of criminals included under this dimension of the law are murderers (who must serve 100 percent of their sentence) and Class X felony sex offenders (those convicted of aggravated criminal sexual assault and predatory criminal sexual assault who must serve 85 percent of their sentence). For those convicted of murder, state law requires that they receive a prison sentence of between 20 and 60 years, or when specific aggravating circumstances are present, a determinate sentence of more than 60 years is allowable, as is a sentence of life in prison or the death penalty. Prior to the implementation of TIS in Illinois (i.e., 1995), the average prison *sentence* length for those convicted of murder was 35 years and the projected average amount of time served for those offenders was slightly less than 17 years (Illinois Department of Corrections, 2001). Thus, depending on how sentence lengths are influenced by the prospect of TIS, the end result could be longer lengths of time served in prison (which is what proponents of the law hoping to incapacitate offenders longer argued would be the benefit), the same lengths of time served in prison (which is what opponents argued would happen as a result of changing sentencing practices), or somewhere in between.

However, after 15 years of actual experience with TIS in Illinois, and thousands of offenders being sentenced under the law, relatively little research regarding the implementation, impact or characteristics of those sentenced under TIS in Illinois has been conducted, and nationally these assessments have been limited. The exception to this in Illinois is a brief summary included in the Illinois Department of Corrections' Annual Statistical Presentation, which provides information on how many inmates are serving sentences under TIS, the average time they have served, and the amount of time left to serve (Illinois Department of Corrections, 2005). Thus, despite the concerns raised by many leading up to the passage of the law, and the potential impact the law was projected to have from fiscal and staff safety perspectives, relatively little systematic assessment of the law has occurred. The current research is designed to fill this gap and represents the only effort to systematically examine the impact that TIS in Illinois has had on sentence lengths, lengths of time to serve, and inmate disciplinary incidents.

Thus, the current study seeks to answer two of the key questions regarding the implementation of Illinois' TIS law as it pertains to convicted murderers and sex offenders: 1) has TIS changed the sentence lengths and lengths of time to serve in prison for murderers and sex offenders, and if so, to what degree, and 2) has TIS had an influence on the extent and nature of disciplinary infractions of inmates admitted to prison for murder and sex offenses subject to the law, and if so, to what degree.

LITERATURE REVIEW

Although truth-in-sentencing types of policies were implemented in many states during the 1980s, it was not until the federal law was passed during the 1990s that the implementation and impact of the law received much attention from researchers. Further, given that many states did not implement the TIS law until the 1990s, many analyses done during that time were premature and were not able to determine the full impact on sentencing and inmate behaviors due to little time elapsing between implementation and evaluation. For the most part, the literature can be divided into three categories: 1) assessments that attempted to examine, at a national level, the impact of TIS on sentencing practices and prison management issues, 2) research designed to assess the implementation and impact of TIS in specific states, and 3) theoretical and philosophical discussions on TIS from the standpoint of being overly punitive, reducing judicial discretion, and equity in sentencing. Although important, this latter area regarding the theoretical and philosophical implications of TIS is not examined in the current report. Despite the considerable change that TIS potentially had on sentencing practices, lengths of time served, and inmate management issues, the literature to date on the implementation and impact of TIS is relatively sparse.

In general, the research that has sought to answer the question regarding how TIS has changed sentencing practices is mixed, and appears to vary from state to state depending on how the state's sentencing laws were structured. For example, in an evaluation of the implementation of TIS in Massachusetts, researchers found very little change in actual *sentences* imposed in the pre- versus post-TIS sentences, but a measurable increase in the *projected length of time to serve* (Massachusetts Sentencing Commission, 2000). In Virginia, evaluators discovered that *sentences* for non-violent offenders were lower, but actual time served *remained the same* under TIS. On the other hand, for violent offenders, both sentence lengths *and* projected time to serve *increased* (Ostrom, Cheesman, Jones, Peterson & Kauder, 2001). In examining TIS in Mississippi, researchers concluded that the response by the court community to TIS has been to adjust sentences to maintain the historic "proportionality in punishment," or same average number of years served in prison. Moreover, they found considerable variation in the enforcement of TIS geographically across the state (Wood & Dunaway, 2003). Thus, in some of the evaluations it was determined that TIS was indeed associated with longer lengths of time in prison, while others found no impact, either due to deliberate changes

in the sentencing structures (i.e., Virginia for non-violent offenders) or changing practices by courtroom practitioners when it came to sentencing, as in the case of Mississippi.

There has also been some limited research that has sought to examine the impact of TIS on changes in the overall size of the prison population. For example, a 2002 study conducted by Sabol, et al. (2002) was concerned with the overall effects of TIS on the prison population size in seven different jurisdictions, one of which was Illinois. In answering one of their research questions, they found that TIS reform had a larger impact on prison populations than did other factors, such as changes in demographic characteristics of offenders or states and the types of offenses resulting in prison sentences.

Given the fact that the impact of the law appears to vary from state to state, depending on the offenses covered under TIS and the overall sentencing structure and/or courtroom culture in place, it is clear that analyses need to be done on a state-by-state basis to take into account the nuances of each state's TIS law and sentencing structure to assess impact on sentence lengths and/or lengths of time to serve. Indeed, it may also be important to examine the impact of TIS across different types of jurisdictions within the same state, as there is evidence from Illinois that the application and use of certain types of criminal sentences—ranging from capital punishment to the imposition of fines-- vary between Cook County (Chicago), suburban and downstate urban counties and rural jurisdictions. For example, research in Illinois by Pierce & Radelet (2002) found greater use of death sentences in rural jurisdictions than more urban areas after statistically controlling for other variables. Similarly, Olson & Ramker (2001) found the odds of having financial conditions ordered as part of probation sentences were higher in rural jurisdictions in Illinois than more urban counties after statistically controlling for other variables.

Similarly, when researchers sought to examine the degree to which TIS has produced management issues for prisons associated with the hypothesized reduction in good conduct incentives, the findings are mixed and limited. For example, in a national study completed in 2003, Turner, Hickman, Green & Fain found some evidence that TIS was associated with higher levels of prison management *concerns*. However, their conclusions are tentative due to the aggregation at a national level, the data only being collected shortly after the implementation of TIS, and the fact that they

were asking administrators about their *perceptions* rather than having actual data available regarding increased incidents or disciplinary problems associated with the law. However, empirical evidence indicating the adverse impact of TIS on disciplinary incidents is evident from research conducted in two states. In separate studies regarding the impact of TIS on inmate behaviors in South Carolina and North Carolina researchers found that those sentenced under TIS did have more behavioral problems and violated prison rules at higher rates and more quickly than did similar inmates admitted to prison sentenced under the old, non-TIS law (North Carolina Criminal Justice Analysis Center, 1998; Fowler et. al., 2002). Specifically, in North Carolina, using multivariate techniques (Cox Regression and Negative Binomial Regression) and controlling for various inmate characteristics, researchers found that those sentenced under North Carolina’s version of Truth-in-Sentencing (referred to as the Structured Sentencing Act, or SSA) had a weighted disciplinary violation rate almost 20 percent higher than the non-Truth-in-Sentencing inmates (North Carolina Criminal Justice Analysis Center, 1998). Further, those sentenced under North Carolina’s version of TIS also had much higher rate for assaultive offenses within the institutions—71 percent higher—than those sentenced under the “old” law (Ibid). Similar findings were also reached when researchers examined the impact of TIS on inmate behavior in South Carolina. Specifically, Fowler et. al. (2002) found that inmates sentenced under South Carolina’s version of TIS were more likely to have disciplinary infractions than those not sentenced under TIS, and that the time-to-infraction was much shorter for the TIS versus the non-TIS inmates, after statistically controlling for offense type, age, length of time to serve and admission date.

In addition to there being some limited research that has examined the relationship between TIS laws and inmate behaviors, there is a much more extensive body of literature that has examined other correlates of inmate disciplinary incidents. For example, some of the extant research on inmate misbehavior comes from that done in other specific states and has tended to focus on comparing the behavior of long term inmates with that of short term inmates. However, there is little or no regard for the TIS status of inmates. For instance, a study conducted by Cunningham, Sorensen and Reidy (2005) in a Missouri state prison sought to determine which risk factors best predict assaultive violence compared across three categories of prisoners: term inmates (inmates serving a specific length of time), inmates serving life sentences, and inmates sentenced to death. Their findings noted that as inmates get older, they are less likely to exhibit assaultive violence. Interestingly, they found

that inmates sentenced to life were 51 percent *less likely* than term inmates to commit a violent assault, and that death-sentenced inmates were 45 percent *less likely* to commit a violent assault while in prison relative to inmates serving a specific term. Thus, while it would appear from these findings that those inmates “with nothing to lose” (i.e., those sentenced to life or death) were less likely to be assaultive, it is important to note that those sentenced to life in the Cunningham et. al (2005) study still had a chance at release by way of parole, and thus may have been less likely to be assaultive given this hope of release.

In a similar study that sought to gauge the effects that “no hope of release” would have on inmates, Sorensen and Wrinkle (1996) conducted a similar study, also in Missouri, comparing the assaultive violence incidents of death-sentenced and life without parole (LWOP) inmates to inmates serving life with the possibility of parole. Using bivariate statistical analyses, Sorensen and Wrinkle found that LWOP inmates were significantly less likely to receive disciplinary infractions for assaultive behavior than inmates serving life with parole. However, in multivariate analyses, these distinctions did not hold up. Their multivariate models found that type of sentence (death, LWOP, or life with parole) did not contribute significantly to the prediction of disciplinary violations, or assaultive behavior. They did, however, find a curvilinear distribution of disciplinary infractions for LWOP inmates, suggesting that the prevalence of disciplinary infractions is high at the beginning of a long sentence, hits a peak in this early period, and then declines once an LWOP inmate settles in to the prison routine. Separate analyses of these data collected in Missouri by Sorensen, Wrinkle and Gutierrez (1998) noted that the highest risk inmates for assaultive offenses were most likely to be young and African-American. Consistent with Sorensen and Wrinkle (1996), their results showed that inmates with no hope of parole do not represent a greater threat to the security of prisons. Instead, they found that more attention should be paid to inmates who do have a chance at release, as their rates of misconduct are higher.

A more recent study in Florida by Cunningham and Sorensen (2006) looked at the same types of inmates by sentence, but also included some TIS offenders in the sample serving at least 85 percent of their sentence. In the study, Cunningham and Sorensen (2006) came to similar conclusions as the previously noted studies. Generally, they found that inmates serving less than 20 years had the highest rates of assaultive behavior. More specifically, they found that shorter-term inmates (10-14

years) had the highest rates of assaultive behavior, followed by inmates serving 15-19 years. Longer-term inmates, especially LWOP inmates had much lower rates of assaultive behaviors. There are some caveats with this study, however. It should be noted that some of the longer-term inmates may have been transferred to lower security classifications, leaving a cohort that was more violent to begin with.

Finally, Berk, Kriegler and Baek (2006) conducted a study in California in order to create a model to forecast which inmates would be most likely to engage in serious misconduct. Using multivariate techniques, the study found the length of sentence to be the biggest predictive factor for serious misconduct, followed by age at first arrest and gang affiliation. Consistent with the studies mentioned above, inmates serving *shorter* sentences (6-10 years in this case) were much *more likely* to be involved in serious misconduct. As well, Berk et al. (2006) also found that younger inmates were much more likely to be involved in serious misconduct.

Thus, while some research has sought to examine the implementation and impact of TIS in the U.S. and across specific, individual states, the findings appear to be limited and parochial. What is available, however, suggests that these types of analyses need to be carried out on a state-by-state basis in order to accurately assess impact and account for unique aspects of TIS laws across the states, and the degree to which courtroom personnel have the capacity to negate the intentions of the law through the use of their discretion when it comes to sentencing. Similarly, in terms of the impact of TIS on institutional behaviors of inmates, there have only been a handful of studies that have examined this dimension of the policy. Although the two studies cited here from North and South Carolina would appear to support the notion that TIS has increased the frequency and nature of inmate disciplinary incidents, the populations subject to TIS in those states appears to be quite broad (i.e., all violent offenders), and therefore requires replication in other states and with more specific offenses targeted under the TIS laws. Further, the existing literature on inmate disciplinary patterns, particularly for those convicted of murder, appears to suggest that longer lengths of time to serve may actually *reduce* the incidence and nature of institutional violence by inmates, and that other inmate characteristics, such as age, need to be statistically controlled.

METHODOLOGY

The methodology that was used to examine TIS in Illinois builds upon the methods used in TIS assessments in other states, and also advances the knowledge due to the unique nature of Illinois' law and its implementation. Given the limited examination of TIS in Illinois, and the fact that each year new crimes are considered for inclusion under the TIS sentencing provisions, it is hoped that this formal, independent evaluation of the impact of Illinois' Truth-in-Sentencing law can inform and guide future policy and practice in Illinois. Specifically, the current research sought to answer the following two research questions:

- 1) Has the implementation of Truth-in-Sentencing affected sentence lengths/projected lengths of time to serve among those sentenced to prison in Illinois for murder and Class X sex offenses (i.e., aggravated criminal sexual assault and predatory criminal sexual assault), and if so, how?
- 2) Have those sentenced under Truth-in-Sentencing for murder and Class X sex offenses been more likely to be involved in disciplinary incidents, particularly for assaultive behaviors, than those not subject to Truth-in-Sentencing, and if so, how?

Data Sources

All of the data used in the current analyses were provided to the research team by the Illinois Department of Corrections (IDOC), and come from administrative records collected and maintained by IDOC during the normal processing of adult inmates. Among the data provided for the evaluation was offender-level information collected during the admission of the inmate into IDOC (i.e., from IDOC admissions data files), including the inmate's demographic characteristics, marital and education status, gang involvement, current conviction offense and sentence imposed, county of conviction, prior prison sentences, and whether they were subject to TIS or not. These data were provided for every adult inmate admitted to IDOC from State Fiscal Year (SFY) 1989 through SFY 2008. These years cover the period from July 1988 through June 2008. The number of murderers included in these data totaled 9,102 and the number of inmates sentenced for Class X sex offenses totaled 7,150.

In addition to these admissions data, offender-level data were also provided regarding the disciplinary history of a smaller sub-sample of the group described above. Specifically, for those adult inmates admitted to IDOC during SFY 2000 and 2001 for murder (N=849) or aggravated criminal sexual assault (N=641), detailed, case-level data for each disciplinary incident from the date of their admission through March 2008 were obtained, including the date of the incident, a description of the incident, and the sanction imposed for the incident. These data were merged with the data obtained from the admissions files for those inmates included in this sub-sample of inmates selected for the analyses of institutional rule violations.

The way the Illinois' TIS law was written, only those whose crime was *committed* after the effective date of the legislation (August 1995) were subjected to the 85 to 100 percent requirement. Thus, given how long some of the more serious crimes take to adjudicate, during the period after the effective date of the TIS law (August 1995) judges were sentencing some murders and sex offenders under the old law (which allowed for good conduct credit to be earned) and the new TIS law (which eliminated or severely limited the amount of good-conduct credit that could be earned). This unique situation provided for an opportunity to conduct a natural experiment of the effect TIS had on sentence lengths: there is clearly a pre-TIS time period, but there is also a time period where defendants being sentenced for similar crimes and subject to different laws related to the ability to earn good conduct credits.

Methodology Used to Examine Impact of TIS on Sentence Length and Length of Time to Serve

For the first research question, if TIS is actually going to result in inmates sentenced for serious crimes being incarcerated for a longer period of time--the objective of many TIS proponents--then there cannot be dramatic reductions in the sentences imposed in the courts in response to TIS restrictions or plea bargaining involving reduced charges. As described in the literature review, evidence from some states, such as Mississippi, has found that court practitioners, including judges, prosecutors and defense attorneys, have responded to TIS by altering their sentence lengths so that, in the end, the length of time spent in prison under TIS is the same as pre-TIS (Wood and Dunaway, 2003). However, the Woods & Dunaway (2003) conclusions were reached through surveys and interviews with practitioners, not examination of actual data on sentence lengths imposed pre- and

post-TIS. In other states, such as Virginia, *ensuring* that non-violent offenders did not serve longer in prison as a result of TIS was *intentionally and directly addressed* by reducing the sentences allowed under sentencing guidelines for these crimes, while for violent crimes, lengths of time to serve were *increased* (and assured) by an upward shift in sentencing guidelines.

In Illinois, however, given that there are no narrow sentencing guidelines when it comes to murder and Class X sex offenses (murderers can receive a prison sentence of between 20 and 60 years without aggravating circumstances and those convicted of Class X sex offenses can receive 6 to 30 years without additional aggravating circumstances), there is the potential that courtroom personnel could maintain historic lengths of time to serve in prison by adjusting their sentencing practices similar to that purported in Mississippi. For example, the average prison sentence imposed on those convicted of murder in Illinois during 1994 (prior to TIS) was 35 years, and the offender would (without Truth-in-Sentencing) serve roughly one-half of that sentence, or 17.5 years. So, if in sentencing the average murderer the judges' intent was for them to spend 17.5 years in prison, under TIS they could impose a sentence of 20 years (with 100 percent of that being served) and come close to achieving their goal of 17.5 years "behind bars." Similarly, the average prison sentence imposed on those convicted of Aggravated Criminal Sexual Assault in 1994 was 13.1 years, and that offender would be expected to serve roughly 7.5 years. Given the allowable sentencing range of 6 to 30 years for Class X felonies, and the requirement under TIS that these offenders serve 85 percent of their sentence, the same objective of 7.5 years "behind bars" (the pre-TIS average) could be achieved by sentencing them to 9 years (9 years X .85 = 7.6 years behind bars).²

In examining the impact of TIS on sentence lengths, prior research confirms that it is important to also statistically control for other offender characteristics when examining variation in sentence lengths. For example, Huang, Finn, Ruback and Friedman (1996) found older, better-educated males

² One additional dimension of sentencing sex offenders that could come into play is the potential of plea bargaining, which could result in charges being reduced from Class X to Class 1 felonies (i.e., reduced from Aggravated Criminal Sexual Assault, a Class X felony, to Criminal Sexual Assault, a Class 1 felony). Although both would require that 85 percent of the sentence be served, the allowable sentencing ranges for Class 1 felonies is 4 to 15 years, as opposed to the 6 to 30 years for Class X felonies. Although possible, it is unlikely that murder charges would be altogether dropped during plea bargaining. Our analyses were not able to include the impact of potential plea bargaining on reductions from Class X sex offenses to Class 1 felonies, and it is unlikely to be an issue in the trial and conviction of first degree murderers.

convicted of a violent crime received longer sentence lengths than those with other characteristics. Research has also suggested that sentences imposed in suburban and rural areas tend to be longer than those imposed in urban areas (Austin, 1991), partly explained by the fact that less urban areas tend to have less serious crime, and therefore courts respond more punitively due to the rarity and social outrage of crimes like murder and rape. Also consistent in the literature is the pattern that the more extensive an individual's criminal history, the longer their prison sentence.

Thus, in the current analyses of the impact of TIS on sentence lengths and lengths of time to serve, we will answer the question of whether sentence lengths of murderers and Class X sex offenders were affected by the implementation of TIS after statistically controlling for other factors that have been found to influence sentence lengths, such as age, race, gender, marital status, education level, if the inmate has children, gang membership, prior criminal history, and jurisdiction of sentencing. In order to accomplish this, we will employ multivariate regression techniques, including ordinary least squares (OLS) regression for analyses of changes in the sentence lengths imposed and logistic regression to examine the impact of the law on the use of sentences over the statutory maximums.

In addition to statistically controlling for age, race, gender, education level, marital status, having children, prior prison admissions, prior criminal history and sentencing jurisdiction, in order to determine the extent to which TIS has independently changed sentence lengths, a variable was created to identify those offenders sentenced under TIS and those sentenced under the law that allowed day-for-day good conduct credit. Specifically, we created an interaction term/variable to group the sentenced offenders into 1 of three groups: 1) those sentenced prior to the passage of the TIS law in Illinois (i.e., those cases sentenced prior to 1997, the reference group); 2) those sentenced who were subject to TIS; and 3) those *sentenced* when TIS was effective, *but were not subject to the law* (i.e., committed their crime prior to the effective date of the legislation). The reason to group the cases into one of these three categories was based on the belief that if judges were imposing sentences on those subject to TIS, and taking into account how long the offender would be serving, that it would potentially have an effect on the sentences they were handing down during the same time period to those who were not subject to the law. Indeed, Emerson (1983) found that judges were affected by the nature of the cases that they sentenced in assigning sentences to individual cases. Their sentencing decisions thus were affected by what types of cases preceded a case. Based

on this research, it is theorized that when judges were sentencing separate offenders, but similar types of cases, under the “old” law and “new” TIS laws, there may be some influence on the sentences imposed. In other words, if a judge was sentencing one murderer under TIS today, and then tomorrow was sentencing another under the old law, the two cases and sentences imposed may affect each other.

In addition to examining the impact TIS had on sentence lengths, we also examined the extent to which TIS changed the use of sentences beyond the statutory maximums. Specifically, we examined whether or not TIS was associated with a reduced proportion of murder and Class X sex offense cases with sentence lengths in excess of the statutory maximum (i.e., 20-60 year range for those convicted of murder and 6-30 years for those convicted of aggravated criminal sexual assault). Under Illinois law, a sentence beyond the statutory maximum, or a sentence of natural life or death in the case of murder, can be sought when specific, aggravating circumstances are present. The hypothesis and potential impact of TIS as it relates to sentences above the statutory maximum, or of natural life and death in murder cases, is that prior to TIS, the maximum sentence that could be imposed on murder without proving aggravating circumstances was 60 years, which, with good conduct credit would translate to 30 years in prison. Thus, for a 30-year-old murderer, odds are they would be released from prison before their death (i.e., they would be 60 years old). If the prosecutor wanted to ensure that this individual would not be released from prison, they would need to prove to the judge that aggravating circumstances were present in order to achieve a projected age of release from prison beyond the typical life expectancy (i.e., 70 or 80 years old). However, with TIS, and a 30 year old convicted murderer, it would now be possible to impose a veritable “life” sentence by sentencing them to prison for 60 years, which under TIS, would require the full 60 years to be served, resulting in that inmate not being eligible for release until the age of 90. To examine this potential change, we used logistic regression to examine the degree to which TIS is associated with a shift towards within-range sentence lengths as opposed to sentences beyond the maximum, after statistically controlling for other offender characteristics. Table 1a summarizes the characteristics of the samples used in the analyses of the sentences imposed on convicted murderers and sex offenders (those cases covering the period from SFY 1989 through SFY 2008).

Table 1a

Demographic, Socio-Economic and Criminal History Characteristics of Sentence Analysis Sample

	Murder Sentences N=9,218	Class X Sex Offender Sentences N=7,150
Age	27.08	32.55
Race		
White	17.3%	40.3%
Non-White	82.7%	59.7%
Total	100.0%	100.0%
Gender		
Male	94%	99.2%
Female	6%	0.8%
Total	100.0%	100.0%
Marital Status		
Married/Common Law	14.6%	27.4%
Single/Divorced	85.4%	72.6%
Total	100.0%	100.0%
Education Level		
No HS/GED	61.1%	52.2%
HS/GED	38.9%	47.8%
Total	100.0%	100.0%
Children		
None	50.7%	48.3%
One or More	49.3%	51.7%
Total	100.0%	100.0%
Gang Member		
No	60.1%	81.1%
Yes	39.9%	18.9%
Total	100.0%	100.0%
Region of Illinois		
Cook County	74.3%	54%
Collar County	6.9%	11%
Other Metropolitan Statistical Area	12.8%	18.9%
Rural Area	6%	16.2%
Total	100.0%	100.0%
Prior Prison Sentences		
None	72.6%	75%
One or More	27.4%	25%
Total	100.0%	100.0%
Truth-in-Sentencing		
Pre-TIS	45.8%	48.4%
Non-TIS	21.4%	12.2%
TIS	32.8%	39.3%
Total	100.0%	100.0%

Methodology Used to Examine Impact of TIS on Inmate Disciplinary Incidents

Because data regarding the disciplinary incidents of inmates in Illinois was not automated statewide at all facilities prior to 1999, we included only those inmates admitted to prison in Illinois for murder and sex offenses between July 1999 and June 2001 in our sample to determine if TIS has any impact on the extent and nature of disciplinary incidents. In addition to including many of the same independent variables as used in the analyses of sentence length (i.e., age, race, gender, marital status, education level, if the inmate has children, gang membership, prior criminal history, and jurisdiction of sentencing, and a dummy variable to indicate if they were subject to TIS), we also included information about how long they were projected to serve in prison (sentence imposed, minus any jail credits and minus any good conduct credit they could potentially earn), length of time served, and the security level of the facility they were housed in. In the analyses, we also performed analyses substituting the security level of the facility with dummy variables for the actual facilities (i.e., Stateville, Pontiac, etc). However, because some individual facilities had very small numbers of cases, the ability to statistically control for the specific facility was limited. All of the independent variables were examined in bivariate analyses to check for multicollinearity and none of the correlations were found to be large enough for this to be a concern in the multivariate analyses.

In terms of the dependent variable, or the institutional disciplinary incidents, we created and examined a number of different measures, including: the total number of disciplinary incidents (a ratio-level measure), an indication of any “serious incidents” (coded as a dichotomous variable, 0=none, 1=1 or more), an indication of any assaults on staff (coded as a dichotomous variable), any assaults on other inmates (coded as a dichotomous variable), and any assaults (combining staff and inmate assaults, and coded as a dichotomous variable). The determination of a “serious incident” was based on a review of IDOC’s disciplinary procedures (Illinois Administrative Code, 2003), and the identification of offenses that could result in the most severe sanctions. Based on this review, serious offenses were defined as 100- and 500-level offenses (i.e., assaults and other violent offenses, and violating a state or federal law; and involvement in a security threat group activities). Table 1b summarizes the characteristics of the sub-samples used in the analyses of the disciplinary incidents among those convicted of murder and sex offenses.

Table 1b
Demographic, Socio-Economic and Criminal History Characteristics of Disciplinary Record
Analysis Sub-Sample

	Murder Disciplinary Incidents (N=849)	Class X Sex Offender Disciplinary Incidents (N=1,405)
Age	26.56	32.27
Race		
White	16.7%	40.2%
Non-White	83.3%	59.8%
Total	100.0%	100.0%
Gender		
Male	92.7%	99.4%
Female	7.3%	0.6%
Total	100.0%	100.0%
Marital Status		
Married/Common Law	9%	21.8%
Single/Divorced	91%	78.2%
Total	100.0%	100.0%
Education Level		
No HS/GED	58.8%	54.6%
HS/GED	41.2%	45.4%
Total	100.0%	100.0%
Children		
None	75.4%	74.7%
One or More	24.6%	25.3%
Total	100.0%	100.0%
Gang Member		
No	63%	83.8%
Yes	37%	16.2%
Total	100.0%	100.0%
Region of Illinois		
Cook County	75.4%	52.2%
Collar County	5.9%	11.5%
Other Metropolitan Statistical Area	10.4%	15.6%
Rural Area	8.4%	20.7%
Total	100.0%	100.0%
Prior Prison Sentences		
None	73.4%	77.2%
One or More	26.6%	22.8%
Total	100.0%	100.0%
Truth-in-Sentencing		
Non-TIS	65%	42.6%
TIS	35%	57.4%
Total	100.0%	100.0%

RESULTS & FINDINGS: IMPACT OF TIS ON SENTENCE LENGTH AND TIME TO SERVE

Results and Findings from the Analyses of the Impact of TIS on Sentence Length and Time to Serve for Convicted Murderers

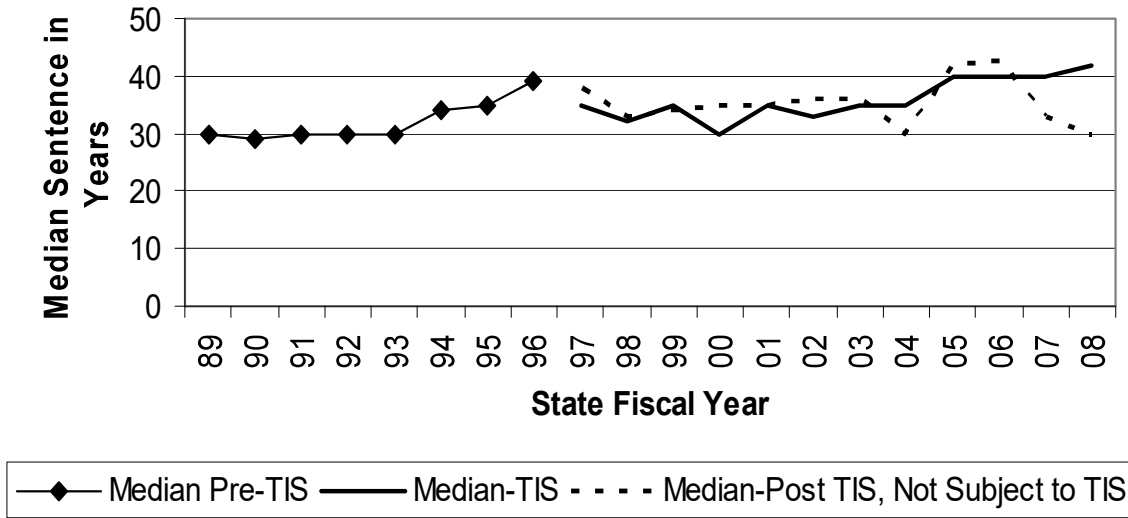
The first set of analyses sought to merely examine the overall pattern of sentences imposed on convicted murderers in Illinois between SFY 1989 and 2008, including analyses of the mean and median sentences imposed (for determinate sentences), and the proportion of murderers that received sentences beyond the statutory maximum of 60 years. These analyses were performed so as to separate out, or distinguish between the sentences imposed prior to the implementation of TIS, those murder sentences that were subject to TIS, and those murderers sentenced after TIS had been implemented, but who were not eligible due to the fact that their crime occurred prior to the passage of the law.

From these analyses, a number of patterns were evident that have implications for understanding the potential impact of TIS on murder sentences. First is the fact that in the years leading up to the passage of TIS in Illinois, the mean and median sentences imposed on convicted murderers that received a determinate sentence (i.e., excluding natural life or death sentences) was increasing (Figure 1). As seen in Figure 1, between SFY 1989 and 1993, the median sentence length imposed on murderers given a determinate sentence was 30 years. However, beginning in 1994, which is when the federal government passed its own version of TIS and increasing attention was being paid to the issue of TIS in the United States and in Illinois, the median sentences imposed on murderers in Illinois began to increase. By SFY 1996, the year before Illinois passed its TIS law, the median sentence lengths imposed on convicted murderers in Illinois had increased to 39 years.³

³ When examining the impact of TIS on sentence lengths, the first thing that needed to be examined was the pre-TIS trend in sentence lengths. Examining the correlation between sentence length (only those 20-60 years) and year (1989-1996), Pearson's r was .18. When examined in a regression model, for each additional year, sentence lengths increased by .87 years ($t=8.6$, $p<.001$). Thus, it appeared that prior to the implementation of TIS in 1997 there was a trend of increased sentence lengths being imposed on murderers in Illinois independent of the effect of the other variables.

Figure 1

**Median Sentence Imposed on Murderers in Illinois
(Excluding Natural Life & Death Sentences)**



The other pattern evident from the analyses of median sentence lengths over time was that following the passage of TIS, the median sentence lengths of those subject to the law, as well as those not subject to the law, appeared to decrease slightly and the upward trend in sentence lengths stopped, at least up until the last few years included in the analyses. By SFY 2008, the median sentence imposed on murderers subject to TIS had increased back to the pre-TIS level in SFY 1996. The trend in the median sentence imposed on those sentenced after the TIS law, but not subject to TIS, becomes statistically unstable after SFY 2003 due to there being relatively few cases in the analyses after that point. Aggregating all of the sentences imposed between SFY 1997 and 2008, the median sentence imposed on those convicted of murder and subject to TIS was 35 years (mean of 38.3 years). For those murderers sentenced after TIS was implemented, but not subject to the law, the median sentence imposed was 35 years (mean of 39.3 years). Thus, without any statistical controls other than whether or not the murderer was subject to TIS indicates the mean and median sentences imposed on convicted murderers subject to TIS were only *slightly lower* than the sentences imposed pre-TIS and when compared to those sentenced after TIS but who were not subject to the law.

The next step of the analyses involved performing bivariate analyses to determine if there were any relationships between the sentence imposed (in years) and the proposed independent variables, including: age, race, gender, marital status, education level, if the inmate has children, gang membership, prior criminal history, and jurisdiction of sentencing. Following the bivariate analyses, multivariate analyses were also conducted to examine the impact of TIS on sentence lengths, while also statistically controlling for the other variables that could influence or change the lengths of sentences imposed on convicted murderers. Those offenders sentenced to natural life or death were not included in these analyses.

In the bivariate analyses, all independent variables revealed statistically significant relationships with sentence length, although not all variables displayed strong relationships to sentence length (Table 2). Further, the fact that statistically significant relationships were found is not necessarily that surprising given the large sample size being used. Specifically, we found that older inmates were more likely to receive longer sentences, although it was a weak relationship (Spearman's $\rho = .068$, $p < .001$). Also, females received shorter sentences than males (32.7 years compared to 38.2 years, respectively), and although this difference was statistically significant ($F = 41.87$, $p < .001$), it was relatively weak (Spearman's $\rho = .092$, $p < .001$). Similarly, when race was coded into three categories (white, African-American, and Hispanic/other) a statistically significant difference in sentence lengths across the three groups was evident ($F = 18.32$, $p < .001$). Specifically, white defendants received longer mean sentences (40.6 years) than African-American (37.6 years) and Hispanic/other inmates (36.6 years). Marital status was also related to sentence length, with married inmates receiving longer sentences, on average, than unmarried inmates (39.7 years compared to 38.3 years, respectively; $F = 4.1$, $p < .05$). Those convicted murderers that had a high school diploma or GED received longer average sentences than those without a high school education (39.2 years compared to 37.8 years, respectively; $F = 7.56$, $p < .01$). Also, inmates who had at least one child received longer average sentences than those without children (39 years versus 36.8 years, respectively; $F = 29.6$, $p < .001$). Further, inmates who were gang members received longer sentences than non-members (38.8 years compared to 37.2 years, respectively; $F = 14.2$, $p < .001$). Where an offender was convicted also had a statistical relationship with mean sentence length. Convicted murderers sentenced outside of Cook County received longer mean sentences than their counterparts in Cook County (43.3 years for the Collar Counties, 39.9 years for other Metropolitan Statistical

Areas, 41.6 years for Micropolitan Statistical Areas versus 36.8 years for Cook County; $F=31.6$, $p<.001$). Finally, murderers with prior incarcerations received longer mean sentences than those with no prior incarcerations (42.7 years versus 36.2 years, respectively; $F=193.0$, $p<.001$).

Table 2

Average Sentences Imposed on Those Convicted of Murder Excluding Life and Death Sentences

	Mean Sentence Length (in Years)
Total	37.88, (sd) 18.60
Age*** (26.72 years) Spearman's Rho=.069, $p<.001$	
Gender*** $F=41.87$, $p<.001$; Spearman=.092, $p<.001$	
Female	32.72, (sd) 15.70
Male	38.22, (sd) 18.72
Race*** $F=18.32$, $p<.001$	
White	40.64, (sd) 23.17
African-American	37.56, (sd) 17.99
Hispanic/Other	36.57, (sd) 15.53
Marital Status* $F=4.1$, $p<.05$; Spearman=.011, $p=.39$	
Married/Common Law	39.66, (sd) 24.48
Divorced/Single	38.27, (sd) 18.11
Education Level** $F=7.56$, $p<.01$; Spearman=.023, $p=.084$	
HS Diploma or GED	39.24, (sd) 22.57
No HS Diploma or GED	37.78, (sd) 17.21
Children*** $F=29.6$, $p<.001$; Spearman=.057, $p<.001$	
None	36.78, (sd) 16.89
1 or More	39.02, (sd) 20.15
Gang Member*** $F=14.18$, $p<.001$; Spearman=.064, $p<.001$	
No	37.24, (sd) 19.54
Yes	38.82, (sd) 17.06
Region of Illinois*** $F=31.57$, $p<.001$	
Cook County/Chicago	36.82, (sd) 17.41
Collar County	43.26, (sd) 20.63
Other Metropolitan Statistical Area	39.86, (sd) 23.81
Micropolitan Statistical Area	41.6, (sd) 16.71
Prior Prison Sentences*** $F=193$, $p<.001$; Spearman=.147, $p<.001$	
None	36.2, (sd) 16.57
One or More	42.68, (sd) 22.75
Truth-in-Sentencing*** $F=11.65$, $p<.001$	
Pre-TIS	36.83, (sd) 18.29
Non TIS	39.3, (sd) 22.58
TIS	38.32, (sd) 15.83

* $p<.05$, ** $p<.01$, *** $p<.001$

When all of these variables were included in the multivariate model, plus a trend variable to account for the pre-TIS trend of increasing sentence lengths and the variable indicating the TIS status of the offender (pre-TIS, TIS and post-TIS but not subject to TIS), we found that TIS was associated with a 3.9 year reduction in the mean sentence length (Appendix 1). In other words, once you statistically take into account the effect of the offender's age, race, gender, marital status, education level, if the inmate has children, gang membership, prior criminal history, jurisdiction of sentencing, and adjust for the pre-TIS trend in sentencing, TIS resulted in a decrease of 3.9 years on the average sentence imposed on murderers.⁴ Given the average murder sentence, this reduction translates into a 10.3 percent reduction in sentence lengths. Thus, while TIS did reduce the length of sentences imposed on convicted murderers *to some degree*, the decrease was nowhere near what some believed it would be (i.e., that sentences would be cut nearly in half to account for the fact that 100 percent will be served under TIS as opposed to the 50 percent served under the old law). Another way the impact of TIS can be considered is by looking at the actual amount of time that will be required to be served by those convicted of murder. Substituting the time to serve for the sentence length reveals that those subject to TIS are expected, on average, to serve 17 years longer in prison than those not subject to TIS after statistically controlling for the other variables in the analyses.

Impact of TIS on Murder Sentences Beyond the Statutory Maximum

The next set of analyses sought to determine if the TIS law was associated with any change in the likelihood that a convicted murderer would receive a sentence beyond the statutory maximum of 60 years. Under Illinois law, a sentence beyond the statutory maximum, or a sentence of natural life or death in the case of murder, can be sought when specific, aggravating circumstances are present. As described previously, the hypothesis and potential impact of TIS as it relates to sentences above the statutory maximum, or of natural life and death in murder cases, is that prior to TIS, the maximum sentence that could be imposed on murder without proving aggravating circumstances was 60 years, which, with good conduct credit would translate to 30 years in prison. Thus, for a 30-year-old murderer, odds are they would be released from prison before their death (i.e., they would be 60 years old). If the prosecutor wanted to ensure that this individual would not be released from prison,

⁴ When the model was rerun using a logarithmic transformation of the dependent variable to account for the slight skew in sentences, the results were consistent. Specifically, TIS was associated with an 8 percent reduction in the sentence imposed on convicted murderers.

they would need to prove to the judge that aggravating circumstances were present in order to achieve a projected age of release from IDOC beyond the typical life expectancy (i.e., 70 or 80 years old). However, with TIS, and a 30 year old convicted murderer, it would now be possible to impose a veritable “life” sentence by sentencing them to IDOC for 60 years, which under TIS would require the full 60 years to be served, resulting in that inmate not being eligible for release until the age of 90.

Similar to the analyses of mean and median sentences imposed on the determinate sentences, we also performed time series analyses to determine if there were any changes during the period included in the analyses (SFY 1989 to 2008) in the proportion of murderers receiving sentences beyond the 60 year maximum (including determinate sentences of more than 60 years, plus natural life or a death sentence), and these analyses were performed so as to separate out, or distinguish between the sentences imposed prior to the implementation of TIS, those murder sentences that were subject to TIS, and those murderers sentenced after TIS had been implemented, but who were not eligible due to the fact that their crime occurred prior to the passage of the law. Unlike the findings from the analyses of median sentence lengths pre-TIS, we did not detect any noticeable trend leading up to the passage of TIS in the proportion of murderers given sentences beyond the statutory maximum. Between SFY 1989 and 1996, roughly 25 percent of convicted murderers received a sentence beyond the statutory maximum of 60 years, and most of these were sentences of natural life (498 of the 879 sentences beyond the statutory maximum). Among those subject to TIS, on the other hand, a much smaller proportion of convicted murderers have received a sentence beyond the statutory maximum (17 percent). Further, among those sentenced after TIS was passed, but who were not subject to it due to having committed their crime prior to the law, 17.4 percent were given sentences beyond the statutory maximum. Thus, the bivariate analyses would suggest that offenders subject to TIS are *less likely* than those murderers sentenced pre-TIS to get a sentence beyond the statutory maximum, whereas those murderers sentenced post-TIS, but not subject to the law, were *more likely* to receive a sentence beyond the statutory maximum than pre-TIS.

As with the analyses of determinate sentences, multivariate analyses were performed to examine the impact of TIS on the likelihood of a sentence beyond the statutory maximum after statistically controlling for other variables. Before these analyses were performed, however, bivariate analyses

examining the relationship between the type of sentence imposed (20-60 years versus 61 or more years/natural life/death) and the independent variables were performed. As seen in Table 3, there were statistically significant, although relatively weak, relationships between the sentence and offender age (older more likely to get sentence beyond 60 years), gender (males more likely to get a sentence beyond 60 years), race (whites more likely to get sentence beyond 60 years), education level (those with higher levels of education more likely to get sentence beyond 60 years), prior prison sentences (those with at least 1 prior prison sentence more likely to get sentence beyond 60 years), and where the offender was convicted (those convicted outside of Chicago/Cook County more likely to get a sentence beyond 60 years).

When multivariate analyses using logistic regression were performed in order to statistically control for the other independent variables, a generally consistent pattern emerged: those subject to TIS were *less likely* to receive a sentence beyond 60 years (relative to the pre-TIS group as well as relative to those convicted during the same time period but not subject to the law). For example, after statistically controlling for the age, race, gender, education level, marital status, gang membership, having children, prior prison sentences and jurisdiction where conviction occurred, those subject to TIS were 57 percent *less likely* to receive a sentence of more than 60 years than those sentenced prior to TIS. On the other hand, those sentenced post-TIS, but not subject to the law, were not any more or less likely to receive a sentence beyond 60 years when compared to those sentenced pre-TIS or those subject to TIS, after statistically controlling for the effect of the other variables.⁵

⁵ The amount of jail time was included as an independent variable and was intended to measure how long the case took to dispose. Thus, jail time was a proxy for the complexity or seriousness of the case (i.e., the longer to dispose of, theoretically the more complex, and potentially more likely to involve a jury trial).

Table 3

Comparison of Demographic, Socio-Economic and Criminal History Characteristics Among Those Sentenced to Less than 61 years versus 61+ years (or life, or death)

	Sentenced to 20-60 years	Sentenced to 61+ years, or receiving Life or Death Sentence	Total
Total	76.9%	23.1%	100%
Age*** (Mean, Years) F=115, p<.001	26.5	28.9	27.1
Gender*	X ² =15.72, 1df, p<.001, Phi=.04 p<.001		
Female	83.9%	16.1%	100%
Male	76.5%	23.5%	100%
Race***	X ² =94.75, 2df, p<.001, Cramer's V=.10, p<.001		
White	68.3%	31.7%	100%
African-American	77.8%	22.2%	100%
Hispanic/Other	82.7%	17.3%	100%
Marital Status**	X ² =8.69, 1df, p<.01, Phi=.04, p<.01		
Married/Common Law	72%	28%	100%
Divorced/Single	76.3%	23.7%	100%
Education Level***	X ² =27.07, 1df, p<.001, Phi=.07, p<.001		
HS Diploma or GED	72%	28%	100%
No HS Diploma or GED	77.7%	22.3%	100%
Children***	X ² =22.36, 1df, p<.001, Phi=.05, p<.001		
None	79%	21%	100%
1 or More	74.8%	25.2%	100%
Gang Member	X ² =.948, 1df, p=.330		
No	77.3%	22.7%	100%
Yes	76.4%	23.6%	100%
Region of Illinois***	X ² =127.28, 3df, p<.001, Cramer's V=.12, p<.001		
Cook County/Chicago	79.8%	20.2%	100%
Collar County	66.6%	33.4%	100%
Other Metropolitan Statistical Area	71%	29%	100%
Rural Area	66.4%	33.6%	100%
Prior Prison Sentence***	X ² =198.37, 1df, p<.001, Phi=.15, p<.001		
None	80.7%	19.3%	100%
One or More	66.8%	33.2%	100%
Truth-in-Sentencing***	X ² =78.95, 2df, p<.001, Cramer's V=.09, p<.001		
Pre-TIS	75.4%	24.6%	100%
Non TIS	73.4%	26.6%	100%
TIS	82.9%	17.1%	100%

* p<.05, ** p<.01, *** p<.001

Thus, our analyses regarding the impact of TIS on murder sentences revealed two substantial findings, which have considerable implications: 1) the average determinate sentence imposed on convicted murderers was reduced *only slightly* as a result of TIS, resulting in offenders serving much longer periods of time in prison, and 2) TIS appears to have reduced the use (or need) to impose sentences beyond the statutory maximum of 60 years. Thus, the passage of TIS has dramatically increased the *actual amount* of time those convicted of murder will spend in prison, and as a result, the cost per murder sentence imposed in Illinois dramatically increased as a result of TIS.

Specifically, among those who received a determinate sentence (i.e., excluding natural life and death sentences), TIS was associated with an average increase of 18 years of time to serve compared to pre-TIS. Using current dollar costs of incarceration in Illinois, and not including any construction costs, the average annual cost to incarcerate an adult in prison is \$22,622 (Illinois Department of Corrections, 2005). Thus, the average cost for incarceration in prison *per murder sentence* pre-TIS was roughly \$400,409 (annual cost of incarcerate per inmate of \$22,622 multiplied by the average length of time to serve of 17.7 years). By comparison, the average cost for incarceration in prison per murder sentence under TIS is roughly \$816,600 (annual cost per inmate multiplied by average length of time to serve of 36.1 years).

In addition to longer periods of incarceration, and therefore higher costs, a much larger proportion of convicted murderers in Illinois will now serve the rest of their life in prison, despite the fact that the actual imposition of natural life sentences has been reduced due to TIS. Because the lengths of time to serve in prison increased so much as a result of TIS, it is projected that 30 percent (886 of the 3,000 sentenced under TIS) of all inmates convicted of murder and subject to TIS will not be eligible for release until after their 75th birthday--the average life expectancy of males in the United States (CDC). However, while 30 percent of murderers sentenced under TIS received sentences that will result in them most likely spending the rest of their life in prison, only a small proportion of these were explicit “natural life” or “death” sentences. Of all the murderers sentenced under TIS and projected to be in prison beyond their 75th birthday, only 13 percent (117/886) had a “natural life” or “death” sentence imposed by the court. By comparison, pre-TIS, only 15 percent of all convicted murderers (627/4,198) received a sentence that would keep them in prison beyond their 75th birthday, and almost all of these (90 percent or 564/627) were court-imposed sentences of “natural life” or “death.” Further, to achieve these sentences of natural life or death, the prosecution had to

prove aggravating circumstances that would allow for the imposition of these sentences.

Impact of TIS on the Sentences Imposed on Sex Offenders

Analyses similar to those performed to examine the impact of TIS on murder sentences were performed to examine the impact of TIS on the sentence lengths of those convicted of aggravated criminal sexual assault. Specifically, we first examined the overall trends in mean and median sentences imposed on those convicted of Class X felony sex offenses (aggravated criminal sexual assault and predatory criminal sexual assault) over time, distinguishing between the TIS-eligible offenders as well as the post-TIS offenders who were ineligible for TIS due to the timing of their offense. Excluded from these analyses were sex offenders that received either natural life sentences or who were sentenced as sexually dangerous persons (SDP) due to the inability to quantify the sentence length in these cases. However, natural life and SDP cases accounted for a very small number of the Class X sex offenders sentenced during the time period examined (12 of the 1,405).

During the time period leading up to the passage of TIS in Illinois, no discernable trend in the mean or median sentence length of those convicted of Class X felony sex offenses in Illinois were evident. The average sentence imposed on Class X sex offenders pre-TIS was 13.5 years. By comparison, among those subject to TIS, the mean sentence length was slightly shorter (12.5 years), and among those sex offenders sentenced post-TIS but not subject to the law, the mean sentence length was just over 13.5 years.

Prior to performing multivariate analyses to examine the impact of TIS on the sentence lengths of sex offenders, bivariate analyses were performed between the independent variables—similar to those used in analyses of murder sentences-- and the sentence length (Table 4). The offender's age, race, educational attainment, having a child, gang status, and prior prison sentences were all found to be statistically related to the mean sentence length. Older Class X sex offenders received longer sentences, on average, although this was a fairly weak relationship (Spearman's $\rho = .135$, $p < .001$). When race was analyzed in three categories (white, African-American and Hispanic/other), it was statistically related to sentence length ($F = 23.43$, $p < .001$). It was found that African-Americans received longer sentences (13.6 years) than whites (12.9 years) and Hispanic/other offenders (11.3

years). Educational attainment was also statistically related to sentence length for Class X sex offenders ($F=3.5$, $p<.10$). Offenders who had a high school education received slightly longer sentences than those without a high school diploma or GED (13.7 years versus 13.2 years). Whether or not an offender had children was also statistically significant ($F=47.6$, $p<.001$). Offenders who had at least one child received longer mean sentences than those without children (13.7 years versus 12.2 years, respectively), although this was a weak relationship (Spearman's $\rho=.092$, $p<.001$). Similarly, an offender's gang status was statistically related to sentence length ($F=88.52$, $p<.001$), with gang members receiving longer mean sentences than non-members (15.2 years versus 12.5 years, respectively; Spearman's $\rho=.096$, $p<.01$). Further, a Class X sex offender's prior prison sentences were found to be statistically related to sentence length ($F=456.5$, $p<.001$). Offenders with at least one prior prison sentence received a mean sentence of 17.1 years, compared to a mean of 11.6 years for those without a prior prison sentence. The relationship between prior prison sentences and sentence length for sex offenders was stronger than the other relationships found, but still relatively weak (Spearman's $\rho=.241$, $p<.001$).

When all of these variables were included in the multivariate model, plus the variable indicating the TIS status of the offender (pre-TIS, TIS and post-TIS but not subject to TIS), we found that TIS was associated with a slight reduction in the mean sentence length of approximately .51 years (i.e., roughly 6 months shorter) (Appendix 2). It does not appear that those sentenced after the TIS law, but who were not subject to it, experienced any change or difference in sentence lengths than did those sentenced pre-TIS. In other words, once you statistically take into account the effect of the offender's age, race, gender, marital status, education level, if the inmate has children, gang membership, prior criminal history, and jurisdiction of sentencing, TIS resulted in a decrease of 0.5 years on the average sentence imposed on Class X sex offenders. Thus, while TIS did reduce the length of sentences imposed on convicted sex offenders to some degree, the decrease was nowhere near what some believed it would be (i.e., that sentences would be cut dramatically to account for the fact that 85 percent will be served under TIS as opposed to the 50 percent served under the old law).

Table 4
Average Sentences Imposed on Those Convicted of Class X Sex Offenses, Excluding Life Sentences
and Finding of Sexually Dangerous Person

	Mean Sentence Length (in Years)
Total	12.98, (sd) 9.56
Age*** (32.45 years) Spearman=.135, p<.001	
Gender F=.332, p=.565	
Female	12.23, (sd) 9.42
Male	12.99, (sd) 9.55
Race*** F=23.43, p<.001	
White	12.88, (sd) 8.75
African-American	13.64, (sd) 10.62
Hispanic/Other	11.32, (sd) 8.00
Marital Status F=2.5, p=.114	
Married/Common Law	13.07, (sd) 9.61
Divorced/Single/Widower	13.55, (sd) 10.09
Education Level F=3.5, p<.10	
HS Diploma or GED	13.68, (sd) 9.87
No HS Diploma or GED	13.16, (sd) 9.99
Children*** F=47.6, p<.001; Spearman=.092, p<.001	
None	12.17, (sd) 8.88
1 or More	13.74, (sd) 10.1
Gang Member*** F=88.52, p<.001; Spearman=.096, p<.001	
No	12.47, (sd) 8.95
Yes	15.2, (sd) 11.61
Region of Illinois F=1.42, p=.234	
Cook County/Chicago	12.85, (sd) 10.14
Collar County	12.65, (sd) 8.61
Other Metropolitan Statistical Areas	13.46, (sd) 8.65
Rural Areas	12.87, (sd) 8.65
Prior Prison Sentences*** F=456.50, p<.001; Spearman=.241, p<.001	
None	11.64, (sd) 8.01
One or More	17.13, (sd) 12.37
Truth-in-Sentencing*** F=5.87, p<.001	
Pre-TIS	13.16, (sd) 10.23
Non TIS	13.68, (sd) 10.59
TIS	12.54, (sd) 8.25

* p<.05, ** p<.01, *** p<.001

Logistic regression analyses were also performed to examine if the TIS law had any impact on the use of sentences beyond the statutory maximum (i.e., more than the 30 years generally allowable for Class X felonies). Using the same independent variables as described above, the TIS-status did not have any statistically significant effect on the imposition of a sentence beyond the 30 year maximum, and is most likely due to the relatively low prevalence of these types of sentences in general. As described previously, only about 5 percent of all Class X sex offenders received a sentence beyond the 30 year maximum pre-TIS, and among those subject to TIS, the prevalence of these sentences were also quite rare (i.e., 4 percent of all Class X felony sex offense TIS sentences). Thus, the impact of TIS on the sentence lengths and lengths of time to serve for sex offenders is somewhat similar to that seen with convicted murderers, although to a lesser degree due to the sentence lengths involved. Still, as a result of TIS, convicted Class X sex offenders are now serving substantially longer periods of incarceration than they did pre-TIS. On average, those sex offenders subject to TIS will serve an average of 9.7 years in prison, compared to the roughly 6.2 years those sentenced prior to TIS served, as well as those sentenced after the TIS law but who were not subject to the law. As a result, the average at release for sex offenders subject to TIS will be roughly 42 years old, compared to an average age at release of 38 for those sentenced pre-TIS. Thus, although there was an increase in the length of time served as a result of TIS for sex offenders, and a subsequent increase in the average age at release, it was nowhere near the magnitude of the increased time served for convicted murderers as a result of TIS and it did not have the kind of impact on age at release and likelihood of dying in prison prior to release as did the TIS law for murderers.

RESULTS & FINDINGS FROM ANALYSES OF IMPACT OF TIS ON DISCIPLINARY INCIDENTS

Impact of TIS on Disciplinary Incidents for Murderers

To determine if TIS had an impact on the extent and nature of disciplinary incidents among murderers in Illinois, data were obtained that allowed for the tracking of disciplinary incidents for a cohort of murderers admitted to prison in Illinois between July 1999 and June 2001. Disciplinary data included those recorded through March 2008, so the average amount of time inmates were at risk of having a disciplinary incident was 8 years. Within the sample of murderers tracked were 300 inmates subject to TIS and 550 that were eligible for day-for-day good conduct credit. The specific disciplinary outcomes examined included: total number of disciplinary tickets, any serious incident (yes or no), any assaults (yes or no), assaults of staff (yes or no), and assaults of other inmates (yes or no). Analyses included bivariate comparisons of inmate characteristics, including whether the inmate was subject to TIS or not, and each of these different measures of disciplinary incidents. Below is a description of the findings from each set of these bivariate analyses as well as a summary of the multivariate models tested to examine the effect of TIS on each measure of institutional rule violations.

Impact of TIS on the Total Number of Disciplinary Incidents on Murderers

Bivariate analyses were performed to determine if there was any statistical relationship between inmate characteristics, including whether they were subject to TIS, and the total *number* of disciplinary tickets (Table 5). With respect to the TIS status of the inmate, the bivariate analyses indicated that inmates who were not subject to TIS averaged 26.75 disciplinary tickets, compared to an average of 18.06 tickets for TIS inmates ($F=25.45$, $p<.001$). Thus, the bivariate analyses suggest that TIS offenders have *fewer disciplinary incidents*, on average, than inmates not subject to TIS.

In addition, there were also statistically significant relationships between number of disciplinary tickets and the inmate's age, gender, race, education level, gang membership, region of Illinois where the inmate was from, projected time to serve, and facility security level. Specifically, female inmates averaged 42.4 disciplinary tickets, compared to 20.5 for males ($F=56.9$, $p<.001$), which ,ay

potentially be explained by differences in the recording of disciplinary incidents between male and female facilities. African-American inmates had an average of 23.4 disciplinary tickets, compared to 21.1 per Hispanic inmate and 17.4 for white inmates ($F=4.12$, $p<.05$). Similarly, inmates with no high-school diploma/GED averaged 25.9 disciplinary tickets, whereas those with a high-school diploma/GED averaged 19.6 tickets ($F=3.91$, $p<.05$). Similarly, gang members had an average of 23.7 tickets, compared to 21 for non-gang members ($F=3.06$, $p<.10$). Inmates from more populous areas of Illinois averaged higher numbers of disciplinary tickets than inmates from other areas, with Cook County inmates averaging 23, “collar” county inmates averaging 19.8, and inmates from other Metropolitan Statistical Areas averaging 21.1, compared to an average of 13.7 disciplinary tickets for inmates from Micropolitan Statistical Areas, and a mean of 15.8 for inmates from all other areas of Illinois ($F=2.13$, $p<.10$). Facility security level was also statistically related to average number of disciplinary tickets. Inmates in medium security (level 4) facilities averaged 37.13 tickets, compared to 20.4 tickets for maximum security (level 1) inmates, 25.8 for secure medium (level 2) inmates, 22.6 for high medium (level 3) inmates, and 13.25 for high minimum (level 5) inmates ($F=6.64$, $p<.001$). A statistically significant relationship was also evident in the comparison of inmate age and the number of disciplinary tickets, with younger inmates having more tickets (Spearman’s $Rho=-.364$, $p<.001$). Finally, a statistically significant relationship between the number of disciplinary tickets an inmate received and the projected length of time they have to serve was found. Inmates with shorter projected sentences were found to have more disciplinary tickets (Spearman’s $Rho=-.351$, $p<.001$). On the other hand, no statistical relationship was found between number of tickets and marital status, whether or not they had children, and prior prison sentences.

Table 5
Average Number of Disciplinary Tickets Received by Those Convicted of Murder

	Mean Number of Disciplinary Tickets
Total	22.02, (sd) 21.68
Age*** (26.56 years) Spearman=-.364, p<.001	
Gender*** F=56.90, p<.001	
Female	42.43, (sd) 48.57
Male	20.52, (sd) 17.35
Race** F=6.93, p<.01	
White	17.39, (sd) 27.02
Non-White	22.88, (sd) 20.46
Race 2* F=4.12, p<.05	
White	17.39, (sd) 27.02
African-American	23.38, (sd) 21.00
Hispanic/Other	21.13, (sd) 18.43
Marital Status F=1.93, p=.166	
Married/Common Law	16.11, (sd) 11.37
Divorced/Single	23.42, (sd) 27.63
Education Level* F=3.91, p<.05	
HS Diploma or GED	19.58, (sd) 24.63
No HS Diploma or GED	25.86, (sd) 28.96
Children F=.381, p=.537	
None	22.29, (sd) 20.19
1 or More	21.19, (sd) 25.85
Gang Member F=3.06, p=.08	
No	21.00, (sd) 22.58
Yes	23.74, (sd) 20.00
Region of Illinois F=2.13, p=.075	
Cook County/Chicago	23.01, (sd) 21.27
Collar County	19.78, (sd) 16.89
Other Metropolitan Statistical Areas	21.13, (sd) 29.82
Micropolitan Statistical Areas	13.67, (sd) 11.65
All Other Areas	15.81, (sd) 14.32
Prior Prison Sentences F=2.65, p=.104	
None	22.75, (sd) 22.34
One or More	19.92, (sd) 19.55
TIS*** F=25.45, p<.001	
Non TIS	26.75, (sd) 21.86
TIS	18.06, (sd) 21.04

Projected Time to Serve*** (33.69 years) Spearman=-.351, p<.001	
Actual Time Served*** (7.73 years) Spearman=.122, p<.001	
Facility Security Level*** F=6.64, p<.001	
Maximum	20.40, (sd) 19.48
Secure Medium	25.80, (sd) 17.15
High Medium	22.60, (sd) 23.57
Medium	37.13, (sd) 45.79
High Minimum	13.25, (sd) 12.47

* p<.05, ** p<.01, *** p<.001

Multivariate analyses were also conducted in order to more accurately determine which variables had an independent impact on the numbers of disciplinary tickets received among the sample of murderers, and also to determine the impact of TIS while statistically controlling for the other variables associated with the number of disciplinary incidents. The first ordinary least squares (OLS) regression model for murder offenders included the independent variables for age at admission, projected time to serve in years, actual years served, gender, race as a dichotomous variable (white or non-white), marital status (married or not married), whether or not the offender had children, educational attainment (HS/GED or no HS/GED), gang status, area where they were sentenced (Cook or non-Cook), whether or not they were previously admitted to prison, the security level of their institution (maximum or other), and whether or not they were subject to TIS. This model was found to be statistically significant at the p<.001 level, with an R² of .188 and an adjusted R² of .175.

Of the 13 independent variables in this model, six were found to be statistically significant. Age at admission (p<.001) was inversely related to total number of tickets. For every year older an offender was, he or she could expect to receive .6 fewer disciplinary tickets (B=-.559). As well, for every year longer an offender was projected to serve (p<.005), he or she could expect to receive .1 fewer disciplinary tickets (B=-.094). On the other hand, for every year longer that an offender had actually served (p<.05), he or she could expect to receive 1.3 more disciplinary tickets (B=1.29). Gender had the strongest impact on the total number of tickets an offender received (N=553, p<.001; Beta=-.322). Male murder offenders could expect to receive about 28 fewer tickets than female murder offenders (N=8,664; B=-27.56). The security level of the offender's parent institution was also significantly related to total number of tickets received (p<.10), with offenders not in maximum

security receiving 3 fewer tickets than maximum security offenders ($B=-3.14$). Finally, an offender's status as TIS ($p<.005$) was also related to fewer disciplinary tickets, with TIS offenders *receiving almost 5 fewer tickets*, on average, than non-TIS inmates ($B=-4.67$). (See Appendix 3)

Impact of TIS on the Prevalence of "Serious" Incidents among Murderers

Bivariate analyses were also performed to determine if there was any statistical relationship, and if so, the strength of that relationship, between inmate characteristics, including whether they were subject to TIS, and whether or not the inmate received a disciplinary ticket for a serious incident (Table 6). Serious incidents were defined as any offenses that carry a maximum penalty of one year of loss or restriction of privileges, grade reduction, good time revocation and/or segregation. Offenses in this category range from violent assaults to participation in a security threat group or unauthorized organization. No statistically significant difference was noted between murderers subject to TIS and those not subject to TIS, with approximately 53 percent of both groups receiving a ticket for a serious incident.

On the other hand, there was a statistically significant relationship between receipt of a ticket for a serious incident and the inmate's age, race, education level, gang membership, and the security level of their institution. Specifically, younger inmates were more likely to have a serious violation, with the mean age of 24.8, compared to 28.1 for those with no serious incidents ($F=31.1$, $p<.001$). Non-white inmates were more likely than whites to have received a ticket for a serious incident (56 percent versus 40 percent, respectively) ($X^2=11.06$, 1df, $p<.001$), although the strength of the relationship was relatively weak ($\Phi=.12$, $p<.001$). Similarly, inmates without a high-school diploma/GED were more likely than those with a high-school diploma/GED to have had a serious incident (56 percent versus 43 percent, respectively; $X^2=5.2$, 1df, $p<.05$), although the strength of the relationship was relatively weak ($\Phi=.13$, $p<.05$). Inmates identified as gang members were more likely than non-gang members to have received a ticket for a serious incident (61 percent versus 50 percent, respectively; $X^2=9.5$, $p<.01$), and the strength of the relationship was weak ($\Phi=.11$, 1df, $p<.01$). Finally, inmates in higher-security institutions were more likely to receive a ticket for a serious incident (57 percent for maximum security inmates and 52 percent for secure medium inmates, compared to 38 percent for high medium, 39 percent for medium, and 0 percent for high

minimum; $X^2=16.67$, 4df, $p<.01$). However, this was also a relatively weak relationship (Cramer's $V=.143$, $p<.01$). No statistical relationship was found between receipt of a ticket for a serious incident and inmate gender, marital status, whether or not they had children, the region of Illinois where they were from, prior prison sentences, projected time to serve and, as noted before, whether or not they were subject to TIS.

Table 6

Comparison of Demographic, Socio-Economic and Criminal History Characteristics among Murderers with No Serious Offenses and Those with at Least One Serious Offense

	No "Serious" Offenses	One or More "Serious" Offenses	Total
Total	46.3%	53.7%	849
Age*** (Mean, Years) $F=31.13$, $p<.001$	28.11	24.83	26.34
Gender	$X^2 =1.27$, 1df, $p=.260$		
Female	53.6%	46.4%	100%
Male	45.8%	54.2%	100%
Race***	$X^2 =11.06$, 1df, $p<.001$, $\Phi=.116$, $p<.001$		
White	59.8%	40.2%	100%
Non-White	43.8%	56.2%	100%
Race 2**	$X^2 =11.74$, 2df, $p<.01$, Cramer's $V=.120$, $p<.01$		
White	59.8%	40.2%	100%
African-American	44.7%	55.3%	100%
Hispanic/Other	40.9%	59.1%	100%
Marital Status	$X^2 =.170$, 1df, $p=.680$		
Married/Common Law	53.6%	46.4%	100%
Divorced/Single	49.5%	50.5%	100%
Education Level*	$X^2 =5.22$, 1df, $p<.05$, $\Phi=.131$, $p<.05$		
HS Diploma or GED	57.3%	42.7%	100%
No HS Diploma or GED	44.0%	56.0%	100%
Children	$X^2 =1.61$, 1df, $p=.204$		
None	45.1%	54.9%	100%
1 or More	50.3%	49.7%	100%
Gang Member**	$X^2 =9.54$, 1df, $p<.01$, $\Phi=.108$, $p<.01$		
No	50.5%	49.5%	100%

Yes	39.3%	60.7%	100%
Region of Illinois	$X^2=5.04, 4df, p=.283$		
Cook County/Chicago	44.3%	55.7%	100%
Collar County	50.0%	50.0%	100%
Other Metropolitan Statistical Area	51.2%	48.8%	100%
Micropolitan Statistical Area	57.1%	42.9%	100%
All Other Areas	57.1%	42.9%	100%
Prior Prison Sentence	$X^2 =.123, 1df, p=.725$		
None	46.0%	54.0%	100%
One or More	47.4%	52.6%	100%
Projected time to Serve (Mean Years) $F=2.08, p=.150$	32	34.93	33.57
Truth-in-Sentencing	$X^2=.165, 4df, p=.685$		
No	43.2%	56.8%	100%
Yes	44.8%	55.2%	100%
Security Level of Institution**	$X^2=16.67, 4df, p<.01, \text{Cramer's } V=.143, p<.01$		
Maximum	43.0%	57.0%	100%
Secure Medium	48.2%	51.8%	100%
High Medium	61.9%	38.1%	100%
Medium	60.5%	39.5%	100%
High Minimum	100.0%	0.0%	100%
Time Served (Mean Years)* $F=6.35, p<.05$	7.65	7.85	7.76

* $p<.05$, ** $p<.01$, *** $p<.001$

Analyses were also performed to examine the relationship between the *number* of serious incidents and inmate characteristics and whether the inmate was subject to TIS. No statistical relationship was found between the number of serious incidents among the murderers and whether they were subject to TIS. However, as with the analyses of whether or not the inmate received any tickets for serious incidents (i.e., dichotomous indication of yes or no), there were statistically significant differences in the average number of serious incidents and the inmates' race, number of children, gang involvement, and their institution's security level. Specifically, Hispanic inmates had an average of 1.56 serious incidents, compared to 1.18 per African-American inmate and .95 for white inmates ($F=4.08, p<.05$). Similarly, inmates with no children averaged 1.31 serious incidents, whereas those with at least one child had .92 serious incidents ($F=6.8, p<.01$). This was a weak relationship, with

Spearman's $Rho=.08$, with $p<.05$. Similarly, gang members had an average of 1.57 serious incidents, compared to 1.01 for non-gang members ($F=18.1$, $p<.001$), although this was a weak relationship as well (Spearman's $Rho=.15$, $p<.001$). Once again, inmates in higher-security institutions averaged more serious incidents. Maximum security inmates averaged 1.33 and secure medium inmates averaged 1.02, while high medium inmates averaged .83, medium inmates averaged .84, and high minimum inmates had 0 ($F=2.46$, $p=.044$). As with the dichotomous analysis, a statistically significant relationship was evident in the comparison of inmate age and the number of serious incidents, with younger inmates having more serious incidents (Spearman's $Rho=-.25$, $p<.001$). A relationship was also found between the projected time to serve and number of serious incidents, although it was weak (Spearman's $Rho=.064$, $p<.10$).

Consistent with the comparison made with the serious incident as a dichotomous variable, no statistical relationship was found between the number of tickets for a serious incident and inmate gender, marital status, the region of Illinois where they were from, prior prison sentences, and, as already noted, whether or not they were subject to TIS. Thus, in the multivariate analyses of any serious incident (i.e., a dichotomous variable using logistic regression) and of the number of serious incidents (i.e., a ratio-level variable using ordinary least squares regression), the TIS variable was not statistically related to whether or not the inmate received a ticket for a serious incident or the number of tickets for serious incidents

Impact of TIS on the Prevalence of Assaults by Murderers

Bivariate and multivariate analyses were also performed to determine if there was any statistical relationship, and if so, the strength of that relationship, between inmate characteristics, including whether they were subject to TIS, and whether or not the inmate received a disciplinary ticket for any assault (Table 7). No statistically significant differences were noted between murderers subject to TIS and those that were not, with roughly 19 percent of both groups receiving a disciplinary ticket for an assault. On the other hand, among the sample of offenders sentenced to IDOC for murder, there were statistically significant relationships between receipt of a ticket for any assault and the inmate's age and gang membership. Specifically, younger inmates were more likely to have received a disciplinary ticket for assault, with a mean age of 24.2, compared to 26.8 for those with no assaults

($F=11.4$, $p<.005$). Similarly, inmates identified as gang members were more likely than non-gang members to have received a ticket for any assault (23 percent versus 16 percent, respectively), but the strength of the relationship was weak ($\Phi=.09$, $p<.05$). No statistical relationships, however, were found between receipt of a ticket for any assault and inmate gender, race, marital status, education level, whether or not they had children, the region of Illinois where they were from, prior prison sentences, projected time to serve and, as indicated previously, whether or not they were subject to TIS.

Table 7
Comparison of Demographic, Socio-Economic and Criminal History Characteristics among Murderers with No Assaults and at Least One Assault

	No Assaults	One or More Assaults	Total
Total	81.7%	18.3%	100%
Age*** (Mean, Years) $F=11.35$, $p<.001$	26.81	24.22	26.34
Gender	$X^2 =.007$, 1df, $p=.936$		
Female	82.1%	17.9%	100%
Male	81.7%	18.3%	100%
Race	$X^2 =1.10$, 1df, $p=.295$		
White	85.0%	15.0%	100%
Non-White	81.1%	18.9%	100%
Race 2	$X^2 =1.58$, 2df, $p=.453$		
White	85.0%	15.0%	100%
African-American	81.7%	18.3%	100%
Hispanic/Other	79.2%	20.8%	100%
Marital Status	$X^2 =.010$, 1df, $p=.919$		
Married/Common Law	82.1%	17.9%	100%
Divorced/Single	81.4%	18.6%	100%
Education Level	$X^2 =1.083$, 1df, $p=.298$		
HS Diploma or GED	83.9%	16.1%	100%
No HS Diploma or GED	79.1%	20.9%	100%
Children	$X^2 =.174$, 1df, $p=.676$		
None	81.4%	18.6%	100%
1 or More	82.7%	17.3%	100%
Gang Member*	$X^2 =6.21$, 1df, $p<.05$, $\Phi=.087$, $p<.05$		

No	84.3%	15.7%	100%
Yes	77.4%	22.6%	100%
Region of Illinois 2	$X^2=3.82, 4df, p=.431$		
Cook County/Chicago	81.9%	18.1%	100%
Collar County	78.3%	21.7%	100%
Other Metropolitan Statistical Area	82.1%	17.9%	100%
Micropolitan Statistical Area	95.2%	4.8%	100%
All Other Areas	76.2%	23.8%	100%
Prior Prison Sentence	$X^2=.347, 1df, p=.556$		
None	82.2%	17.8%	100%
One or More	80.4%	19.6%	100%
Projected time to Serve (Mean Years) $F=.150, p=.699$	33.76	32.74	33.57
Truth-in-Sentencing	$X^2=.008, 1df, p=.931$		
No	80.7%	19.3%	100%
Yes	81.0%	19.0%	100%
Security Level of Institution	$X^2=1.74, 4df, p=.784$		
Maximum	81.3%	18.7%	100%
Secure Medium	80.7%	19.3%	100%
High Medium	82.5%	17.5%	100%
Medium	86.8%	13.2%	100%
High Minimum	100.0%	0.0%	100%
Time Served (Mean, Years) $F=.882, p=.348$	7.74	7.84	7.76

* $p<.05$, ** $p<.01$, *** $p<.001$

Analyses were also performed to examine the relationship between the *number* of assaults and inmate characteristics. As with the analyses of whether or not the inmate received any tickets for assault (i.e., dichotomous indication of yes or no), there were statistically significant differences in the average number of assaults and the inmates' age and gang involvement. Specifically, gang members had an average of .35 assaults, compared to .24 for non-gang members ($F=2.9, p<.1$). A statistically significant relationship was evident in the comparison of inmate age and the number of assaults, with younger inmates having more assaults (Spearman's $Rho=-.134, p<.001$). Also consistent with the comparison made with the assaults as a dichotomous variable, no statistical relationship was found between the number of tickets for assault and inmate gender, race, marital status, education level, whether or not they had children, the region of Illinois where they were from,

prior prison sentences, projected time to serve and whether or not they were subject to TIS.

The analyses also examined separately the prevalence and patterns of assaults specifically against IDOC staff as well as assaults committed by inmates against other inmates, and examined if the inmate's TIS status was at all related to these forms of violence. The prevalence of staff assaults was low for both TIS and non-TIS murderers, with fewer than 9 percent of both groups receiving a ticket for assaulting a staff member and no statistically significant differences between the TIS and non-TIS inmates was evident. Although the prevalence of assaults of other inmates was slightly higher—at roughly 12 percent—there were also no statistically significant differences between the TIS and non-TIS inmates. In the multivariate analyses of any assault (i.e., a dichotomous variable using logistics regression) and of the number of assaults (i.e., a ratio-level variable using ordinary least squares regression), the TIS variable was not statistically related to whether or not the inmate received a ticket for a serious incident or the number of tickets for serious incidents. Similar findings were evident when the specific nature of the assault (staff assaults or assaults on other inmates) was examined between TIS and non-TIS inmates.

Impact of TIS on the Sanctions Imposed on Murderers with Disciplinary Incidents

Analyses were also performed to determine if an inmate's TIS status had any influence on the types of sanctions imposed by IDOC for disciplinary incidents, including placement in segregation, loss of good conduct credit, loss of commissary privileges, and loss of gym/yard privileges. As with the analyses described above, bivariate and multivariate analyses were performed to determine if the inmate's TIS status had an independent relationship to any of these sanctions being imposed.

Among the sample of murderers, the most prevalent form of sanction imposed on those receiving disciplinary tickets was loss of commissary privileges, and no statistical difference was noted between those murderers subject to TIS and those not: roughly 90 percent of both groups lost their commissary privileges for a period of time as a result of a disciplinary incident. For the other forms of punishment, including placement in segregation, loss of gym/yard privileges, and loss of good conduct credit, no statistical differences were identified based on the inmates' TIS status.

Impact of TIS on Disciplinary Incidents for Class X Sex Offenders

To determine if TIS had an impact on the extent and nature of disciplinary incidents among Class X sex offenders in Illinois, data were obtained that allowed for the tracking of disciplinary incidents for a cohort of sex offenders admitted to prison in Illinois between July 1999 and June 2001.

Disciplinary data included that recorded through March 2008, so the average amount of time inmates were at risk of having a disciplinary incident was 8 years. Within the sample of sex offenders tracked were 806 inmates subject to TIS and 599 that were eligible for day-for-day good conduct credit. The specific disciplinary outcomes examined included: total number of disciplinary tickets, any serious incident (yes or no), any assaults (yes or no), assaults of staff (yes or no), and assaults of other inmates (yes or no). Analyses included bivariate comparisons of inmate characteristics, including whether the inmate was subject to TIS or no, and each of these different measures of disciplinary incidents. Below is a description of the findings from each set of these bivariate analyses as well as a summary of the multivariate models tested to examine the effect of TIS on each measure of institutional rule violations.

Impact of TIS on the Total Number of Disciplinary Incidents for Sex Offenders

Bivariate analyses were performed to determine if there was any statistical relationship between inmate characteristics, including whether they were subject to TIS, and the total *number* of disciplinary tickets among the sample of sex offenders (Table 8). No statistically significant relationship was found between the number of tickets for a serious incident and whether or not they were subject to TIS: among both the TIS and non-TIS inmates the average number of disciplinary tickets was roughly 23. As well, no statistical relationship was found between the number of tickets and whether or not they had children, prior prison sentences, or their projected time to serve.

Among the sample of offenders sentenced to IDOC for Class X sex offenses, there was a statistically significant relationship between number of disciplinary tickets and an inmate's age, gender, race, marital status, educational attainment, gang status, and the region of Illinois where they were from. Specifically, younger inmates were more likely to have higher numbers of disciplinary tickets (Spearman's $Rho = -.513$, $p < .001$). Female inmates far outpaced their male counterparts in mean

number of disciplinary tickets, with females having a mean of 49.8, compared to 22.5 for males (F=5.17, p<.05). This statistical relationship held even with the very small number of females (N=5) in this population. African-American inmates had an average of 29.8 tickets, compared to 17.9 per white inmate and 17.5 for Hispanic inmates (F=15.41, p<.001). Inmates who were divorced or single averaged 25.2 tickets, compared to 14.1 for those who were married (F=7.63, p<.01). Similarly, inmates with no high-school diploma/GED averaged 28.8 disciplinary tickets, whereas those with a high-school diploma/GED had 16.8 tickets (F=9.35, p<.005). As well, gang members had an average of 33.6 disciplinary tickets, compared to 20.8 for non-gang members (F=17.56, p<.001). Finally, the region of Illinois where an inmate was from was found to be statistically related to total number of disciplinary tickets. Inmates from Cook County averaged 26 tickets, while those from the collar counties averaged 18.6, those from other metropolitan statistical areas averaged 20.3, those from micropolitan statistical areas averaged 22.8, and those from all other areas averaged 18.4 tickets (F=2.34, p<.10).

Table 8
Average Number of Disciplinary Tickets Received by Those Convicted of Class X Sex Offenses

	Mean Number of Disciplinary Tickets
Total	23.19, (sd) 32.75
Age*** (32.27 years) Spearman=-.513, p<.001	
Gender* F=5.17, p<.05	
Female	49.8, (39.34 sd)
Male	22.51, (26.62 sd)
Race*** F=26.22, p<.001	
White	17.43, (31.27 sd)
Non-White	26.90, (33.17 sd)
Race 2*** F=15.41, p<.001	
White	17.86, (27.75 sd)
African-American	29.84, (28.19 sd)
Hispanic/Other	17.46, (14.94 sd)
Marital Status** F=7.63, p<.01	
Married/Common Law	14.08, (22.89 sd)
Divorced/Single	25.18, (26.49 sd)
Education Level* F=9.35, p<.05	
HS Diploma or GED	16.80, (19.97 sd)

No HS Diploma or GED	28.76, (34.95 sd)
Children F=2.47, p=.117	
None	23.70, (27.86 sd)
1 or More	19.64, (22.97 sd)
Gang Member*** F=17.56, p<.001	
No	20.83, (26.37 sd)
Yes	33.57, (26.87 sd)
Region of Illinois F=2.34, p=.054	
Cook County/Chicago	26.04, (26.15 sd)
Collar County	18.59, (19.12 sd)
Other Metropolitan Statistical Areas	20.31, (24.1 sd)
Micropolitan Statistical Areas	22.80, (42.63 sd)
All Other Areas	18.41, (28.64 sd)
Prior Prison Sentences F=2.29, p=.13	
None	21.84, (27.75 sd)
One or More	25.82, (23.15 sd)
TIS F=.11, p=.745	
Non TIS	22.19, (32 sd)
TIS	22.97, (24.25 sd)
Projected Time to Serve (7.91 years) Spearman=.031, p=.451	

* p<.05, ** p<.01, *** p<.001

Impact of TIS on the Prevalence of “Serious” Incidents among Sex Offenders

Bivariate analyses were performed to determine if there was any statistical relationship, and if so, the strength of that relationship, between inmate characteristics, including whether they were subject to TIS, and whether or not the inmate received a disciplinary ticket for a serious incident among the sample of sex offenders (Table 9). In the bivariate analyses, we found that inmates subject to TIS were more likely to have one or more serious incidents than non-TIS inmates (34.9 percent versus 27.6 percent, respectively; $X^2=3.06$, 1 df, $p<.10$), although in the multivariate model that controlled for amount of time served, among other things, the inmates’ TIS status was no longer associated with the prevalence of serious incidents. Thus, the relationship between serious incidents and the inmate’s TIS status in the bivariate relationship is primarily due to the fact that sex offenders subject to TIS in the sample were incarcerated for a longer period of time, and therefore had more of an “opportunity” to engage in a serious incident. The multivariate analyses found that for every additional year a sex offender spent in prison, the likelihood of their getting a ticket for a serious

incident increased 12 percent.

In addition, statistically significant relationships between receipt of a ticket for a serious incident and the inmate's age, race, marital status, whether or not they had children, gang membership, prior prison sentence, projected time to serve, and security level of the facility were also found.

Specifically, younger offenders were more likely than older inmates to have received a ticket for a serious incident with the average age of an inmate who received a ticket being 28.96 and the average age of those not receiving tickets being 33.78 ($F=26.66$, $p<.001$). Similarly, non-white inmates were more likely than white inmates to have had a serious incident (37.8 percent versus 26.3 percent, respectively) ($X^2 =7.85$, 1 df, $p=.005$), although the strength of the relationship was relatively weak ($\Phi=.115$, $p=.005$). An inmate's marital status also had a statistically significant relationship with the occurrence of a serious offense as divorced/single inmates were more likely to have received a ticket than married inmates (34.3 percent versus 19.2 percent, respectively) ($X^2 =4.38$, 1df, $p<.05$), however this relationship was weak ($\Phi=.132$, $p<.05$). Inmates who had no children were more likely to have a serious incident compared to those with at least one child (34.7 percent versus 26.2 percent, respectively; $X^2 =3.52$, 1df, $p<.10$). However, this was a relatively weak relationship ($\Phi=.007$, $p<.10$). In addition, inmates identified as gang members were more likely than non-gang members to have received a ticket for a serious incident (47.2 percent versus 30.1 percent, respectively), and the strength of the relationship was weak ($\Phi=.129$, $p<.005$). Prior prison sentence also had a significant relationship with the likelihood of an inmate receiving a ticket as those with prior sentences were more likely than those without to have received a ticket for a serious incident (41.8 percent versus 30.1 percent, respectively) ($X^2 =6.46$, 1df, $p<.05$), while this relationship was weak ($\Phi=.104$, $p<.05$). An inmate's projected time to serve was also statistically significant, with those having one or more serious incidents having longer mean sentences than those with no serious incidents (9.4 years versus 7.7 years, respectively; $F=12.14$, $p<.005$). Finally, inmates housed in the highest security level facility were more likely to have received a ticket for a serious incident than those housed in lower security facilities (54.2 percent versus 15.9 percent, respectively) ($X^2 =40.63$, 4df, $p<.001$) and this relationship was relatively weak (Cramer's $V=.262$, $p<.001$). On the other hand, no statistical relationship was found between receipt of a ticket for a serious incident and inmate gender, education level, or the region of Illinois where they were from.

Table 9

Comparison of Demographic, Socio-Economic and Criminal History Characteristics among Class X Sex Offenders with No Serious Offenses and at Least One Serious Offense

	No Serious Offenses	One or More Serious Offenses	Total
Total	66.8%	33.2%	100%
Age*** (Mean, Years) F=26.66, p<.001	33.78	28.96	32.27
Gender	$X^2 = 1.71, 1df, p = .192$		
Female	40.0%	60.0%	100%
Male	67.5%	32.5%	100%
Race**	$X^2 = 7.85, 1df, p < .01, \Phi = .115, p = .005$		
White	73.7%	26.3%	100%
Non-White	62.8%	37.8%	100%
Race 2*	$X^2 = 9.05, 2df, p < .05, \text{Cramer's } V = .123, p < .05$		
White	73.7%	26.3%	100%
African-American	61.0%	39.0%	100%
Hispanic/Other	67.0%	33.0%	100%
Marital Status*	$X^2 = 4.38, 1df, p < .05, \Phi = .132, p < .05$		
Married/Common Law	80.8%	19.2%	100%
Divorced/Single	65.7%	34.3%	100%
Education Level	$X^2 = .015, 1df, p = .901$		
HS Diploma or GED	68.4%	31.6%	100%
No HS Diploma or GED	67.6%	32.4%	100%
Children	$X^2 = 3.52, 1df, p = .061$		
None	65.3%	34.7%	100%
1 or More	73.8%	26.2%	100%
Gang Member**	$X^2 = 9.96, 1df, p < .01, \Phi = .129, p < .01$		
No	69.8%	30.2%	100%
Yes	52.8%	47.2%	100%
Region of Illinois	$X^2 = 5.9, 4df, p = .207$		
Cook County/Chicago	63.3%	36.7%	100%
Collar County	65.4%	34.6%	100%
Other Metropolitan Statistical Area	72.7%	27.3%	100%
Micropolitan Statistical Area	76.9%	23.1%	100%
All Other Areas	71.3%	28.8%	100%

Prior Prison Sentence*	$X^2=6.46, 1df, p<.05, Phi=.104, p<.05$		
None	69.9%	30.1%	100%
One or More	58.2%	41.8%	100%
Projected time to Serve (Mean, Years)** $F=12.14, p<.01$	7.676	9.427	100%
Truth-in-Sentencing	$X^2=3.06, 1df, p=.08$		
No	72.4%	27.6%	100%
Yes	65.1%	34.9%	100%
Security Level*** $X^2=40.63, 4df, p<.001, Cramer's V=.262, p<.001$			
Maximum	45.8%	54.2%	100%
Secure Medium	53.0%	47.0%	100%
High Medium	69.8%	30.2%	100%
Medium	75.7%	24.3%	100%
High Minimum	84.1%	15.9%	100%

* $p<.05$, ** $p<.01$, *** $p<.001$

Multivariate analyses were conducted in order to more accurately determine which variables have the greatest impact on the prevalence of serious incidents, while statistically controlling for certain factors. The first model included independent variables for age at admission, projected time to serve in years, actual years served, gender, race in three categories (white, African-American and Hispanic/other), marital status, whether or not the offender had children, educational attainment, gang status, area where they were sentenced, whether or not they were previously admitted to prison, the security level of their institution, and whether or not they were subject to TIS. The dependent variable in the first model was whether or not an offender committed a serious offense, as defined above. This first model was found to be statistically significant at the $p<.001$ level, with an R^2 of .159 and a pseudo- R^2 of .221. The independent variables improved the predictive ability of this model from 67.3 percent to 70.5 percent.

Of the 13 independent variables, seven were found to be statistically significant when other factors were controlled. Age at admission ($p<.001$) was statistically significant, and it was found that for every year older an offender is, his or her likelihood of receiving a disciplinary ticket for a serious offense decreases by about 5 percent (odds ratio of .955). The amount of time that an offender has served was also significant ($p<.05$). It was found that for every year longer that an inmate served, his or her chances of receiving a disciplinary ticket for a serious offense increased by about 12

percent (odds ratio of 1.121). Further, whether or not an offender had children ($p < .10$) acted as a protective factor against receiving a ticket for a serious offense. Offenders who had at least one child were over 40 percent less likely to receive a ticket for a serious offense (odds ratio of .593). In some cases, where an offender was sentenced also had an impact on his or her likelihood of being ticketed for a serious offense. It was found that offenders sentenced in metropolitan statistical areas other than Cook County were about 45 percent less likely to receive a ticket for a serious offense than those coming from Cook County (odds ratio of .548). As well, the security level of the institution where these Class X sex offenders were housed was also found to be statistically significant. Inmates housed in high medium facilities ($p < .005$) were 60 percent less likely to have a ticket for a serious offense than their counterparts in maximum security facilities (odds ratio of .397). Those housed in medium security facilities ($p < .001$) were about 74 percent less likely to have a serious offense ticket than those in maximum security (odds ratio of .259). Finally, the most influential variable in this model ($p < .001$; Wald=21.4) was found to be placement in a high minimum security facility. Compared to maximum security inmates, those placed in high minimum were 85 percent less likely to be ticketed for a serious offense.

Impact of TIS on the Prevalence of Assaults by Sex Offenders

Bivariate and multivariate analyses were also performed to determine if there was any statistical relationship, and if so, the strength of that relationship, between inmate characteristics, including whether they were subject to TIS, and whether or not the inmate received a disciplinary ticket for any assault (Table 10). No statistically significant differences were noted between sex offenders subject to TIS and those that were not, with roughly 8 to 10 percent of both groups receiving a disciplinary ticket for an assault. This lack of a statistical relationship between TIS status and assaults was also evident in the multivariate analyses, as well as when assaults specifically against staff were examined (roughly 4 to 6 percent of both groups received tickets for this) and assaults against other inmates (with roughly 6 percent of both groups receiving tickets for this).

Table 10

Comparison of Demographic, Socio-Economic and Criminal History Characteristics among Class X Sex Offenders with No Assaults and at Least One Assault

	No Assaults	One or More Assaults	Total
Total	89.8%	10.2%	100%
Age*** (Mean, Years) F=11.44, p<.001	32.95	27.87	32.27
Gender*	X ² =5.45, 1df, p<.05, Phi=-.093 p<.05		
Female	60.0%	40.0%	100%
Male	90.7%	9.3%	100%
Race*	X ² =6.52, 1df, p<.05, Phi=.102, p<.05		
White	94.0%	6.0%	100%
Non-White	87.9%	12.1%	100%
Race 2***	X ² =16.11, 2df, p<.001, Cramer's V=.16, p<.001		
White	94.0%	6.0%	100%
African-American	84.9%	15.1%	100%
Hispanic/Other	95.3%	4.7%	100%
Marital Status***	X ² =8.52, 1df, p<.001, Phi=.18, p<.001		
Married/Common Law	100.0%	0.0%	100%
Divorced/Single	86.4%	13.6%	100%
Education Level	X ² =1.22, 1df, p=.27		
HS Diploma or GED	91.4%	8.6%	100%
No HS Diploma or GED	87.0%	13.0%	100%
Children	X ² =.162, 1df, p=.687		
None	90.2%	9.8%	100%
1 or More	91.3%	8.7%	100%
Gang Member***	X ² =13.53, 1df, p<.001, Phi=.146, p<.001		
No	92.3%	7.7%	100%
Yes	80.4%	19.6%	100%
Region of Illinois	X ² =6.92, 4df, p=.14		
Cook County/Chicago	89.3%	10.7%	100%
Collar County	87.4%	12.6%	100%
Other Metropolitan Statistical Area	90.4%	9.6%	100%
Micropolitan Statistical Area	90.2%	9.8%	100%
All Other Areas	97.8%	2.2%	100%
Prior Prison Sentence***	X ² =11.981, 1df, p<.001, Phi=.138, p<.001		

None	92.7%	7.3%	100%
One or More	83.2%	16.8%	100%
Projected time to Serve (Mean Years) F=2.36, p=.125	8.036	9.26	100%
Truth-in-Sentencing	X ² =.972, 1df, p=.324		
No	92.2%	7.8%	100%
Yes	89.7%	10.3%	100%
Security Level*** X ² =19.36, 4df, p<.001, Cramer's V=.18, p<.001			
Maximum	79.6%	20.4%	100%
Secure Medium	85.1%	14.9%	100%
High Medium	92.3%	7.7%	100%
Medium	92.4%	7.6%	100%
High Minimum	96.3%	3.7%	100%

* p<.05, ** p<.01, *** p<.001

Impact of TIS on the Sanctions Imposed on Sex Offenders with Disciplinary Incidents

As with the examination of the disciplinary incidents of the murderers, analyses were also performed with the sex offender sample to determine if an inmate's TIS status had any influence on the types of sanctions imposed by IDOC for disciplinary incidents, including placement in segregation, loss of good conduct credit, loss of commissary privileges, and loss of gym/yard privileges. As with the analyses described above, bivariate and multivariate analyses were performed to determine if the inmate's TIS status had an independent relationship to any of these sanctions being imposed.

Among the sample of sex offenders, the most prevalent form of sanction imposed on those receiving disciplinary tickets was loss of commissary privileges, and no statistical difference was noted between those sex offenders subject to TIS and those not: roughly 80 percent of both groups lost their commissary privileges for a period of time as a result of a disciplinary incident. For the punishments of placement in segregation and loss of gym/yard privileges, again, differences between the TIS and non-TIS inmate were not evident. Finally, and interestingly, when comparisons of whether or not the inmate lost good conduct credit and their TIS status were made, we found that those inmates subject to TIS were more likely than those not subject to TIS to lose good conduct credit. This pattern was also evident in the multivariate analyses performed. This is interesting to

note because there were not any substantive differences noted in the prevalence or pattern of disciplinary incidents among the TIS and non-TIS sex offenders, and sex offenders subject to TIS are only eligible to earn up to a 15 percent reduction in their sentence (i.e., 85 percent must be served under TIS) as opposed to the non-TIS sex offenders, who can earn up to 50 percent off of their sentence through good conduct credits.

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APPENDIX 1

Ordinary Least Squares Results for Analyses of Murder Sentence Lengths (in Years)

OLS Results for Maximum Sentence Length for Murder

	Unstandardized Coefficients		Standardized Coefficients		
	B	S. E.	Beta	t	Sig.
Constant***	37.590	1.396		26.920	0.000
Admission Age in Years	0.048	0.026	0.023	1.845	0.065
Gender (Female=0, Male=1) ***	6.081	0.868	0.079	7.003	0.000
Race (White=0, Non-White=1) ***	-2.370	0.620	-0.047	-3.826	0.000
Marital Status (Single=0, Married=1) **	2.769	1.034	0.047	2.677	0.007
Education Level (No High-School/GED=0, High-School or GED=1)	0.547	0.700	0.015	0.781	0.435
Children (None=0, 1 or more=1) **	1.302	0.497	0.035	2.620	0.009
Gang Status (None=0, 1=Yes)	-0.118	0.472	-0.003	-0.251	0.802
Region of Illinois (Rest of Illinois=0, Cook County=1)***	-3.334	0.521	-0.077	-6.403	0.000
Prior Prison Sentences (None=0, 1 or more=1) ***	4.938	0.520	0.117	9.505	0.000
Year Trend 1989...1996 and 1997-2008***	1.101	0.137	0.161	8.031	0.000
Not TIS Relative to Pre-TIS	-1.648	0.857	-0.036	-1.923	0.055
TIS Relative to Pre-TIS***	-3.941	0.798	-0.101	-4.938	0.000

* p<.05, ** p<.01, *** p<.001

Logistic Regression Results for Murder Sentences

Logistic Regression Results with Dependent Variable as Maximum Sentence at or under Statutory Maximum (Coded as 0) or over Statutory Maximum, Including Life and Death (Coded as 1)

	B	S.E.	Wald	df	Sig.	Exp(B)
Age at Admission***	0.021	0.003	39.563	1	0.000	1.021
Gender						
Female (reference)						
Male***	0.468	0.139	11.292	1	0.001	1.597
Race						
White (reference)***			29.834	2	0.000	
African-American***	-0.350	0.080	19.130	1	0.000	0.705
Hispanic/Other***	-0.589	0.114	26.732	1	0.000	0.555
Marital Status						
Single/Divorced (reference)			0.382	2	0.826	
Married/Common Law	0.055	0.090	0.369	1	0.544	1.056
Education Level						
No HS/GED (reference)			3.550	2	0.170	
HS/GED	0.127	0.070	3.294	1	0.070	1.135
Children						
No Children (reference)						
One or More Children	-0.029	0.070	0.168	1	0.682	0.972
Gang Status						
No Affiliation (reference)						
Affiliated	-0.121	0.067	3.195	1	0.074	0.886
Region of Illinois						
Cook County (reference)***			67.783	3	0.000	
Collar County***	0.705	0.102	47.542	1	0.000	2.023
Other Metro Area***	0.459	0.084	29.787	1	0.000	1.582
Rural Area***	0.472	0.120	15.590	1	0.000	1.603
Prior Prison Sentences						
No Prior Sentences (reference)						
One or More Sentences***	0.688	0.068	102.105	1	0.000	1.990
Truth-in-Sentencing						
Pre-TIS (reference)***			143.742	2	0.000	
Non-TIS	-0.023	0.081	0.081	1	0.776	0.977
TIS***	-0.835	0.074	125.886	1	0.000	0.434
Constant***	-2.540	0.172	217.086	1	0.000	0.079

* p<.05, ** p<.01, *** p<.001

APPENDIX 2

Ordinary Least Squares Results for Analyses of Class X Sex Offender Sentence Lengths (in Years)

OLS Results for Maximum Sentence Length for Class X Sex Offenders

	Unstandardized Coefficients		Standardized Coefficients		
	B	S.E.	Beta	t	Sig.
Constant***	9.693	1.335		7.261	0.000
Admission Age in Years ***	0.069	0.011	0.081	6.460	0.000
Gender (Female=0, Male=1)	-0.187	1.289	-0.002	-0.145	0.884
Race (White=0, Non-White=1)	0.157	0.278	0.008	0.563	0.574
Marital Status (Single=0, Married=1)	-0.078	0.520	-0.003	-0.149	0.881
Education Level (No High-School/GED=0, High-School or GED=1)	0.125	0.407	0.006	0.308	0.758
Children (None=0, 1 or more=1) **	0.451	0.280	0.024	1.611	0.107
Gang Status (None=0, 1=Yes)***	1.041	0.323	0.042	3.229	0.001
Region of Illinois (Rest of Illinois=0, Cook County=1)	-0.508	0.269	-0.026	-1.888	0.059
Prior Prison Sentences (None=0, 1 or more=1) ***	4.954	0.282	0.223	17.578	0.000
Not TIS Relative to Pre-TIS	0.274	0.385	0.009	0.711	0.477
TIS Relative to Pre-TIS*	-0.505	0.251	-0.026	-2.011	0.044

* p<.05, ** p<.01, *** p<.001

APPENDIX 3

Ordinary Least Squares Results for Analyses of Murder Disciplinary Tickets

OLS Results for Total Disciplinary Tickets Received by Murder Offenders

	Unstandardized Coefficients		Standardized Coefficients		
	B	S.E.	Beta	t	Sig.
Constant***	56.870	6.493		8.759	0.000
Admission Age in Years ***	-0.559	0.095	-0.217	-5.909	0.000
Projected Time to Serve in Years ***	-0.094	0.027	-0.125	-3.470	0.001
Years Served *	1.297	0.590	0.071	2.197	0.028
Gender (Female=0, Male=1) ***	-27.562	3.034	-0.322	-9.084	0.000
Race (White=0, Non-White=1)	2.057	2.108	0.034	0.976	0.329
Marital Status (Single=0, Married=1)	0.534	4.050	0.004	0.132	0.895
Children (None=0, 1 or more=1)	-2.803	2.055	-0.055	-1.364	0.173
Education Level (No High-School/GED=0, High-School or GED=1)	-1.643	2.277	-0.027	-0.722	0.471
Gang Status (None=0, 1=Yes)	2.453	1.669	0.055	1.470	0.142
Region of Illinois (Rest of Illinois=0, Cook County=1)	0.818	1.844	0.016	0.443	0.658
Prior Prison Sentences (None=0, 1 or more=1)	2.567	2.126	0.052	1.208	0.228
Maximum Security (No=0, Yes=1)	-3.144	1.796	-0.065	-1.751	0.080
Truth-in-Sentencing (No=0, Yes=1) **	-4.666	1.599	-0.103	-2.919	0.004

R²=.188, Adjusted R²=.175; Model F=14.125, p<.001
p<.05, ** p<.01, *** p<.001