

# **Testing the Deterrent Effect of Sentence Lengthening Legislation on Violent Crime**

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## **Abstract**

This paper explores the assumed deterrent effect of longer sentence lengths on the frequency at which violent crime is committed. To do this, an econometric analysis of state panel data using a two-way fixed effects model examines the effects of sentence lengthening legislation by comparing a treatment group of states that have enacted stricter sentencing policies (including truth-in-sentencing, three-strike rules, and determinate sentencing) to a control group of states that have not done so. Many policymakers and voters believe that longer sentencing increases the costs of crime which discourages potential criminals, thus having a determinant effect on the rate at which violent crime is committed. This paper tests the hypothesis that the supply of violent crime is, for the most part, inelastic to costs such as sentence lengths. Some possible explanations for this lack of elasticity might be the complexity of the legal system itself and/or a misunderstanding of the motivations that incentivize both criminals and law enforcement.

## **Introduction:**

Reducing violent crime rates is an incredibly complex process which has led to a variety of potential solutions, not all of which have proven to be effective. Many voters and policy makers believe that increased punishments for violent crime increases the cost of committing these crimes which will reduce the supply of violent crime across sectors (Marvel and Moody, 2001). After all, as costs of normal goods or services (including social goods) increase, their supplies decrease. Because it is seemingly simple to manipulate, this cost is often sentence length which can be adjusted through sentencing policies, largely by implementing truth-in-sentencing legislation, three-strike rules, and/or determinate sentencing. The underlying logic is that if sentence lengths are increased for violent crimes, potential criminals will be discouraged from committing them. While this idea is theoretically sound according to the economic theory of supply and demand, such policies assume that the relationship between violent crime and sentencing length is elastic; this research paper intends to test this assumption.

Many states have varying degrees of penalties for violent crimes that have changed over time. Whether or not these increased costs actually have a negative effect on the frequency at which violent crimes are committed is an important issue to address because if the answer is no, resources are being misallocated at great public cost. Crime in and of itself is costly to society through indirect and direct costs including external costs such as increased medical and mental health spending for individuals, reduced quality of life for victims affected by violent crime, and lost productivity as well as social costs such as communal pain and suffering (Chalfin, 2015). Some of these direct costs include increased prison space, taxes to support the cost of living for these inmates, and staffing requirements as exemplified by the steadily rising ratio of dollars spent on corrections to dollars spent on education in the United States (Orrick and Vieraitis, 2015). Other indirect costs include the economic impact of less individuals in the labor force (which can be quantified as approximately 2%-6% of annual GDP), as well as the more personal impact of these longer sentences (Chalfin, 2015). This might include the emotional and economic consequences for the family members of these prisoners (for example, the lost earnings of a family who depends financially on a person convicted of a violent crime or the health consequences of incarceration). Costs in anticipation of crime otherwise known as defensive or precautionary expenditures are also important to consider as well as the opportunity costs of those who commit crimes and societal resources responding to and preparing against crimes (Brand, et al., 2000).

There are many different ways states can go about implementing sentence lengthening policies. One of these is to adopt what are called “three-strike” laws. A “strike” can be defined as a conviction for a violent crime such as murder, rape, robbery, attempted murder, assault with intent to rape/rob, etc. (Helland and Tabarrock, 2007). Typically, three-time violators of these types of serious, violent crimes are sentenced to mandated life imprisonment, otherwise known as a three-strike rule (Hinds, 2005). The goal of this type of legislation is to decrease crime rates by increasing the cost of crime and keeping likely offenders imprisoned. Unfortunately, current

literature does not contain a wide variety of studies regarding the deterrent effects of three-strike laws on crime rates or prison populations. More research is needed to inform policy, hence the purpose of this paper.

Truth-in-sentencing policies are another way in which states have attempted to discourage violent crime within their jurisdictions. Because actual time served and time sentenced are often very different, states enact truth-in-sentencing that requires offenders to serve a substantial amount of time sentenced (Ditton, et al., 1999). Most of these policies require convicts to serve at least 85% of their sentence, but specific requirements vary by state between 50-100% (Ditton, et al., 1999). These types of policies were encouraged to states by the federal government through the Violent Crime Control and Law Enforcement Act of 1994, as amended in 1996, that offered federal grants to increase prison space for states who do choose to implement them (Sabol and W.J., 2002). Current literature indicates that truth-in-sentencing lowers criminal activity in the adopted state but causes increased crime levels in neighboring states due to the geographic migration of such activities (Ross, 2012). Another unintended consequence of the state-wide implementation of these stricter policies is a state and local government response of decreasing spending on police protection which may offset any benefits of the legislation (Ross, 2012).

Many states have implemented, and abolished, determinate sentencing in their legal sentencing structure as well. Key to determinate sentencing policies are fixed prison sentences for offenses so as to increase time served and ensure a proportional punishment to crimes committed (Lawrence, 2015). Different types of determinate sentencing exist such as determinate discretionary where a narrow range of sentence lengths is established (examples include Indiana, Illinois, and Maine) and presumptive sentencing where specific sentences are set for different crimes (examples include Arizona, California, Colorado, and New Mexico).<sup>1</sup> Some states have implemented this type of legislation with the goal of reducing bias within the legal system; for example, Minnesota's decision to implement determinate sentencing did result in a diminished disparity in sentencing practices based on socioeconomic factors; however, this improvement has been offset by bias introduced in presentencing decisions instead (Miethe and Moore, 1985). It is unclear whether or not these findings are still significant today or if the results are applicable across states. This paper can help to answer that question. The overarching goal, however, of Minnesota and other states who have implemented determinate sentencing and, more specifically, presumptive sentencing is to increase the costs of violent crime so as to discourage criminals. Current literature has found that determinate sentencing does have a significant impact on reducing historical crime rates, but it is unclear whether this is true today (Stemen and Rengifo, 2011). Other than in the state of Indiana, research also indicates that determinate sentencing does not significantly increase prison populations which would suggest

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<sup>1</sup> State of Arkansas Legislative Council. (1980). Discussion of Determinate and Indeterminate Sentencing Procedures and Laws. The Bureau of Legislative Research.

that it might disincentivize violent crime or not function within the legal system in the way it was intended (Marvell and Moody, 1996).

Current literature indicates a complicated relationship between different types of sentence lengthening policy and the rate at which criminals are convicted. Using a hierarchical multivariate linear model, Zhang et al. was able to determine that there is no statistically significant correlation between sentencing reform focused on increasing sentence lengths of violent crime and changes in overall prison populations (2009). This would indicate that many of the aforementioned costs of imprisonment on society including emotional and economic costs to prisoners themselves as well as their families, the economic impact of a smaller labor force, and an increased need for prison space and staffing which result in higher taxes would not be addressed. Sorenson and Stemen, on the other hand, found conflicting evidence as to the relationship between mandatory sentencing, productive guidelines, and the implementation of the three-strike rule on crime rates (2002). Similarly, Meritt et al. found that Measure 11, a mandatory minimum sentencing policy, had a much smaller impact on crime rates than what was anticipated which indicates a lack of understanding of the possible unintended consequences of such legislation (2006). According to the study, this is largely due to the complexity of the legal system which supports the idea that reducing crime rates is a complex economic issue as opposed to a straightforward problem that merits a simple solution (Meritt, et al., 2006). Other possible unintended consequences might include criminals attempting to avoid convictions by killing victims, witnesses, and police officers and/or bribing law enforcement (Kovandzic, et al., 2004). Other evasive actions might include criminals moving to different jurisdictions or to other types of crime to avoid these increased punishments (Marvel and Moody, 2001).

Most academic literature regarding the determinant effect of these sentencing policies on crime rates is done by studying the change in crime before and after the implementation of these types of sentencing rules. This paper can contribute to this research through looking more specifically at violent offenders and measuring the frequency at which violent crimes are committed as opposed to the severity of them. If a determinant effect exists, one expects crime rates to decrease as a result of the increase in costs of committing violent crimes; otherwise, one can assume that the supply of crime within society is inelastic to sentence lengthening policies and legislation decisions should be reflective of that finding.

If the relationship between violent crime and its cost is in fact inelastic, increasing prison sentences will not have important implications on crime. Instead of assuming that this is a simple problem (where one action triggers a clear response), it should be addressed as an economic problem which would imply that there are more complex underlying relationships that might require more innovative solutions to address the root issue. This research question intends to highlight this reality by studying the reverse deterrent effect of sentence lengthening legislation on violent crimes rates with the goal of contributing to current research affecting public policy related to violent crime reduction so as to improve regulation efforts.

The data and variables used to examine this question are outlined in the data section of this paper along with a discussion of important assumption violations and a description of the variables of interest. The model created for this paper is highlighted in the methods section along with the mathematical justification of the model, further explanations for variables, and a continued discussion of assumption violations. The results section of the paper answers the research question using real data and assigns a potential omitted variable bias. Finally, the conclusion suggests further areas of research and provides a justification for the results of the model.

### **Data:**

The outcome variable of interest of this paper is the supply of crime within society. The data used to obtain this variable includes Jacob Kaplan's Concatenated Files: Uniform Crime Reporting: Offenses Known and Clearances by Arrest.<sup>2</sup> This is a cleaned data set extracted from the Uniform Crime Reports published by the FBI from the years 1961-2020; however, this analysis will focus on the years 1984-2019 as those years contain the data necessary to create a two-way fixed effects model studying the impact of the implementation of sentence lengthening policies over the time period in which these policies have been enacted. The UCR data is collected by the FBI from over 18,000 city, university/college, county, state, tribal, and federal law enforcement agencies, making it the national repository for crime data. Although the data is collected across a wide range of agencies, a manual outlining uniform classification as well as scoring techniques was provided to ensure consistent and comparable data across states. Kaplan's files clean the data provided by the UCR and compile the yearly data into one panel data set that includes crime statistics organized by agency for all 50 states. The data would not be considered a random sample of the population because it measures the population itself. Every crime of which the police or other law enforcement agencies are aware is included in this data set.

The data was then cleaned again for the purposes of this paper. Observations not pertaining to specific states were dropped which resulted in 1,312 observations being dropped out of 926,142 (approximately .15% of the data set). This is because this paper is only interested in measuring crime rates by state and should not double count any crimes. Because of the very small portion of the data set being dropped and the nature of the observations, this is not a concern to the validity of this model.

Despite the thoroughness of Kaplan's files, the years 1997, 2012, 2013, 2014, 2016, and 2017 were excluded from the data set. For that reason, supplementary data was used as collected

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<sup>2</sup> Kaplan, Jacob. (2022). Jacob Kaplan's Concatenated Files: Uniform Crime Reporting (UCR) Program Data: Offenses Known and Clearances by Arrest (1960-2021)

by the FBI through the Uniform Crime Reporting Program<sup>3</sup>. As explained above, this data is collected through the submission of statistics by regional law enforcement agencies that participate in the program which are then used to estimate for non-reporting or partially reporting agencies. This data was cleaned to only include violent crime totals.

Kaplan's concatenated files contain the total crimes committed in each state; therefore, a new variable was created by dividing Kaplan's data by total population data as collected by the U.S. Census Bureau which outlines the number of residents within each state as of July 1<sup>st</sup> between the years 1984-2019 and published by the Iowa State University Iowa Community Indicators Program<sup>4</sup>. The U.S. Census Bureau calculates their population estimates based on current birth, death, and migration data to create yearly time-series estimates beginning with the last decennial census data.<sup>5</sup> This birth and death data<sup>6</sup> was collected from the NCHS and state departments. Migration data<sup>7</sup> was collected using the American Community Survey (a survey of a random sample of 3.5 million households within the U.S. that gathers information related to demographics and residency), the Current Population Survey (a survey of a random sample of about 60,000 households within the U.S. that are held in the survey for four consecutive months, released from the survey for 8, and re-entered for four months before leaving permanently), and the decennial census conducted by the U.S. Census Bureau that counts every resident of the United States.

The independent variables of interest of this model are three non-mutually exclusive dummy variables that take on a value of one in the year in which the state enacted truth-in-sentencing, three-strike rules, or determinate sentencing respectively and a value of 0 otherwise. This variable turns on in the year the policy was enacted and stays on during subsequent years.

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<sup>3</sup> Criminal Justice Information Division. (1997). Section II: Crime Index Offenses Reported [Index of Crime: Region, Geographic Division, and State, 1996-1997]. Federal Bureau of Investigation.

Criminal Justice Information Division. (2013). 2013 Crime in the United States [Crime in the United States by Region, Geographic Division, and State, 2012-2013]. Federal Bureau of Investigation.

Criminal Justice Information Division. (2014). 2014 Crime in the United States [Crime in the United States by Region, Geographic Division, and State, 2013-2014]. Federal Bureau of Investigation.

Criminal Justice Information Division. (2015). 2015 Crime in the United States [Crime in the United States by Region, Geographic Division, and State, 2014-2015]. Federal Bureau of Investigation.

Criminal Justice Information Division. (2017). 2017 Crime in the United States [Crime in the United States by Region, Geographic Division, and State, 2016-2017]. Federal Bureau of Investigation.

Criminal Justice Information Division. (2018). 2018 Crime in the United States [Crime in the United States by Region, Geographic Division, and State, 2017-2018]. Federal Bureau of Investigation.

<sup>4</sup> Iowa Community Indicators Program. (2020). Annual Estimates of the Total Population for States. Iowa Community Indicators Program. <https://www.icip.iastate.edu/tables/population/states-estimates>

<sup>5</sup> U.S. Census Bureau (2021). Population Estimates and Projections. Retrieved from <https://census.gov/data/developers/data-sets/popest-popproj.html>.

<sup>6</sup> U.S. Census Bureau (2021). Births and Deaths. Retrieved from <https://www.census.gov/topics/health/births-deaths.html#:~:text=The%20U.S.%20Census%20Bureau%20uses,in%20our%20population%20projections%20program>.

<sup>7</sup> U.S. Census Bureau (2021). Surveys and Programs Contributing to Migration/Geographic Mobility. Retrieved from <https://www.census.gov/topics/population/migration/surveys-programs.html>.

The control group of each variable included in this study will be states that never enact the policy of interest (truth-in-sentencing, three-strike rules, and determinate sentencing) while the treatment group of each variable includes states that have enacted the policy of interest at any point. This data was collected using a variety of online resources including official state and federal reports, reliable news sources, and academic papers.

Truth-in-sentencing and three-strike law data was collected using two primary sources; the first being a Bureau of Justice Statistics Report written by Ditton, et al. (1999) and the second being a research report published by the Urban Institute Policy Justice Center (Sabol and W.J., 2002). These sources, while extremely useful, were not complete and were therefore validated using other resources including legal and news articles as well as academic papers. Supplementary resources were used to validate truth-in-sentencing laws in Nevada,<sup>8</sup> New Hampshire,<sup>9</sup> Idaho,<sup>10</sup> South Carolina,<sup>11</sup> the District of Columbia,<sup>12</sup> and others.<sup>13</sup> An analysis of the applications of three-strike rules was used as a check for all states (Corbett, 2021). Further validation regarding three-strike laws was conducted for Arizona,<sup>14</sup> New York,<sup>15</sup> and Idaho.<sup>16</sup> Determinate sentencing data was also collected for all 50 states primarily using an article published by the Robina Institute of Criminal Law and Justice of the University of Minnesota (Reitz, et al., 2018). This information was validated by a report published by the National Conference of State Legislatures (Lawrence, 2015). Additional validation was performed for the District of Columbia,<sup>17</sup> Connecticut,<sup>18</sup> Alaska,<sup>19</sup> Colorado,<sup>20</sup> New Jersey,<sup>21</sup> Pennsylvania,<sup>22</sup>

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<sup>8</sup> Anthony, N. C. (2016). History of Criminal Sentencing Categories in Nevada. Advisory Commission on the Administration of Justice.

<sup>9</sup> Whidden, J. (2022). 'Tough on crime' attitude leads to explosive growth of NH prison system. Valley News.

<sup>10</sup> Criminal Procedures, Title 19 § 2513 (1970)

<sup>11</sup> S.C. Gen. Assemb. H. 3186. Sess. 115, 2003-2004 (2004).

<sup>12</sup> Truth in Sentencing Amendment Act of 1998, D.C. Law 12-165, D.C. Code § 24-403.01 (1998).

<sup>13</sup> Ditton, P. M., & Wilson, D. J. (1999). Truth in sentencing in state prisons. US Department of Justice, Office of Justice Programs, Bureau of Justice Statistics

<sup>14</sup> Arizona State Senate Research. (2006). Fact Sheet for S.C.R. 1010. Arizona State Senate Forty-Seventh Legislature.

<sup>15</sup> N.Y. Penal Law Sentences of Imprisonment §70.08(3). (2013).

<sup>16</sup> Criminal Procedures, Title 19 § 2514 (1970).

<sup>17</sup> Sentencing Reform Amendment Act of 2000, D.C. Law 13-302 (2000).

<sup>18</sup> Program Review and investigations Staff. (2006). An Evaluation of Connecticut's Justice Reinvestment Initiative. P.A. Compliance Project

<sup>19</sup> Torgerson, M. (1986). The Impact of presumptive Sentencing on Alaska's Prison Population. Alaska State Legislature House Research Agency.

<sup>20</sup> Colorado Legislative Council. (1994). Brief History of Sentencing Legislation in Colorado. U.S. Department of Justice Office of Justice Programs.

<sup>21</sup> New Jersey Criminal Sentencing & Disposition Commission. (2019). Annual Report.

Fairchild, M. (1980). Determinate Sentencing Laws. National Criminal Justice Reference Service.

<sup>22</sup> State of Arkansas Legislative Council. (1980). Discussion of Determinate and Indeterminate Sentencing Procedures and Laws. The Bureau of legislative Research

Tennessee,<sup>23</sup> New York,<sup>24</sup> and others.<sup>25</sup> Figures 1-3 visually depict the implementation of truth-in-sentencing, three-strike rules, and determinate sentencing, respectively, across states and years. Tables 1-3 organize the information illustrated in Figures 1-3 into control and treatment groups where the treatment groups are separated by classification as early adopters, adopters, and/or late adopters.

**Figure 1 - Truth-in-Sentencing**

	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019		
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<sup>23</sup> State of Arkansas Legislative Council. (1980). Discussion of Determinate and Indeterminate Sentencing Procedures and Laws. The Bureau of legislative Research

<sup>24</sup> New York State Permanent Commission on Sentencing. (2014). A Proposal for “Fully Determinate” Sentencing for New York State. John Jay College of Criminal Justice.

<sup>25</sup> Fairchild, M. (1980). Determinate Sentencing Laws. National Criminal Justice Reference Service





<b>Table I - Truth-in-Sentencing</b>		
<i>Early Adopters</i>	<i>Adopters</i>	<i>Control</i>
UT	DC	AL
ID	WI	AK
NH	NY	AZ
PA	OK	CO
	NJ	HI
	IA	IN
	OH	KY
	SC	MD
	CT	MA
	FL	MT
	GA	NE
	IL	NM
	LA	RI
	ME	SD
	MS	TX
	ND	VT
	TN	WV
	VA	WY
	AR	
	CA	
	MI	
	MO	
	NV	
	NC	
	KS	
	MN	
	DE	
	OR	
	WA	

<b>Table II - Three-Strike Rules</b>			
<i>Early Adopters</i>	<i>Adopters</i>	<i>Late Adopters</i>	<i>Control</i>
DE	AR	NY	AL
ID	FL	MA	AK
MD	MT		AZ
TX	NV		DC
	NJ		HI
	ND		IL
	PA		IA
	SC		KY
	TN		ME
	UT		MI
	VT		MN
	CA		MS
	CO		MO
	CT		NE
	GA		NH
	IN		OH
	KS		OK
	LA		OR
	NM		RI
	NC		SD
	VA		WV
	WI		WY
	WA		

<b>Table III - Truth-in-Sentencing</b>		
<i>Early Adopters</i>	<i>Adopters</i>	<i>Control</i>
AK	DC	AL
CA	WI	AR
CO	OH	GA
CT	NY	HI
FL	VA	ID
IL	AZ	IA
IN	NC	KY
ME	WA	LA
MN	KS	MD
NJ	DE	MA
NM	OR	MI
TN		MS
		MO
		MT
		NE
		NV
		NH
		ND
		OK
		PA
		RI
		SC
		SD
		TX
		UT
		VT
		WV
		WY

Because this model compares a control group of states to a treatment group of states, omitted variable bias is not an extremely concerning issue. Despite this, the model includes covariates in order to reduce any possible omitted variable bias so as to better account for the assumption of zero-conditional mean. These variables include current-dollar GDP<sup>26</sup> as well as median household income<sup>27</sup> both measured by state between the years 1984-2019. Divorce rates<sup>28</sup> by state were also included to control for the effects of a single-parent household on crime rates. Unemployment data<sup>29</sup> was controlled for as calculated by the U.S. Bureau of Labor and

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<sup>26</sup>U.S. Bureau of Economic Analysis. (2020). Gross Domestic Product by State, Fourth Quarter and Annual [Annual GDP by State and Industry]. U.S. Department of Commerce.

U.S. Bureau of Economic Analysis. (2004). Comprehensive Revision of Gross State Product, 1977-2002, and Accelerated GSP Estimates for 2003 [GDP by State and Industry – SIC Industries]

<sup>27</sup> St. Louis FED. (2021). Release Tables [Median Household Income by State, Annual]. Economic Research – Federal Reserve Bank of St. Louis.

<sup>28</sup> National Center for Health Statistics. (2021). Marriage and Divorces [Divorce rates by State: 1990, 1995, and 1999-2020]. Center for Disease Control and Prevention.

An imputed mean method as used for missing data of all states between the years 1984-2000 and for missing data of the states Georgia, Hawaii, Louisiana Minnesota, New Mexico, and Oklahoma. California and Indiana were left as missing due to a lack of data.

<sup>29</sup> U.S. Bureau of Labor Statistics. (2019). Local Area unemployment Statistics [Unemployment rates for States]. United States Department of Labor.

Statistics to control for potential economic downturns. Demographic data<sup>30</sup> such as categorical variables for race and sex were also included.

This data is summarized in Tables 4-6 which highlight the mean of each of these covariates for a treatment and control group of truth-in-sentencing policies, three-strike rules, and determinate sentencing legislation, respectively. Most of the differences between the means are statistically insignificant due to their extremely high p-values, but some remain statistically different. That finding is reflected in the robustness checks of this paper and is discussed in further detail in the results section.

This model does face some inherent assumption violations including that of random assignment. States are not randomly assigned these policies; individual states consider each policy carefully and make an active choice to implement or exclude them from state legislature. An extremely likely contributing factor correlated with the decision to eliminate these types of legislation that may account for a great deal of this random assignment issue is the popular ideology of the state. For that reason, strong nominate measures of state government ideology were controlled for and incorporated into the model.<sup>31</sup> This data, however, was only available between the years 1960-2017 for all states except the District of Columbia but this will not cause a problem to the study as no states implemented these practices after the year 2017 and a mean imputation method was used to make predictions for those two missing years for each state as well as for all years for the District of Columbia.

## Methods:

To test the potential determinant effect of sentence lengthening legislation on crime rates within the U.S., this paper uses a two-way fixed effects model that compares a treatment group of states to a control group of states for each policy (truth-in-sentencing, three-strike rules, and determinate sentencing). In this model, the control group represents states that have not implemented sentence lengthening policies. The treatment group represents states who have chosen to implement these policies. In math type, this model can be described as

$$Y_{it} = \lambda_i + \gamma_t + \beta_1(T_{it}) + \beta'X + u_{it}$$

where  $Y_{it}$  represents violent crime rates per 100,000 people in state  $i$  and year  $t$ ,  $\lambda_i$  represents dummy variables for state  $i$ ,  $\gamma_t$  controls for the fixed effects of year  $t$ , and  $T_{it}$  is a

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<sup>30</sup> CDC WONDER. (2020). Bridged-Race Population Estimates 1990-2019. Center for Disease Control and Prevention.

This data was collected between the year 1990-2019. Data prior to 1990 was unavailable and therefore the mean imputation method was used. This is acceptable because race and sex compositions of states are not expected to vary greatly between years.

<sup>31</sup> Berry, William D., Richard C. Fording, Evan J. Ringquist, Russell L. Hanson and Carl Klarner. 2010. "Measuring Citizen and Government Ideology in the American States: A Re-appraisal." *State Politics and Policy Quarterly* 10: 117-35.

dummy variable that turns on when state  $i$  has implemented the respective policy in year  $t$ .  $\beta'$  represents the covariates included in the model to control for any other variables across states that might also have an effect on crime rates (annual GDP, median household income, unemployment rates, sex, race, divorce rates, and government ideology) and  $u_{it}$  represents the error term. While these variables reduce the omitted variable bias and random assignment violation to account for the zero-conditional mean assumption, it is possible that there are still variables not included in this model that might cause biased estimators. Four models will be run to examine the distinct effects of truth-in-sentencing policies, three-strike rules, and determinate sentencing legislation on crime rates per 100,000 people with robustness checks for each.

As previously mentioned, the variable of interest of this model is the supply of crime within society. This is measured as crime rates by state and is incorporated into the model as a continuous variable. More specifically, the model specifies crime rates per 100,000 people so as to ease the interpretation of coefficients and increase their applicability as it relates to policy decisions on the state and federal level.

The two-way fixed effects model allows this paper to most effectively control for both the year and state to identify not only the impact of implementation on one state but rather the overall expected effects across states. A possible variation of the two-way fixed effects model called a Mundlak regression is popular but would not be appropriate for this specific research question as it would test both the average of states across years and the average of years across states (Hedges, 1994). While this may be useful in certain contexts, this model is simply interested in observing the averages of states across time, hence the use of the two-way effects model. This model can also allow for certain assumption violations in heterogenous trends as well as spillover effects (Hedges, 1994). After analyzing different model types and variations, Harwell, et al. found that two-way fixed effects models are generally less sensitive to assumption violations than are other models (1992).

The model also faces a potential violation of the homoskedasticity assumption that the variance of the error term by state is constant. This assumption would not be appropriate for this specific model but will be accounted for using the `vce(cluster)` function in Stata which clusters the standard errors of each observation by state, thus accounting for the assumption of homoskedasticity.

As discussed, potential limitations of this model primarily revolve around the lack of random assignment that violates the assumption of zero-conditional mean. This is accounted for through the use of covariates but likely does not account for all potential biases. This bias will be assigned in the results section of this paper. Another limitation is the data collected. Not all agencies reported crime statistics which led the FBI to create estimates based on surrounding agencies and, while agencies were provided with guidelines as to how to report these crimes, there is a possibility that not every agency followed them exactly which would lead to

inconsistencies. Lastly, a simple mean imputation method was used to account for missing data which may not accurately depict the true data.

## **Results:**

After running multiple two-way fixed effects models regressing crime rates per 100,000 people on different treatment groups of states, this paper finds that truth-in-sentencing, three-strike rules, and determinate sentencing have varied impacts on violent crime rates. These results are highlighted by Table VII.

According to the base model, a state that enacts truth-in-sentencing can expect a decrease in crime rates per 100,000 people of approximately 3.24 points. This figure, however, is not statistically or economically significant by any reasonable standards. When controlled for covariates (state GDP, median household income, population, unemployment rates, divorce rates, race, sex, and government ideology), truth-in-sentencing is expected to increase crime rates per 100,000 people by 218.9 points which is economically and statistically significant at the 99% confidence level. When controlled for three-strike rules and determinate sentencing implementation, this figure changes again to suggest that truth-in-sentencing decreases crime rates per 100,000 people by 42.77 points but this figure is not statistically significant at any relevant level. Although the coefficient changes to suggest an increase in crime rates per 100,000 people of 112.2 points, the same conclusion is true when covariates are added to this model.

Three-strike rules, however, are statistically significant at the 99% confidence level across all models run by this paper. When analyzed by itself, three-strike rules are expected to increase crime rates per 100,000 people by 110.6 points. When covariates are controlled for, this magnitude increases to 313.4 points. After controlling only for the effects of truth-in-sentencing and determinate sentencing, three-strike rules are expected to increase crime rates per 100,000 people by 156.8 points. This is most likely due to controlling for the effects of determinate sentencing as opposed to truth-in-sentencing implementation due to the statistical insignificance of truth-in-sentencing. Finally, after controlling for determinate sentencing, truth-in-sentencing and covariates, three-strike rules are expected to increase crime rates per 100,000 people by 269.8 points, and remains significant at the 99% confidence level.

**Table VII**  
*The Impact of Sentence Lengthening Legislation on Crime Rates*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Truth-in-Sentencing	-3.24 (-0.05)	218.93*** (3.30)					-42.77 (-0.77)	112.25 (1.55)
Three-Strike Rules			110.64*** (3.91)	313.37*** (6.27)			156.79*** (3.35)	269.84*** (5.25)
Determinate Sentencing					-81.05 (-0.67)	238.47*** (5.17)	-110.11 (-0.96)	118.11* (1.77)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Constant	506.54*** (18.02)	6362.88 (1.34)	459.98*** (39.93)	5982.66 (1.22)	538.02*** (10.86)	7976.58* (1.68)	506.41*** (11.89)	4809.70 (1.03)
Controls	-0.0005	0.1267	0.0015	0.1317	-0.0001	0.1244	0.0020	0.1337
No. of Obs.	1,836	1,743	1,836	1,743	1,836	1,743	1,836	1,743
R-squared	0.0000	0.1337	0.0021	0.1387	0.0005	0.1314	0.0036	0.1417
Number of states								

The effects of determinate sentencing, however, are not as clear. When run as a base model, determinate sentencing is expected to decrease crime rates per 100,000 people by 81.05 points; however, this figure is not statistically significant meaning that this paper cannot confidently conclude that determinate sentencing, by itself, has an impact on crime rates. When controlled for covariates, this magnitude increases to 238.5 points and is significant at the 99% confidence level. After controlling for truth-in-sentencing and three-strike rule implementation, this estimate decreases to indicate a decrease in crime rates per 100,000 people of 110.1 points but becomes statistically insignificant, which is similar to the robustness pattern observed in truth-in-sentencing implementation. Lastly, when controlled for truth-in-sentencing and three-strike rules as well as covariates, determinate sentencing is expected to increase crime rates per 100,000 people by 118.1 points which is economically and statistically significant, but only just barely at the 90% confidence level.

The statistical significance of truth-in-sentencing and determinate sentencing when only accounting for covariates is likely due to them being proxy variables (albeit poor ones) for the implementation of three-strike rules. This is because the magnitude of truth-in-sentencing implementation and determinate sentencing implementation when accounting for possible covariates is similar to that of three-strike rules which remains statistically significant at the 99% confidence level throughout each model.

Three-strike rules remain statistically and economically significant in every model run, but not in the way that policy makers expected. Instead of decreasing crime rates, implementation of these rules increased them. The potential causes of this phenomenon are explained in the conclusion section.

Determinate sentencing, however, remains significant at the 90% confidence level after accounting for truth-in-sentencing and three-strike rules as well as covariates. Because of its higher p-value, if one uses the standard acceptable confidence level of 95%, the coefficient is not statistically significant. This might indicate that determinate sentencing has a possible

encouraging effect on violent crime rates, but additional research is needed. Further explanation for this phenomenon is included in the conclusion.

Because the model compares a treatment to a control group, omitted variable bias is not a huge concern to this model but was accounted for using covariates that include state GDP, unemployment rates, divorce rates, median household income by state, state populations, race, sex, and government ideology. Given that some of the means of these covariates were statistically different between the control and treatment groups as outlined in Tables 4-6 (likely due to the lack of random assignment of this model), the statistical significance and magnitude of each model changed with the addition of said covariates. The statistical significance of three-strike rules, however, remained robust with these added covariates although the magnitude changed significantly. Therefore, this model concludes that three-strike rules increase violent crime rates.

**Table IV - Summary Statistics: Truth-in-Sentencing**  
*Sample Descriptive Statistics by Control and Treatment Groups*

Variable	Control (1)			Treatment (2)			Difference (3)	P-Value
	Mean	Median	N	Mean	Median	N		
Crime Rates	496.64	365.34	963	514.22	309.60	873	-17.58	0.6372
Annual GDP	140,926.3	71,085	961	311,907.6	196,638	853	-178,901.2	0.0000
Income	60,655.51	58,865	963	64,636.83	63,578	873	-3,981.32	0.0000
Population	4,491,508	2,952,876	963	6,763,507	4,763,390	873	-2,271,999	0.0000
Unemployment	5.76%	5.5%	963	5.49%	5.1%	873	0.28%	0.0022

**Table V - Summary Statistics: Truth-in-Sentencing**  
*Sample Descriptive Statistics by Control and Treatment Groups*

Variable	Control (1)			Treatment (2)			Difference (3)	P-Value
	Mean	Median	N	Mean	Median	N		
Crime Rates	470.92	307.65	1,089	554.69	370.38	747	-83.77	0.0269
Annual GDP	150,640.30	81,251	1,067	322,294.80	195,289.20	747	-171,654.50	0.0000
Income	61,167.90	60,618	1,089	64,561.40	63,519	747	-3,393.51	0.0000
Population	4,539,207	3,247,111	1,089	7,077,199	4,972,195	747	-2,537,992	0.0000
Unemployment	5.83%	5.50%	1,089	5.34%	5.00%	747	0.49%	0.0000

**Table V - Summary Statistics: Determinate Sentencing**  
*Sample Descriptive Statistics by Control and Treatment Groups*

Variable	Control (1)			Treatment (2)			Difference (3)	P-Value
	Mean	Median	N	Mean	Median	N		
Crime Rates	482.99	325.62	1,088	537.01	353.83	748	-54.02	0.1537
Annual GDP	139,136.1	76,536.50	1,086	343,936.2	237,598.10	728	-204,800.10	0.0000
Income	60,296.70	59,118.50	1,088	65,824	64,377.50	748	-5,527.35	0.0000
Population	4,015,299	2,856,381	1,088	7,835,853	5,700,437	748	-3,820,553	0.0000
Unemployment	5.51%	5.20%	1,088	5.80%	5.50%	748	-0.29%	0.0015

The fixed effects of state and years are highly correlated with the explanatory variables of the model which indicates that the variables included are impactful on crime rates and appropriate for this model. Although the R-squared value is not included in this type of model, the RHO is calculated which can be used as a substitute for the traditional R-squared. Approximately 20% of the variation in violent crime rates can be explained by the policy types

and covariates included in the model which indicates relevance but some possible omitted variable bias if these variables are also correlated with the sentencing legislation being examined.

Some possible variables contributing to this bias might be prison populations and/or other sentence lengthening legislation. Prison populations by state would most likely be negatively correlated with a decision to implement the sentence lengthening policies included in this model (being that sentence lengthening policies generally increase prison populations, they are not as likely to be implemented when prisons are over-crowded; instead, more parole and early-release programs are more likely to be instituted) and would be positively correlated with violent crime, leading to a negative bias. If one assumes that other types of sentence lengthening legislation are negatively correlated with violent crimes (as is their purpose) and are positively correlated with the sentencing legislation included by the model, then there is another example of a negative bias, which would lead this paper to conclude that the coefficients estimated by this model are underestimates.

## **Conclusion:**

The only consistent and highly relevant policy highlighted by the results of this paper is that of three-strike rules. This is likely due to the unintended changed incentives of both criminals and police officers after its implementation. For example, increases in the intensity of violent crime is a potential unintended consequence of these sentence lengthening policies and might further explain the expected increase of violent crime given the implementation of three-strike rules (Marvell, et al., 2001). A criminal that already has two strikes might choose to murder a victim or witnesses to reduce the likelihood of being caught which has the opposite effect of the purpose of these laws. Some states and local governments might respond to three-strike rules by decreasing police presence which might offset any potential deterrent effect of three-strike rules that may have existed (Ross, 2012). Police might also be more apt to charge offenders with more violent felonies as opposed to less serious charges due to potential biases or preconceived notions regarding that offender. For example, instead of charging an offender with assault, they might choose to charge them with aggravated assault.

Three-strike rules are the only type of sentence lengthening legislation that fundamentally changes the incentives of criminals as well as police officers. These incentive changes more than offset any potential deterrent effect of three-strike rules on violent crime.

Truth-in-sentencing and determinate sentencing have little to no statistical significance, depending on the confidence level used. This is because there are little to no fundamental changes to incentives of parties involved in the criminal justice process when truth-in-sentencing or determinate sentencing policies are implemented. Determinate sentencing combines aspects of truth-in-sentencing and three-strike rules in that it outlines a minimum sentence (similar to the minimum percentage of the sentence served as outlined by truth-in-sentencing policies) but is

based specifically on the crime committed (in a way that is similar to three-strike rules). This combination means that it may be statistically significant but only at the 90% confidence level being that the changes in incentives are not as obvious or meaningful as those for three-strike rules.

Such results would lead this paper to conclude that the relationship between violent crime and sentence lengthening legislation is inelastic and different policy measures are needed to accomplish the goal of reducing violent crime. This might be explained by the complexity of the legal system within the United States. Not only does the federal government have policies, standards, and procedures that are applicable to all 50 states and the District of Columbia, but each state has its own system and approach to law enforcement. This does not only include sentence related legislation, but the process of charging, convicting, and housing inmates (Meritt, et al., 2006).

According to this model, state governments should focus on alternatives of sentence lengthening legislation to decrease violent crime rates. If states are to implement sentence lengthening legislation, they should focus primarily on eliminating three-strike rules. Further research is needed to examine these policies in more depth, possibly on the state or local level as opposed to the whole country and to study specifically the potential changes in crime severity across strikes (specifically, how does the severity of violent crime differ for a criminal with one, two, or three strikes). Research regarding the effects of minimum sentence lengths, indeterminate sentencing, and the choice to eliminate some of these policies on violent crime might also highlight important implications for policy makers.

Other areas of potential research might include different ways in which governments can discourage violent crime. This might include efforts to better public education systems, to encourage core family units that include two-parent households, to improve the state's economy (as suggested by the GDP covariate included in Table 7), to decrease unemployment rates (as suggested by the unemployment rate covariate included in Table 7), or to increase accessibility to mental health resources. Research should be conducted to study the effectiveness of these possible alternatives before resources should be allocated to pursuing them so as to ensure the optimal allocation of said resources.

The pervasion of violent crime within society is an extremely complex issue that merits a more comprehensive and analytic approach than simply increasing its cost through sentence lengthening policies being that their relationship is inelastic. Policymakers should be careful to ensure that they completely understand the implications of these types of legislation as well as possible unintended consequences before they decide to implement them.

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