

# Intergenerational Effects of Mass Incarceration

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## Abstract

We exploit variation in whether and if states implemented “three strikes and you’re out” policies to estimate the intergenerational effect of mass incarceration. We find the only states that experienced a significant increase in incarceration rates were California and Georgia. Our difference-in-differences approach finds decreased income mobility in California and Georgia for children nine or younger compared to those ten or above. Younger children were negatively affected in states that used three strikes laws to incarcerate many people, while there was no effect in states without detectable increases in incarceration rates.

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# 1 Introduction

The United States has one of the highest incarceration rates in the world as a result of its criminal justice policies. Data from the Bureau of Justice Statistics indicate that incarceration rates rose steadily during the 1980s and 1990s; from 500,000 adults incarcerated in 1980 to 1.5 million in 1990 and later to more than 2.3 million in 2008 (Glaze and Parks (2011) and Kaeble, Glaze, Tsoutis, and Minton (2015)). At the same time the literature indicates that the number of children whose parents are incarcerated has increased. Approximately 600,000 children had a parent in state or federal prison in 1986, compared with over 1.3 million children in 1997 (Johnson and Waldfogel (2002)).

Against this background, several studies investigate the impact of mass incarceration on children and the results have generally suggested that parental incarceration is associated with negative child outcomes. For instance, parental incarceration has been associated with depressive symptoms, aggression, delinquency, criminal behavior, and social exclusion that persists into adulthood (Foster and Hagan (2015)). Furthermore, Haskins (2016) posit that children whose parents are incarcerated may be at risk for academic difficulties. She reports that paternal incarceration negatively affects children's cognitive capacities which is detrimental to academic achievement.

Despite what appears to be converging evidence that parental incarceration poses a significant threat to child development, this area of research has yet to overcome important methodological challenges related to selection bias. Incarceration is not random and many of the same factors that predict parental imprisonment also predict a child's educational success and subsequent lifetime income. It is well documented that the socially and economically disadvantaged children and families are most affected by mass incarceration in the United States (Ewert, Sykes and Pettit (2014) and Wakefield and Wildeman (2013)). Thus, children with imprisoned parents often suffer socio-structural disadvantages that may foster low intergenerational mobility. As a result, Noyes, Paul and Berger (2016) discuss how it is often unclear whether the difficulties that have been observed among children whose parents are incarcerated are due to the incarceration itself or to

other adversities they experience throughout their lives. The implications for policy rely heavily on whether and the extent to which there are intergenerational effects of incarceration. Thus, the contribution of this paper is to provide causal estimates of the intergenerational impacts of the “three strikes and you’re out” policies. This paper uses the variation in timing and implementation of three strikes policies as an exogenous policy shock to examine how children are affected by such policies. We find that three strikes laws significantly increased the level of incarceration in California and Georgia while having no substantial impact on the other 22 three strikes states. We then use intergenerational mobility data from Chetty, Hendren, Kline and Saez (2014) to show that California and Georgia experience decreases in income mobility for children under nine years old compared to those ten or over at the time of implementation. This is likely at least partly driven by an increase in the strength of the relationship between parental income and college attendance rates in Georgia for children five or younger when the policy was implemented. In California there is a levelling of the playing field for college attendance for younger kids compared to older kids. However, those children may be less likely to graduate or may be attending lower quality schools.

The most closely related literature on whether the associations of parental incarceration with child outcomes is causal was conducted using penal reforms in Denmark (see Wildeman and Andersen (2017)). The authors exploit a policy change in Denmark, in which some individuals qualified for a non-custodial sentence, to compare the child’s risk of being charged with a crime in a difference-in-differences framework. They find that the policy, which reduces the likelihood that fathers are incarcerated, significantly reduces the likelihood that male children are charged with a crime. However, there is a concern about selecting into the type of crime that allows one to receive a non-custodial sentence. Additionally, there are concerns about not controlling for time trends and macro-economics trends (year fixed effects). Our paper builds on the existing literature by exploiting the three strikes policy to see how children of different ages are affected in terms of intergenerational mobility measures.

Between 1993 and 1996, 25 states and the federal government passed the “three strikes and

you're out" legislation.<sup>1</sup> This policy mandates significant sentence enhancements for repeat offenders, such as life sentences without parole for at least 25 years on conviction of the third violent offense.<sup>2</sup> Though the degree of adaptation varies across states, by mid-1998, four years after adopting this penal reform, Georgia and California sentenced approximately 2,000 offenders and more than 40,000 offenders, respectively, under the three strike provision (Kovandzic, Sloan III, and Vieraitis (2004)). The implementation of this law may have made significant contributions to mass incarceration in the United States and will therefore be used in our investigation of the link between mass incarceration and intergenerational mobility.<sup>3</sup> While the laws might impact children directly since they could later be sentenced under them, we compare effects across age groups to show younger children are more affected. Both younger and older children could be sentenced under these policies, but we expect younger children to be more affected by incarceration of family members due to the literature on the importance of early childhood environments.<sup>4</sup>

The rest of this paper is organized as follows. In Section 2 we provide an overview of three strikes law, Section 3 provides a discussion of the three strikes literature with emphasis on the effect of this punitive reform on crime and incarceration. In Section 4, we present details on the data while Section 5 describes the empirical strategy. Section 6, presents the main results, as well as some robustness checks. Finally we conclude the paper in Section 7.

## 2 Institutional Details

This paper extends the research on the intergenerational effects of parental incarceration by using three strikes policies and a difference-in-difference framework. To accurately describe the impact of the three strikes law on state's incarceration rates it is important to discuss the defining

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<sup>1</sup>Although the federal three strikes law received much attention during passage of the 1994 crime-bill, application of the federal version of three strikes law resulted in very little convictions. According to Dickey and Hollenhorst (1999) passage of this federal law appears to have been a largely symbolic act.

<sup>2</sup>In some sates individuals could strike out after the second convicted offense.

<sup>3</sup>Zimring, Hawkins and Kamin (2001) describe three strikes in California as the most important effort to achieve an abrupt increase in criminal punishment in modern times.

<sup>4</sup>The Center for the Economics of Human Development has produced a plethora of work providing evidence on the importance of quality development programs from birth to age five. For some recent examples, see Garca, Heckman, Leaf and Prados (2016) and Garca, Heckman, Leaf and Prados (2017).

features of this law. With the exception of Kansas, all three strikes states had pre-existing habitual or repeat offender statutes. However, it may be argued that the implementation of the three strikes legislation was added to existing laws for enhanced sentencing because the existing laws were not achieving desired outcomes. Notably, in all three strikes states, the new legislation represented a reform to the penal system either through increases in the length of incarceration for violent crimes, an expansion of the crimes that triggered enhanced sentencing or both.

For the most part, this initiative mandated 25-years-to-life sentences without the possibility of parole for repeat offenders. Clark, Austin and Henry (1997) posit that although these statutes share the same title, “three strike and you’re out”, their meanings varied across states. Three main differences have been highlighted. First, what constituted a strike differed across states. For most states, violent crimes; like murder, rape, robbery and assault were included as strikes in the legislation. However, the sale of drugs constituted as a strike in Indiana, Louisiana and California; while escape qualified as a strike in Florida. Second, there were variations in terms of the number of strikes required to be out. In most states, three strikes were required but in states like Arkansas, California, Connecticut, Georgia, Kansas, Montana, Pennsylvania and Tennessee, enhanced sentencing was inflicted after two strikes.<sup>5</sup> The third area of difference is with regards to what it means to “strike out” - what sanctions are imposed when sufficient strikes have been accumulated. Generally, a felon was given mandatory life sentences without the possibility of parole but in some states like New Mexico and California after serving a minimum number of years (30 and 25, respectively) offenders became eligible for parole. Table 1 provides more details on the nature of this sentence enhancing policy in each state as well as the year in which the law was adopted.

Following implementation, we expect that the most dramatic changes to the criminal justice system to occur in California and Georgia. As a result, we explicitly control for these states in our econometric models. The scope of three strikes law in California is dramatically different from both pre-existing laws and that which was adopted by the other 24 three strikes states. Specifically, after three strikes was adopted in 1994, the law no longer required the offender to

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<sup>5</sup>Georgia, Maryland and Louisiana also made provisions for a fourth strike.

have served prison time for a listed felony to count as a first or second strike. Additionally, the third strike, which triggered a term of 25-years-to-life, did not have to be considered a violent one. As a result, the three strikes law in California promoted enhanced prison sentences for non-violent offenses, like residential burglary.<sup>6</sup> Consistent with this finding, two-thirds of California’s strikers are imprisoned for non-violent offenses (Zimring et al. (2001)). While no state comes close to California in terms of the number of people convicted under three strikes provisions, five years following implementation Dickey and Hollenhorst (1999) assert that Georgia experienced substantial increases in the use of one strike and two strike policies. Under Georgia’s pre-existing sentencing laws, life without parole was only given after the fourth conviction for a violent crime. After the implementation of the three strikes law in 1994, individuals were subject to lengthy sentences after the first or second strike. The first strike received a minimum of ten years without parole and (for some violent crimes) the second strike earned life without parole. Our hypothesis of a detectable effect in California and Georgia is also motivated by results from the assessment of Kovandzic et al. (2004). Their examination highlights California and Georgia as states with a large number of persons in prison under three strikes as opposed to the other states that implemented such laws

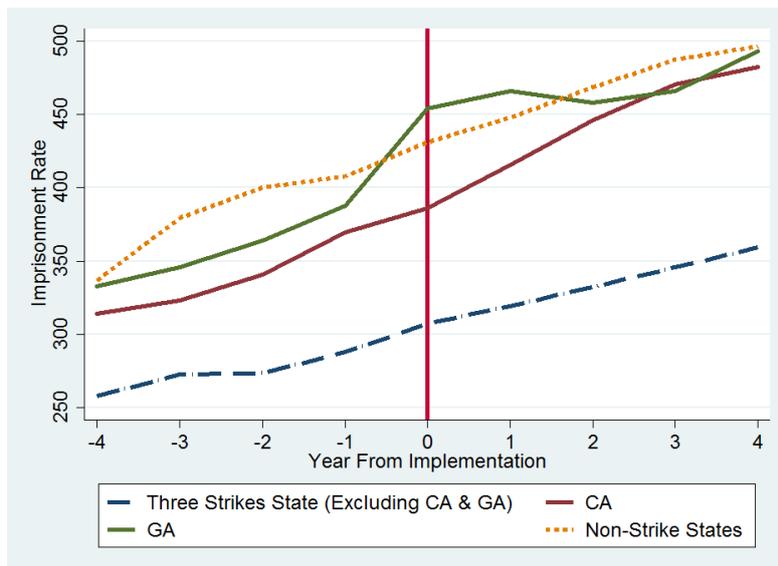


Figure 1: Imprisonment Rates pre and post Three Strikes Implementation

<sup>6</sup>A conviction for residential burglary is a sufficient condition for three strikes and many of those who become eligible for the extended sentences are burglars (Zimring, Hawkins and Kamin (2001)).

Figure 1 highlights the change in imprisonment rates following the adoption of the three strikes law - with implementation date set equal to time 0. As outlined in the figure, incarceration rates across states increased during the 1990's. Furthermore, Schiraldi, Colburn, and Lotke (2004) report that incarceration rates increased more during the 1990s than in any previous decade. Given this upward trend, our models incorporate time fixed effects and state (or commuting zone or county level) specific linear time trends.

### 3 Literature Review

Several studies investigate the effects of three strikes law. No consensus has been identified across these studies and conflicting effects have persisted within studies. For example, Kovandzic et al. (2004) report that while states like Nevada and Pennsylvania experienced significant increases in crime following the adoption of the three strikes law, states like California experienced a decline in crime. Against this background, they conclude that they were unable to identify credible statistical evidence that the passage of three strikes laws reduce crime by deterring potential criminals or incapacitating repeat offenders. Similarly, Schiraldi et al. (2004) argue that though the three strikes movement largely targeted violent perverse criminals, with promises of great impact, comparative analysis of crime across the United States revealed disappointing results ten years after most strikes laws were enacted. With regard to crime, they report that three strikes states fared no better than states that did not adopt three strikes laws. In the current paper, we are less concerned about the effect of three strikes on crime directly but more focused on the level of imprisonment after enacting this law. Specifically, we hope to shed light on how this policy may have increased the number of persons imprisoned in the United States and hence contributed to the state of mass incarceration.

While not a direct test of three strikes policy, Hunt and Peterson (2014) examine the effect of retroactive sentence reductions on the impact of recidivism for those sentenced under crack cocaine guidelines. They compare people released right before the new policy was put into place with those who qualified under the sentence reduction to be released right after implementation. The

paper concludes that those released under a reduced sentence were not more likely to recidivate than those who had longer sentences. They argue that severe sentence lengths do not have any marginal benefit in terms of reducing recidivism.

Although previous research on children with an incarcerated parent has been methodologically weak in assessing causality, these studies consistently document significantly more behavioral problems among children, including but not limited to, aggressive behavior, depression, hyperactivity, withdrawal, sleep and eating disorders and poor school grades. Furthermore, Johnson and Easterling (2012) find that parental separation resulting from incarceration may pose unique risks in its effects on children and the family, relative to parental separation due to divorce. Specifically, a prison sentence may be described as a death sentence of a father's relationship with his child. Since the three strikes law lengthened the sentences for repeat offenders, a significant and positive link between this policy and incarceration implies that there was likely increased separation.

## 4 Data

In this section, we describe the data sources and key variable definitions. Variation in the treatment variable is associated with the year each state first implemented the three strikes law. Given that the date each state adopted this punitive reform is not available from a unified source, we compile information on states that enacted the three strikes law between 1993 and 1996 from Clark et al. (1997) and Dickey and Hollenhorst (1999).<sup>7</sup>

Table 1 outlines the date each state accepted the three strikes law as well as comments on the details of each legislation. Specifically, Washington became the first three strikes state in 1993, this was followed by California and ten other states in 1994. In 1995 twelve states followed suit and Alaska joined the group in 1996. Our main analyses are based on data from 1989 to 2000: four years before 1993 and four years after 1996. The data does not include 1988 because of

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<sup>7</sup>A total of 25 states adopted the three strikes law between 1993 and 1996. A decade later, in 2006, the 26<sup>th</sup> state (Arizona) began using the sentence enhancement legislation. Later in 2012, Massachusetts implemented the law as well. Given that the last two states to accept this law did so ten or more years after 1996, we restrict the discussion to the first 25 three strikes states.

other crime related policies that went into effect that year. For example, California passed The California Street Terrorism Enforcement and Prevent Act of 1988 <sup>8</sup> which included a sentence enhancement for a felony committed for the benefit of a criminal street gang. Thus, by starting in 1989 we include years that have more similar crime policies leading up to three strikes.

Information on state real per capita income was taken from Bureau of Economic Analysis and unemployment data are from the Bureau of Labor Statistics. Table 2 presents characteristics of three strikes states compared with non-strike states. This table illustrates average real per capita GDP growth among three strikes states was higher than that of non-strike states between 1989 and 2000. Similarly, average unemployment rates across three strikes states was relatively in line with the average rate of unemployment in non-strikes states. This seems to suggest that three strikes states were not predominantly poorer with high levels of unemployed and hence idle workers.

In the initial stage of our analyses, the outcome variable represents the measure of incarceration for state  $s$  in year  $t$ . Measures of crime was taken from the FBI's Uniform Crime Reports. Specifically, we used data on the rates of violent and property crime across state over time. The imprisonment rate, the number of prisoners under state or federal jurisdiction sentenced to more than 1 year per 100,000 United States residents, provides information about incarceration rates. It is published by the Bureau of Justice Statistics. Since 1978, approximately 70 percent of the incarcerated population in U.S. is housed in state and federal prisons. Since the prison population makes up the largest proportion of the incarcerated population and given that these inmates are most likely to be impacted by enhanced sentencing laws (like three strikes), our analyses place focus on this group. Details presented in Table 2 imply that three strikes states had relatively higher rates of violent and property crime on average when compare to non-strike states. The average imprisonment rate was higher for three strikes states than that for non-strike states, with the highest average recorded for California and Georgia.

The measures of intergenerational mobility are downloaded from the Equality of Opportunity Project website and come from Chetty et al. (2014). We employ the Intergenerational Mobility

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<sup>8</sup>Cal. Pen. Code 186.20, et. seq. also known as the STEP Act

Estimates by Commuting Zone and Birth Cohort data set and the Intergenerational Mobility Estimates by County and Birth Cohort data set. The authors measure intergenerational mobility through parent-child tax linkages. The area the child grew up in is based on the address listed on the tax return when the parent listed the child as a dependent. The number of years the data can be matched is restricted by when a social security number had to be listed for dependents. Those social security numbers are used to follow the children into adulthood. The tax returns filed by children provide information on college attendance and income. College attendance is based on information provided in the 1098-T form which is a tuition statement reported by the educational institution. For more details, see Chetty et al. (2014).

In the commuting zone data there is an observation for each birth cohort by commuting zone from 1980 to 1993. The intergenerational mobility measures are how parental national income rank affects the slope and intercept for the child's national income rank at age 26 as well as how parental national income rank affects the slope and intercept for the child's college attendance at age 19. If social mobility is decreasing we would expect to see a decrease in the intercept indicating those kids at the bottom of the parental national income distribution have lower incomes themselves or are less likely to attend college as a result of the three strikes. Additionally, we might expect that three strikes strengthened the relationship between parental income and child income and the relationship between parental income and the child's probability of attending college which would show up through a steep slope. The data set also includes a variable specifying the number of children in the commuting zone by birth cohort.

Similar to the commuting zone level data, in the county level data an observation is a county level by birth cohort measure.<sup>9</sup> However, there are fewer birth cohorts as only those born from 1980 to 1988 are in the data set.<sup>10</sup> Instead of the outcome measures being slopes and intercepts, the outcome variables are broken down by where the child started in the national income distribution (by using the parental measure of income). Specifically, the data includes college attendance and

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<sup>9</sup>A county is a smaller area than a commuting zone and multiple counties can be contained within a commuting area. 18 observations are dropped due to counties not existing throughout 1990 to 2000.

<sup>10</sup>The college attendance data is available for cohorts born 1980 to 1988 and the income at age 26 is available for cohorts born 1980 to 1986

rank in the national income distribution of those who started at the 25th and 75th percentile. This will allow us to test whether those most likely affected by the policies (those who started at the 25th percentile) are more negatively affected than those who were unlikely affected by these policies (those who started at the 75th percentile). We expect three strikes will have a negative impact on income and college attendance for children who started out at the 25th percentile of the national income distribution. This data set provides county level population measures in 1990 and 2000 instead of the number of children. To create the weight for the analysis, Census Intercensal County Population Data from 1980 to 1988<sup>11</sup> is merged with the County and Birth Cohort data set. Each county's population under the age of five is matched with the county level intergenerational mobility measures by birth cohort. Due to changes in counties, 27 observations are dropped because they could not be matched to the Census data.

The summary statistics for the intergenerational mobility measures can be found in the bottom half of Table 2. In California the slope between the parental income rank in the national distribution and the child's college attendance at age 19 is much flatter than the other states. There are large differences in college attendance intercepts across states such that Georgia has a much lower intercept than California. In addition to the flatter slope for college attendance, California also has a flatter slope for parental income rank and child income rank at age 26 than the other states. Consistent with the income gaps in college attendance discussed in Bailey and Dynarski (2011), there is a large gap in attendance between children born at the 25th percentile of the national income distribution and children born at the 75th percentile of the national income distribution. These gaps also exist in the rank in the national income distribution at age 26.

## 5 Research Design

Before conducting an analysis of the impact of three strikes on children's college attendance and social mobility, we test whether three strikes laws had an impact on incarceration rates. Our difference-in-differences strategy considers the year of implementation of the three strikes law in

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<sup>11</sup>Available at <https://www.census.gov/data/tables/time-series/demo/popest/1980s-county.html>

each state to trigger the treatment. For our identification strategy to yield causal estimates of higher incarceration rates, it is important to establish that the timing of three strikes adoption appears to be exogenous. Since states adopted three strikes law at different times, we compare the number of people in prison per 100,000 residents - the imprisonment rate- across states with and without the legislation. We expect the number of sentenced prison inmates to increase following the adoption of enhanced sentences under the three strikes law. The following represents our baseline model:

$$Y_{st} = \alpha + \delta \text{strikes}_{st} + \lambda_t + \eta_s + X_{st}\beta + \theta_s t + \epsilon_{st} \quad (1)$$

In this model,  $Y_{st}$  represents the log of imprisonment rate in state  $s$  at time  $t$ . The independent variable of interest,  $\text{strikes}_{st}$ , equal 1 if the “three strikes and you’re out” legislation is implemented in state  $s$  at time  $t$  and zero otherwise.<sup>12</sup> Crime and hence incarceration may be impacted by the macroeconomy. According to Schiraldi et al. (2004) reduction in crime in California and New York (during the 1990s) strike and non-strike states, respectively, may be reflective of improvements in both states’ job markets rather than their criminal justice policies. It is therefore important that these regressions control for observable state-specific factors that might have changed over time. Consequently, we include the state unemployment rate and real GDP per capita (represented in the vector  $X_{st}$ ) to control for differences in local market conditions that may affect an individual’s decision to commit crime.

Estimating a single aggregate effect of three strikes on imprisonment and crime ( $\hat{\delta}$ ) may be particularly misleading given the differences in the scope of the three strikes legislation across states (see Table 1 for details) as well as publicity surrounding passage of laws. California, and Georgia are identified as states where three strikes laws are severe and frequently enforced (see Kovandzic et al. (2004)). Additionally, California and Georgia are described as the only two states that have applied this legislation with consistency. This was evident in the large number

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<sup>12</sup>Specifically, the strike dummy associated with Washington state, takes a value of one from 1993 onwards and zero otherwise since three strikes was implemented in 1993 in that state. For Nevada, the strike dummy takes a value of one from 1995 onwards and zero before the policy was adopted.

of offenders sentenced under two and three strikes provisions only four years after enactment.<sup>13</sup> Thus, we estimate separate treatment effects for California and Georgia. For the rest of the states with such laws, we aggregate them into one treatment effect called “other”. To control for fixed state or year characteristics, we include state and year fixed effects ( $\eta_s$  and  $\lambda_t$ , respectively).<sup>14</sup>  $\theta_{st}$  presents state specific linear time trends. For all models, we cluster the standard errors by state because implementation occurred at the state level. Thus, the preferred model we estimate is:

$$Y_{st} = \alpha + \delta_1 \text{strikes}_{CA,t} + \delta_2 \text{strikes}_{GA,t} + \delta_3 \text{strikes}_{OTHER,t} + \lambda_t + \eta_s + X_{st}\beta + \theta_{st} + \epsilon_{st} \quad (2)$$

For the regressions on intergenerational mobility our treatment variable is defined based on the age the child was when the law went into effect. Given the large literature on the importance of early childhood, we expect the effect of three strikes policies to be larger in magnitude for younger children than for older children since they are more likely to be affected by family or community changes due to incarceration. Similar to Haskins (2016) we break children into age groups such that we look at outcomes for young children (five years old or younger), children in early elementary (six years old to nine years old), and older children (above nine years old when the policy was implemented). Due to only having income and college attendance data on some of the birth cohorts, we compare two age groups.<sup>15</sup> Specifically, in the commuting zone data we have income data such that we can compare children in early elementary school to older children and we have college attendance data such that we can compare young children with early elementary children. In the county level data we are comparing elementary children to older children. Thus, we are testing whether children who were younger when three strike laws went into effect had worse intergenerational mobility than children who were older when these laws went into effect.

For intergenerational mobility outcomes, we estimated the following model:

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<sup>13</sup>More than 40,000 and almost 2,000 offenders were sentenced in California and Georgia, respectively (Dickey and Hollenhorst (1999)).

<sup>14</sup>A similar justification was presented by Kovandzic et al. (2004) in their analysis of three strikes on crime.

<sup>15</sup>Income at age 26 is available for the 1980 to 1986 birth cohorts and college attendance at age 19 is available for the 1984 to 1993 birth cohorts.

$$Y_{cb} = \alpha + \delta_1 \text{strikes}_{CA,b} + \delta_2 \text{strikes}_{GA,b} + \delta_3 \text{strikes}_{OTHER,b} + \lambda_b + \eta_c + \theta_c b + \epsilon_{cb} \quad (3)$$

where  $c$  is the commuting zone or county and  $b$  is birth cohort.

Since we have data at the commuting zone, instead of state fixed effects and state specific linear time trends, we use commuting zone fixed effects and commuting zone specific linear time trends. Similarly, in the county level data we use county level fixed effects and county specific linear time trends. The standard errors are still clustered at the state level since that is the level at which the policy was implemented. Weights are also used in these regressions to control for how many people went into estimating each outcome variable. To avoid smaller commuting zones driving the result, each observation is weighted by the percent of the total number of children it represents in the sample (the weights sum to 1). In the county level data we use the Census data to construct weights based on the percent of the total number of children under five each county represents.

## 6 Results

To investigate the impact of three strikes on children’s educational outcomes and social mobility, we use quasi-experimental methods by exploiting the variation across states and in the timing of three strikes policy implementation.

### 6.1 Incarceration Outcomes

*Does the implementation of “Three Strikes and You’re Out” lead to changes in imprisonment rates?*

We begin our analysis by estimating difference-in-differences models of incarceration rates. Table 3 details the link between three strikes implementation and the log of imprisonment rates. The results from this assessment provides evidence that three strikes legislation contributed to

higher imprisonment rates. The effect, though in line with expectation, was not identified across all three strikes states.

To ensure that the significant result identified was not driven by states that rarely implemented the law, we separated the  $Strikes_{st}$  variable in all subsequent model estimation and explicitly controlled for the policy shock in California versus Georgia versus all other states.<sup>16</sup> Column 2 of Table 3 reveals that the policy shock experienced by California and Georgia contributed to significant increases in log imprisonment rates. For Georgia, this significant and positive effect remained even when we included control variables (see Columns 3 and 4 of Table 3). Following the inclusion of log per capita real GDP, the effect of the treatment variable for California became insignificant. Investigation for a potential reason revealed that the treatment variable for California significantly affects both per capita GDP (negative impact) and the state unemployment rate (positive impact). This calls into question the validity of these covariates and thus our preferred specifications do not include them.

### 6.1.1 Event Study

The results largely indicate a positive and statistically significant link between the treatment variable and incarceration in California and Georgia. To better test for the parallel trends assumption, we employ an event study. We include an indicator for whether or not a state implemented a three strikes law starting in period  $t - k$ . Specifically, we estimate the following equation.

$$Y_{st} = \alpha + \sum_{k=-4}^{k+4} \gamma_{t-k}(S_{t-k}) + \eta_s + \lambda_t + \epsilon_{st} \quad (4)$$

The  $S_{t-k}$  is a series of dummy variables that capture the number of years before and after the three strikes law was implemented. For example,  $S_{k=0}$  is set equal to one in the year a state first implements the three strikes law.  $\gamma_{t-k}$  is an estimate ( $k$  years after the three strikes law was

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<sup>16</sup>Since California's three strikes and you're out law was drafted to qualify any felony as the triggering third strike and since this state reported the largest number of felons under the three strike law, we wanted to ensure that it was not driving our initial results.

enacted) of the treatment effect relative to one year prior to implementation. As in our main specification, the event study model includes state ( $\eta_s$ ) and year ( $\lambda_t$ ) fixed effects.

If the parallel trends assumption holds, the event study treatment variables prior to the date of implementation should be insignificant. Treatment variables at or after implementation are expected to be positive and statistically significant. Our event study results are displayed in Figure 2 and Table 4. Given our difference-in-differences results in California and Georgia, we run event study regressions for those states specifically.

California and Georgia exhibited no significant pre-trend at the five percent level in the standard event study framework as shown in Column 1 and Column 3 of Table 4. In California there is a delayed increase, but then an increase at the ten percent level one year after, and significant impacts at the five percent level two and three years after. This is consistent with Clark et al. (1997) which stated that in California strike eligible cases were pending longer than before as defendants did not want to accept plea deals that would count as a strike and thus increase their time incarcerated. In Georgia, on the other hand, there was an immediate significant effect which carried into one year after the law went into effect.

Borusyak and Jaravel (2016) discuss identification problems in using unit fixed effects, time fixed effects and linear time trends. They suggest restricting pre-trends such that you start with a fully dynamic framework and drop any two terms corresponding to the pre-trend. The next step is to run a F-test on the remaining pre-trends. However, they note that this test only has power against non-linear pre-trends, although it is unlikely the pre-trends would be exactly linear. The recommendation is to drop two time periods far away from each other<sup>17</sup> so we drop  $k = -1$  and  $k = -3$ . However, the results are robust to dropping  $k = -1$  and  $k = -4$ , which is consistent with the claim in the paper that even in finite samples it should not matter. This suggestion is implemented in Column 2 and Column 4 of Table 4. In both cases we cannot reject the hypothesis that the coefficients on the pre-periods are statistically the same. This provides further evidence that our results are not driven by significant pre-trends in incarceration rates in California and Georgia.

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<sup>17</sup>Specifically, do not drop  $k = -1$  and  $k = -2$ .

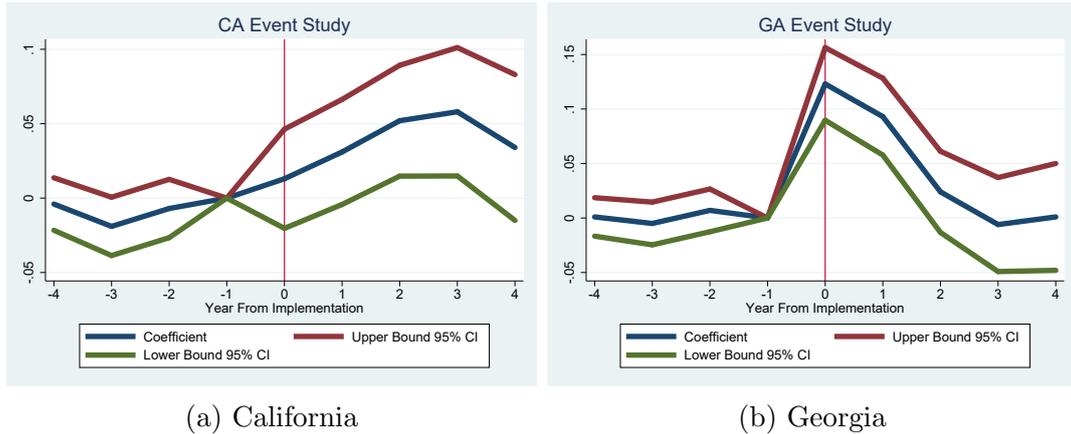


Figure 2: Event study estimation of the effects of three strikes. One year prior to implementation ( $k=-1$ ) is normalized to 0.

Throughout our investigation, Georgia displays evidence of increased levels of incarceration following three strike law implementation. This may suggest that after adopting this enhanced sentencing policy in 1994 a larger number of individuals (potentially, parents as well) were separated from their family due to incarceration. Chetty et al. (2104) posit that the level of upward mobility in an urban area is correlated with residential segregation, income inequality, quality of school, strengths of social networks and family structure. In terms of intergenerational mobility, Atlanta, Georgia was ranked 49 out of 50 suggesting that children in this commuting zone had one of the lowest chances for upward mobility.<sup>18</sup> Our results show that Georgia has experienced an increased incarceration, which may have promoted weaker or single parent family structure, an impediment to upward mobility.

<sup>18</sup>Chetty et al. (2014) discussion included the 50 largest commuting zones.

## 6.2 Intergenerational Mobility Outcomes

*Does the implementation of “Three Strikes and You’re Out” lead to changes in intergenerational mobility?*

The first intergenerational mobility outcome of interest is college attendance at age 19. Being a college graduate improves labor market outcomes, so this measure can give us an idea of how these children will fare economically as adults. We estimate separate point estimates for California and Georgia since our previous findings confirm larger incarceration results consistent with the literature. The other states are aggregated into one treatment effect.

Table 5 compares college attendance at age 19 by commuting zone across younger and older children. The younger children are under six years old while the older kids are at least six years old. The slope and intercept refer to the linear relationship between parental rank in the national income distribution and the child’s probability of attending college at age 19. In the preferred specification with the commuting zone specific linear time trends, the slope is significantly steeper in Georgia while there is no effect on the intercept. Thus, younger kids in Georgia saw a stronger correlation between their parent’s income and their college attendance at age 19. However, in California the slope is significantly negative and the intercept is significantly positive which indicates there is a weaker correlation between parent’s income and their child’s college attendance for younger kids in California. Unfortunately, there is no information on the sector of attendance (for-profit or not, two or four year school) or quality of the school. There is no effect in the other states that passed three strike rules but saw no detectable change in incarceration rates. In the other states, the treatment effects are precisely estimated zeros.

The impact of three strikes laws on commuting zone measures of income mobility is in Table 6. The slope and intercept refer to the linear relationship between parental rank in the national income distribution and the child’s rank in the national income distribution at age 26. In the preferred specification that includes commuting zone specific time trends, the income slope is significantly positive and the income intercept is significantly negative for California and Georgia. This means that the relationship between a parent’s income rank and the child’s income rank is

stronger for younger kids (nine or younger) in these states. This is consistent with the hypothesis that younger children are more negatively affected by mass incarceration policies than older children. Despite the levelling of the playing field in California with respect to college attendance at age 19, there is still an increase in importance of parental income for a child's income at age 26 in California. This might be due to children who grew up in lower income families attending lower quality colleges or not completing degrees. Again, there are no significant effects in the other states that implemented three strikes policies. These results are consistent with the other states only implementing the law for the most severe criminals, while California and Georgia incarcerated so many people that it was detrimental. The severity of the laws seem to impact intergenerational mobility.

### **6.2.1 Differences in Mobility by Starting Point in the Distribution**

It would be a concern if the results we find are driven by changes in those who grew up in the top of the income distribution and not those at the lower end of the income distribution. Given that those who were affected by the laws tended to be low income, as a robustness check we compare treatment effects for those growing up in the 25th percentile and those growing up in the 75th percentile. Table 7 shows the impact on college attendance at age 19. Both the 25th and 75th percentile experience a significant decrease in the probability of attending college in California and Georgia. The other states with these laws did not experience any significant changes at the five percent level.

Table 8 shows the impact on rank in the national income distribution at age 26. In both California and Georgia the effects on those at the 25th percentile are negative but insignificant while the point estimates for the 75th percentile are positive and insignificant. This is suggestive evidence that the negative effect is being driven by lower income families which were harder hit by three strikes laws. Again, there is no significant effect for the other states regardless of whether (county specific) linear time trends are included.

## 7 Conclusion

This paper highlights the effects of “three strikes and you’re out” legislation on incarceration and intergenerational mobility. It adds to the literature by providing causal estimates of the effect on intergenerational mobility by using the gradual roll-out of this law at the state level to identify the effect of three strikes separately from other policy changes occurring during the 1990s. Our difference-in-differences models reveal that following the adoption of this penal reform the level of incarceration increased in California and Georgia. This result was statistically significant and robust to the inclusion of state specific linear time trends as well as state and time fixed effects. We find a worsening intergenerational income mobility in California and Georgia and no significant impact in the other states that implemented three strikes.

We posit that the differences across states may be partly due to variation in the scope of the legislation; not only in degree but also in the kind of criminal that were eligible to be convicted under this law in California, in particular, relative to other three strikes states. For example, though the ratio of the state populations of California and Washington was six to one over the sample period; the ratio of sentences under three strikes was 334 to 1.<sup>19</sup> This is reflective of not only a larger criminal justice system in states like California relative to other three strikes states, but may also be explained by differences in legal practice.

Our results are important for the current debate over policies credited with causing mass incarceration. When Eric Holder was Attorney General the policy was to reduce sentence lengths for non-violent offenders. However, Attorney General Sessions has recently stated he wants to go back to stricter sentencing policies. Opponents of such policies argue that these policies have disproportionately hurt poor minority communities, and are not cost effective. Proponents argue it is a matter of public safety. Our results suggest that intergenerational mobility is causally related to the severity of sentencing policies and thus needs to be included in any cost-benefit analysis. Even though some states have changed their laws, the effect will continue to be felt in California and Georgia since more than one generation was negatively impacted.

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<sup>19</sup>Though the population ratio was calculated using the entire sample period (1989-2000) the sentencing ratio was restricted to 1998 based on data reported by Dickey and Hollenhorst (1999).

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Table 1: Comparison of State Strikes Laws

State	Year Law Enacted	Features of the Strikes Legislation
Alaska	1996	
Arkansas	1995	No parole sentences for third conviction for specified violent felonies
California	1994	Mandatory doubling of sentence for any felony if one prior serious or violent felony conviction
		Mandatory life for any third felony if two prior serious or violent felony convictions
Colorado	1994	Mandatory life in prison with no parole eligible for 40 years for third conviction for Class 1 or 2 felony or Class 3 felony that is violent
Connecticut	1994	Up to life in prison for third conviction for many violent offenses
Florida	1995	For third conviction for specified violent offense, life if first-degree felony, 30-40 years if second-degree felony, 10-15 years if third-degree felony
Georgia	1994	Mandatory life without parole for second specified violent felony conviction
Indiana	1994	Mandatory life without parole for third specified violent felony conviction
Kansas	1994	Allows court to double sentencing guidelines for second and third convictions for many "person felonies"
Louisiana	1994	Mandatory life without parole for third specified felony conviction, including at least one conviction for specified felonies
Maryland	1994	Life without parole for fourth violent felony conviction for which separate prison terms were served for the first three
Montana	1995	Mandatory life without parole for second conviction for certain offenses and third conviction for other offenses
Nevada	1995	Range of options for enhancing sentence on third conviction for violent felony
New Jersey	1995	Mandatory life without parole for third conviction for certain violent felonies
New Mexico	1994	Mandatory life with parole eligible after 30 years for third violent felony conviction
North Carolina	1994	Mandatory life without parole for third conviction for violent offense
North Dakota	1995	Enhanced sentences for second conviction for class A,B, or C felony
Pennsylvania	1995	mandatory minimum enhanced sentence of 10 years for second conviction for crime of violence and 25 years for third such conviction
South Carolina	1995	Mandatory life without parole for second conviction for specific felonies
Tennessee	1995	Mandatory life without parole for second conviction for designated violent felonies; same for third conviction for other violent felonies
Utah	1995	Second- and -third-degree felony offenders sentenced as first degree felons, and first-degree felons not eligible for probation, if they have two prior conviction for any felonies and a present conviction for a violent felony
Vermont	1995	Up to life with no suspended sentence or no probation eligibility and no early release for third conviction for crime of violence; up to life for fourth felony conviction of any kind
Virginia	1994	Mandatory life without parole on third conviction for specified violent felonies or drugs distribution charges
Washington	1993	Mandatory life without parole on third conviction for specified violent felonies
Wisconsin	1994	Mandatory life without parole on third conviction for specified serious felonies
Federal Law	1994	

Sources: Clark, Austin, Henry (1997) and Dickey and Hollenhorst (1999)

Table 2: Summary Statistics

	Three Strikes States (Excluding CA & GA)	Non-Strike States	CA	GA
Crime Related Measures				
Imprisonment rate	307.9	297.7	399.0	427.7
Violent crime rate	539.3	461.9	908.6	655.7
Property crime rate	4463.2	4077.8	4694.9	5378.2
Real GDP per capita	\$31,720.38	\$30,841.59	\$34,503.17	\$32,326.92
Unemployment rate	5.3%	5.1%	7.0%	5.0%
Intergenerational Mobility				
College at age 19 Slope	.72	.73	.59	.73
College at age 19 Intercept	.14	.16	.20	.10
Number of Kids in CZ for Coll.	5,490	5,349	25,651	3,823
Income at age 26 Slope	.27	.27	.20	.33
Income at age 26 Intercept	.38	.38	.40	.31
Number of Kids in CZ for Inc.	4,664	5,557	20,739	3,458
Attend college at 19: 25th pctl	32.5	33.2	39.1	26.3
Attend college at 19: 75th pctl	68.6	68.5	68.7	63.6
Income Rank at 26: 25th pctl	45.7	46.1	45.9	40.4
Income Rank at 19: 75th pctl	59.2	58.5	54.7	55.5
Kids Under 5 in Cty	4,760	5,239	35,049	2,824

The crime values above represent averages over the sample period, 1989 to 2000. The intergenerational mobility measures reflect averages over all of the available birth cohorts.

Table 3: Impact of three strikes law implementation on state imprisonment rates

	Column1	Column 2	Column 3	Column 4
$strikes_{st}$	.070***(.024)			
$strikes_{CA_t}$		.049*** (.017)	.028 (.019)	.019 (.023)
$strikes_{GA_t}$		.092*** (.017)	.090*** (.018)	.092*** (.017)
$strikes_{OTHER_{st}}$	.008 (.024)	.008 (.025)	0.003 (.025)	.005 (.026)
state fixed effects	X	X	X	X
year fixed effects	X	X	X	X
state specific linear time trend	X	X	X	X
Controls:				
Unemployment rate			X	
Log(real per capita GDP)				X
Observations	600	600	600	600

The outcome variable is the log of imprisonment rate. Each Column reflects a separate regression model of the outcome variable on the treatment. The other states include any state other than California or Georgia that implemented a three strikes law. In addition to state specific linear time trends, state and year fixed effects, control variables; the unemployment rate and log of real per capita GDP are included in the model reported in Column 3 and Column 4, respectively. All standard errors are clustered by state. Standard errors are in parentheses and \*\*\*, \*\* and \* indicate that the estimates are statistically significant at the 1%, 5% and 10% levels.

Table 4: Comparison of State Three Strikes Laws

$Treatment$	$CA$		$GA$	
	Column 1	Column 2	Column 3	Column 4
$S_{t-4}$	-.004 (.009)	.003 (.008)	.001 (.009)	.003 (.008)
$S_{t-3}$	-.019*(.010)	second omitted	-.005 (.010)	second omitted
$S_{t-2}$	-.007 (.010)	-.001 (.008)	.007 (.010)	.008 (.008)
$S_t$	.013 (.017)	.019 (.017)	.123*** (.017)	.125*** (.017)
$S_{t+1}$	.031* (.018)	.038** (.018)	.093*** (.018)	.095*** (.018)
$S_{t+2}$	.052*** (.019)	.058*** (.020)	.024 (.019)	.026 (.020)
$S_{t+3}$	.058** (.022)	.064*** (.023)	-.006 (.022)	-.004 (.023)
$S_{t+4}$	.034 (.025)	.040 (.026)	.001 (.025)	.003 (.026)
Borusyak and Jaravel F-Test		.7927		.6806
Number of Obs	500	500	500	500

The outcome variable is the log of imprisonment rate. All models include state and year fixed effects and standard errors are clustered by state. Standard errors are in parentheses and \*\*\*, \*\* and \* indicate that the estimates are statistically significant at the 1%, 5% and 10% levels. The Borusyak and Jaravel limited pre-trends fix means dropping two pre-treatment time periods and running a F-test on the remaining coefficients in the pre-trend time period. The results of the F-test do not depend on whether  $t = -3$  or  $t = -4$  is the second dropped period and they specifically say not to drop  $t = -1$  and  $t = -2$ .

Table 5: Impact of Three Strikes Laws on Commuting Zone College Attendance Measures

<i>Variable</i>	<i>College Measure</i>			
	College Slope	College Intercept	College Slope	College Intercept
<i>strikes_CA<sub>t</sub></i>	-.015*** (.005)	.000 (.003)	-.015*** (.005)	.029*** (.003)
<i>strikes_GA<sub>t</sub></i>	.022*** (.005)	.013*** (.003)	.013** (.005)	.001 (.003)
<i>strikes_Other<sub>t</sub></i>	.006 (.009)	-.006 (.008)	.007 (.005)	-.003 (.003)
CZ fixed effects	X	X	X	X
year fixed effects	X	X	X	X
CZ specific linear time trend			X	X
Weights for number of children	X	X	X	X
Observations	6,339	6,339	6,339	6,339

College attendance is measured at age 19. Those under the age of six when the policy were implemented are the treated group, while those six and older are the control group within the state. CZ stands for commuting zone. All standard errors are clustered by state and are in parentheses. The other states include any state other than California or Georgia that implemented a three strikes law. \*\*\*, \*\* and \* indicate that the estimates are statistically significant at the 1%, 5% and 10% levels.

Table 6: Impact of Three Strikes Laws on Commuting Zone Measures of Income Mobility

<i>Variable</i>	<i>Income Measure</i>			
	Income Slope	Income Intercept	Income Slope	Income Intercept
<i>strikes_CA<sub>t</sub></i>	.007** (.003)	-.022*** (.003)	.011*** (.002)	-.006*** (.002)
<i>strikes_GA<sub>t</sub></i>	-.024*** (.003)	.015*** (.003)	.013*** (.002)	-.005** (.002)
<i>strikes_Other<sub>t</sub></i>	.001 (.004)	-.001 (.004)	.000 (.003)	-.001 (.003)
CZ fixed effects	X	X	X	X
year fixed effects	X	X	X	X
CZ specific linear time trend			X	X
Weights for number of children	X	X	X	X
Observations	4,476	4,476	4,476	4,476

Income is measured at age 26. Those under the age of ten when the policy were implemented are the treated group, while those ten and older are the control group within the state. CZ stands for commuting zone. All standard errors are clustered by state and are in parentheses. The other states include any state other than California or Georgia that implemented a three strikes law. \*\*\*, \*\* and \* indicate that the estimates are statistically significant at the 1%, 5% and 10% levels.

Table 7: Impact of Three Strikes Laws on County Level College Attendance Measures

<i>Variable</i>	<i>College Measure</i>			
	25th Percentile	75th Percentile	25th Percentile	75th Percentile
<i>strikes_CA<sub>t</sub></i>	-4.644*** (.439)	-3.754*** (.542)	-2.556*** (.360)	-1.842** (.775)
<i>strikes_GA<sub>t</sub></i>	-2.144*** (.440)	-2.485*** (.543)	-3.763*** (.361)	-3.530*** (.776)
<i>strikes_Other<sub>t</sub></i>	.139 (.679)	.373 (.657)	.757* (.410)	1.065 (.676)
Cty fixed effects	X	X	X	X
year fixed effects	X	X	X	X
Cty specific linear time trend			X	X
Census Weights	X	X	X	X
Observations	12,056	12,056	12,056	12,056

College attendance is measured at age 19. Those under the age of six when the policy were implemented are the treated group, while those six and older are the control group within the state. Cty stands for county. All standard errors are clustered by state and are in parentheses. Census weights are created by the county level population under five and sum to one. The other states include any state other than California or Georgia that implemented a three strikes law. \*\*\*, \*\* and \* indicate that the estimates are statistically significant at the 1%, 5% and 10% levels.

Table 8: Impact of Three Strikes Laws on County Level Measures of Income Mobility

<i>Variable</i>	<i>Income Measure</i>			
	25th Percentile	75th Percentile	25th Percentile	75th Percentile
<i>strikes_CA<sub>t</sub></i>	-2.116*** (.249)	-1.727*** (.209)	-.301 (.208)	.101 (.200)
<i>strikes_GA<sub>t</sub></i>	.470* (.249)	-.620*** (.210)	-.064 (.209)	.268 (.198)
<i>strikes_Other<sub>t</sub></i>	-.328 (.294)	-.128 (.225)	-.263 (.275)	-.192 (.243)
Cty fixed effects	X	X	X	X
year fixed effects	X	X	X	X
Cty specific linear time trend			X	X
Census Weights	X	X	X	X
Observations	9,474	9,474	9,474	9,474

Income is measured at age 26. Those under the age of ten when the policy were implemented are the treated group, while those ten and older are the control group within the state. Cty stands for county. All standard errors are clustered by state and are in parentheses. Census weights are created by the county level population under five and sum to one. The other states include any state other than California or Georgia that implemented a three strikes law. \*\*\*, \*\* and \* indicate that the estimates are statistically significant at the 1%, 5% and 10% levels.