Intergenerational Effects of Mass Incarceration

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Abstract

We exploit variation in whether and when states implemented "three strikes and you're out" policies to estimate the intergenerational effect of mass imprisonment. Using difference-in-differences, an event study, and synthetic control we find an increase in the imprisonment rate in California but not in other three strike states. We measure intergenerational mobility with data from Chetty et al. (2014) which is based on child-parent tax linkages. Using two data sets and three measures of income mobility, we provide evidence of worsening income mobility for more disadvantaged kids who were ten or younger and eleven or younger in California. The coefficients on kids age nine and under are also the expected direction, but are not significant. To test whether human capital accumulation is the mechanism, we use Census data to provide suggestive evidence that the worse income mobility was driven by lower levels of educational attainment.

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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) posits 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 first contribute to the literature by showing three strikes laws significantly increased the level of imprisonment in California while having no substantial impact on the other three strikes states. We then add to the literature by using intergenerational mobility data from Chetty, Hendren, Kline and Saez (2014) to show that those who were more disadvantaged and younger when the three strikes law went into effect in California had worse income mobility. Our conceptual framework suggests that lower investments in children, which results in lower human capital accumulation, is the mechanism at play. To test this we use Census data to show that these younger cohorts with lower income mobility were less likely to have attended college and were less likely to have completed college. When we adjust the standard errors to take into account a small number of treatment states, the coefficients lose significance but remain of the expected sign and are more reasonable in terms of magnitude. Thus, the increased sentencing lengths imposed under three strikes for mainly non-violent crimes in California led to reduced income mobility, likely through reduced human capital accumulation.

Three recent papers using Scandinavian data have sought to move from correlations to causal estimates. One 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, the author do not include time trends or macroeconomic fixed effects. Dobbie, Grnqvist, Niknami, Palme and Priks (2018) uses Swedish data and variation in judge sentencing harshness to find that parental incarceration among the more disadvantaged population leads to worse medium-run outcomes for children. Specifically, they find that parental incarceration increases teen pregnancy, increases teen crime and reduces early life employment. Bhuller, Dahl, Lken and Mogstad (forthcoming) use Norwegian data and variation in judge harshness to find no impact on criminal activity or academic performance for the children of the incarcerated. Using the same method of variation in judge harshness, Arteaga (2017) uses Colombian data finds that the impact on parental incarceration depends on the potential for violence and conviction rates. She finds that overall there are positive impacts on education, but that incarceration is more disadvantageous for marginally convicted parents.

Billings (2017) uses United States data and a fixed effect model to find children are harmed by parental arrest but children benefit from parental incarceration. Our paper builds on the existing literature by using exogenous policy variation to estimate the causal impact on intergenerational mobility in the United States.

Between 1993 and 1996, 25 states and the federal government passed the "three strikes and you're out" legislation.[1] 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.[2] Though the degree of adaptation varies across states, by mid-1998, four years after adopting this penal reform, California sentenced more than 40,000 offenders 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.[3]

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 imprisonment of family members due to the literature on the importance of

[1] 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.

[2] In some sates individuals could strike out after the second convicted offense.

[3] 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.

early childhood environments. [4] We expect income mobility to be negatively affected because parental imprisonment could negatively impact human capital accumulation. The more human capital accumulated, the higher wages the individual is expected to receive in the labor market. If parents are incarcerated this means the parent has less money and less time to invest in their child's human capital accumulation. Cuhna and Heckman (2007) discuss the idea of dynamic complementary which states that skills acquired earlier in life help later investments be more productive. Thus, we expect the earlier in the life the parent is unable to invest in their child, the larger the detriment to human capital accumulation.

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 and criminal justice policies related to three strikes. Section 4 is the conceptual framework, Section 5 presents details on the data while Section 6 describes the empirical strategy. Section 7, presents the main results, as well as some robustness checks. Lastly, we conclude the paper in Section 8.

2 Institutional Details

This paper extends the research on the intergenerational effects of parental imprisonment by using three strikes policies as an exogenous policy shock. To accurately describe the impact of the three strikes law on state's imprisonment rates it is important to discuss the defining 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 exiting 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 imprisonment 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

[4] 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).

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.[5] 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. A felon was generally given mandatory life sentences without the possibility of parole, but in some states offenders became eligible for parole.[6] 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 the most dramatic changes to the criminal justice system to occur in California. As a result, we explicitly allow for a separate treatment effect for California 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 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.[7] Consistent with this finding, two-thirds of California's strikers are imprisoned for non-violent offenses (Zimring et al. (2001)). Our hypothesis of a detectable effect in California is also motivated by results from the assessment of Kovandzic et al. (2004).

[5]Georgia, Maryland and Louisiana also made provisions for a fourth strike.

[6]For example, after serving a minimum of 30 years in New Mexico and 25 years in California the individual may become eligible for parole.

[7]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)).

Their examination highlights California as a state with a large number of persons in prison under three strikes as opposed to the other states that implemented such laws.

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 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 were released right after implementation and qualified for the new policy. The authors conclude 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 Conceptual Framework

We follow Cuhna and Heckman (2007) for an overlapping generations model of human capital formation for our conceptual framework. Individuals live for $2T$ periods in which the first T years are childhood and the remaining $T+1$ to $2T$ years are as an adult. Upon reaching adulthood parents draw an initial skill level for their child (θ_1) from a distribution J(θ_1). The technology of skill formation is $\theta_{t+1} = f_t(h, \theta_t, I_t)$ where h is parental characteristics, θ_t is the previous skill and I_t is the investment. It is assumed that f_t is strictly increasing and strictly concave in I_t. The parent's problem is given by

$$V(h, b, \theta) = max(u(c_1) + \beta * u(c_2) + \beta^2 \delta * E[V(h', b', \theta_1')]) \tag{1}$$

where β is the discount rate, $u(\cdot)$ is the utility function, c is household consumption, b is the bequest received upon adulthood and δ denotes the parental altruism toward the child. Index 1 and 2 refer to time periods during the child's first T years. The parent faces a budget constraint of

$$c_1 + I_1 + \frac{c_2 + I_2}{(1+r)} + \frac{b'}{(1+r)^2} = wh + \frac{wh}{(1+r)} + b \tag{2}$$

where r is the interest rate, w is the wage and h is the previously mentioned parental characteristics (skills). The parent is also constrained by the liquidity constraint such that $b' \geq 0$. The production technology that determines a child's human capital as an adult (h') is

$$h' = m_2(h, \theta_1, [\gamma(I_1)^\phi + (1-\gamma)(I_2)^\phi]^{\frac{1}{\phi}}) \tag{3}$$

where γ is a skill multiplier and $\frac{1}{(1-\phi)}$ determines how easy it is to substitute between investments in different periods. The constraints on ϕ and γ are such that $\phi \leq 1$ and $0 \leq \gamma \leq 1$.[8]

Incarceration has been shown to disproportionately affect low income families who already have low b values to begin with, which leads to lower investments made by parents. Incarcerated individuals earn very low wages. For example, according to the California Code of Regulations Title 15 Crime Prevention and Corrections, the hourly wage ranges from \$.08 to \$.37 for half time or partial full time employment. This would lead to even lower available funds to invest in children. Even after the person has been released, Finlay (2011) shows they have a hard time finding employment because of the availability of background checks. Those with criminal record are also restricted in terms of welfare eligibility such as SNAP which could also contribute to the liquidity constraint binding even more.

Even if parents are able to later invest in their children, previous literature provides evidence that ϕ is relatively small which means that low levels of early investments are not easily made up for with larger investments at older ages. A key point in Cuhna and Heckman (2007) is human capital models must allow for self-productivity (earlier investments help you acquire skills from later investments since skills beget skills) and dynamic complementarity (investments in later periods are more productive the higher the current skill level).

Thus, we expect that longer sentences from three strikes further reduce the investments parents/caregivers make in their children. This is not easily remedied by later investments which would lead to lower human capital for these children. These lower levels of human capital would lead to lower wages as an adult and worsening income mobility. While separating from parents could theoretically be a good thing if the child is endangered, many individuals given extended sentences in California were non-violent offenders. It's not clear that people who commit bur-

[8] ϕ represents the degree of complementarity (or substitutability) between early and late investment in producing skills)

glaries are bad parents in the sense they are not optimally investing in their children given the constraints they face. We now turn to how we empirically test for lower levels of income mobility and if educational attainment appears to be the mechanism.

5 Data

In this section, we describe the data sources and key variable definitions. We employ data on state imprisonment rates, violent and property crime rates as well as whether and when each state adopted three strikes law. Measures of macroeconomic activity is also of importance and so we include states' unemployment rate and real GDP per capita. Regarding measures of mobility, we use data on children's income rank from Chetty et al. (2014). To test for human capital as the mechanism, we use American Community Survey Data.

Variation is based on whether and when states implement three strikes. 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). [9]

Table 1 outlines differences in three strike laws across states. Specifically, Washington became the first three strikes state in 1993, this was followed by California and eleven other states in 1994. California passed legislation in March of 1994 and had a referendum on the policy in November of 1994. In 1995 eleven states followed suit and Alaska joined the group in 1996.[10] Our main analyses are based on data from 1989 to 2000: four years before 1993 and four years after 1996 which provides a large enough time frame for the policy to have an impact on imprisonment without being confounded with other policy changes.

Information on state real per capita income was taken from Bureau of Economic Analysis and

[9] A total of 25 states adopted the three strikes law between 1993 and 1996. A decade later, in 2006, the 26^{th} state (Arizona) begun using the sentencing enhancement legislation. Later in 2012, Massachusetts implemented the law as well. Given that the last two states to accept this law did so ten years or more after 1996, we restrict the discussion to the first 25 three strikes states.

[10] There is a debate in the related literature about whether Alaska's law is considered a three strike law or not. All of our results are robust to including Alaska as part of the control states.

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 that average real per capita GDP growth among non-strikes states was marginally higher than that of three strike states between 1989 and 2000. Additionally, 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 imprisonment 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.

The measures of intergenerational mobility are from the Equality of Opportunity Project website and come from Chetty et al. (2014). We employ the Intergenerational Mobility 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 which cover approximately 95 percent of children in each birth cohort. 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 income as an adult. Parental income is defined as mean family income when the child is 15-19 years old. If parents did not file a tax return, then the information for income is pulled from W-2 forms, unemployment benefits, social security benefits and disability benefits information. In the case that there is no tax return filed or no information available, those individuals are given a zero for family income. 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 1986. 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. If social mobility is decreasing we would expect to see a decrease in the intercept indicating those kids from families at the bottom of the parental national income distribution have lower income ranks themselves. Additionally, we might expect that three strikes strengthened the relationship between parental income and child income would show up through a steeper 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.[11] Those born from 1980 to 1986 are in the data set. 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 the rank in the children national income distribution at age 26 for 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). 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, vital statistics records on births per year per county are merged with the County and Birth Cohort data set.

Our mobility outcome variables follow Chetty et al. (2014): relative mobility, absolute mobil-

[11] 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.

ity, and absolute upward mobility. The measure for relative mobility (slope measure) captures the association between a childs position in the income children distribution and his parents position in the parental income distribution. Children are ranked based on their income at age 26 relative to other children in the same birth cohort. Parents are ranked based on their income relative to their own cohort. As a result, this measure of mobility is referred to as the rank-rank specification and captures the degree to which differences in children's income are determined by their parents income. The measure for absolute mobility is aimed at detecting the outcomes of children from families of a given income level in absolute terms. This measure allows us to examine the mean outcomes of children who grew up in low-income families. We use children who start at the very bottom (the intercept) for this measure of mobility. Like Chetty et al. (2014) we compute absolute upward mobility as the intercept plus 0.25 times the slope estimate. In the county level data we can directly use the children who started at the 25th percentile instead of constructing it.

The summary statistics for the intergenerational mobility measures can be found in the bottom half of Table 2. California also has a flatter slope for parental income rank and child income rank at age 26 than the other states.

To test for whether human capital accumulation is the mechanism behind the intergenerational mobility results we look at high school dropout, whether the individual ever attended college and college completion. Education data comes from the 2006-2012 American Community Survey (ACS). Unlike the CPS, the ACS contains information on an individual's county of birth, which is important for us to determine exposure to worse income mobility as young children.[12] Our outcomes of interest are high school dropout, whether the individual ever attended college and college completion by age 26. We restrict the data to only include those born in the United States since we are looking at variation across state imprisonment policies when these individuals were children. This data allows us to know which state an individual was born in which we are using as a proxy for which state they lived in as a kid.

[12]We assume that an individual spends their childhood in the county of birth.

6 Research Design

6.1 imprisonment

6.1.1 Difference-in-Differences

Before conducting an analysis of the impact of three strikes on children's income mobility, we test whether three strikes laws had an impact on imprisonment rates. Our difference-in-differences strategy considers the year of implementation of the three strikes law in each state to trigger the treatment. For our identification strategy to yield causal estimates of higher imprisonment rates, it is important to establish that the timing of three strikes adoption appears to be exogenous. 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:

$$Y_{st} = \alpha + \delta strikes_{st} + \lambda_t + \eta_s + X_{st}\beta + \theta_s t + \epsilon_{st} \tag{4}$$

In this model, Y_{st} represents the log of imprisonment rate in state s at time t. The independent variable of interest, $strikes_{st}$, equal 1 if the "three strikes and you're out" legislation is implemented in state s at time t and zero otherwise.[13] Crime and hence imprisonment may be impacted by the macroeconomy. According to Schiraldi et al. (2004) reduction in crime in California (strike state) and New York (non-strike state) during the 1990s 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.

[13]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.

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 is identified as a state where three strikes laws are severe and frequently enforced (see Kovandzic et al. (2004)). Additionally, California is described as having applied this legislation with consistency. This was evident in the large number of offenders sentenced under two and three strikes provisions only four years after enactment (see Table 1). Thus, we estimate separate treatment effects for California compared to the rest of the three strike states. For the rest of the states with such laws, we pool them into one treatment effect called "other". We control for unobserved state and year characteristics by including state and year fixed effects (η_s and λ_t, respectively).[14] Given that imprisonment was trending upward during this time period, it is important that we include $\theta_s t$ which are state specific linear time trends. Thus, the preferred model we estimate is:

$$Y_{st} = \alpha + \delta_1 strikes\_CA_t + \delta_2 strikes\_OTHER_t + \lambda_t + \eta_s + X_{st}\beta + \theta_s t + \epsilon_{st} \tag{5}$$

We calculate our standard errors two ways. The first is by clustering our standard errors at the state level. However, identification of the key parameter arises from changes in policy for one state for our estimates in California. This small number of treatment violates the usual assumption that the number of groups changing policy is large and so the treatment effect point estimators are not consistent. Consequently, we use Conley-Taber method to construct confidence intervals via test statistic inversion (see Conley and Taber, 2011). Conley and Taber (2011) show that treatment effects that appear significant with clustered standard errors are not always significant when the standard errors are adjusted for, so it is important to show the results hold when the correct standard errors are used.

[14] A similar justification was presented by Kovandzic et al. (2004) in their analysis of three strikes on crime.

6.1.2 Event Study

To better test for the parallel trends assumption, we employ an event study. We include an indicator for whether or not a state implemented three strikes law starting in period $t-k$. Specifically, we estimate the following equation.

$$Y_{st} = \alpha + \sum_{k=-4}^{4} \gamma_{t-k}(S_{t-k}) + \eta_s + \lambda_t + \epsilon_{st} \tag{6}$$

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. γ_{t-k} is an estimate (k years after the three strikes law was enacted) of the treatment effect relative to one year prior to implementation. As in our main specification, the event study model includes state (η_s) and year (λ_t) fixed effects. We expect the effect will be delayed since Clark et al. (1997) discuss how data on Los Angeles showed that strike court cases were pending longer than non-strike court cases since defendants were less likely to accept plea deals that would result in a strike.[15]

6.2 Intergenerational Mobility

Based on our results from the impacts on imprisonment rates, we use those changes to explore whether there was worsening income mobility for kids who experienced higher levels of imprisonment in their state. We try multiple age cutoffs and focus around the age ten cutoff. While we would like to also estimate the impact on cutoffs much earlier than nine, given the birth cohorts in the data it is not feasible. Additionally, Hopkins and Bracht (1975) find that around age ten is a critical time period as IQ becomes stable at this point. Thus, this is has been called a critical or sensitive time period for development. For intergenerational mobility outcomes (Y_{cb}), we estimated the following model:

[15]Goode (2012) reports that 94 percent of state cases end in plea bargains while Clark et al. (1997) reports the Los Angeles data suggests there was a 25 percent increase in jury trials. Thus, even with defendants being less likely to accept plea deals, many cases were still settled out of court.

$$Y_{cb} = \alpha + \delta * dosage_{cb} + \lambda_b + \eta_c + \theta_c b + \epsilon_{cb} \tag{7}$$

where c is the commuting zone or county and b is birth cohort. We define dosage as a dummy variable for being at or below the age cutoff when the law went into effect.

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 clustered at the state level since that is the level at which the policy was implemented. We also calculate Conley-Taber standard errors to adjust for only California being treated. 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 create weights from the Vital Statistics county level birth data. Additionally, as a robustness check we use Census Intercensal County Population Data.[16] 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.

6.3 Mechanism: Human Capital Accumulation

To test for whether human capital accumulation is the mechanism through which intergenerational income mobility is affected, we estimate the following model

$$Y_{cb} = \alpha + \delta * younger_{cb} + \lambda_b + \eta_c + \theta_c b + \epsilon_{cb} \tag{8}$$

where Y_{cb} is our educational outcome of interest, c is the county, b is their birthcohort and younger is a dummy variable equal to one if the individual was a younger child (1984 and 1985

[16]available at https://www.census.gov/data/tables/time-series/demo/popest/1980s-county.html

birthcohort) born in a county in California.

Specifically, for each county, we compute the share of residents without a high school diploma by age 26 (High School Dropouts), the share of residents with some college experience (Some College) and the share of residents with a 4 year degree or higher (College Graduate).[17] 'Younger' is a dummy variable that takes a value of one if the respondent was born in California in 1984 or 1985 which corresponds with the birthcohorts that had worse income mobility. We hypothesize that δ will be positive when the outcome is high school dropout and negative when the outcome is ever attended college or college completion.

7 Results

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

7.1 Imprisonment 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 imprisonment 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. Specifically, we find that the three strikes law in California resulted in a 5.3 percent increase in imprisonment.[18] The Conley-Taber 95 percent confidence intervals for

[17]To construct these outcome variables we use respondents' educational attainment, as measured by the highest year of school or degree completed.

[18]Since the California legislature passed the law in March and a referendum was held in November about it being a constitutional amendment, it is not clear that 1994 should be considered treated. Thus, we drop 1994 from the analysis and consider years to be treated if they had the policy in effect for the entire calendar year and control years to be years without the policy for any length of time. If we instead consider 1994 to be treated the point estimate decreases in magnitude to a 4 percent increase, which is not surprising given that the law wasn't implemented for the entire year.

California is (.009,.1) indicating the results are significant at the conventional level. In columns 2 and 3 we add other macroeconomic controls usually found in the crime literature. With these controls the result in California becomes insignificant. We find the reason for this is that if instead of having imprisonment as the outcome of interest we use the state unemployment rate or the state log(real per capita GDP), these are impacted by the three strikes law. So many people were incarcerated that the unemployment rate significantly decreased and the per capita GDP decreased. Thus, moving forward we do not use these as controls given that they themselves are impacted by the policy.

7.1.1 Event Study

We use the event study to test for pre-trends. Changes due to the treatment should be statistically insignificant before adoption. Treatment variables at or after implementation are expected to be positive and statistically significant; an indication that following implementation, imprisonment rates increased within treated states. Our event study results, investigating this validity of treatment effect, are displayed in Figure 1 and Table 4. Given our difference-in-differences results in California, we run event study regressions for that state specifically.

California 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 one percent level two and three years after. This is consistent with the story that defendants did not want to accept plea deals that would count as a strike and thus increase their time incarcerated.

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.

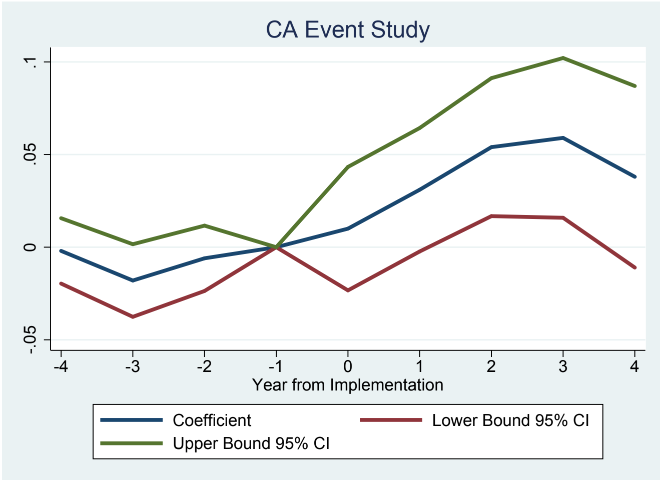

Figure 1: Differences in California Imprisonment Rates Over Time

The recommendation is to drop two time periods far away from each other so we drop $k = -1$ and $k = -3$.[19] 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 of Table 4. 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 imprisonment rates in California.

[19] Specifically, do not drop $k = -1$ and $k = -2$.

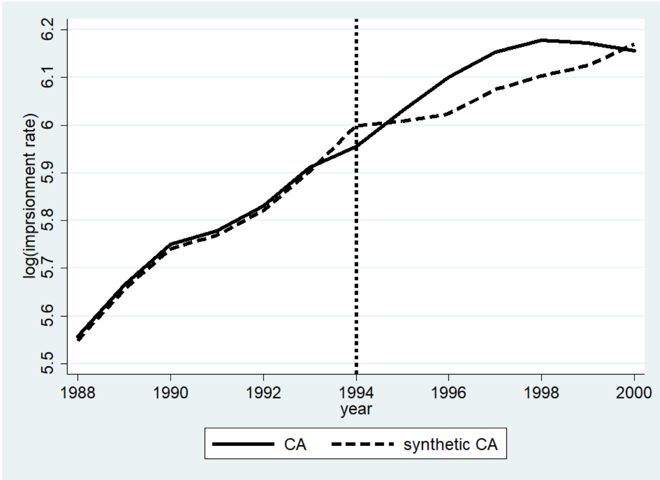

Figure 2: Trends in Imprisonment Rates in California Relative to Synethetic California

7.1.2 Synthetic Control

We conduct sensitivity analysis by using the synthetic control method of Abadie and Gardeazabal (2003) and Abadie et al. (2010). Our donor pool for this analysis includes the remaining 49 states and the District of Columbia (DC). As a robustness check we restrict our donor pool to include only the control states and our results are qualitatively the same.

The results displayed in Figure 2 compare the pre-treatment data of actual California with the synthetic California. Prior to 1994, California and Synthetic California are extremely similar; however, following three strikes implementation (in 1994) the imprisonment rate in California rose above the average rate for Synthetic California, in support of our results reported above.

7.2 Intergenerational Mobility Outcomes

Does the implementation of "Three Strikes and You're Out" lead to changes in intergenerational mobility?

The impact of three strikes laws on commuting zone measures of income mobility is in Table 5. Kids age nine and under only experienced the year of implementation in which there was no significant impact on imprisonment due to the backlog in the courts. However, the coefficients have the expected signs as there is a strengthening in the correlation between parental and child rank in their respective income distribution (relative mobility) and worsening absolute and absolute upward mobility. Using an age ten or eleven cutoff (meaning the kids experienced the significant increase in imprisonment) we find very significant relationships between the imprisonment dosage and worsening income mobility. Parental rank matters more for the child's rank as measured by the relative mobility. The absolute mobility and absolute upward mobility show that the strengthen is driven by worsening outcomes for those who are the most disadvantaged to begin with, consistent with Dobbie et al. (2018).

Specifically, for kids age ten or younger, our absolute mobility coefficient means that for kids starting out at the very bottom of the income distribution, the increase in imprisonment leads to a reduction of .61 points in the expected income rank at age 26. For kids ten and under starting at the 25th percentile, we find the increase in imprisonment leads to a reduction of .34 points in the expected income rank at age 26. Given a slope of .197 in the descriptive statistics for California prior to implementation, that means that as a kid whose parents income rank increased by ten points, the kid's rank increased by 1.97 points. Increasing the relative on average mobility due to three strikes by .0105 means that moving up ten points in the parental income distribution increases the child's rank by an additional 1.05 rank points for a total of 3.02 rank points.

In addition to clustering our standard errors, we also calculate Conley-Taber standard errors. For age nine and under the coefficients remain insignificant. For kids ten and under the relative mobility is still significant at the five percent level. Additionally, the absolute mobility measure is significant with Conley-Taber standard errors at the five percent level and the upward income mobility is significant with Conley-Taber standard errors at the ten percent level. For the eleven and under age cutoff, the absolute upward mobility remains insignificant, the absolute mobility measure remains significant at the ten percent level and the relative mobility remains significant at the five percent level. Thus, our results are not merely driven by us failing to take into account only one state was treated.

7.2.1 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 6 shows the impact on rank in the national income distribution at age 26. We find evidence of a significant decline in income mobility for children who started at the 25th percentile and were 11 years or younger at the time three strikes was implemented in California. This suggests that young children raised in a county with relatively high levels of imprisonment (due to the adoption of three strikes law) experience a .58 point decline in their expected income rank at age 26. For those who started at the 75th percentile, the coefficients are marginally significant (ten percent level) for kids age nine or younger but are insignificant for the other age cutoffs. This further suggests that those who started off more disadvantaged were negatively impacted by the increase in imprisonment. These results are qualitatively the same if we use the Census weights instead of the Vital Statistics weights.

To take into account only California was treated, we again apply the Conley-Taber method. For the eleven and under age cutoff, the estimate for those who started at the 25th percentile remains negative and statistically different from zero, supporting our earlier findings that relatively poor young children in California experienced a decline in their expected income rank at age 26. For those who started at the 75 income percentile, the coefficient associated with children 9 years or younger was no longer statistically different from zero. This indicates that the California three strike policy negatively impacted more disadvantaged kids while having no impact on more advantaged kids.

8 Mechanism

One reason that intergenerational mobility could be impacted is through exposure to crime. To test this hypothesis, we run the same difference-in-differences as before but have the outcome variable defined as the log of violent crime or the log of property crime. When we cluster our standard errors there is no impact on violent crime, but there is on property crime. There is no impact on either violent crime or property as a result of three strikes laws in California (see Tables 7) with the Conley-Taber method.

As discussed in the conceptual framework, we hypothesize the mechanism for the worsening income mobility is through reduced human capital accumulation. To test for evidence of this, we investigate whether the birthcohorts in California with worse income mobility had lower educational attainment. Results displayed in Table 8 reveals worsening educational attainment. There is a significant increase in high school dropouts for the cohorts affected by the worsening income mobility. Also, our estimates indicate that those cohorts in California with worse income mobility were 6.7 percentage points less likely to have attended college and were 9.8 percentage points less likely to have completed college. We do a falsification test in which the other states that implement three strikes laws are considered treated for the same birthcohorts that were treated in California. In the other states that at some point implemented three strikes, we find no evidence of worse educational attainment for the same birthcohrts.

We also adjust our estimates with the Conley-Taber method. The new coefficients are smaller in magnitude. The increase in high school dropouts is .3 percentage points, the decrease in college attendance is .3 percentage points and the decrease in college completion is .4 percentage points. While these changes are insignificant, they do seem more reasonable in terms of the magnitude of changes given our intent-to-treat framework.

9 Conclusion

This paper highlights the effects of "Three Strikes and You're Out" legislation on imprisonment and intergenerational mobility. The first contribution is to show that three strikes increased imprisonment in California even with state trends, state fixed effects and year fixed effects included. Previous policy briefs have simply used percentage change as a way to argue the law increased imprisonment. We find no evidence of increases in imprisonment in the aggregated other states that implemented a three strikes law.

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 criminals that were eligible to be convicted under this law. 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.[20] 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.

This paper further adds to the literature by using multiple measures of income mobility and two different data sets to show a consistent pattern of worsening income mobility due to California's three strike law. Specifically, we find that kids ten and younger or eleven and younger who were more disadvantaged to begin with had worse income mobility as a result of the increased imprisonment. The finding that disadvantaged kids were the ones affected is consistent with the findings in Dobbie et al. (2018). In terms of mechanisms, we show that kids did not experience a significant change in exposure to crime. We provide evidence supporting the mechanism of lower human capital accumulation through reduced college attendance and college completion. Although the results are not significant, there are the expected signs and a reasonable magnitude.

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 stated he wants to go back to stricter sentencing polices. Opponents of such policies argue that these policies have dispro-

[20] 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).

portionately 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 California has changed its three strike law, the effect will continue to be felt by those who were young children in California when the policy was implemented.

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Table 1: Comparison of State Strikes Laws

State (Year)	Least number of strikes required to trigger enhanced sentencing	Types of Crime	Number imprisoned under three strikes law after 10 years
Alaska (1996)*			
Arkansas (1995)	Two	Violent Crime	5
California (1994)	Two	Any Crime	42,322
Colorado (1994)	Three	Violent Crime	4
Connecticut (1994)	Three	Violent Crime	1
Florida (1995)	Three	Violent Crime	1,628
Georgia (1995)	Two	Violent Crime	7,631
Indiana (1994)	Three	Violent Crime	38
Kansas (1994)	Three	Violent Crime	N/A
Louisiana (1994)	Three	Violent Crime	N/A
Maryland (1994)	Four	Violent Crime	330
Montana (1995)	Two	Violent Crime	0
Nevada (1995)	Three	Violent Crime	304
New Jersey (1995)	Three	Violent Crime	10
New Mexico (1994)	Three	Violent Crime	0
North Carolina (1994)	Three	Violent Crime	22
North Dakota (1995)	Two	Violent Crime	10
Pennsylvania (1995)	Two	Violent Crime	50
South Carolina (1995)	Two	Violent Crime	14
Tennessee (1994)	Two	Violent Crime	14
Utah (1995)	Three	Violent Crime	N/A
Vermont (1995)	Three	Violent Crime	16
Virginia (1994)	Three	Violent Crime	328
Washington (1993)	Three	Violent Crime	209
Wisconsin (1994)	Three	Violent Crime	9

Sources: Schiraldi, Colburn and Lotke (2004) and Dickey and Hollenhorst (1999). *There is a debate in the criminology literature about whether Alaska's law is considered a three strike law.

Table 2: Summary Statistics

Variable	*CA*		*Other Treated*		*Control*	
	Before 1994	*After 1994*	*Before 1994*	*After 1994*	*Before 1994*	*After 1994*
Crime Related Measures						
Imprisonment rate	327.23	450.22	267.53	345.31	282.36	380.09
	(30.17)	(36.64)	(107.00)	(124.49)	(218.52)	(271.75)
Violent crime rate	1062.06	798.93	566.14	528.52	575.66	501.12
	(54.19)	(156.96)	(282.48)	(263.75)	(480.57)	(388.37)
Property crime rate	5593.16	4053.31	4774.86	4503.53	4305.92	4036.12
	(152.70)	(794.08)	(994.12)	(993.47)	(1345.94)	(1296.05)
Real GDP per capita	29,373	38,168	26,001	35,849	27,497	37,906
	(811)	(8,255)	(6,620)	(9,496)	(12,353)	(18,487)
Unemployment rate	7.5	6.6	5.9	4.6	6.2	4.8
	(2.0)	(1.4)	(1.2)	(1.2)	(1.8)	(1.3)
Intergenerational Mobility						
Income at 26 Slope	.197	.198	.281	.276	.270	.274
	(.037)	(.043)	(.073)	(.065)	(.071)	(.068)
Income at 26 Intercept	.402	.382	.373	.379	.393	.397
	(.024)	(.032)	(.064)	(.057)	(.062)	(.060)
Income Rank 26: 25th pctl	.465	.453	.449	.455	.459	.464
	(.024)	(.021)	(.054)	(.052)	(.050)	(.049)
Income Rank 26: 75th pctl	.553	.541	.587	.588	.590	.596
	(.020)	(.021)	(.035)	(.039)	(.036)	(.039)
Educational Attainment (Mechanism)						
High School Dropout	.0711	.068	.066	.061	.058	.055
	(.004)	(.003)	(.027)	(.033)	(.027)	(.024)
Some college	.579	.602	.631	.64	.659	.67
	(.014)	(.006)	(.065)	(.07)	(.072)	(.072)
College Graduate	.293	.308	.378	.378	.399	.407
	(.283)	(.002)	(.077)	(.077)	(.081)	(.088)

The crime values above represent averages over the sample period, 1989 to 2000. For the intergenerational mobility measures the 'before' time period corresponds to birth cohorts before 1984 while 'after' corresponds to 1984 and 1985. Numbers in parentheses represent standard deviations.

Table 3: Impact of Three Strikes Law Implementation on State Imprisonment Rates

	Column1	Column 2	Column 3
$strikes\_CA_t$	.0557*** (.02)	.018 (.02)	.0099 (.023)
$strikes\_OTHER_{st}$	-.0077 (.02)	-.0039 (.0247)	-.0007 (.0249)
state fixed effects	X	X	X
year fixed effects	X	X	X
state specific linear time trend	X	X	X
Controls:			
Unemployment rate		X	
Log(real per capita GDP)			X
Observations	612	612	612

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 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 2 and Column 3, 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. Conley-Taber 95 percent confidence intervals for California are (.009,.1).

Table 4: Comparison of State Three Strikes Laws

Treatment	*CA*	
	Column 1	Column 2
S_{t-4}	-.002 (.009)	.004 (.007)
S_{t-3}	-.018*(.010)	second omitted
S_{t-2}	-.006 (.009)	.000 (.008)
S_t	.010 (.017)	.016 (.017))
S_{t+1}	.031* (.017)	.037** (.018)
S_{t+2}	.054*** (.019)	.060*** (.019)
S_{t+3}	.059*** (.022)	.065*** (.022)
S_{t+4}	.038 (.025)	.044* (.026)
Borusyak and Jaravel F-Test		.7510
Number of Obs	510	510

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 Dosage on Commuting Zone Measures of Income Mobility

	Cluster Standard Errors		
Variable	Age 9	Age 10	Age 11
Relative Mobility	.0048 (.0029)	.0105*** (.0016)	.0089*** (.0014)
Absolute Mobility	-.0016 (.0024)	-.0061*** (.0017)	-.0061*** (.0018)
Absolute Upward Mobility	-.0004 (.0018)	-.0034** (.0014)	-.0039** (.0015)
	Conley-Taber 95 Percent CI		
Variable	Age 9	Age 10	Age 11
Relative Mobility	(-.0143, .0306)	(.0025, .0262)	(.0008, .0145)
Absolute Mobility	(-.0177, .0152)	(-.0133, -.0005)	(-.0090, .0009)
Absolute Upward Mobility	(-.0133, .0085)	(-.0086, .0010)	(-.0056, .0009)
Observations	4,476	4,476	4,476

All regressions include commuting zone fixed effects, year fixed effects, commuting zone specific linear time trends and weights for the number of children. Income is measured at age 26. The measure for relative mobility captures the association between a childs position in the income children distribution and his parents position in the parental income distribution. Absolute mobility captures the mobility for those at the very bottom of the income distribution. Absolute upward mobility measures a child's expected rank in the national income distribution at age 26, conditioning on being born to parent's that were at the 25th percentile of their national income distribution. The top portion of the table shows standard errors clustered by state. ***, ** and * indicate that the estimates are statistically significant at the 1%, 5% and 10% levels. The bottom part of the table shows the 95 percent confidence intervals when the Conley-Taber method is used to take into account only one state is treated.

Table 6: Impact of Three Strikes Dosage on County Level Measures of Income Mobility

	Cluster Standard Errors		
Variable	Age 9	Age 10	Age 11
Absolute Upward Mobility (25th percentile)	.0011 (.0018)	-.0025 (.0015)	-.0058***(.0019)
75th percentile	.002* (.0011)	.0004 (.0013)	-.00003 (.0014)
	Conley-Taber 95 Percent CI		
Variable	Age 9	Age 10	Age 11
Absolute Upward Mobility (25th percentile)	(-.0096, .0129)	(-.0104, .0015)	(-.0098, -.0012)
75th percentile	(-.0024, .0137)	(-.0043, .0066)	(-.0034, .0058)
Observations	9,453	9,453	9,453

All regressions include county fixed effects, year fixed effects, county specific linear time trends and birth record weights. Income is measured at age 26. Absolute upward mobility measures the mobility of those who started at the 25th percentile (based on parents' rank in their national income distribution). As a placebo test we estimate whether more advantaged kids who should not be affected had a change in expected income rank. All standard errors are clustered by state and are in parentheses. ***, ** and * indicate that the estimates are statistically significant at the 1%, 5% and 10% levels.

Table 7: Impact of Three Strikes Law Implementation on State Violent and Property Crime Rates with and Without Correction Made for One Treatment State

	CA with Clustered SE	CA with Conley-Taber
Violent Crime	-.0027 (-.0706, .0652)	.0283 (-.0456, .0948)
Property Crime	-.0569 (-.1, -.0142)	-.0307 (-.08, .0057)
state fixed effects	X	X
year fixed effects	X	X
state specific linear time trend	X	X
Number of states	51	51
Observation	561	561

Confidence intervals for parameters are presented in parentheses. We use the $\hat{\Gamma}^*$ formula to construct the Conley-Taber standard errors which are found under the Conley-Taber column. The column denoting clustered SE include the confidence intervals from when we cluster standard errors by state.

Table 8: Impact of Three Strikes on Educational Attainment

	Clustered Standard Errors		
Variable	High School Dropout	Some College	College Graduate
$Strikes\_CA_t$	.0123***(.0038)	-.0671***(.0131)	-.0975***(.0169)
$Strikes\_OTHER_t$	.0051(.0067)	-.0265(.0178)	-.0248(.0216)
	Conley-Taber 95 Percent CI		
$Strikes\_CA_t$	.0028 (-.078, .0504)	-.0027 (-.0721, .187)	-.0039 (-.0759, .1541)
Observations	13,420	13,420	13,420

Sources: 2006-2012 American Community Survey . All regressions include county and year fixed effects. The dependent variable is based on highest reported level of education attained by age 26 for birthcohort 1980-1985. The treatment variable takes a value one if the respondent was born between 1984 and 1985 in California ($Strikes\_CA_t$) or in any of the other 23 strike states ($Strikes\_OTHER_t$). All standard errors are clustered at state level and presented in parentheses. ***, ** and * indicate that the estimates are statistically significant at the 1%, 5% and 10% levels.