Intergenerational Effects of Enhanced Sentencing

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Abstract

We show that three strike laws lead to worse intergenerational mobility for children aged eight to nine at implementation. The effects are focused in states that actually used their law. Our results complement the literature that examines the impact on the extensive margin (are parents incarcerated or not) since we show that the intensive margin (sentence length) matters also. Using several data sets and multiple measures of mobility from Chetty et al. (2014) we find worse income mobility and reduced college attendance for these children. Negative impacts on income and college attendance are also found using the American Community Survey.

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1 Introduction

The United States has one of the highest imprisonment rates in the world as a result of its criminal justice policies.[1] 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. Glaze and Maruschak (2008) report between 1991 and midyear 2007, there was a 79 percent increase in parents held in state and federal prisons and an 80 percent increase of children with incarcerated parents. They note that this increase has resulted in an estimated 2.3 percent of the U.S. resident population under age 18 having an incarcerated parent.

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) provides empirical evidence that paternal incarceration is associated with lower cognitive capacities for children which is detrimental to academic achievement.[2]

Despite what appears to be converging evidence that parental incarceration poses a significant threat to child development, this area of research has an important methodological challenge related to selection bias. Incarceration is not random and many of the same factors that predict parental incarceration 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

[1]The imprisonment rate is defined by the Bureau of Justice Statistics as "the number of persons under the jurisdiction, or legal authority, of state or federal correctional officers per 100,000 U.S. residents."

[2]Additionally, a Pew Charitable Trusts Foundation 2010 report discusses literature showing that people who have been incarcerated have worse economic outcomes, literature showing many of these people have kids, and literature showing kids who grow up disadvantaged tend to stay disadvantaged. The report is making an argument based on the law of transitivity and not examining an exogenous policy change that impacts incarceration and intergenerational mobility.

most affected by mass incarceration in the United States (Ewert, Sykes and Pettit (2014) and Wakefield and Wildeman (2013)). Thus, children with incarcerated 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. It is important for public policy to know whether associations are driven by general disadvantages or whether there is a causal impact of our criminal justice policies.

The contribution of this paper is to provide causal estimates of the intergenerational impacts of the 'three strikes and you're out' policies. As opposed to the related literature which examines impacts by whether parents are incarcerated or not, this paper provides a complementary analysis that focuses on changes in time spent incarcerated. This paper uses the variation in timing and implementation of three strikes policies as an exogenous policy shock to estimate the effects of enhanced sentence lengths on intergenerational mobility. Between 1993 and 1996, 25 states and the federal government passed the 'three strikes and you're out' legislation.[3] This policy mandates significant sentence enhancements for repeat offenders, such as 25 year sentences or life sentences without parole on conviction of the third violent offense.[4] The implementation of this law is credited with contributing to mass incarceration in the United States.[5]

Using quasi-experimental methods with two data sets that include multiple measures of intergenerational mobility from Chetty, Hendren, Kline and Saez (2014), we estimate changes in income and education mobility among three strike states. Since the criminal justice literature reports that some states used their laws much more often in practice than others, we estimate separate treatment effects for high and low usage states. Passing a law and implementing a law are two fundamentally different treatments so we expect the effect is larger in states that actually

[3]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.

[4]In some states individuals could strike out after the second convicted offense.

[5]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.

used their law. We find that states that rarely or never used their statutes did not experience a change in intergenerational mobility, but states that actually used their laws had changes in intergenerational mobility. Specifically, kids eight to nine years old at implementation had worse intergenerational income mobility in high use states. In robustness checks, we use American Community Survey (ACS) data to provide supporting evidence that young children born in three strikes states earn lower annual income. Thus, our results still hold even with individual level data.

Our conceptual framework suggests that lower investments in children, which results in lower human capital accumulation, is a mechanism at play. To test this we use the mobility data based on college enrollment at age 19 from Chetty et al. (2014). We find evidence of worse college attendance for the group with worse intergenerational income mobility. Additionally, for a robustness check we use ACS data to show that there is a reduction in college attendance in high usage states for these young children. This is consistent with these children faring worse in the labor market due to reduced human capital accumulation. These results have important policy implications given the current political debate over whether to reduce sentences for non-violent offenders.

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 related literature. Section 4 is the conceptual framework which focuses on the role of human capital accumulation. Section 5 discusses how some states used their three strikes law more in practice than others. Section 6 shows the impact on intergenerational income mobility across ages at which the policies were implemented. Section 7 discusses evidence about the role human capital accumulation plays. Section 8 provides additional robustness checks including estimating effects using the American Community Survey data. Section 9 concludes and discusses the importance of our results for public policy.

2 Institutional Details

This paper extends the research on the intergenerational effects of parental incarceration by using three strikes policies as an exogenous policy shock. To accurately describe the impact of the three strikes law on prisoner's sentence lengths 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. 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 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 from prison 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. Georgia, Maryland and Louisiana also made provisions for a fourth strike. 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. Table I 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, Georgia and Florida. As a result, we explicitly allow for a separate treatment effect 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 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. A conviction for residential burglary is a sufficient condition for three strikes in California and many of those who become eligible for the extended sentences are burglars (Zimring et al. (2001)). 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, 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. The hypothesis of a detectable effect in California, Georgia, and Florida is also motivated by results from the assessment of Kovandzic et al. (2004) and Clark et al. (1997). Kovandzic et al. (2004) 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.

In a review of three strikes policy on U.S. prison population Zhang, Maxwell, and Vaughn (2009) highlight Dickey and Hollenhorst's (1999) argument that the impact of three strikes laws on prison and crime across the U.S. had little national impact because few offenders outside of California, Florida, and Georgia were ever sentenced under this provision. Additionally, Neal and Rick (2013) outline that while California, Georgia, and Florida handed down numerous enhanced sentences to many offenders under these laws, a significant number of states defined their strike zones so narrowly that the statutes are rarely used.

3 Literature Review

Several studies investigate the effects of three strikes on crime, with mixed results. 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 or perverse criminals, with promises of great impact, comparative analysis of crime across the United States revealed disappointing results ten years after most three 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.

Stolzenberg and D'Alessio (1997) use maximum likelihood estimation to determine there was no reduction in serious crime or petty theft rates due to California's three strike law. Marvell and Moody (2001) find three strikes laws increase homicide. Their explanation for this is that due to the increased punishment, criminals have an incentive to murder witnesses and victims who may identify them. Chen (2008) concludes that despite California's law being the broadest and most widely applied, California did not experience a greater reduction in crime than other three strike states.

However, these papers did not focus specifically on individual's who would be impacted by three strikes. Helland and Tabarrok (2007) examine whether three strikes impacted behavior of those who already had two strikes in California. They find that California's laws reduced felony arrest rates by 17 to 20 percent among this group of individuals. Thus, the policy may not have impacted crime overall, but it appears to have reduced criminal behavior for those who would have received their third strike. Shepherd (2002) builds a theoretical model to argue that there should be a deterrent effect for individuals with fewer than two strikes as well. She raises the concern that previous papers looking for a deterrent effect do not adequately control for crime impacting the implementation of three strikes laws. Specifically, she finds the results in Marvell and Moody (2001) change when taking into account the simultaneity of crime and the introduction of three strikes in her empirical analysis. Her results are consistent with full deterrent effects.[6] Thus, research has shown that three strikes deters crime that would have counted towards a strike.

While previous literature has estimated effects from three strikes on potential criminals, we

[6]She finds effects on crime that would count towards the first strike and not just the third strike. This is possible to empirically test because the law in California has different rules for what counts as a first and third strike.

test whether there are changes in intergenerational mobility. Specifically, we examine how implementation of three strikes affects intergenerational income mobility and the college attendance gradient (how parental income rank relates to college attendance). Given the variation across states in the severity and usage of the policy, we expect to find larger impacts in states with higher usage levels. This is because the media coverage may have made the enhanced sentencing more salient to those who would be affected by three strikes. In other states that technically had the law implemented but never used it, the effect on behavior is expected to be small and insignificant.

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. 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. Thus, previous work has found that longer sentence lengths have no significant benefit in terms of recidivism while this paper finds that longer sentence lengths impose a significant cost on children eight to nine years old.

Four recent papers have sought to move from correlations to causal estimates of the effects of parental incarceration using data from Denmark, Sweden, Norway and Colombia. Wildeman and Andersen (2017) 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. Dobbie, Grönqvist, 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, Løken and Mogstad (2018) use Norwegian data and variation in judge harshness to find no impact on criminal activity or academic performance for the children of the incarcerated. Arteaga (2018) uses Colombian data and variation in judge harshness to find that parental incarceration increases educational attainment by .8 years for kids whose parents are on the margin of incarceration.

In terms of the United States context, Billings (2017) uses a fixed effect model to find children are harmed by parental arrest but children benefit from parental incarceration. Specifically, they benefit in end of grade exams and on behavioral measures. The literature outside the United States mentioned previously has used exogenous variation to estimate impacts while Billings (2017) is not using exogenous variation. Cho (2009) examines the impact of an incarcerated mother on a child's outcomes in Chicago. She compares the impacts on children whose mothers are in prison (treatment group) to children whose mothers are in Cook County Jail (control). The test scores of children in the two groups are not statistically different.

Concurrent with this paper, another working paper (Norris, Pecenco and Weaver (2018)) uses quasi-experimental methods to estimate a causal impact within the United States. Specifically, they use variation in judge harshness to look at spillover effects on siblings and children in Ohio. They find that parental incarceration increases teen pregnancy and reduces high school graduation.[7] We contribute to the literature by showing sentence length also matters with our finding of worse intergenerational income and education mobility for kids ages eight through nine years in states that increased their sentence lengths due to three strikes.

Parental separation due to incarceration likely has a different effect from other types of parental separation. For example, 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. Glaze and Maruschak (2008) report that there is a negative association between time the parent expects to be incarcerated and the likelihood of

[7]Aizer and Doyle (2005) look at incarceration of children directly in the United States context. They use variation in judge harshness to find that juvenile incarceration leads to worse outcomes for those incarcerated in the United States. Specifically, these individuals have worse high school completion rates and are more likely to be incarcerated as adults.

having weekly contact with their children.[8] Since the three strikes law lengthened the sentences for repeat offenders, this could result in reduced weekly contact of kids with their incarcerated parents.

Our research also relates to the literature that more broadly looks at the impact of spillovers related to incarceration and crime. Bhuller, Dahl, Løken, and Mogstad (2018) find causal spillover effects on criminal networks and brothers of defendants sent to prison in Norway. Specifically, people in a defendant's criminal or brother network are less likely to be charged with a crime over the next four years. While not focused on parental incarceration, this paper provides evidence of spillovers within social networks. Gershenson and Tekin (2018) estimate the impacts on test scores from the 2002 'Beltway Sniper' attacks. They find that schools closer to where people were being attacked had worse test scores, especially among third grade (math and reading) and fifth grade (math) students. Thus, previous literature has shown there are spillovers to the broader community from incarceration and crime.

4 Conceptual Framework

In this section, we explore one potential mechanism through which adult imprisonment may affect children. We do this by looking at the effect of enhanced sentencing on human capital formation. Parental incarceration may benefit or harm children, depending on the magnitude of the costs associated with being separated from one's parent, and benefits from a better and more productive environment. With this in mind, the main aim here is to provide the reader with a way of thinking about how parental imprisonment affects children's human capital. In keeping with Cuhna and Heckman (2007), we model human capital accumulation as a multi-stage process. For simplicity, we assume that childhood spans three periods. This three-stage human capital production process allows us to distinguish between early, middle and late childhood.

A child begins life with an endowment of human capital (h_0) and in each period t (for $t \in \{1, 2, 3\}$) receives contribution (I_t) to augment their initial stock. Therefore, the human capital

[8] Specifically, they report that 47 percent of parents who expect to be released within six months have weekly contact with their children, 39 percent have contact if they expect to be released in 12 to 59 months and only 32 percent have contact if they expect to get out in 60 or more months.

accumulates according to

$$h = h_0 + I_1 + I_2 + I_3. \tag{1}$$

The total contribution in each period reflects the sum of family and environmental inputs such that, $I_t = E_t + F_t$. Parental or family contributions in period t is F_t and may be viewed as time spent with the child (i.e. on homework after school, providing healthy meals, and safe transportation to and from school) or money spent on the child (i.e. for tutors, extracurricular activities, and summer camps). Environment-specific contributions, E_t, capture the social interactions one typically has, as determined by peers and role models in the neighborhood. This is based on the idea from Aliprantis and Carroll (2018) in which each neighborhood environment has a different total factor productivity.

Two features of E_t and F_t are important for our analysis. Firstly, both F_t and E_t can differ due to incarceration and secondly they can differ over time. We assume that E_t increases when parents are incarcerated. Thus, when a child is separated from his or her parent due to imprisonment, it is likely that they enter a better and more productive environment.[9]

Family contribution, F_t, falls when parents are incarcerated. This is because imprisoned parents were involved in the children's care and provided financial support prior to becoming incarcerated. Glaze and Maruschak (2008) report that approximately 64 percent of mothers and 47 percent of fathers in state prison report living with their children prior to arrest or incarceration. In addition to providing daily care, 52 percent of mothers and 54 percent of fathers report being the primary source of financial support for their children. Once incarcerated, parents may lose contact with children due to restrictions on visiting hours, high prices for phone calls and restrictions on access to technology.[10] Additionally, parental incarceration could create emotional trauma, increase stigma and social alienation which negatively affects cognitive development (Bhuller et al. (2018)).

[9]This assumption is supported by prior work which shows that children living in disadvantaged communities, characterized by high levels of crime, unemployment, low-quality housing, and inadequate access to health care are more likely to experience parental incarceration than children living in less-distressed neighborhoods; and those impacted by incarceration are more socially and economically disadvantaged, to begin with (Western, 2006; Ewert, Sykes, and Pettit (2014); as well as Wakefield and Wildeman (2013)).

[10]A Times article states that the FCC reported 15-minute calls could cost $15 prior to federal rules that were implemented in 2014 (Gustin (2014)).

The second key feature, timing, captures the idea that the effectiveness of investment in human capital is age dependent. Parental and environmental investments are most effective during the first stage of childhood and decrease over time. This aspect of the model is related to Cuhna and Heckman (2007). They argue that early childhood investment is more productive and complementary with later investment. This feature is also related to Knudsen et al. (2006). They cite evidence from economics, neurobiology, and sociology showing that different abilities and skills are formed in different stages of the life cycle and some essential skills are developed in early childhood.

To capture these two features, we define E_t and F_t as

$$\{E_t, F_t\} = \begin{cases} \{\phi_{e,t}e, \quad \phi_{f,t}f\} & \text{if } z = 0 \\ \{\phi_{e,t}(1+\delta_e)e, \quad \phi_{f,t}(1-\delta_f)f\} & \text{if } z = 1. \end{cases} \tag{2}$$

Here z is an indicator equal to one if parents are incarcerated in period t and zero otherwise. With this specification human capital depends on three sets of parameters. The first set of parameters, f, and e, captures the level of family and environmental investment, respectively, for the child when parents are not incarcerated. The relative size of these inputs provides information about how much human capital depends on the family and the environment. For example, if $e > f$ then the environment-specific investment is larger than family investment.[11]

The second set of parameters, δ_f and δ_e, gauges how family and environmental investment change in response to imprisonment. To be consistent with the above discussion, these parameters are positive such that family investments decrease and environmental investments increase with imprisonment. So the gains from environmental changes exceed the losses from parental separation due to imprisonment if $\delta_e > \delta_f$.

The third set of parameters, $\phi_{e,t}$ and $\phi_{f,t}$, gauges how time-sensitive investments are. Since we know that later investment is less productive, we set $\phi_{e,1} > \phi_{e,2} > \phi_{e,3}$ and $\phi_{f,1} > \phi_{f,2} > \phi_{f,3}$, with a smaller $\phi_{e,t}$ or $\phi_{f,t}$ indicating that the same level of investment yields smaller increments

[11] On the other hand, family investment is larger than or equal to environmental investment if $e < f$ or $e = f$, respectively.

of human capital through time. We place no restriction on the relative size of $\phi_{e,t}$ and $\phi_{f,t}$ but if for example, $\phi_{e,t} > \phi_{f,t}$ then the effectiveness of environmental inputs in period t is larger than the effectiveness of family inputs.

We use the definition of E_t, F_t to show that human capital can be expressed as the human capital of someone whose parents are not incarcerated (h_n) plus changes due to incarceration. If parents are not incarcerated, a child receives a combination of E_t and F_t each period, so at the end of childhood, human capital is equal to

$$h_n = h_0 + \sum_{t=1}^{3}(\phi_{e,t}e + \phi_{f,t}f). \tag{3}$$

Here h_0 is the initial stock of human capital, which is augmented in periods one to three with effective units of environmental and family investment, $\phi_{e,t}e$, and $\phi_{f,t}f$, respectively. The effectiveness of both types of investment falls through time and is determined by the relative size of $\phi_{e,t}$ and $\phi_{f,t}$. If parents experience incarceration, then human capital ($h_{b,a}$) accumulates according to

$$h_{b,a} = h_n + \begin{cases} \phi_{e,1}\delta_e e - \phi_{f,1}\delta_f f & \text{if } a = 1 \\ \phi_{e,2}\delta_e e - \phi_{f,2}\delta_f f & \text{if } a = 2 \\ \phi_{e,3}\delta_e e - \phi_{f,3}\delta_f f & \text{if } a = 3. \end{cases} \tag{4}$$

Here a captures age at separation. Given our three-stage model, $a \in \{1, 2, 3\}$ with $a = 1$ if a child is exposed to parental imprisonment during early childhood. If $a = 2$ or $a = 3$, a child is separated from their parents due to imprisonment in middle or late childhood, respectively.

A comparison of Equations (3) and (4) indicates that parental imprisonment may increase, decrease or have no effect on the stock of human capital. This depends partly on the relative size of the gains in environmental investment ($\delta_e e$) and losses in parental investment ($\delta_f f$). The overall effect also has a timing component, since the importance of these gains and losses ($\phi_{e,a}$ and $\phi_{f,a}$) varies with age at separation, a. This age-dependent effect of imprisonment identified here is similar to results outlined in Arteaga (2018). She provides evidence that children benefit when their convicted parents are incarcerated. However, this effect varied with age at incarceration, with those 0 to 5 years and 10 to 15 years experiencing larger increases in years of schooling relative to those 5 to 10 years.

For a simple interpretation of how parental imprisonment may affect the stock of human capital at the end of childhood, we consider the case where $\phi_{e,a} = \phi_{f,a} = 1$. With this adjustment, the change in human capital does not vary with a, and may be negative if losses in family investment outweigh gains in environmental investment (i.e. if $\delta_f f > \delta_e e$). Alternatively, imprisonment may increase or have no effect on human capital if $\delta_f f < \delta_e e$ or $\delta_f f = \delta_e e$.

We model 'three strikes and you're out' laws as an extension to the number of periods parents are incarcerated. With enhanced sentencing laws, we assume that parents remain in prison for an additional period. For example, if parents are incarcerated at the beginning of $t = 1$, instead of being released at the end of period one, they remain in prison and are reunited with children after $t = 2$. In line with the overall objective of this paper, we examine the effect of enhanced sentencing on human capital accumulation and how this effect changes with age at exposure. This is achieved by first investigating the difference between the human capital stock of children exposed to parental imprisonment with ($h_{l,a}$) and without ($h_{b,a}$) enhanced sentencing; $\Delta_a \equiv h_{l,a} - h_{b,a}$. We then consider how this difference varies with age at exposure by comparing $\Delta_{a=i}$ and $\Delta_{a=j}$ for $i \neq j$. Accordingly, we show

$$h_{l,a} - h_{b,a} \equiv \Delta_a = \begin{cases} \phi_{e,2}\delta_e e - \phi_{f,2}\delta_f f & \text{if } a = 1 \\ \phi_{e,3}\delta_e e - \phi_{f,3}\delta_f f & \text{if } a = 2 \\ 0 & \text{if } a = 3. \end{cases} \tag{5}$$

Equation (5) indicates that the effect of enhanced sentencing on human capital accumulation also varies with age at exposure. The effects on human capital accumulation for kids whose parents are incarcerated in the $a = 1$ period of childhood depends on differences in investments at age $a = 2$ because the enhanced sentencing results in the parent now being incarcerated at $a = 2$ and $a = 1$ instead of just at $a = 1$. Enhanced sentencing has no effect on the human capital stock of older ($a = 3$) children because regardless of whether there is enhanced sentencing or not, the parent is gone for the rest of childhood. Enhanced sentencing may have a positive, negative or null effect on the human capital stock of young and middle-aged children. For the young and middle-aged child, separation from parents due to enhanced sentencing leads to additional gains in environmental investment as well as additional losses in family investment. However, the overall change is dependent on the relative size and importance of these gains and losses.

For the reasons presented here, we argue that exposure to enhanced sentencing affects human capital development. The effect varies with age at exposure and may be non-monotonic. Older children are not affected by enhanced sentencing policies and the effect on younger children depends on changes in the relative importance of investment through time. The effect of enhanced sentencing on human capital formation may be monotonic in age if the relative effectiveness of environmental and family investment does not change over time. In our empirical analysis, we test these hypotheses.

5 Does the implementation of "three strikes and you're out" impact sentence length?

Related criminal justice literature suggests that California, Georgia and Florida should have larger effects because they actually used their three strikes laws. To test for an effect of this policy on sentence length across strike states, we use offender level data to examine the likelihood of receiving a maximum sentence of 25 years or more in high usage strike states (as defined by the prior literature) versus low usage strike states versus the control states.

Data for this section of the analysis was taken from the National Corrections Reporting Program administered by the Bureau of Justice Statistics since 1983. The National Corrections Reporting Program (NCRP) compiles data on admissions and releases from state prison, post-confinement community supervision and year-end prison custody records. Specifically, we use data on adult offenders sentenced to one or more years between 1989 and 2000 following conviction of a new offense (986,971 inmates from 42 states and the District of Colombia).[12]

[12]The period 1989 to 2000 spans four years before Washington states adopted three strikes law and four after Alaska joined in 1996.

We estimate a difference-in-differences model to compare changes in the probability of receiving maximum sentence of 25 years or more.[13] We estimate the following regression model:

$$Y_{ist} = \alpha + \beta_1 Used_{st} + \beta_2 Unused_{st} + X_{ist}\gamma + \lambda_t + \eta_s + \theta_s t + \epsilon_{ist} \tag{6}$$

where Y_{ist} is a dummy variable that takes a value of 1 if offender i (convicted in state s and admitted in year t) received a maximum sentence of 25 years to life and 0 otherwise. X_{ist} is a vector of control variables. We control for age at admission, number of prior offense, crime rates and race and ethnicity; with dummy variables for Black, White, Hispanic and 'Other'. We include year fixed effects (λ_t), state fixed effects (η_s), and state specific linear time trends ($\theta_s t$). $Used$ refers to states that were reported to have actually used their laws while *unused* refers to states that passed laws but never or rarely used them. We vary the definitions of *used* (top three states (California, Georgia and Florida), states with more than 100 convicted offenders and states where usage exceeded .01 percent of all admissions in 1998).[14]

The results are shown in Table III. Once controls are added, we find that offenders convicted in California, Georgia and Florida were 2.7 percentage point more likely to receive a maximum sentence of 25 years or more following adoption of three strikes law. Given that the overall probability of receiving a maximum sentencing of 25 years or more post-1994 was 8 percent, this reflects a 33.8 percent higher probability. The results are slightly smaller in magnitude if we expand the definition of *used*, as done in panel two and three of Table III. The coefficient for *unused* is not significantly positive, consistent with them not actually using the law in practice.

The ability of the difference-in-differences model to estimate a causal effect of three strikes and you're out policy on the probability of receiving a maximum sentence of 25 years or more depends on whether the control group forms a valid counterfactual. The estimated treatment coefficient, δ_1 is biased if in the absence of the treatment, the probability of receiving a maximum sentence of 25 years or more follows a different trend in treatment and control states. We use an event study to test whether the common-trend assumption is satisfied. Specifically, we estimate

[13] In our main results we look at convictions stemming from all types of crime; violent, property, drug, public order or other. The results are invariant to restricting the analysis to violent and property crime, which are the main strike triggers.

[14] The analysis should also include Virginia, but that is not one of the states reporting for the data set.

a lag-lead model of the treatment effect in high usage states (we run it with all three definitions of used) and data between 1989 and 2000. We use the following model specification:

$$Y_{ist} = \alpha + \sum_{k=-5}^{5} \gamma_k (S_{t+k}) + X_{ist} + \eta_s + \lambda_t + \epsilon_{ist} \tag{7}$$

where S_{t+k} is a series of dummy variables that capture the number of years before and after the three strikes law was implemented in high usage states. For example, S_{t+k} when $k = 0$ is set equal to one in the year a state first implements the three strikes law and $k = 5$ denotes at least five years after implementation. γ_k is an estimate (k years after the three strikes law was enacted) of the treatment effect relative to one year prior to implementation. X_{ist} is a vector of controls for age at admission, number of prior offense, race and ethnicity as well as state violent and property crime rates. We also control for unobserved state and year characteristics by including state and year fixed effects (η_s and λ_t, respectively).

Figure 1 presents the effects of three strikes and you're out policy adoption in high usage states. Table IV presents more detailed results. There is no apparent pattern in the probability of receiving 25 years or more prior to the implementation of three strikes. The lead coefficients are never significant, which provides support for the parallel trends assumption. The lag coefficients are larger in magnitude and significant. Hence the lag-lead specification provides no evidence that the probability of receiving 25 years or more in treatment and control states in the absence of the policy change differ significantly. Estimates in Columns 1 of Table IV suggest that the probability of receiving 25 years or more increased significantly one year post implementation in California, Georgia and Florida (first panel of table). The treatment effect remained significant and increased in magnitude up to six years after adoption and did not vary much when we expand the definition of high usage states (see panel two and three of Table IV).

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 so we drop k = -1 and k = -3. This suggestion is implemented in Column 2 of Table IV. We cannot reject the hypothesis that the coefficients on the pre-periods are statistically the same. Specifically, for the first panel (CA, GA, & FL) the F-test result is 0.6822, for the second panel (>100 uses) the result is 0.7151, and for the third panel (use is > .01 of admissions in 1998) the result is 0.7138. This provides further evidence that our results are not driven by significant pre-trends in sentence lengths in our high usage states.

As a robustness check, we examine changes in the distribution of sentence lengths assigned to offenders in treatment and control states. The data set includes information on six sentence lengths, namely: 1 to 1.9 years, 2 to 4.9 years, 5 to 9.9 years, 10 to 24.9 years, greater than or equal to 25 years and life in prison. We use the most conservative definition for high usage states and expect significant changes in sentence lengths across strike states. In California, for example, the adoption of enhanced sentencing often resulted in an offender receiving the minimum of three times the sentence otherwise provided for the current conviction, 25 years, or the sentence provided by law for the current charge plus any applicable sentence enhancements.[15] Results from our analysis are presented in Figure 2 and reveal that sentencing lengths of 10 years or more increased in high usage states following the adoption of three strikes law. These figures plot represent results from lag lead estimation similar to that reported in Figure 1.

We also investigated why there was a significant decrease for *StrikesOther* in the difference-in-differences framework. When we run the event study framework for this group, we find that the pre-trend assumption required for difference-in-differences is violated (not shown). Thus, the difference-in-differences estimate of a reduction in the sentence length should be viewed with skepticism.

5.1 Does the implementation of "three strikes and you're out" lead to changes in arrest rates?

Given the changes in sentence lengths, one might wonder if arrest rates are also changing during

[15] According to Austin et al. (2000), any person convicted for serious felonies is to receive a five-year sentence enhancement for each prior conviction.

this time period. We compare arrest rates across the treated and control states by estimating the following model:

$$
\begin{aligned}
Y_{ct} = &+\beta_1 Used_{ct} + \beta_2 * Blacks_{ct} + \beta_3 Used_{ct} * Black_{ct} + \beta_4 Unused_{ct} \\
&+ X_{ct}\gamma + \lambda_t + \eta_s + \theta_c t + \epsilon_{ct}
\end{aligned} \tag{8}
$$

where Y_{ct} is defined as log of adult arrest in county c at time t. $Used$ and $Unused$ are dummy variables, which allows the treatment effect to vary based on how much these states enforced the three strikes law. X_{ct} is a vector of socio-demographic characteristics. We expect that if the implementation of enhanced sentencing rendered the criminal justice system more punitive in general, then it may be reflected in higher arrest rates.

The FBI Uniform Crime Reporting (UCR) system collects data on crimes and arrests through reports from local law enforcement agencies. Since reporting patterns of the different agencies vary across time and counties, some have cautioned against the use of UCR data in longitudinal analyses (Maltz and Targonski, 2002). In 1994, NACJD modified the algorithm that imputes missing data when one or more agencies fail to report their records. As a result, arrest reports prior to 1994 are not comparable to reports for 1994 and on-wards. So we focus on the treatment effect in states that adopted three strikes in 1995 and 1996. All other treatment states were dropped from the analysis.

Socio-demographic indicators characterizing the population and labor market within each county are included and represent variables largely promoted by discussions of the link between neighborhood disadvantages, crime and incarceration (see for example Sharkey and Torrats-Espinosa (2017)). Changes in the severity of correction policies have had a larger impact on black communities because arrest rates have historically been greater for blacks than whites (Neal and Rick (2014)). As a result, we incorporate racial and ethnic dummy variables (Black, Hispanic, 'other' and non-Hispanic white).[16] The interaction term $Used_{ct} * Black_{ct}$ tests if the treatment

[16]Data on the racial distribution within county over time are obtained from the Census Bureau. Specially, we collect race data from the U.S. Intercensal County Population Data compiled by NBER. Data on poverty rates were obtained directly from the Census Bureau website, Table 21 of the Historical Poverty Tables: People and Families - 1959 to 2016.

effect differs in areas with a large proportion of Blacks. We also control for the proportion of families living below the poverty line.

Table V displays the regression results. In line with a priori expectations, areas with a larger proportion of Blacks also had higher arrest rates. The coefficients on the treatment dummies, $Used$ and $Unused$ are never significant at the conventional levels, and suggest that arrest rates in the treatment states were not statistically different from the rates in control states. This result suggest the treatment effect of enhanced sentence lengths is not being confounded with changes in arrest rates.

6 Does the implementation of 'three strikes and you're out' lead to changes in intergenerational mobility?

6.1 Data

To measure intergenerational mobility, we use data from the Equality of Opportunity Project website (Chetty et al. 2014), which includes a variety of intergenerational mobility measures. From the Equality of Opportunity Project, we employ mobility estimates recorded in the Commuting Zone (or County) and Birth Cohort data set. The authors measure intergenerational mobility through parent-child tax linkages. Chetty at al. (2014) use administrative tax records of more than 40 million children and their parents which covers approximately 95 percent of children born between 1980 and 1993 who are U.S. citizens as of 2013.

Parental income is defined as mean family income and measured when the child was between 15 and 19 years old. Chetty at al. (2014) state that "estimates of the rank-rank slope are insensitive to the age of parents and children at which parents income is measured, provided that parents are between 30 and 55."[17] Thus, using income measured when kids are between 15 and 19 should not greatly affect our results. 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.

[17]Chetty et al. (2014) provide more information on how their method is robust to various ways of measuring parental and child income.

We use the child's county (or commuting zone) of residence between ages 15 and 19 to proxy for where they grew up and to determine if they would be exposed to three strikes. In the commuting zone data there is an observation for each birth cohort by commuting zone for children born between 1980 and 1986. All commuting zones available in the data set are included, except those in Texas since there was always a policy in place.[18] We use the number of children, by commuting zone and birth cohort to weight the analysis.

The commuting zone data provides information on the relative mobility which measures the correlation between parental income rank and child income rank. The rich data available at the commuting zone level allows us to examine the mean outcomes of children who grew up in low-income families, both to parents at the very bottom of the income distribution (absolute mobility) and those raised by parents at the 25th percentile of the income distribution (absolute upward mobility).[19] If there is worsening income mobility for children raised in the treatment states, we expect a positive coefficient for the relative mobility measure, which would indicate a stronger relationship between a parent's rank and their child's rank. For the measures of absolute mobility and absolute upward mobility, a negative number would indicate that those at the bottom of the income distribution are more negatively affected by the three strikes policy. The summary statistics for the intergenerational mobility measures can be found in Table II.

At the county level, an observation is a county by birth cohort. We have data on the expected income rank for children born between 1980 and 1986. Observations in Texas are again dropped due to Texas always having the policy in place.[20] County level measures of intergenerational mobility reflect the expected income rank at age 26 for children born to parents at the 25th and 75th percentile of the income distribution.

This data allows us to test whether those more likely affected by the criminal justice policy (those born to parents at the 25th percentile) are more negatively affected than those who were

[18] Helland and Tabarrok (2007) state that Texas has been wrongly categorized as a control state in previous work since it had a three strikes law from the 1950's.

[19] 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.

[20] 18 observations are also dropped due to counties not existing throughout 1990 to 2000.

less likely to be directly affected by these policies (those who started at the 75th percentile). Unfortunately, county level data include population measures in 1990 and 2000 instead of the number of children. So, to create weights for this part of the analysis, we merge in data on the number of births in each county and birth cohort from Vital Statistics. For these measures of mobility, a negative coefficient would suggest that children are negatively impacted by the three strikes policy.

6.2 Research Design

We test whether children who grew up in states that implemented three strikes laws had changes in their intergenerational mobility. We hypothesize that there are larger effects in states that actually used their laws, as opposed to states that passed laws but did not implement them in practice. Thus, we create two treatment variables as before: *used* which is a dummy variable for the state actually using their three strike law and *unused*, which is a dummy variable for the state passing a three strikes law but not using it in practice.

Due to the birth cohorts available in the data, we can not compare kids born before and after the policy implication. However, we can compare kids that were younger and older when the policy went into place. While we would like to compare very young kids (younger than eight) to older kids, that is not possible given the data.

We also hypothesize that the effects vary by age, as discussed more in the conceptual framework. To test for this, we use different age cutoffs to determine older and younger kids at implementation. Thus, our difference-in-differences framework compares the difference in intergenerational mobility between younger and older kids across treatment and control states. For intergenerational mobility outcomes (Y_{cb}), we estimated the following model:

$$Y_{cb} = \alpha_1 * exposure_{cb} * Used_c + \alpha_2 * exposure_{cb} * Unused_c + \lambda_b + \eta_c + \theta_c b + \epsilon_{cb} \tag{9}$$

where c is the commuting zone or county and b is birth cohort. Since we have data at the commuting zone or county, we use commuting zone or county fixed effects (η_c) and commuting zone or county specific linear time trends ($\theta_c b$). λ_b is a birth cohort fixed effect that is constant across locations. We define *exposure* as a dummy variable for being at or below the age cutoff when the law went into effect. $Used$ is a dummy variable that equals one if county or commuting zone c is located in a state that has a three strikes law used in practice, while $Unused$ is a dummy variable that equals one if the county or commuting zone is located in a state that has a three strikes law that is unused (or rarely used). We use different cutoffs to define high usage to show how robust the results are to our definition.

The standard errors are 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 geographical areas 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.

6.3 Results

The results from our most restrictive definition of $Used$ are outlined in panel one of Table VI. In the other two panels we use less restrictive definitions of $Used$ to demonstrate the robustness of our results. First, our results are consistent with our hypothesis that high use states saw more of an effect than states that passed three strikes laws but did not use them in practice. The coefficients on $Unused$ are never significantly different from zero, indicating that passing the law alone did not impact intergenerational mobility. In order for enhanced sentencing to result in longer separations of children and parents, it needs to actually be used.

We see evidence of worsening intergenerational income mobility for kids who are age eight to ten years in high use states regardless of our definition of $Used$. For the age eleven cutoff, there is a significant decrease in the slope indicating a reduction in the relationship between parental income rank and child income rank (better intergenerational mobility) in high use states. Consistent with our hypothesis that usage matters, the coefficients in the $Used$ states becomes smaller in magnitude as we use less conservative definitions of a high usage state.

Specifically, for kids who are eight or nine (nine cutoff), our absolute mobility coefficient means that kids starting at the very bottom of the income distribution experience a reduction of 1.47

percentile points in the their income rank at age 26. For kids eight or nine starting at the 25th percentile, we find a reduction of .99 percentile points in the expected income rank at age 26. Given that the average income rank of children 9 or younger in the control states corresponds to annual income of $31,000 (the 46 percentile in the child family income distribution), the .99 percentile rank decline reflects annual loss of approximately $900 for children born to parents at the 25th percentile of the income distribution.[21] For a back of the envelope calculation, the mean value for the slope in California, Georgia, and Florida was .273 during this time period, meaning that as a parent's income rank increases by ten points, the kid's rank increases by 2.73 percentile points. Increasing the relative mobility by .0195 means the slope becomes .273+.0195=.2925. This means the same ten point increase in parental rank now leads to a 2.925 percentile point increase for children.

The results, while smaller, indicate worse income mobility for children ages eight to ten years (ten cutoff) when the policy went into place in high usage states. For those raised at the very bottom, the expected income rank at age 26 is .57 percentile points lower in high usage states. For those raised at the 25th percentile of parent income distribution, there is a decrease in their income rank at age 26 of .35 percentile points. These results show that those who are disadvantaged to begin with are negatively impacted by the policy, which is consistent with Dobbie et al. (2018).

At the age eleven cutoff there is flattening of the slope, which indicates better mobility, but the measures of absolute mobility and absolute upward mobility are insignificant. These results are again limited to the states with high usage of the their strikes policy ($Used = 1$). Arteaga (2018) finds that there is a U-shaped pattern in the effects on educational attainment by the child's age at parental incarceration. Specifically, she finds better educational outcomes from kids 10-15 than for kids 5-10. The fact that we find a change in effects also around the age ten cutoff suggests her results are not limited to educational attainment for Colombian children. This also consistent with Johnston (1995) which compares outcomes by child ages and states that having a parent incarcerated during the middle childhood years (ages seven to ten) is associated with some

[21] This estimate was derived from the national income estimates corresponding to centile reported by Chetty et. al, 2014

of the worst outcomes. Specifically, anxiety and aggression start around this time.[22]

We also use the county level measures of income mobility measured at the 25th and 75th percentile of the parental income distribution to test for differences across the parental income distribution. Given that those who were affected by the laws tended to be low income, we expect worse outcomes for kids starting at the 25th percentile.[23]

Table VII shows the impact on income rank in the national income distribution at age 26 using county level data. Similar to our results in Table VI we find negative coefficients on absolute upward mobility for children ages eight to ten years at the time of implementation in high usage states. This effect is largely in line with estimates from the commuting zone by birth cohort data. Younger kids who start out at the 25th percentile have an expected income rank at age 26 that is 1.05 percentile points lower than older kids. The negative effects gets smaller when we examine children eight to ten years (ten cutoff) and becomes positive when younger includes eleven year olds. This switching of signs across the age ten cutoff is consistent with our results in the commuting zone data and the shifting in Arteaga (2018) between middle-age kids (five to ten) and older kids (eleven to fifteen). The coefficients on the 25th percentile again becomes smaller in magnitude as we use a less restrictive definition of high usage states, which is consistent with our findings in the commuting zone data. Again, the significant results in the high usage states are consistent with our hypothesis that states must actually use their three strikes law to impact intergenerational mobility.

Children from more affluent backgrounds, those raised at the 75th percentile, have smaller effect sizes than those from more disadvantaged backgrounds. This is consistent with the criminal justice research showing enhanced sentencing policies disproportionately impact low-income individuals.

Overall, we find negative effects on intergenerational income mobility for kids ages eight to ten

[22]The related literature also suggests the benefits to older kids may be due to role-model effects. If the parent is a negative role-model, then separating the children from the parent would lead to better outcomes for the child. This would be consistent with Bhuller, Dahl, Løken, and Mogstad (2018) which found brothers of the incarcerated were less likely to be charged with a crime.

[23]Location matters more for children growing up in low income families: the expected rank of children from low-income families varies more across geographical areas than the expected rank of children from high income families (Chetty et al., 2014).

years at implementation in high usage states. When we include kids age eleven at implementation in the definition of young, the estimated treatment effect changes sign and indicates that young children (ages 8-11 years) benefit from enhanced sentencing in high usage states. The change between ten and eleven year old kids is consistent with variation by age found in Arteaga (2018). Our results are quite robust to the definition of high usage states and as we incorporate more states that used the policy less often, our results become smaller in magnitude.

6.4 Robustness Checks

To further provide evidence that our reported estimates are causal, we run placebo or refutability tests similar to Gavrilova et al. (2017) and Abadie et al. (2010). To conduct these tests we randomly assign treatment to the twenty-five control states, shifting the actual treatment states to the donor pool and re-estimate Equation 9 using data at commuting zone level.

Specifically, we randomly select states from the initial control group (non-strike states) to represent those that enforced the law and those that rarely used three strikes. We focus on the least restrictive definition of "used", so we assign at random nine states to represent areas where three strikes law was enforced. The other 16 states represent treatment states that rarely used these laws. The actual treatment states are considered the control group. Since we are concerned about the income mobility of young relative to older children, the treatment is triggered based on age at implementation (in 1993, 1994 and 1995), coinciding with the actual treatment dates.[24]

We perform 1600 replications and rank the actual treatment effects of three strikes among the effects from the placebo regression models. If the estimated effects outlined in Table VI are driven by strong heterogeneity in trends, the placebo estimates will often find an effect similar in magnitude and our actual treatment effect ($\hat{\alpha_1}$). This means our treatment effect will be in the thick of the distribution of the placebo estimates. However, if our estimates are capturing actual treatment effects, the coefficients will be in the far left or right tail (depending on whether the estimate was positive or negative) of the distribution of the placebo-coefficients, indicating that the actual treatment effect is significantly different from the placebo estimates.

[24] We also used the entire sample as a donor pool and randomly assigned treatment to 25 states. The results were qualitatively the same. Thus, to ensure that the actual treatment effect was not driving results from the placebo tests we restrict the donor pool to the 25 control states and present those results here.

Figures 3, 4, and 5 illustrate results from the placebo tests and provide supporting evidence that children ages 8 and 9 at implementation of enhanced sentenced experienced worse intergenerational income mobility. When we include older children in our definition of young, we consistently find no significant difference between the placebo and actual estimate. This suggests that enhanced sentencing has no effect on older kids which is consistent with the prediction from Equation 5 of our conceptual framework. Given that both the enhanced sentencing regime and the previous sentencing regime would remove the incarcerated parent from the rest of childhood, we did not expect there to be any effect for the oldest age group. Since we still find negative impacts on children in the middle age group (eight to nine year old children specifically) this would suggest that for these kids $\phi_{e,3}\delta_e e - \phi_{f,3}\delta_f f < 0$. This means the cost from the reduced parental investment exceeds the benefits from being in a different environment after the parent is incarcerated.

7 Does the implementation of 'three strikes and you're out' lead to changes in the college attendance gradient?

7.1 Data

We also measure intergenerational mobility by how parental income relates to a child's college attendance (college attendance gradient) with data from the Equality of Opportunity Project website (Chetty et al. (2014)). We again use the commuting zone and county level data sets. College attendance in this data is determined by the 1098-T form. Since the form is filed directly by colleges, there is no concern about individuals inaccurately reporting their college going behavior.

In the commuting zone data set, the intergenerational educational mobility measure is defined as the college attendance rate at age 19. The birth cohorts available are not identical to those used in the income mobility data. Specifically, we have college attendance at age 19 for birth cohorts 1984 to 1993. However, a benefit to having more birth cohorts is that we can look at the impact on college attendance for kids even younger than age eight.

As with the income mobility data, county level data provides measures of college attendance for children raised in families at the 25th and 75th percentile. We again use the college attendance variable that is based on if the individual enrolled in college at age 19 to better compare results across geographic units of observation. Data is available for children born between 1980 to 1988.

Since Georgia had the Georgia HOPE policy change during our measured years of college attendance, we drop observations in Georgia after the change in 2007 to focus on a time when the policy environment was the same. Summary statistics for the intergenerational impacts on college enrollment are reported in Table II. Over time there was an improvement in the college enrollment rate across treatment and control states so we control for this with linear time trends in our regressions.

7.2 Empirical Methods

As discussed in the conceptual framework, we hypothesize a mechanism for the worsening income mobility is through reduced human capital accumulation. To test for evidence of this, we investigate whether kids in high usage states had worse college attendance at age 19. Specifically, we estimate the following model:

$$Y_{cb} = \alpha_1 * exposure_{cb} * Used_c + \alpha_2 * exposure_{cb} * Unused_c + \lambda_b + \eta_c + \theta_c b + \epsilon_{cb} \tag{10}$$

where Y_{cb} is college attendance at age 19, c is the commuting zone or county, b is their birth cohort and *exposure* is a dummy variable equal to one if the individual was exposed to a three strikes law by the age cutoff. We again include birth cohort fixed effects (λ_b), commuting zone or county fixed effects (η_c) and commuting zone or county specific linear time trends ($\theta_c b$). We again test whether the effects vary across how enforced the three strikes law was with our $Used$ and $Unused$ variables. To test how sensitive our results are to the definition of $Used$, we again use multiple definitions. To test our hypothesis of differences in impacts by age, we estimate the effect at a variety of age cutoffs. We use the commuting zone weights for number of children and the Vital Statistics weights for the county level data. We cluster the standard errors by state.

7.3 Results

Results are shown in Table VIII for the commuting zone data. Due to the birth cohorts not being the same as the income mobility data, we do not have old enough cohorts for the eleven cutoff. However, we do have younger birth cohorts so we can look at education patterns for even smaller cutoffs through to the age ten cutoff. Again, each panel of the table shows the results using a different definition of a high usage state (top three versus all states with at least 100 uses versus states with usage exceeding .01 percent of all admissions in 1998).

Across the definitions of high usage, the impact on kids age seven or younger is always insignificant. At age eight we start seeing evidence of worse college attendance as measured by the absolute mobility (how likely a kid starting at the very bottom of the parental income distribution attends college) and the absolute upward mobility (how likely a kid starting at the 25th percentile of the parental income distribution attends college) in high usage states. The results for these two measures are larger in magnitude for those age eight or nine when the law went into effect. At age ten the results indicate a flattening of the relationship between parental income rank and child's college attendance, but also shows a decrease in the absolute upward mobility. These results indicate that the age of a child impacts whether children are better or worse off.

Our results from the county level data are in Table IX. The county level data has more birth cohorts so we are again able to examine the impact at different age cutoffs. For the age eight cutoff, the coefficient on absolute upward mobility is again negative as in the commuting zone data, although the standard error is too large for it to be significant. The coefficient for the 75th percentile is positive, but insignificant.

The age nine absolute upward mobility measure is negative and significant similar to the commuting zone coefficient. The 75th percentile is also negative and significant, consistent with the reduced intercept and unchanged slope in the commuting zone data (which indicates the line shifted downward). For age ten we again find worse absolute upward mobility and worse mobility for those starting at the 75th percentile. Using the age eleven or younger cutoff, we find the significant negatives have disappeared, again consistent with a change impact around age ten similar to Arteaga (2018). This is also consistent with our commuting zone results.

Similar to our previous results, we still find evidence that usage matters. As we use less restrictive definitions of high usage (moving from top panel to bottom panel) the significant coefficients at the nine and ten cutoff become smaller in magnitude. This is again consistent with states needing to actually use the laws for them to have any impact.

Overall, our results are consistent with age at implementation mattering. For those eight to ten years at implementation of three strikes, we tend to find negative impacts on the college attendance gradient (although not all are significant). Putting these results together with our income mobility results suggest that children ages eight to nine years are the ones most negatively impacted by three strikes laws.

8 Robustness Checks

8.1 Varying Treatment Effects

It is possible that the length of exposure matters in addition to whether or not a child was exposed to three strikes. To test this hypothesis, we compare the income rank of young children and older children in the treatment and control states. In these checks we go a step further and compare differences in income rank based on the number of years of exposure to enhanced sentencing while young. The number of years of exposure varies with or definition of young. For example, a child born in 1986 and raised in California is exposed to the 1994 three strikes laws for approximately two years before age eight, approximately three years before age nine, approximately four years by age ten and approximately five years by age eleven.

Results from this assessment (not shown here but available upon request) indicate that children exposed to three years of enhance sentencing due to three strikes law, while young, are more negatively affected. In terms of birth cohorts, this results was largely associated with children born in 1984 and 1985. The less significant effects for those exposed for four or five years by the age cutoff is consistent with the idea discussed in our conceptual framework that exposure to better environment while young may have a larger positive effect. Hence, the negative effect of being separated from parents for 4 or 5 years while young may be offset by exposure to better home environment.

8.2 American Community Survey Data

To further assess the robustness of our results, we use ACS data from 2006 to 2012 to estimate treatment effects using individual level data. In order to better compare our results, we focus on adult earnings and college attendance among children born between 1980 and 1986 (ages 26 to 32 when we look at outcome variables). We use the state of birth question in the ACS to determines if the individual was in one of the $Used$, $Unused$ or control states.

We use a difference-in-differences approach with varying age cutoffs and all three definitions of high usage ($Used$). Like our baseline analysis we compare the difference in outcome variables between younger and older children raised in three strikes and no-strikes states. Standard errors are again clustered at the state level. Specifically, we estimate the following model:

$$Y_{sb} = \alpha_1 * exposure_{sb} * Used_s + \alpha_2 * exposure_{sb} * Unused_s + X\alpha_3 + \lambda_b + \eta_s + \theta_s b + \epsilon_{sb} \tag{11}$$

where Y_{sb} is either household income or college attendance, λ_b is a birth cohort fixed effect, η_s is a state fixed effect and $\theta_s b$ is a state specific linear time trend. X is a vector of individual characteristics such as race, gender, and marital status.

Table X reveals that household income was significantly lower for children born in high usage states who were exposed to enhanced sentencing before age 9 relative to young children born in the control states.[25] Specifically, exposure to three strikes between eight and nine years results in a 3.9 percent lower household income at age 26, relative to the income of similar children born in non-strike states at 26. Children ages eight or nine at implementation were also less likely to attend college as shown in Table XI. This supports earlier claims that children exposed to enhanced sentencing by age nine are negatively affected.

[25] Household income in defined by the U.S. Census Bureau as the total monetary income of all household members age 15 or older during the previous year.

9 Conclusion

Previous and concurrent papers on the spillover effects of incarceration have examined the impact on the extensive margin (parents who are incarcerated versus those who are not), while this paper examines the impact of the intensive margin (sentence length). Thus, this paper provides complementary results to the growing literature on the effects of parental incarceration. We use two different data sets and multiple measures of mobility from Chetty et al. (2014) to show a consistent pattern of worsening income mobility for those age eight to nine years at implementation. We provide additional evidence of an effect by showing it also exists using individual level data from the ACS. This significant effect on kids who are nine is consistent with Gershenson and Tekin (2018), which finds negative impacts on math and reading scores for 3rd graders as a result of the 'beltway sniper'. We find negative effects specifically for more disadvantaged kids (absolute and absolute upward mobility measures) which is consistent with the findings in Dobbie et al. (2018).

We provide evidence that one mechanism behind the reduced intergenerational income mobility is through reduced human capital accumulation. We estimate the impact on how college attendance of children is impacted by parent's income rank with two data sets and multiple measures from Chetty et al. (2014). We find evidence of worse educational mobility for kids ages eight to ten years. As a robustness check, we show that using individual level data from the ACS also results in finding worse college attendance for these kids. These results are complementary to Norris et al. (2018) which finds parental incarceration decreases the probability of graduating from high school. We are finding that the sentence length matters and it is not just whether or not parents are incarcerated at all.

Our results are important for the current debate over the impact of sentencing lengths. Opponents argue that these policies have disproportionately hurt poor minority communities, and are not cost effective. Proponents argue it is a matter of public safety. When Eric Holder was Attorney General the policy was to reduce sentence lengths for non-violent offenders. However, Attorney General Sessions was against this policy. Representative Doug Collins sponsored the

First Step Act (H.R.5682) which passed the House of Representatives in spring of 2018, but it did not become law.

Since Attorney General Session's resignation, there have been more serious talks about criminal justice reform, including reforming the federal three strikes policy. Kim and Dawsey reported in the Washington Post in November of 2018 that a new policy drafted by the Senate would impact the three strikes law by reducing the sentence from life behind bars to 25 years and give judges some additional discretion. However, Cohen and Yang (forthcoming) find Republican-appointed judges give longer sentences to black defendants and this difference is larger when judges have more discretion. This paper shows that sentence lengths not only impact those incarcerated, but that there are spillover effects as well. Given that our results show the sentence length matters for kids, this finding should be taken into considering when drafting new criminal justice legislation.

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Table I: 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 (.0008%)
California (1994)	Two	Any Crime	42,322 (.3%)
Colorado (1994)	Three	Violent Crime	4 (.0006%)
Connecticut (1994)	Three	Violent Crime	1 (.0006%)
Florida (1995)	Three	Violent Crime	1,628 (.06%)
Georgia (1995)	Two	Violent Crime	7,631 (.5%)
Indiana (1994)	Three	Violent Crime	38 (.004%)
Kansas (1994)	Three	Violent Crime	N/A
Louisiana (1994)	Three	Violent Crime	N/A
Maryland (1994)	Four	Violent Crime	330 (.03%)
Montana (1995)	Two	Violent Crime	0
Nevada (1995)	Three	Violent Crime	304 (.06%)
New Jersey (1995)	Three	Violent Crime	10 (.0006%)
New Mexico (1994)	Three	Violent Crime	0
North Carolina (1994)	Three	Violent Crime	22 (.002%)
North Dakota (1995)	Two	Violent Crime	10 (.01%)
Pennsylvania (1995)	Two	Violent Crime	50 (.0005%)
South Carolina (1995)	Two	Violent Crime	14 (.002%)
Tennessee (1994)	Two	Violent Crime	14 (.002%)
Utah (1995)	Three	Violent Crime	N/A
Vermont (1995)	Three	Violent Crime	16 (.02%)
Virginia (1994)	Three	Violent Crime	328 (.03%)
Washington (1993)	Three	Violent Crime	209 (.03%)
Wisconsin (1994)	Three	Violent Crime	9 (.001%)

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. In paranthesis we represent the number of inmates sentenced under three strikes as a proportion of total admissions in 1998.

Table II: Summary Statistics

Variable	*CA, GA, & FL*		*Other Treated*		*Control*	
	Before Adoption	*After Adoption*	*Before Adoption*	*After Adoption*	*Before 1994*	*After 1994*
Max sentence 25 yrs or more	.047	.035	.219	.058	.084	.059
	(.213)	(.184)	(.414)	(.234)	(.278)	(.237)
Male	.916	.896	.945	.919	.932	.907
	(.277)	(.305)	(.227)	(.273)	(.251)	(.290)
Age 18-24	.320	.274	.377	.339	.405	.363
	(.467)	(.446)	(.485)	(.473)	(.491)	(.481)
Age 25-34	.415	.370	.385	.350	.380	.346
	(.493)	(.483)	(.487)	(.477)	(.485)	(.476)
Age 35-44	.197	.262	.172	.230	.162	.218
	(.398)	(.440)	(.378)	(.421)	(.369)	(.413)
Age 45-54	.051	.0758	.048	.065	.039	.058
	(.219)	(.267)	(.213)	(.246)	(.194)	(.233)
Age 55 or older	.017	.018	.017	.017	.013	.014
	(.128)	(.134)	(.131)	(.129)	(.114)	(.118)
White	.307	.323	.362	.356	.049	.201
	(.461)	(.468)	(.480)	(.479)	(.217)	(.401)
Black	.366	.308	.248	.354	.130	.432
	(.482)	(.462)	(.432)	(.478)	(.334)	(.495)
Other Race	.062	.076	.268	.205	.736	.212
	(.241)	(.266)	(.443)	(.404)	(.441)	(.409)
Hispanic	.266	.294	.122	.085	.084	.155
	(.442)	(.455)	(.328)	(.279)	(.277)	(.362)
Violent Crime	.338	.301	.515	.399	.351	.277
	(.473)	(.459)	(.50)	(.490)	(.477)	(.447)
Property Crime	.259	.264	.199	.219	.309	.257
	(.438)	(.441)	(.399)	(.414)	(.462)	(.437)
Other Crime	.403	.435	.286	.382	.340	.466
	(.491)	(.496)	(.452)	(.486)	(.474)	(.499)
Arrest Rates						
Intergenerational Mobility	*9 or younger*	*11 or younger*	*9 or younger*	*11 or younger*	*9 or younger*	*11 or younger*
Income at 26 Slope	.253	.254	.277	.277	.277	.277
	(.066)	(.066)	(.069)	(.067)	(.066)	(.068)
Income at 26 Intercept	.353	.356	.386	.385	.396	.395
	(.039)	(.038)	(.057)	(.058)	(.061)	(.063)
Income Rank 26: 25th pctl	.431	.433	.457	.457	.465	.463
	(.034)	(.033)	(.054)	(.053)	(.050)	(.051)
Income Rank 26: 75th pctl	.545	.548	.597	.596	.599	.596
	(.029)	(.028)	(.039)	(.038)	(.039)	(.038)
College at 19 Slope	.253	.260	.277	.276	.277	.277
	(.066)	(.064)	(.069)	(.068)	(.066)	(.066)
College at 19 Intercept	.353	.351	.386	.387	.396	.395
	(.039)	(.037)	(.057)	(.059)	(.061)	(.0617)
College at 19: 25th pctil	.335	.332	.347	.342	.356	.346
	(.106)	(.104)	(.089)	(.090)	(.098)	(.101)
College at 19: 75th pctil	.676	.675	.712	.711	.722	.706
	(.088)	(.089)	(.080)	(.078)	(.080)	(.092)

The crime data represent averages over the sample period 1989 to 2000. Statistics have been computed using state population weights. For the intergenerational mobility measures the non-treated states are given a year of implementation of 1994 to get the variables by age cutoff. Numbers in parentheses represent standard deviations. Since Texas always had a three strikes policy, observations in Texas are dropped from the regressions. Sources: National Corrections Reporting Program, 1991-2015, Dataset 001: Term Records and the Equality of Opportunity Project website (Chetty et al. 2014).

Table III: Impact of Three Strikes Law on the Probability of Receiving Maximum Sentence of 25 year or more

	Top Three Used States: CA, GA, FL	
Variable	Column 1	Column 2
KeyStrikes	.0175 (.0146)	.0305*** (.0104)
OtherStrikes	-.0317 (.0459)	-.0318 (.0426)
Controls		X
state fixed effects	X	X
year fixed effects	X	X
state specific linear time trend	X	X
	States with Over 100 uses: CA, GA, FL, MD, NV, WA	
Variable	Column 1	Column 2
KeyStrikes	.017 (.0145)	.0294*** (.0103)
OtherStrikes	-.1049*** (.0382)	-.1002** (.0403)
Controls		X
state fixed effects	X	X
year fixed effects	X	X
state specific linear time trend	X	X
	State's three strike use is $>$ .01 *of admissions in 1998*	
Variable	Column 1	Column 2
KeyStrikes	.017 (.0145)	.0294*** (.0103)
OtherStrikes	-.1049*** (.0382)	-.1002** (.0403)
Controls		X
state fixed effects	X	X
year fixed effects	X	X
state specific linear time trend	X	X
Observations	1,620,448	1,620,448

Dependent variable is binary, and equals one if the offender receives maximum sentence of 25 years or more, and zero otherwise. 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. All estimates include controls for age at admission, number of prior offense, race and ethnicity as well as state violent and property crime rates. All models include state population weights. Since Texas always had a three strikes policy, observations in Texas are dropped from the regressions. Source: National Corrections Reporting Program, 1991-2015, Dataset 001: Term Records.

Table IV: Comparison of State Three Strikes Laws

Treatment	*CA, GA & FL*	
	Column 1	Column 2
S_{t-5}	.021 (.065)	.030 (.057)
S_{t-4}	-.003 (.037)	.008 (.026)
S_{t-3}	-.018 (.022)	omitted
S_{t-2}	-.011 (.012)	-.003 (.004)
S_{t}	.014 (.010)	.020 (.018)
S_{t+1}	.043*** (.012)	.051** (.019)
S_{t+2}	.063*** (.017)	.071*** (.023)
S_{t+3}	.074*** (.020)	.082*** (.003)
S_{t+5}	.083*** (.024)	.091*** (.030)
S_{t+5}	.092*** (.029)	.100*** (.034)
	States with Over 100 uses: CA, GA, FL, MD, NV, WA	
S_{t-5}	.0202 (.069)	.031 (.061)
S_{t-4}	-.006 (.038)	.007 (.026)
S_{t-3}	-.020 (.023)	omitted
S_{t-2}	-.012 (.012)	-.0037 (.004)
S_{t}	.015 (.011)	.0227 (.018)
S_{t+1}	.046*** (.013)	.054*** (.021)
S_{t+2}	.064*** (.018)	.072*** (.025)
S_{t+3}	.073*** (.022)	.082*** (.028)
S_{t+5}	.083*** (.027)	.091*** (.033)
S_{t+5}	.091** (.034)	.100** (.039)
	State's three strike use is > .01 *of admissions in 1998*	
S_{t-5}	.020 (.069)	.031 (.061)
S_{t-4}	-.006 (.038)	.007 (.026)
S_{t-3}	-.020 (.023)	omitted
S_{t-2}	-.012 (.012)	-.012 (.012)
S_{t}	.015 (.011)	.015 (.011)
S_{t+1}	.046** (.013)	.046** (.013)
S_{t+2}	.064*** (.018)	.064*** (.018)
S_{t+3}	.073*** (.022)	.073*** (.022)
S_{t+5}	.082*** (.027)	.082*** (.027)
S_{t+5}	.091** (.034)	.091** (.034)
Number of Obs	1,629,662	1,629,662

Dependent variable is binary, and equals one if the offender receives maximum sentence of 25 years or more, and zero otherwise. 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. All estimates also include controls and state population weights. Column 2 applies the Borusyak and Jaravel (2016) test which means dropping two pre-treatment time periods and running a F-test on the remaining coefficients in the pre-trend time period. For the top section (CA, GA, & FL) the F-test result is 0.6822, for the middle section (>100 uses) the result is 0.7151, and for the last section (use is > .01 of admissions in 1998) the result is 0.7138. These results indicate no evidence of pre-trends.

Table V: Impact of Three Strikes Law Implementation on State Arrest

	State's three strike use is > .01 *of admissions in 1998*	
Variable	Column 1	Column 2
$Used$	-.136 (.111)	-.057(.125)
$Unused$	.070 (.072)	.072 (.072)
Black	.122***(.014)	.12***(.014)
$Used * Black$		.035* (.02)
$povertyrate$	.194***(.058)	.192*** (.058)
Hispanic	.054** (.021)	.053** (.021)
Other race	.026 (.020)	.027 (.020)
county fixed effects	X	X
year fixed effects	X	X
county specific linear time trend	X	X
Observations	11,972	11,972

Each column reflects a separate regression model. The outcome variable in Columns 1 and 2 is the log of adult arrest rate by county from 1994 to 1999 for 2,826 counties. $Used$ is a binary variable that take a value of one for high usage states and zero otherwise. Due to data collection changes in 1994, it is advised to not compare post-1994 data to pre-1994 data, so we drop states that implemented the three strikes law in 1994 or earlier. Consequently, high usage states for this analysis included Georgia, Florida, North Dakota, Nevada and Vermont. The variable $Unused$ is binary, is set equal to one for all other strike states and zero otherwise. The models incorporate data for adult arrest rates associated with both violent (murder, rape, robbery, and assault) and property crimes (burglary, larceny, motor vehicle theft, and arson). White, non-Hispanic is the omitted race/ethnicity category. 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. All models include county population weights. Since Texas always had a three strikes policy, observations from Texas are excluded from all models.

Table VI: Impact of Three Strikes Exposure on Commuting Zone Measures of Income Mobility

	Top Three Used States: CA, GA, FL			
Variable	Age 8	Age 9	Age 10	Age 11
Relative Mobility (*Used*)	.0025 (.0350)	.0195*** (.0068)	.0086*** (.0015)	-.0098*** (.0025)
Relative Mobility (*Unused*)	-.0050 (.0054)	.0011 (.0023)	.0001 (.0020)	.0019 (.0028)
Absolute Mobility (*Used*)	-.0001 (.0032)	-.0147** (.0063)	-.0057*** (.0016)	.0060 (.0036)
Absolute Mobility (*Unused*)	.0039 (.0042)	-.0024 (.0030)	-.0023 (.0024)	-.0010 (.0024)
Absolute Upward Mobility (*Used*)	.0005 (.0025)	-.0099** (.0047)	-.0035** (.0015)	.0036 (.0035)
Absolute Upward Mobility (*Unused*)	.0027 (.0032)	-.0023 (.0025)	-.0023 (.0020)	-.0005 (.0018)
	States with Over 100 uses: CA, GA, FL, MD, NV, VA, WA			
Variable	Age 8	Age 9	Age 10	Age 11
Relative Mobility (*Used*)	.0020 (.0041)	.0148*** (.0054)	.0067*** (.0020)	-.0063* (.0034)
Relative Mobility (*Unused*)	-.0071 (.0058)	.0004 (.0026)	-.0001 (.0023)	.0017 (.0030)
Absolute Mobility (*Used*)	-.0001 (.0036)	-.0118** (.0050)	-.0049*** (.0016)	.0044* (.0026)
Absolute Mobility (*Unused*)	.0054 (.0042)	-.0023 (.0033)	-.0022 (.0028)	-.0013 (.0027)
Absolute Upward Mobility (*Used*)	.0004 (.0027)	-.0081** (.0038)	-.0032*** (.0014)	.0029 (.0025)
Absolute Upward Mobility (*Unused*)	.0036 (.0033)	-.0022 (.0029)	-.0022 (.0024)	-.0009 (.0021)
	State's three strike use is $>$ .01 *of admissions in 1998*			
Variable	Age 8	Age 9	Age 10	Age 11
Relative Mobility (*Used*)	.0020 (.0041)	.0143*** (.0053)	.0063*** (.0021)	-.0061* (.0034)
Relative Mobility (*Unused*)	-.0071 (.0058)	.0006 (.0026)	.0001 (.0024)	.0016 (.0030)
Absolute Mobility (*Used*)	-.0001 (.0036)	-.0115** (.0049)	-.0047*** (.0016)	.0042 (.0025)
Absolute Mobility (*Unused*)	.0054 (.0042)	-.0024 (.0034)	-.0024 (.0028)	-.0012 (.0028)
Absolute Upward Mobility (*Used*)	.0004 (.0027)	-.0079** (.0037)	-.0031** (.0014)	.0027 (.0024)
Absolute Upward Mobility (*Unused*)	.0036 (.0033)	-.0023 (.0029)	-.0023 (.0024)	-.0008 (.0021)
Observations	4,097	4,097	4,097	4,097

All regressions control for commuting zone fixed effects, birth cohort fixed effects and commuting zone specific linear time trends. All regressions include 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 (positive number indicates parental rank matters more for kids outcomes). Absolute mobility captures the mobility for those at the very bottom of the income distribution (negative number indicates worse mobility). Absolute upward mobility measures a child's expected rank in the national income distribution at age 26, conditioning on being born to parents that were at the 25th percentile of their national income distribution (negative number indicates worse mobility). The coefficient for each measure is based on a separate regression. State's three strike use is $>$ .01 of admissions in 1998 corresponds to CA, GA, FL, MD, NV, VA, WA, ND, and VT. Standard errors are clustered by state. ***, ** and * indicate that the estimates are statistically significant at the 1%, 5% and 10% levels. Since Texas always had a three strikes policy, observations in Texas are dropped from the regressions. *Used* is a dummy variable based on whether the state actually used it's policy and varies by section of the table.

Table VII: Impact of Three Strikes Exposure on County Level Measures of Income Mobility

	Top Three Used States: CA, GA, FL			
Variable	Age 8	Age 9	Age 10	Age 11
Absolute Upward Mobility (*Used*)	.0013 (.0022)	-.0105* (.0056)	-.0022 (.0037)	.0047 (.0047)
Absolute Upward Mobility (*Unused*)	.0008 (.0034)	-.0039 (.0027)	-.0016 (.0022)	.0008 (.0020)
75th percentile (*Used*)	.0014 (.0014)	-.0012 (.0012)	.0018 (.0025)	.0006 (.0051)
75th percentile (*Unused*)	-.0008 (.0026)	-.0039* (.0021)	-.0016 (.0019)	.0015 (.0018)
	States with Over 100 uses: CA, GA, FL, MD, NV, VA, WA			
Variable	Age 8	Age 9	Age 10	Age 11
Absolute Upward Mobility (*Used*)	-.0005 (.0030)	-.0094** (.0042)	-.0010 (.0029)	.0044 (.0031)
Absolute Upward Mobility (*Unused*)	.0026 (.0037)	-.0034 (.0031)	-.0023 (.0025)	.0001 (.0022)
75th percentile (*Used*)	.0011 (.0016)	-.0023 (.0014)	.0005 (.0020)	.0016 (.0036)
75th percentile (*Unused*)	-.0014 (.0031)	-.0037 (.0023)	-.0015 (.0022)	.0011 (.0021)
	State's three strike use is $>$ .01 *of admissions in 1998*			
Variable	Age 8	Age 9	Age 10	Age 11
Absolute Upward Mobility (*Used*)	-.0005 (.0030)	-.0091** (.0041)	-.0008 (.0029)	.0042 (.0030)
Absolute Upward Mobility (*Unused*)	.0026 (.0037)	-.0035 (.0031)	-.0025 (.0025)	.0001 (.0022)
75th percentile (*Used*)	.0011 (.0016)	-.0023 (.0014)	.0003 (.0019)	.0017 (.0035)
75th percentile (*Unused*)	-.0014 (.0031)	-.0037 (.0024)	-.0014 (.0022)	.0010 (.0021)
Observations	8,895	8,895	8,895	8,895

All regressions include county fixed effects, year fixed effects, county specific linear time trends and birth record weights. Absolute upward mobility measures a child's expected rank in the national income distribution at age 26, conditioning on being born to parents that were at the 25th percentile of their national income distribution (negative number indicates worse mobility). State's three strike use is $>$ .01 of admissions in 1998 corresponds to CA, GA, FL, MD, NV, VA, WA, ND, and VT. 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. Since Texas always had a three strikes policy, observations in Texas are dropped from the regressions. The available birth cohorts in this data are 1980 to 1986.

Table VIII: Impact of Three Strikes Exposure on Commuting Zone Level Measures of College Attendance at 19

	Top Three Used States: CA, GA, FL			
Variable	Age 7	Age 8	Age 9	Age 10
Relative (*Used*)	-.0020 (.0070)	.0046 (.0113)	-.0007 (.0124)	-.0381*** (.0094)
Relative (*Unused*)	-.0065 (.0054)	-.0007 (.0066)	-.0012 (.0057)	-.0066 (.0064)
Absolute (*Used*)	-.0008 (.0034)	-.0121*** (.0028)	-.0157*** (.0037)	-.0066 (.0064)
Absolute (*Unused*)	-.0002 (.0047)	-.0003 (.0048)	.0020 (.0045)	.0057 (.0046)
Absolute Upward (*Used*)	-.0013 (.0032)	-.0109** (.0047)	-.0158*** (.0034)	-.0090* (.0045)
Absolute Upward (*Unused*)	-.0019 (.0043)	-.0005 (.0039)	.0017 (.0046)	.0040 (.0048)
	States with Over 100 uses: CA, GA, FL, MD, NV, VA, WA			
Variable	Age 7	Age 8	Age 9	Age 10
Relative (*Used*)	-.0029 (.0056)	.0053 (.0087)	.0017 (.0102)	-.0345*** (.0111)
Relative (*Unused*)	-.0068 (.0064)	-.0024 (.0072)	-.0030 (.0056)	-.0070 (.0065)
Absolute (*Used*)	.0002 (.0038)	-.0100** (.0037)	-.0141*** (.0047)	-.0015 (.0045)
Absolute (*Unused*)	-.0009 (.0054)	.0003 (.0054)	.0029 (.0045)	.0072* (.0043)
Absolute Upward (*Used*)	-.00056 (.0033)	-.0087* (.0046)	-.0137*** (.0046)	-.0102** (.0044)
Absolute Upward (*Unused*)	-.0027 (.0049)	-.0003 (.0042)	.0022 (.0046)	.0054 (.0045)
	State's three strike use is > .01 of admissions in 1998			
Variable	Age 7	Age 8	Age 9	Age 10
Relative (*Used*)	-.0026 (.0054)	.0050 (.0086)	.0021 (.0099)	-.0309** (.0126)
Relative (*Unused*)	-.0072 (.0065)	-.0023 (.0073)	-.0034 (.0056)	-.0081 (.0067)
Absolute (*Used*)	.0002 (.0037)	-.0094** (.0038)	-.0137*** (.0047)	-.0019 (.0046)
Absolute (*Unused*)	-.0010 (.0055)	.0000 (.0054)	.0030 (.0045)	.0079* (.0042)
Absolute Upward (*Used*)	-.0004 (.0033)	-.0082* (.0046)	-.0131*** (.0046)	-.0097** (.0045)
Absolute Upward (*Unused*)	-.0028 (.0050)	-.0006 (.0042)	.0022 (.0047)	.0059 (.0044)
Observations	5,626	5,626	5,626	5,626

All regressions include county fixed effects, birth cohort fixed effects, commuting zone linear time trends and weights. The measure for relative mobility captures the association between a childs probability of college attendance at age 19 and his parents position in the parental income distribution (positive number indicates parental rank matters more for kids outcomes). Absolute mobility captures the college attendance probability for those at the very bottom of the income distribution (negative number indicates worse mobility). Absolute upward mobility measures a child's probability of college attendance at age 19, conditioning on being born to parents that were at the 25th percentile of their national income distribution (negative number indicates worse mobility). The coefficient for each measure is based on a separate regression. 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. The birth cohorts with this data available are 1984 to 1993 so we can not identify changes using the same age cutoffs for income mobility. Since Texas always had a three strikes policy, observations in Texas are dropped from the regressions.

Table IX: Impact of Three Strikes Exposure on County Level Measures of College Attendance at 19

	Top Three Used States: CA, GA, FL			
Variable	Age 8	Age 9	Age 10	Age 11
Absolute Upward Mobility (*Used*)	-.0109 (.0076)	-.0297*** (.0035)	-.0306*** (.0065)	-.0092 (.0059)
Absolute Upward Mobility (*Unused*)	.0038 (.0048)	.0070 (.0044)	.0067 (.0071)	.0055 (.0085)
75th percentile (*Used*)	-.0064 (.0089)	-.0254*** (.0052)	-.0310*** (.0054)	-.0126 (.0079)
75th percentile (*Unused*)	.0028 (.0055)	.0025 (.0063)	.0025 (.0069)	.0055 (.0085)
	States with Over 100 uses: CA, GA, FL, MD, NV, VA, WA			
Variable	Age 8	Age 9	Age 10	Age 11
Absolute Upward Mobility (*Used*)	-.0082 (.0057)	-.0236*** (.0051)	-.0234*** (.0075)	-.0016 (.0076)
Absolute Upward Mobility (*Unused*)	.0045 (.0056)	.0101** (.0045)	.0109 (.0080)	.0039 (.0098)
75th percentile (*Used*)	-.0041 (.0076)	-.0196*** (.0060)	-.0239*** (.0076)	-.0042 (.0099)
75th percentile (*Unused*)	.0028 (.0057)	.0040 (.0069)	.0058 (.0073)	.0042 (.0091)
	State's three strike use is > .01 *of admissions in 1998*			
Variable	Age 8	Age 9	Age 10	Age 11
Absolute Upward Mobility (*Used*)	-.0075 (.0056)	-.0226*** (.0053)	-.0226*** (.0076)	-.0012 (.0076)
Absolute Upward Mobility (*Unused*)	.0042 (.0056)	.0099** (.0046)	.0109 (.0081)	.0037 (.0100)
75th percentile (*Used*)	-.0039 (.0075)	-.0188*** (.0060)	-.0233*** (.0075)	-.0044 (.0098)
75th percentile (*Unused*)	.0028 (.0058)	.0038 (.0069)	.0059 (.0074)	.0044 (.0092)
Observations	11,276	11,276	11,276	11,276

All regressions include county fixed effects, year fixed effects, county specific linear time trends and birth record weights. Absolute upward mobility measures a child's expected college attendance at age 19, conditioning on being born to parents that were at the 25th percentile of their national income distribution (negative number indicates worse mobility). College attendance for the 75th percentile is also based on if the individual attended college at 19. 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. The birth cohorts with data on college attendance at age 19 are 1980 to 1988. Since Texas always had a three strikes policy, observations in Texas are dropped from the regressions.

Table X: Robustness Check using the American Community Survey Data: Household Income

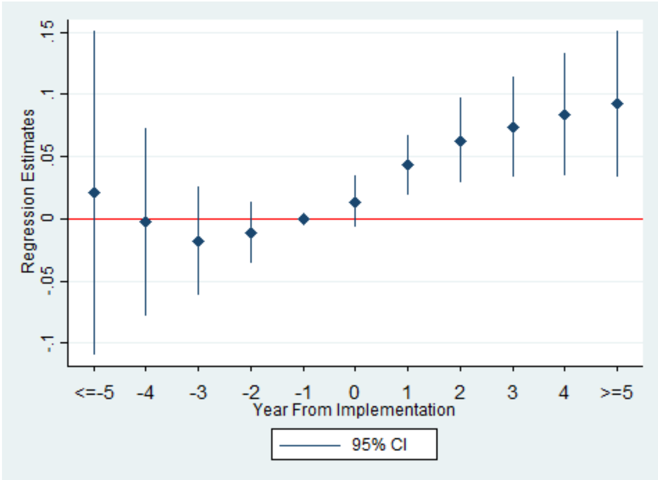
	Household Income			
	Top Three Used States: CA, GA, FL			
Variable	Age 8	Age 9	Age 10	Age 11
Used	-.014 (.012)	-.039** (.019)	-.040 (.027)	.011 (.020)
Unused	.022 (.021)	-.020 (.018)	.018 (.021)	-.019 (.017)
	States with Over 100 uses: CA, GA, FL, MD, NV, VA, WA			
Variable	Age 8	Age 9	Age 10	Age 11
Used	.007 (.024)	-.028* (.016)	-.031 (.023)	.002 (.021)
Unused	.010 (.022)	-.026 (.020)	.023 (.022)	-.018 (.019)
	State's three strike use is $>$ .01 *of admissions in 1998*			
Variable	Age 8	Age 9	Age 10	Age 11
Used	.007 (.024)	-.023 (.016)	-.027 (.022)	-.002 (.022)
Unused	.010 (.022)	-.030 (.020)	.021 (.022)	-.016 (.019)
state fixed effects	X	X	X	X
birth cohort fixed effects	X	X	X	X
state specific linear time trend	X	X	X	X
Observations	161,344	161,344	161,344	161,344

The dependent variable the log of annual household income. The variable earnings is defined as income earned from wages or a person's own business or farm for the previous year. *Used* is a binary variable that take a value of one if a respondent was born in high usage strike states and zero otherwise. The variable *Unused* is also a binary one and is set equal to one for all other strike states and zero otherwise. All estimations include controls for gender, marital status, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Source: American Community Survey 2006 to 2012. 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. All models include person weights, which indicates how many persons in the U.S. population are represented by a given person in the sample. Since Texas always had a three strikes policy, observations from Texas are excluded from all models.

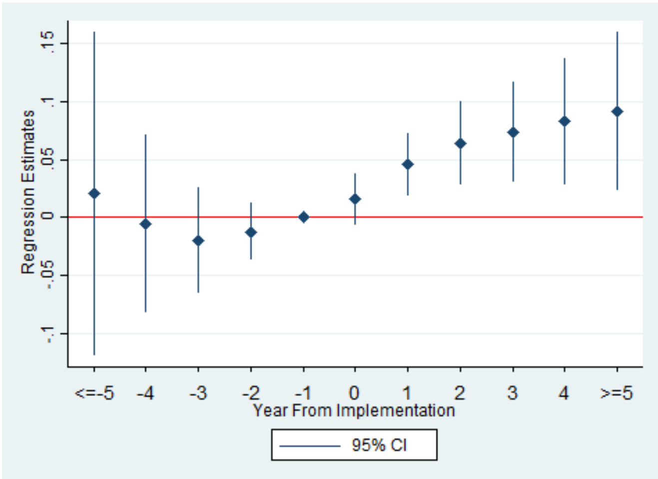
Table XI: Robustness Check using the American Community Survey Data: College Attendance

	College Attendance			
	Top Three Used States: CA, GA, FL			
Variable	Age 8	Age 9	Age 10	Age 11
Used	.006 (.005)	-.017** (.008)	-.016* (.008)	-.008 (.009)
Unused	.005 (.014)	-.003 (.011)	.004 (.011)	.014 (.009)
	States with Over 100 uses: CA, GA, FL, MD, NV, VA, WA			
Variable	Age 8	Age 9	Age 10	Age 11
Used	.005 (.008)	-.004 (.011)	-.011* (.006)	-.005 (.010)
Unused	.005 (.018)	-.011 (.012)	.005 (.013)	.018 (.009)
	State's three strike use is $>$ .01 *of admissions in 1998*			
Variable	Age 8	Age 9	Age 10	Age 11
Used	.005 (.008)	-.004 (.011)	-.009 (.007)	-.001 (.011)
Unused	.005 (.018)	-.011 (.012)	.003 (.013)	.014 (.009)
state fixed effects	X	X	X	X
birth cohort fixed effects	X	X	X	X
state specific linear time trend	X	X	X	X
Observations	167,724	167,724	167,724	167,724

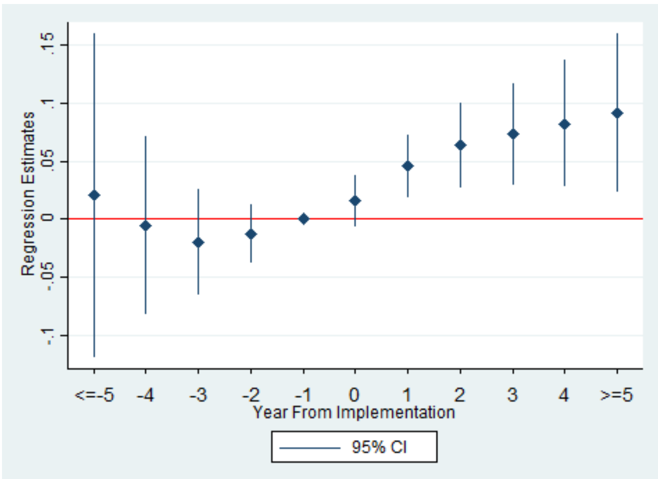
The dependent variable is a measure of college attendance. College Attendance, is binary, and equals one if the respondent report having at least one year of college. *Used* is a binary variable that take a value of one if a respondent was born in high usage strike states and zero otherwise. The variable *Unused* is also a binary one and is set equal to one for all other strike states and zero otherwise. All estimations include controls for gender, marital status, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Source: American Community Survey 2006 to 2012. 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. All models include person weights, which indicates how many persons in the U.S. population are represented by a given person in the sample. Since Texas always had a three strikes policy, observations from Texas are excluded from all models.



(a) Top Three Used States: CA, GA, FL



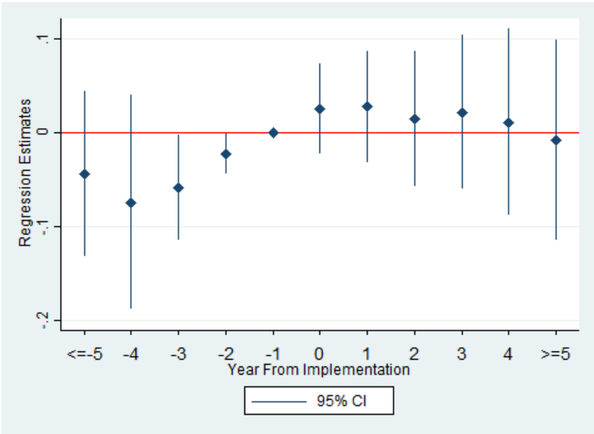
(b) States with Over 100 uses: CA, GA, FL, MD, NV, VA, WA



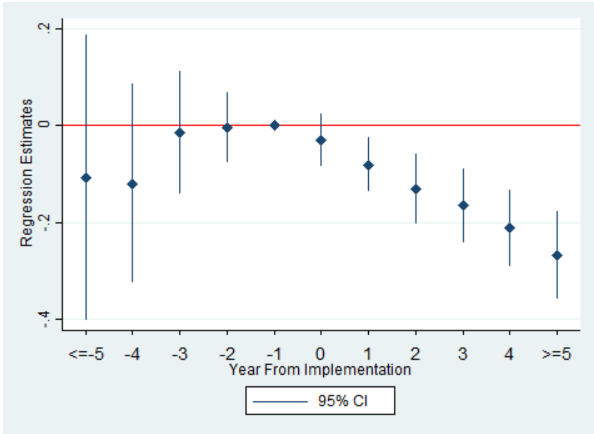
(c) State's three strike use is $>.01$ of admissions in 1998

Figure 1: Relative Mobility: Distribution of Coefficients

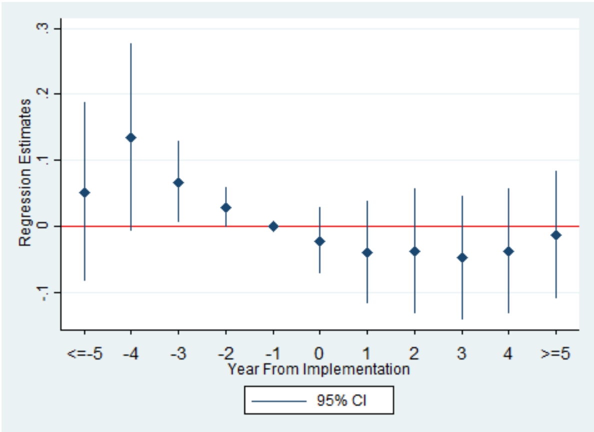
The figure plots coefficients from an event-study analysis. The reference period is one year before three strikes law was implemented Source: National Corrections Reporting Program, 1991-2015, Dataset 001: Term Records.



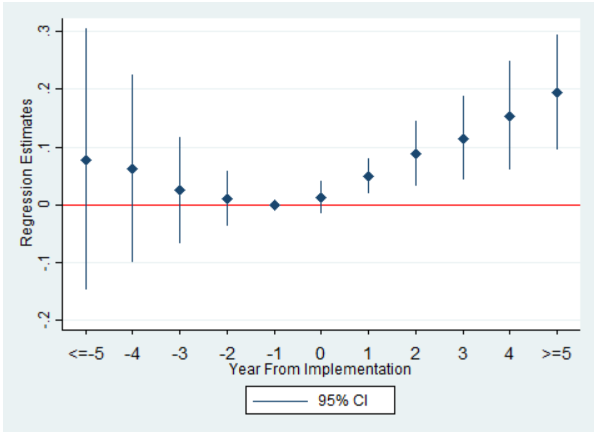
(a) Sentence Length = 1-1.9 years



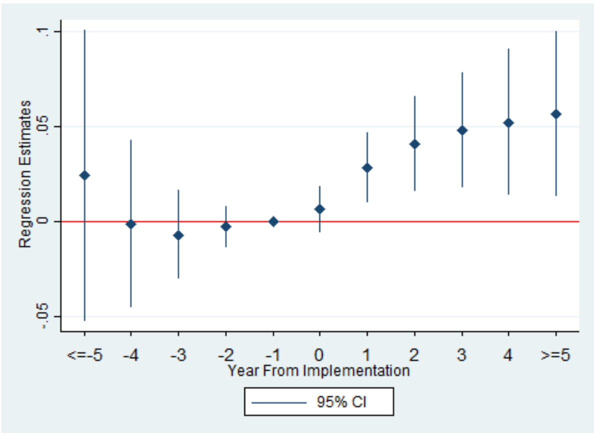
(b) Sentence Length = 2-4.9 years



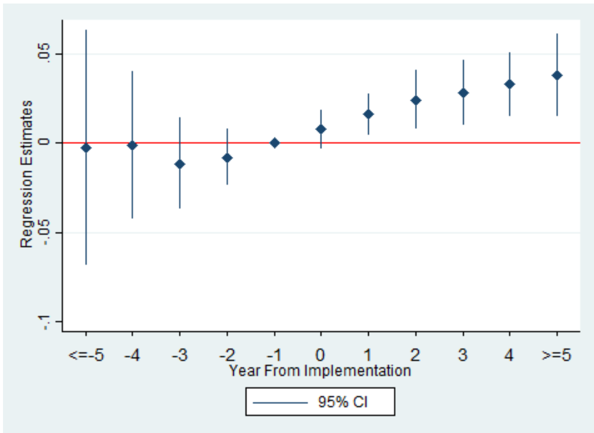
(c) Sentence Length = 5-9.9 years



(d) Sentence Length = 10-24.9 years

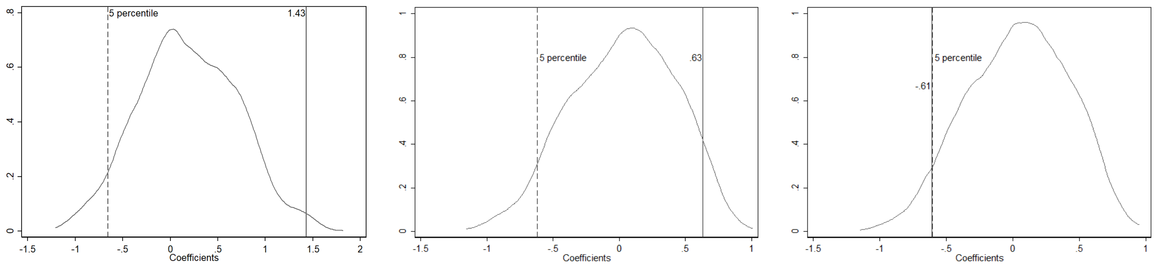


(e) Sentence Length = 25 years



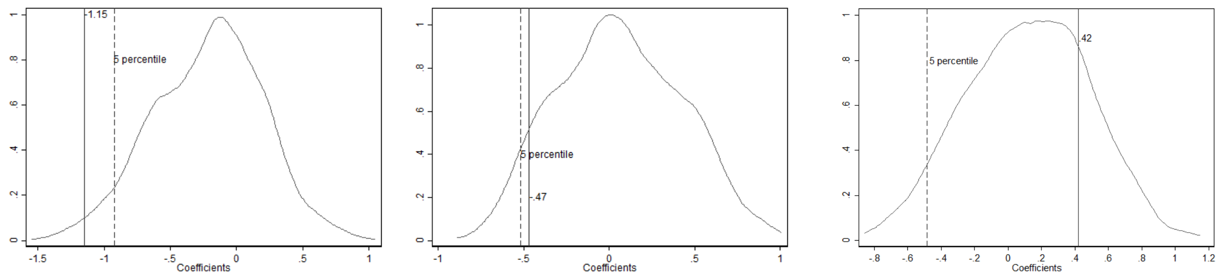
(f) Sentence Length = Life in Prison

Figure 2: Sentence length



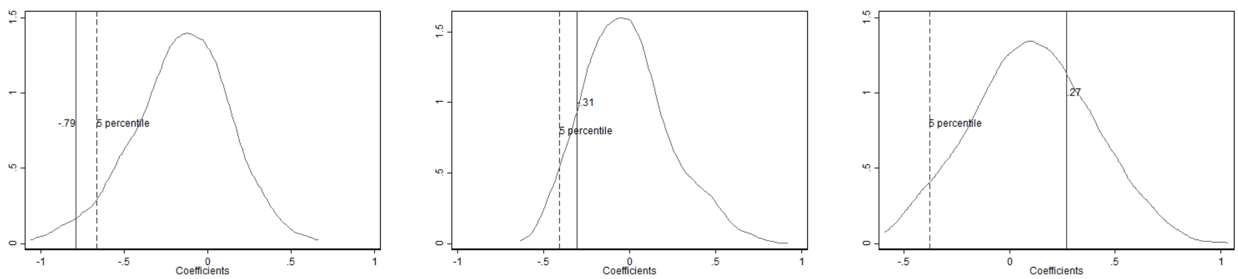
(a) Young = Age 8 & 9 (b) Young = Ages 8 - 10 (c) Young = Ages 8 - 11

Figure 3: Relative Income Mobility: Distribution of Coefficients



(a) Young = Age 8 & 9 (b) Young = Ages 8 - 10 (c) Young = Ages 8 - 11

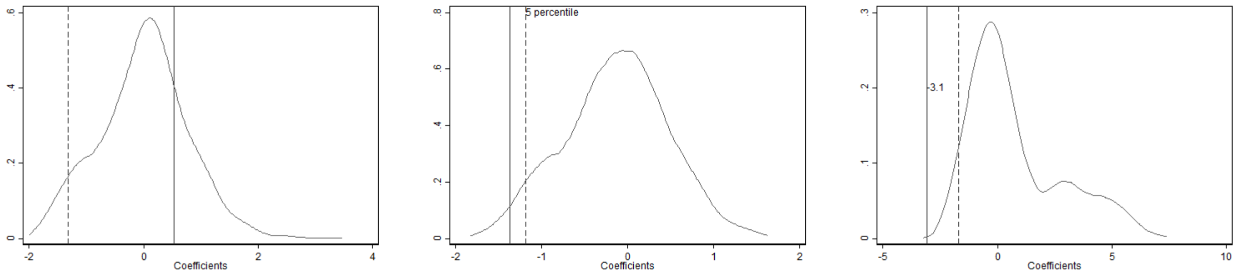
Figure 4: Absolute Income Mobility: Distribution of Coefficients



(a) Young = Age 8 & 9 (b) Young = Ages 8 - 10 (c) Young = Ages 8 - 11

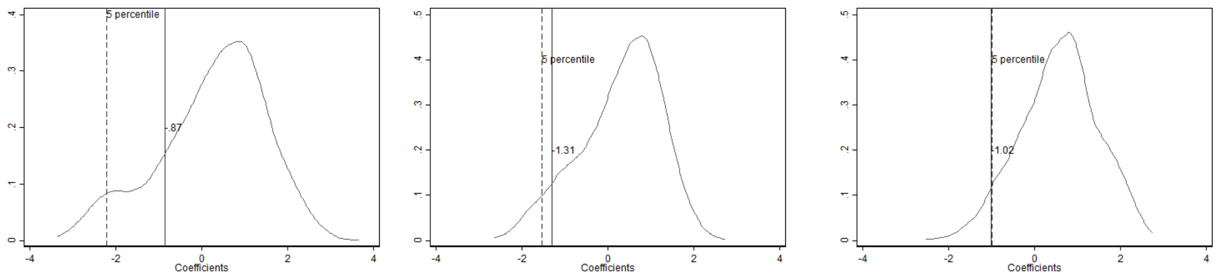
Figure 5: Absolute Upward Income Mobility: Distribution of Coefficients

Figures 3, 4, and 5 show the distribution of the coefficient on $exposure_{cb} * Used_c$ (α_1) in 1600 replications. Using the 25 control states as a donor pool, we randomly assign treatment to reflect 9 states where the policy was enforced and 16 states that rarely used the policy. Each model included commuting zone and birth cohort fixed effects as well as commuting zone specific linear trends. The broken line denotes the lower 5% of the distribution. The solid line shows our actual estimate as reported in panel three of Table VI.



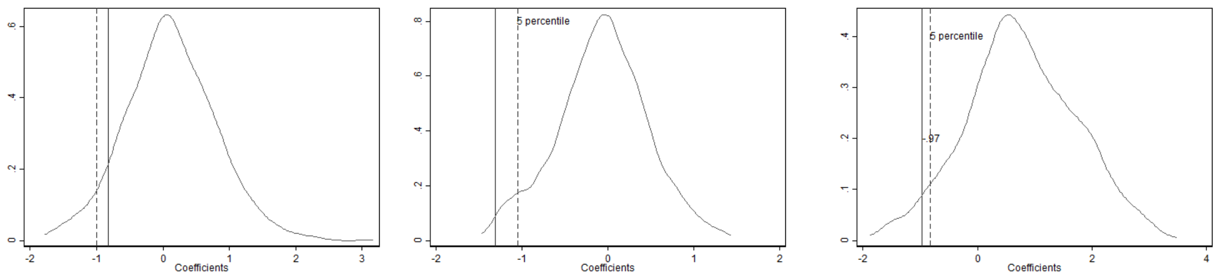
(a) Young = Age 7 - 8 (b) Young = Ages 7 - 9 (c) Young = 7 - 10

Figure 6: College Attendance at 19, Relative Mobility: Distribution of Coefficients



(a) Young = Age 7 - 8 (b) Young = Age 7 - 9 (c) Young = Age 7 - 10

Figure 7: College Attendance at 19, Absolute Income Mobility: Distribution of Coefficients



(a) Young = Age 7 - 8 (b) Young = Ages 7 - 9 (c) Young = 7 - 10

Figure 8: College Attendance at 19, Absolute Upward Mobility: Distribution of Coefficients

Figures 6, 7, and 8 show the distribution of the coefficient on $exposure_{cb} * Used_c$ (α_1) in 1600 replications. Using the 25 control states as a donor pool, we randomly assign treatment to reflect 9 states where the policy was enforced and 16 states that rarely used the policy. Each model included commuting zone and birth cohort fixed effects as well as commuting zone specific linear trends. The broken line denotes the lower 5% of the distribution. The solid line shows our actual estimate as reported in panel three of Table VI.