Essays on the economics of crime and occupational licensing

by

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M.S., University of the West Indies, Mona, Jamaica, 2012

M.A., Kansas State University, 2017

AN ABSTRACT OF A DISSERTATION

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Abstract

This dissertation consists of three essays on issues associated with labor market regulations and the criminal justice system. The first chapter seeks to examine occupational licensing regulations and the native-immigrant wage gap, while the second and third essays examine the effects of an increase in the incidence of adult incarceration on children's short and long term outcomes in the United States.

The first essay, co-authored with Dr. Hugh Cassidy, examines the incidence and impact of occupational licensing on immigrants using two sources of data: the Current Population Survey and the Survey of Income and Program Participation. We find that immigrants are much less likely to have a license than similar natives, and that this gap is largest for non-naturalized immigrants, men, and for workers with the highest level of educational attainment. While, a lack of English proficiency reduces the probability of an immigrant having a license, the licensing rate increases with years since migration, and shows large variations by immigrant region of origin. The wage premiums to having a license are much larger for women than men, but seem to be the same for natives and immigrants after controlling for English language ability.

In the second essay co-authored with Dr. Amanda Gaulke, we utilize quasi-experimental methods to provide causal estimates of the intergenerational impact of mass imprisonment. Which parents are sent to prison is not random, and many of the same factors that predict adult imprisonment also predict children's outcome. To minimize bias, we use variation in the timing and implementation of 'Three Strikes and You're Out' laws to identify the effect of exposure to adult imprisonment on children's long term outcomes. Using the American Community Survey and difference-in-differences approach to estimate the long-run effects, we show that Three Strikes laws impact male children but not female children. Specifically, Black males are less likely to be employed, but among Black males who enroll in college

there is no significant decrease in employment and there is an increase in annual earnings. We show sentence length matters which complements the literature on whether parents are incarcerated matters.

In the third essay, I assess whether the association between paternal incarceration and children's outcomes varies with age at exposure and gender by comparing children who were 0-18 years when their biological father or father figure entered jail or prison. I use data from the National Longitudinal Study of Adolescent to Adult Health to estimate the association. The results confirm existing research by providing additional evidence that children in the United States are adversely affected by paternal incarceration, but they also show that the adverse outcomes identified are mainly associated with children exposed during early and late childhood. In particular, I find that children exposed during early childhood, primarily before age six, perform poorly in high school relative to those never exposed. Those separated from their father at an early age due to incarceration (mainly boys) earn less income when compared with boys never exposed. Children exposed during late childhood are also negatively affected by paternal incarceration. Notably, females exposed during late childhood are less likely to complete college, and receive lower income relative to females never exposed.

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Dedication

For my mother, Donnarie Graham; may I inherit half her strength.

Chapter 1

Occupational Licensing and Immigrants

1.1 Introduction

Occupational licensing has become an increasingly prevalent aspect of the US labor market over the past several decades. Approximately 29% of US workers are required to have an occupational license; as a result, occupational licensing directly impacts more workers than either the minimum wage or unionization.[1]

Proponents of occupational licensing highlight improvements in the quality of services provided and reductions in catastrophes such as unintended fires or misdiagnoses as some of the main benefits. Higher quality services may result from licensing requirements creating greater incentives for individuals to invest in more occupation-specific human capital. Notwithstanding these potential benefits, by imposing a cost to working in an occupation, licensing restricts the supply of labor and therefore may drive up its cost.

Immigrants may be particularly impacted by occupational licensing for several reasons. First, acquiring a license takes time. Newly arrived immigrants are essentially new entrants in the US labor market, and some licenses have a residency and/or citizenship requirement

[1] See Kleiner and Krueger (2010) and Kleiner (2000).

that would naturally impact immigrants more so than natives. Second, a license may require educational credentials that may not transfer easily (if at all) between an immigrant's source country and the United States. Additionally, language barriers may interfere with acquiring a license; for example, licensure may involve taking an exam in English.[2] Natives and immigrants may also differ in the types of occupational licenses they obtain, and may therefore differ in the wage premiums they obtain by having a license. A 2015 report on occupational licensing from the Obama White House, recognizing the potentially important connection between immigrants and occupational licensing, says: "Our licensure system can also prevent immigrants who have considerable training and work experience abroad from applying their skills in the United States, since often they do not meet the relevant licensing requirements" (pg. 8).[3]

An occupational license is a form of labor market investment, since licenses are costly in terms of time and monetary expenses. A common finding in the occupational licensing literature is that a worker with a license tends to enjoy a wage premium, with estimates across previous studies typically ranging from 10% to 15%.[4] This wage premium helps incentivize workers to acquire licenses. In the presence of such a wage premium, any difficulty acquiring an occupational license by immigrants may contribute to their well-known wage gap with natives. Furthermore, as discussed in Borjas (2015), more recent immigrant cohorts to the United States have experienced slower earnings assimilation relative to earlier cohorts. The increasing prevalence of occupational licensing may help to explain this assimilation slowdown if immigrants are being partly excluded from licensed occupations. Additionally, being an investment, an occupational license is only worth acquiring if the worker has sufficient confidence that he or she will spend enough time working to pay back the (often substantial) cost of acquiring the license. Immigrants who may return migrate, therefore, would probably be less likely to invest in a license.

An occupational license, being a costly investment, also helps to serve as a signal about

[2] Federman et al. (2006) observed an increase in the number of Vietnamese manicurists following the introduction of the Vietnamese language exam. They posit that policies of offering English-only licensing exams affect immigrants by acting as a barrier to entry.

[3] See U.S. Department of the Treasury Office of Economic Policy and of Labor (2015).

[4] See Kleiner and Krueger (2013).

the worker.[5] Employers in the United States are likely to have less reliable information about immigrant workers compared to otherwise similar native workers (e.g., it may be difficult to assess the quality of an education acquired abroad). Thus, immigrants may turn to occupational licenses to help alleviate the asymmetric information problems that are more severe for them than for natives. Therefore, we might expect higher licensing rates for immigrants than natives.

Unfortunately, due in part to a lack of large-scale data on occupational licensing that spans all occupations and states, there is little research on occupational licensing and immigrants. Of the limited number of occupational licensing studies that focus on immigrants, most focus on high-skilled occupations such as doctors and how their credentials transfer (or not) internationally. Kugler and Sauer (2005), explores this phenomenon of relicensing immigrant physicians in their work.[6] There is also a small literature studying immigrants and particular occupations, including Federman et al. (2006), which looks at the effects of occupational licensing of manicurists and Vietnamese immigrants.

However, to our knowledge, there is no work that uses large-scale, representative data to explore the relationship between immigration and occupational licensing in the United States. Gomez et al. (2015) investigate the occupational licensing of immigrants to Canada. They find that immigrants are less likely to have a license than natives and that immigrant men may benefit in terms of wages from having a license more so than natives. In addition to studying a different country, Gomez et al. (2015) impute licensing status based on occupation code alone-they do not observe if the worker is licensed (i.e., actual licensing attainment). Also, they are not able to control for English or French language proficiency. Tani (2018), using longitudinal data on immigrants in Australia, finds that occupational licensing improves wages of licensed workers, though many immigrants do not continue to work in their pre-migration occupation due to the occupational licensing hurdle, leading to skill wastage.

In this paper, we are primarily interested in the following questions: How do natives and

[5] See Blair and Chung (2017).

[6] See also Peterson et al. (2014), as well as McDonald and Worswick (2015) for the case of immigrant physicians in Canada.

immigrants differ in their probability of having an occupational license? Do we see evidence of immigrants and natives acquiring different types of occupational licenses in terms of requiring an exam, etc.? How does this gap change with years since migration (YSM) in the country, (i.e., do immigrants tend to "assimilate" toward natives)? What is the importance of English language proficiency on having a license? Do natives and immigrants differ in the wage premium associated with having a license? And what is the relationship between licensing status and hours worked for natives and immigrants?

To answer these questions, we make use of two sources of data: the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP). The CPS and the SIPP each provide distinct advantages, and neither alone allows us to answer all the questions posed above. The CPS, which added occupational licensing questions to their monthly survey in 2015, includes information about the immigrant year of migration (and thus years since migration and age at migration) as well as immigrant birthplace. It is also a much larger survey than the SIPP, and since the licensing questions are now a standard part of the survey, the CPS will continue to be a valuable source of occupational licensing information going forward.

The SIPP, during Wave 13 of the 2008 sample, included a topical module on certification and licensing.[7] The SIPP includes data on English language proficiency, which is of obvious importance when studying immigrants. Information on the requirements necessary to earn and maintain a license is also available in the SIPP, and these additional details about workers' licenses allow us to see if immigrants and natives differ in the types of licenses acquired. Lastly, while the CPS and SIPP each has its unique advantages, using both provides a robustness check on some of our results where the CPS and SIPP overlap.

Both the CPS and SIPP allow us to directly measure occupational licensing attainment of workers. In contrast, due to a lack of direct licensing attainment data, much of the literature has relied on licensing coverage (i.e., whether or to what degree an occupation in a given state is licensed).[8] By directly measuring licensing attainment, we can estimate the relationships

[7]The 2014 SIPP panel includes occupational licensing information.

[8]See, for example, Gittleman and Kleiner (2016), which uses the National Longitudinal Survey of Youth 1979 and a database maintained by the US Department of Labor of occupations by state that require a

between licensing status and both wages and hours worked, where these relationships are identifiable by variation in who reports having a license within an occupation in a state as well as variation in licensing rates by occupation across states.

The results from both the CPS and SIPP show that immigrants are around 30-35% less likely than otherwise similar natives to have an occupational license. This licensing gap is much larger for men (45-50%) than women (15-20%). Immigrant licensing rates increase with years since migration, consistent with labor market assimilation, as well as English language proficiency. However, we also find that most of the characteristics of the licenses held by natives and immigrants are the same. For example, they are equally likely to have taken an exam to receive their license.

Notably, we find that the licensing gap between natives and immigrants is much larger for workers with more than a bachelors degree compared to other education levels. In fact, for women with a bachelors degree or less, there is only a modest gap in licensing between natives and immigrants. For native workers, both men and women, there is a substantial jump in the licensing rate between BA and GRAD levels of education (about 15 and 20 percentage points for men and women, respectively). For immigrants, however, there is only a modest increase between BA and GRAD education levels (around 5 percentage points for both men and women), and thus a substantial license gap between natives and immigrants opens up at the highest education level.

We also find large variations in licensing rate by immigrant region of origin. Immigrants from three regions in particular-the Caribbean, Southeast Asia, and Africa-show high licensing rates, especially among women. These results are consistent with immigrants from these regions clustering into occupations that are typically licensed, notably nursing and cosmetology.

We find an overall wage premium from having a license of 8.5% in the CPS and 6.5% in the SIPP, and in both the CPS and SIPP, women have a much higher licensing wage premium than men. The wage premium is higher for immigrants than natives, especially in the CPS, where the licensing premium is 7 percent higher for immigrants than natives.

license.

However, controlling for English ability in the SIPP reduces the positive immigrant and license interaction term, suggesting that licensing status may be serving as a proxy for English ability (which is not measured in the CPS). Similar to Blair and Chung (2017), we find that the wage gap between natives and immigrants with an occupational license is much smaller than the wage gap between natives and immigrants without a license.

Our paper demonstrates the usefulness of the new CPS certification and licensing questions. While not the central focus of our paper, we provide one of the first estimates of the wage premium associated with occupational licensing using the new CPS variables, finding lower premiums than previous studies of occupational licensing, though similar returns to those from the SIPP.[9]

Our paper has the following structure. Section 1.2 describes our two data sources: the CPS and the SIPP. Our main results are shown in Section 1.3, where 1.3.1 discusses the occupational licensing rate results for both the CPS and SIPP, and 1.3.2 discusses the relationship between licensing and both wages and hours worked, again for both the CPS and the SIPP. Section 1.4 concludes.

1.2 Data

1.2.1 Current Population Survey

The first dataset used in our analysis is the Current Population Survey (CPS), drawn from the Integrated Public Use Microdata Series (IPUMS) (Flood et al. (2017)). The CPS is a monthly representative dataset in the United States that interviews each household for four consecutive months, followed by an eight-month period during which the household is not interviewed; then the household is interviewed for an additional four months before exiting the survey permanently. The fifth month refers to the fifth month an individual is in the sample which, because of the eight-month break, is one year after the first month.

[9] A recent working paper by Kleiner and Soltas (2018) also uses the CPS, though it focuses mostly on licensing coverage by occupation and state as opposed licensing attainment. The authors also do not consider natives and immigrants separately, which is the focus of our paper.

During months four and eight, when households are about to be "rotated out" of the survey, households are part of the Outgoing Rotation Group (ORG). As such, they are asked more detailed income questions than are asked during the other three months.

Beginning in 2015, in their first and fifth survey months, respondents are asked three questions related to occupational certification and licensing: (1) Do you have a currently active professional certification or a state or industry license?, (2) Were any of your certifications or licenses issued by the federal, state, or local government?, and (3) Is your certification or license required for your job?. Due to issues with the implementation of the survey, information from the third question is unavailable for 2015. Note also that the second question asks if any license or certification was issued by federal, state, or local government; for individuals with both a certification and a license, this may lead to an overstatement of the fraction of credentials that are licenses. We find that while the credential rate is very similar between the CPS and SIPP, the fraction of these credentials that are reported as licenses (i.e., issued by some level of government) is higher in the CPS than the SIPP. Also note that the wording of question 3 makes it unclear whether the certification or license is legally required, or whether it is required only by the worker's employer.

We say an individual has a credential if they answer "yes" to the first question; an individual is licensed if they answer "yes" to all three questions, while they are certified if they have a credential that is not a license.[10] The results presented here follow Kleiner and Soltas (2018) and use the strictest category of licensed.

We utilize the IPUMS-created variable cpsidp to link individuals across survey months.[11] Only very limited income information is available in the first and fifth survey months of the monthly basic CPS, so to explore the relationship between occupational licenses, wages, and hours worked, we link these months with the ORG months (i.e., months four and eight).

The CPS contains two important immigrant-related variables not included in the 2008 SIPP: year of immigration and birthplace. Year of immigration allows us to calculate an

[10] Utilizing the third question requires that we drop data from 2015, as the third question was not included that year.

[11] See Flood and Pacas (2016) for further discussion.

immigrant's years since migration as well as his or her age at migration.[12]

Our dataset includes CPS basic monthly observations from January 2016 to January 2019. We use two different estimation samples from the CPS-one for exploring the probability of having a license, and another for exploring the relationship between licensing and both wages and hours worked. All samples include respondents between the ages of 18 and 64 who are in the labor force and currently working, but who are neither self-employed nor in the armed forces.[13] We exclude childhood immigrants-defined as immigrants who arrived in the United States at age 17 or younger-from all samples.[14] Omitting childhood immigrants, which we are unable to do with the SIPP samples since age at migration cannot be calculated, does make the SIPP and CPS immigrant samples different, so care is needed when comparing the CPS and SIPP results.

The probability sample includes only the first and fifth survey months since those are the months during which licensing questions are asked. We have 428,455 person-months of observations, from 335,557 unique individuals.[15] Our wage and hours worked sample uses the Outgoing Rotation Group. We use the licensing status in the first (fifth) month and assign it to the ORG sample in the fourth (eighth) month. We use directly reported hourly wage for workers paid-by-the-hour and calculate hourly wage of salary workers based on their weekly earnings and usual hours worked per week. We include workers with hourly wages

[12] We follow Bleakley and Chin (2004) and, for each year of immigration range, use the maximum year, which results in years since migration being minimized and so age at migration maximized. Thus, some immigrants who actually arrived as children will be classified as adult immigrants and are therefore included in our sample.

[13] We drop self-employed from all samples in the CPS as well as the SIPP because the ORG sample does not contain earnings data for the self-employed.

[14] There are a number of compelling reasons to focus on adult immigrants only: (1) childhood immigrants are much more likely to be proficient in English than adult immigrants, (2) the effect of years since migration is likely to vary greatly between adult and childhood immigrants, (3) a childhood immigrant will acquire nearly all of their work experience in the United States, and (4) childhood immigrants will likely have at least some exposure to the US school system. Results in which we include childhood immigrants do not differ qualitatively from those presented here.

[15] While matching CPS panels is a popular means of forming panel data sets, because the CPS does not follow residential movers these panel data sets suffer high attrition. Neumark and Kawaguchi (2001) analyze whether attrition in the CPS leads to bias in the longitudinal estimation of the effects of unions on wages; and the marriage wage premium for males. Their results for the longitudinal analysis of union wage effects reveal small and statistically insignificant evidence of attrition bias despite the high attrition rate in the matched CPS files. In contrast, their longitudinal analysis of the marriage premium for males finds statistically significant attrition bias. They argue however, that the amount of bias does not seem to be serious in an economic sense. As a result, we are not concerned about attrition bias in the current paper.

between \$5 and \$100 and drop observations with an imputed wage. This sample includes 212,184 observations from 176,572 unique individuals.

Summary statistics of the probability sample are shown in Table 1.1. We show the full sample as well as the subsamples of licensed and unlicensed separately by natives and immigrants, where an immigrant is defined as someone born outside of the United States.[16] Immigrants are much less likely than natives to have a credential, license, or certification. Workers with licenses are older and more educated, more likely to be married, and are disproportionately white. Comparing natives and immigrants with and without a license, licensed natives are much more educated than unlicensed natives, while the education level difference between licensed and unlicensed immigrants appears to be less dramatic. We discuss the relationship between educational attainment and licensing further in Section 1.3.1. Years since migration is higher for licensed than unlicensed immigrants.

1.2.2 Survey of Income and Program Participation

The 2008 panel of the SIPP is a nationally representative longitudinal survey of the United States. Every four months respondents answer a core group of questions about the preceding four months. These responses provide detailed monthly information about demographics, employment situations, wages, and a variety of other characteristics. Occupational licensing information is derived from the Wave 13 Professional Certifications, Licenses, and Educational Certificates topical module collected between September and December of 2012.

A respondent is said to have a "credential" if they respond "yes" to the following question: "Do/Does you/he/she have a professional certification or state or industry license?" Respondents are also asked: "Who awarded this certification or license?", as well as: "Is this certification or license required for current or most recent job?" If a certificate or license was awarded by the federal, state, or local government, and if it is also required for their job, we refer to the credential as a "license", otherwise, we refer to it as a "certificate".[17] Thus, as

[16] Individuals from Puerto Rico, Guam, Northern Mariana Islands, American Samoa, and the US Virgin Islands are considered born in the United States and are thus natives.

[17] This distinction between certificate and license follows definitions 1 and 2 from Gittleman et al. (2018).

in the CPS, we use the strictest definition for licensed.

The SIPP licensing and certification module asks several additional questions about occupational licensing not included in the CPS, such as: "Did you take courses or training to earn this certification or license?" These additional licensing questions allow us to go into more depth regarding the differences in occupational licensing between natives and immigrants. The SIPP, unlike the CPS, contains a self-reported measure of English speaking proficiency.

Our sample includes workers aged 18 to 64. We use a single observation per worker from Wave 13, which is either month four (in 93% of cases) or the nearest month for those not employed in month four. As in the CPS, we keep workers who are not self-employed and whose hourly wages are between \$5 and \$100, and we drop observations with an imputed wage. An immigrant is defined as anyone born outside of the United States. At 21,269 observations, the SIPP sample is much smaller than either the CPS probability or wage samples. Unlike the CPS, we have only one SIPP sample that we use to explore licensing probability, wage premium, and hours worked.

Descriptive statistics of our SIPP sample are shown in Table A.2, which follows the same format as the descriptive table for the CPS. The credential rate is higher in the SIPP than the CPS, though a larger fraction of these credentials are licenses in the SIPP than the CPS, which leads to an overall lower licensing rate of natives in the SIPP (17.6%) than the CPS (19.8%). Otherwise, very similar patterns are observed in the SIPP as in the CPS: licensed workers are older, more educated, less likely to be immigrants, and have higher wages. Licensed immigrants have much higher English proficiency than unlicensed immigrants. While this is consistent with a lack of language proficiency imposing a barrier to obtaining a license, it does not, of course, control for other worker characteristics such as age or level of education.

In Table A.3, we take advantage of the richer certificate information provided in the SIPP to see if immigrants and natives are acquiring different types of licenses. For workers with a license, we show (separately for natives and immigrants) the fraction of licenses that (1) required coursework or training, (2) required a skills demonstration or exam, (3) require maintenance in the form of continuing education, and (4) were acquired for work or personal

Table 1.1: Summary Statistics, Probability Sample, CPS

	All		License		No License	
	Native mean	Imm. mean	Native mean	Imm. mean	Native mean	Imm. mean
Female	0.492	0.435				
Credentials:						
Credential	0.254	0.163	1.000	1.000	0.070	0.042
License	0.198	0.127	1.000	1.000	0.000	0.000
Certificate	0.056	0.036	0.000	0.000	0.070	0.042
Education:						
HS Dropout	0.045	0.222	0.016	0.062	0.052	0.246
HS Grad	0.270	0.243	0.138	0.157	0.302	0.255
Some Coll.	0.315	0.152	0.281	0.198	0.323	0.146
BA	0.245	0.205	0.280	0.290	0.237	0.193
GRAD	0.125	0.178	0.285	0.293	0.085	0.161
Race/Ethnicity:						
White	0.714	0.158	0.780	0.204	0.697	0.151
Black	0.133	0.118	0.104	0.215	0.140	0.104
Asian	0.020	0.284	0.018	0.337	0.021	0.276
Other	0.133	0.440	0.099	0.245	0.141	0.469
Hispanic	0.113	0.451	0.081	0.252	0.121	0.479
Other:						
Age	39.850	43.957	42.170	45.658	39.277	43.710
Married	0.505	0.695	0.623	0.725	0.476	0.691
# Children	0.783	1.189	0.967	1.181	0.738	1.191
YSM		15.051		16.548		14.833
Observations	382,161	46,294	78,510	5,858	303,651	40,436

Notes: Numbers are the mean of the variable. "YSM" refers to years since migration of immigrants. Source: Current Population Survey, 2016-2019.

reasons. We detect no major differences between natives and immigrants in any of these license characteristics. While only a small number of descriptives, these results suggest that the types of licenses acquired by natives and immigrants may not differ much in their characteristics.

1.3 Results

We begin by discussing the licensing rate results from both the CPS and SIPP in Section 1.3.1, and then proceed to discuss the relationships between licensing and both hourly wage and hours worked in Section 1.3.2, again for both the CPS and SIPP. The estimation models are as similar as possible between the two datasets, with notable differences including years since migration in the CPS, and language ability in the SIPP.

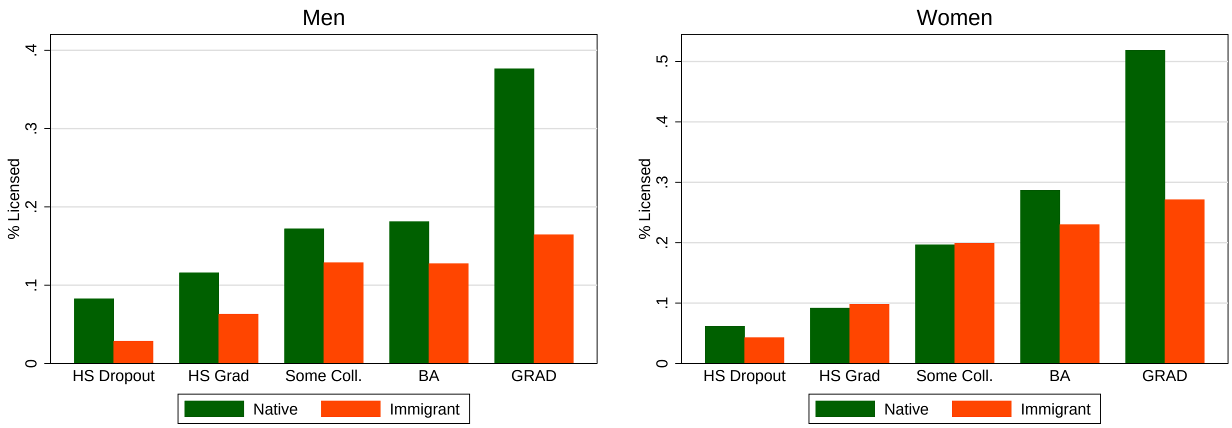
1.3.1 Probability of Having an Occupational License

We start by exploring the relationship between being an immigrant and the probability of being licensed. The summary statistics for both the CPS and SIPP indicate that licensed workers tend to be more educated than unlicensed workers,[18] though the licensing rate appears to increase more strongly with education among natives than among immigrants.

To visualize the relationship between licensing rates and educational attainment, Figure 1.1 shows the fraction of people at each education level who report having a license, separately for men and women as well as separately for natives and immigrants, from the CPS probability sample. The increasing trend of licensing with education for all groups is clear; however, licensing increases with educational attainment more rapidly for natives than immigrants. The native/immigrant licensing gap is substantially larger for the most educated group (GRAD) than any other, particularly for women. In fact, native and immigrant women exhibit little difference in their licensing rates across the other four education levels. Thus, it seems plausible that the of occupational licensing on the labor market may be more relevant for highly educated immigrants.

To give an idea of the type of occupations licensed workers perform, Table A.4 shows the ten most common occupations reported among licensed workers, again from the CPS probability sample. This is done separately for men and women as well as natives and immigrants for the full sample of licensed workers. Also, given the apparent relevance of the most educated group of workers in understanding the native/immigrant licensing gap, we

[18] See Kleiner and Vorotnikov (2017).



(a) Men

(b) Women

Figure 1.1: Occupational Licensing by Education Level, Natives and Immigrants

repeat this exercise but for only the workers with the most education.

For both natives and immigrants, licensed women are frequently in nursing or a closely related occupation: nearly a fifth of licensed immigrant women are registered nurses, a further 17% are nursing orderlies, 3.1% are licensed practical nurses, 4.8% are physicians, 1.9% are health aides, and 1.7% are physical therapists. Thus, licensed immigrant women are well represented in the medical industry. After nursing "hairdressers and cosmetologists" is the most common occupation category for licensed immigrant women at 8.6%, which is much higher than the rate for native licensed women (2.3%).

For workers at the highest education level with a license, native workers are much more likely than immigrants to be teachers; in total, 37% of native women with a GRAD level of education are some form of school teacher, while only 20% of immigrant women are teachers. For men, 23% of natives but only 11% of immigrants are teachers. So while likely not the full story, it seems that this occupation alone may be a large contributor to the significant native/immigrant licensing gap at the highest level of education; the data suggest that the teaching profession may be disproportionately unattractive or difficult to enter for immigrant workers. Native women with a license, in addition to also being frequently in nursing and closely related occupations, are much more likely than immigrants to be teachers.

On the other hand, while nursing is a common occupation among licensed workers, the rates are much lower for men than for women, especially for native male workers. Truck driver is a common occupation for both native and immigrant male licensed workers. Licensed immigrant men are much more likely to be physicians than native men, at 8.6% and 3.1%, respectively.

Considering the most educated group, where the native/immigrant licensing gap is most severe, we see for both immigrant men and women, physician is by far the most common occupation. Lawyer is the most common occupation among highly educated licensed male natives, but only 2.5% of highly educated licensed immigrant men are lawyers. Thus, for both men and women, while there are some interesting differences in the type of occupations that licensed workers engage in, there is a lot of overlap.

The descriptive statistics suggest large differences between natives and immigrants in their licensing rate. In order to statistically test whether, and to what extent, otherwise similar natives and immigrants differ in their probability of being licensed, we turn to our econometric model. We estimate a series of linear probability regressions where the dependent variable equals one if the individual reports having an occupational license, and zero otherwise:

$$License_{it} = \beta_X X_{it} + \beta_{Imm} Imm_i + \beta_{YSM} YSM_{it} + \beta_{YSM2} YSM_{it}^2 + \delta_s + \epsilon_{it} \tag{1.1}$$

where $License_{it}$ equals one if worker i has a license in month t, and zero otherwise; X_{it} is a vector of individual and other characteristics; δ_s is the fixed effect for state of residence s; and Imm_i equals one if the worker is an immigrant, and zero otherwise. Years since migration values (YSM_{it} and YSM_{it}^2) equal zero for natives. We control for state of residence, age (introduced as a third-order polynomial), educational attainment (five categories), survey year, month of sample (i.e., first or fifth month), month of the year, whether the individual appears once or twice in the sample, racial and ethnic dummy variables (Black, Asian, other, and Hispanic), a married dummy variable, and number of children.[19] This estimation

[19] Attrition between the first and fifth survey months may not be random; thus including the fifth month

is performed on the full sample, as well as separately for men and women.

The results from these estimations are shown in Table 1.2. As our focus is on immigration and licensing, our primary coefficients of interest are those on the immigrant dummy and years since migration, though we also show the coefficients for gender and educational attainment. The row Mean shows the mean of the dependent variable, (i.e., the fraction of the sample that has an occupational license).

First note the gender gap in licensing rates: controlling for other characteristics, women are 5.4 percentage points more likely than men to be licensed. While the focus of this paper is immigration and licensing, it is worth speculating briefly regarding this large gender licensing gap that has received little attention in the literature. In the presence of asymmetric information that might be more severe for women than men,[20] women may be more likely to acquire an occupational license, since a license may serve as a signal of worker characteristics such as ability or labor market attachment. Alternatively, it may be that occupations that are more attractive to women than men (possibly due to non-pecuniary characteristics like time flexibility), [21] are more likely to be licensed, so accessing these occupations requires holding a license. These crucial questions are left for future work to explore.

Consistent with Figure 1.1, educational attainment has a strong, positive relationship with licensing status; workers with a BA are 12.5 percentage points more likely than high school dropouts to be licensed, while workers with more than a BA (GRAD) are 31.1 percentage points more likely.

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results. With this in mind, we repeated the estimates using data from only the first month. The results were nearly identical.

[20]See Milgrom and Oster (1987) for a discussion of the Invisibility Hypothesis, and Cassidy et al. (2016) for an application of that hypothesis to promotion signaling by gender.

[21]See Goldin (2014) for a discussion of time flexibility and the gender pay gap within occupations.

Table 1.2: Linear Probability Models, Occupational License, CPS

	(1) All	(2) Men	(3) Women
Female	0.054***		
	(0.0014)		
Immigrant	-0.097***	-0.089***	-0.095***
	(0.0042)	(0.0050)	(0.0073)
YSM/10	0.036***	0.014**	0.057***
	(0.0054)	(0.0064)	(0.0094)
$YSM^2/100$	-0.004***	-0.000	-0.009***
	(0.0016)	(0.0018)	(0.0027)
Education:			
HS Grad	0.018***	0.022***	0.018***
	(0.0021)	(0.0027)	(0.0034)
Some Coll.	0.089***	0.075***	0.117***
	(0.0023)	(0.0030)	(0.0036)
BA	0.126***	0.079***	0.186***
	(0.0026)	(0.0033)	(0.0040)
GRAD	0.311***	0.232***	0.396***
	(0.0033)	(0.0044)	(0.0048)
Mean	0.189	0.156	0.224
Observations	428,455	217,081	211,374
R^2	0.088	0.059	0.111

Notes: Dependent variable is binary, and equals one if the worker has an occupational license, and zero otherwise. All estimations include controls for age (as a third-order polynomial), state of residence, industry controls (at the three-digit level), month in sample, month of the year, survey year, number of times individual is observed, marital status, number of children, full time dummy variable, government worker dummy variable, and racial and ethnic dummy variables (Black, Asian, other, and Hispanic). Occupation controls are at the three-digit level. Omitted education group is high school dropout. Row "Mean" shows the mean of the dependent variable. Source: Current Population Survey, 2016-2019. Standard errors in parentheses, and are clustered at the individual level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels.

The coefficient on the immigrant dummy variable is negative, statistically significant, and economically large on all estimations. Consistent with labor market assimilation, the probability an immigrant is licensed increases with years since migration: ten years after migration, the probability increases by 3.6 percentage points. In the full sample, we find that ten years after migration, immigrants are 6.5 (9.7-3.6+0.4) percentage points less likely to have an occupational license; given the overall rate of 18.9%, this corresponds to a 34.4%

lower probability.

Considering the results by gender, the licensing gap ten years after migration is 7.5 percentage points for men and 4.7 percentage points for women, so the licensing gap between natives and immigrants is greater for men than women. Furthermore, the overall licensing rate for women is quite a bit higher than men (22.4% versus 15.6%). In percentage terms, immigrant men and women, ten years after migration, are 48.1% and 20.1%, respectively, less likely to be licensed than natives. Also, the effect of years since migration is weaker for men than women (i.e., male immigrants seem to assimilate more slowly in terms of licensing status than female immigrants). Ten years after migration, the licensing probability increases by only 1.4 percentage points for men but by 4.8 percentage points for women, and thus the gender licensing gap for immigrants grows with years since migration.

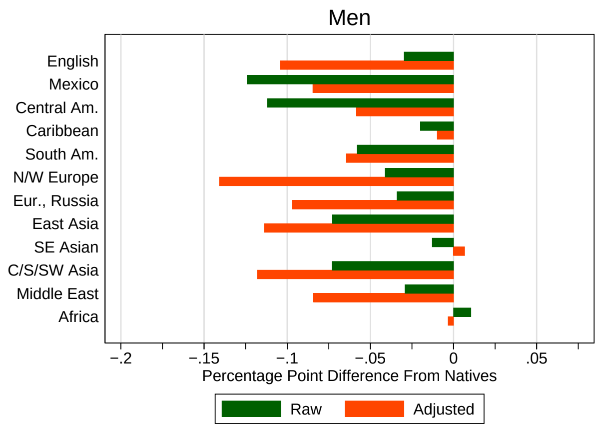
A prominent phenomenon among immigrant workers in the United States is occupational clustering-immigrants of the same nationality tend to work in similar occupations, especially when clustered in the same city.[22] A well-known example in the occupational licensing literature is Federman et al. (2006), which studies Vietnamese manicurists. In the presence of occupational clustering, where certain occupations may require a license, we might expect variation in occupational licensing rates by birthplace, as is the case for Vietnamese manicurists.

We explore this idea by dividing immigrants by region of birth.[23] Figure 1.2 shows the license rates of each region relative to natives, for men and women separately. Two values per region are shown: (1) the raw difference in licensing rate, and (2) the adjusted difference that accounts for worker characteristics. These adjusted differences are computed by adding region of birth to the regression specification in Equation (1.1), and then using the coefficients on these region dummy variables, adjusted such that years since migration equals 15, which is approximately the mean of the sample.

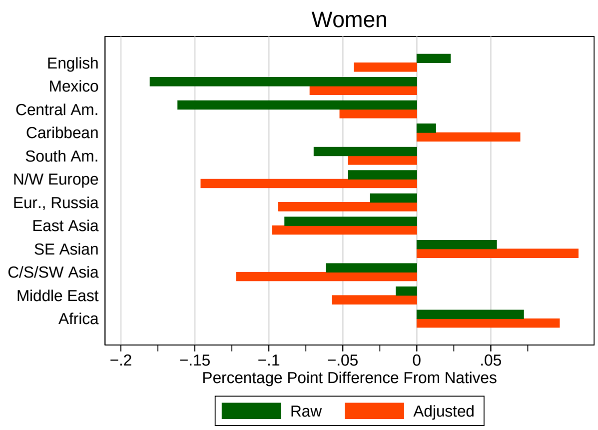
Licensing rates are lowest for immigrants from Mexico and Central America for both

[22]See Patel and Vella (2013).

[23]Regions are English-speaking developed countries (Canada, Australia, New Zealand, United Kingdom, and Ireland); Mexico, Central America, the Caribbean, South America, Northern and Western Europe, the rest of Europe and Russia, East Asia, Southeast Asia, Central/South/Southwest Asia, the Middle East, and Africa.



(a) Men



(b) Women

Figure 1.2: Occupational Licensing Rate Deviation from Natives by Immigrant Region of Origin

men and women. Interestingly, and consistent with occupational clustering, we find high licensing rates for immigrants from the Caribbean, Southeast Asia, and Africa. Immigrants from these regions, especially women, tend to work in occupations such as nursing, for which occupational licensing is more prevalent. For example, 12.0% of immigrant women from Southeast Asia work as "Miscellaneous personal appearance workers," a category that includes manicurists, while only 0.2% of native women work in that occupation.

Adjusting for worker characteristics expands the licensing gaps for many regions; the main driver for this difference appears to be educational attainment. For many regions of origin educational attainment is relatively high, and since licensing rates increase with educational attainment, these immigrants have larger adjusted licensing rates and thus a large gap relative to otherwise similar natives. For example, female immigrants from English-speaking developed countries have a mean licensing rate that is higher than rates for natives; however, their adjusted licensing rate is lower due largely to their high levels of education. Immigrants from Mexico, on the other hand, have a smaller adjusted licensing rate gap than their raw gap (though both are negative), due largely to their low average education levels.

Note that in addition to male immigrants from English-speaking developed countries, both male and female immigrants from Northern and Western Europe have low adjusted licensing rates. This finding is somewhat curious since immigrants from developed (especially English-speaking) countries may be more familiar with the US labor market than immigrants from other regions, and are the least likely to be illegal immigrants, which would make acquiring a license less difficult; nevertheless, they are substantially under-licensed relative to natives. One reason for this large adjusted gap is that a substantial fraction of immigrants from these regions are highly educated (around 52% for those from Northern and Western Europe), and so comparing them to otherwise-similar natives means comparing them to the most-educated (and as previously discussed, most-licensed) group of workers, leading to a large potential licensing gap that does indeed appear in the data.[24] This suggests that a lack of familiarity with the US labor market may not be a large contributing factor in explaining the gap in licensing rates between natives and immigrants.

We now turn to discuss the licensing probability results using SIPP data. Since many of the results are quite similar between the datasets, our discussion focuses on the novel contributions of the SIPP and the instances where the CPS and SIPP results diverge either in direction or substantially in magnitude.

We first estimate a linear probability regression model, as described in Equation (1.1), where the dependent variable equals one if the worker has a license, and zero otherwise. We include similar controls as the CPS probability estimations. Specifically, all estimations con-

[24] The fact that immigrant regions of origin with high average levels of education have the potential for high licensing gaps does not, of course, imply that a large licensing gap will appear. Recall that the adjusted licensing gaps shown in Figure 1.2 control for education. However, for immigrants from regions with low average levels of education, there is simply little scope for a large licensing gap with otherwise-similar natives, since even native workers with low levels of education have low rates of licensing.

trol for an immigrant dummy variable, state of residence, age (introduced as a third-order polynomial), educational attainment (five categories), racial and ethnic dummy variables (Black, Asian, other, and Hispanic), a married dummy variable, number of children, a union dummy variable, government worker dummy variable, service worker dummy variable (derived from industry code), and a paid-by-the-hour dummy variable. Notably, unlike the CPS, we cannot control for years since migration. We then add a dummy variable for immigrants' English proficiency.

Results are shown in Table A.5. Without language ability controls, immigrants are 4.8 percentage points less likely to have a license; given the overall licensing rate of 16.4%, this represents a 29.3% lower probability. Immigrants proficient in English are 23.2% less likely than natives to have a license, while immigrants who do not speak English well are 43.3% less likely to have a license.

Male immigrants are 5.6 percentage points (42.1%) less likely to have a license relative to natives, while female immigrants are only 3.6 percentage points (18.2%) less likely. Female immigrants who speak English very well are only 11.6% less likely to have a license than native women, while male immigrants who speak English very well are 35.3% less likely than male natives to have a license. The values for female and male immigrants who do not speak English well are 32.8% and 55.6%, respectively. The difference between the licensing rates of proficient and non-proficient English speakers differs at the 10% significance level for both men and women.

Comparing the SIPP and CPS results, we find that at the mean years since migration (about 15 in the CPS), the results are very similar at 29.3% lower probability in the SIPP and 27.5% lower probability in the CPS. Notably, the SIPP results provide suggestive evidence that language proficiency, or the lack thereof, is an important contributor to the native/immigrant licensing difference, especially for female immigrants.

We end our discussion of occupational licensing attainment and immigrants by investigating one potentially critical immigrant characteristic that we have thus far not discussed: citizenship. As documented in Calvo-Friedman (2014), there are numerous examples of states in which eligibility for particular occupational licensure is restricted to citizens or to citizens and permanent residents. For example, a funeral home director in Massachusetts is required to be a citizen, while a funeral home director in New York must be either a citizen or a permanent resident.[25] Thus, it seems quite plausible that a lack of citizenship may impede an immigrant's ability to acquire an occupational license.

Indeed, as expected, immigrants who are citizens are more than twice as likely to have an occupational license as non-citizens. However, citizens are also on average more educated and have been in the country longer; these traits are positively related to the probability of having an occupational license, as the results above show. To see the relationship between citizenship status and occupational licensing while controlling for other characteristics, we repeat the linear probability model estimation described in Equation (1.1), but we add a control for whether the immigrant is a citizen. Also, we include only immigrants in the estimation, since we are interested in whether citizen status affects licensing rates within the immigrant group, and for brevity we only include the results from the CPS.

The results are shown in Table 1.3 and include the full sample as well as the sample of men and women separately. We also repeat the estimation with and without the citizenship dummy variable, so each of our three samples has a pair of estimations. First, before discussing the citizenship results, it is worth noting that when focusing only on immigrants, the educational gradient in occupational licensing is much flatter than when considering all workers: immigrants with the highest level of education (GRAD) are only 15.9 percentage points more likely to be licensed than high school dropouts, whereas in the full sample that includes natives (Table 1.2), this value was 31.1 percentage points. This result mirrors what is shown in Figure 1.1.

[25]See Calvo-Friedman (2014), Appendix A, for many other examples.

Table 1.3: Linear Probability Models, Occupational License, Immigrants Only, CPS

	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Citizen		0.058***		0.051***		0.063***
		(0.0044)		(0.0056)		(0.0070)
Female	0.063***	0.059***				
	(0.0037)	(0.0037)				
YSM/10	0.051***	0.036***	0.035***	0.022***	0.064***	0.046***
	(0.0060)	(0.0060)	(0.0072)	(0.0072)	(0.0103)	(0.0103)
$YSM^2/100$	-0.009***	-0.007***	-0.005**	-0.004	-0.013***	-0.011***
	(0.0017)	(0.0017)	(0.0020)	(0.0020)	(0.0029)	(0.0029)
Education:						
HS Grad	0.030***	0.022***	0.029***	0.023***	0.038***	0.028***
	(0.0039)	(0.0039)	(0.0045)	(0.0045)	(0.0069)	(0.0070)
Some Coll.	0.097***	0.082***	0.086***	0.073***	0.121***	0.105***
	(0.0058)	(0.0058)	(0.0073)	(0.0073)	(0.0092)	(0.0093)
BA	0.122***	0.109***	0.096***	0.085***	0.163***	0.146***
	(0.0059)	(0.0059)	(0.0071)	(0.0071)	(0.0098)	(0.0098)
GRAD	0.159***	0.149***	0.135***	0.127***	0.207***	0.193***
	(0.0070)	(0.0069)	(0.0084)	(0.0083)	(0.0118)	(0.0118)
Mean	0.127	0.127	0.094	0.094	0.170	0.170
Observations	46,294	46,294	25,455	25,455	20,839	20,839
R^2	0.075	0.081	0.050	0.056	0.089	0.094

Notes: Dependent variable is binary, and equals one if the worker has an occupational license, and zero otherwise. Sample includes only immigrants. All estimations include controls for age (as a third-order polynomial), state of residence, month in sample, month of the year, survey year, the number of times individual is observed, marital status, number of children, and racial and ethnic dummy variables (Black, Asian, other, and Hispanic). Omitted education group is high school dropout. Row "Mean" shows the mean of the dependent variable. Standard errors in parentheses, and are clustered at the individual level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: Current Population Survey, 2016-2019.

Citizenship status, even in the presence of a rich set of other controls, is strongly related to occupational licensing attainment; in the full sample, immigrants who are citizens are 5.8 percentage points (45.7%) more likely to have a license than non-citizens. When looking at men and women separately, these values are 54.3% and 37.1%, respectively, suggesting that citizenship may be more relevant for male than female immigrants. Also, while years since migration continues to be informative even when controlling for citizenship status, the magnitude of the coefficients declines, suggesting that part of the "assimilation" process that years since migration captures is the naturalization process. Lastly, though not shown here, repeating these estimations for the SIPP we find that controlling for English proficiency only somewhat lowers the coefficient on the citizenship dummy variable. Thus it is unlikely that citizenship in the CPS is merely proxying for English language ability.

An obvious concern with these results is the strong potential for selection bias-immigrants who become citizens are probably not a random sample of the immigrant population. Since acquiring an occupational license is a costly investment, immigrants who acquire a license may also be the type who are likely to stay in the United States for a long period of time, perhaps permanently, and thus are also more likely to become citizens. It is beyond the scope of this paper to address this important issue. However, two things are clear: (1) many states restrict occupational licensing to only citizens and permanent residents for certain occupations, and (2) non-citizen immigrants are much less likely to be licensed than otherwise similar citizens. Using data from the 1990s, Bratsberg et al. (2002) find that naturalization of US immigrants accelerates their wage growth and reduces employment barriers. The increasing prevalence of occupational licensing in the economy since that time suggests that the returns to naturalization may have risen, a topic that deserves closer attention.

1.3.2 Licensing, Wages, and Hours Worked

We turn now to the relationship between occupational licensing and labor market outcomes, specifically wages and hours worked. We estimate the following regression model:

$$Y_{it} = \beta_X X_{it} + \beta_{Lic} License_{it} + \beta_{Imm} Imm_i + \beta_{LicXImm} License_{it} * Imm_i + \delta_s + \epsilon_{it} \tag{1.2}$$

where Y_{it} is the labor market outcome of interest (i.e., either log wage or hours worked) for individual i in month t, $License_{it}$ is a dummy variable that equals one if the worker has a license in month t, Imm_i is a dummy variable that equals one if the worker is an immigrant, X_{it} is a vector of control variables, and δ_s is the fixed effect for state of residence s. The interaction term $License_{it} * Imm_i$ tests if the effect of having a license differs between immigrants and natives.

The baseline estimation includes the same set of controls used in the license probability regressions. In addition, we include a number of other job-related controls that are not introduced in the probability estimations. These include controls for whether a worker is full-time or part-time, paid-by-the-hour, a union member, or a government worker. We again perform these estimates on the full sample as well as separately for men and women. For brevity, only the immigrant dummy variable and the occupational licensing variables are shown. Table 1.4 shows the CPS results for both log wage (Panel A) and hours worked (Panel B), while Table A.6 shows the SIPP results, again for both log wage and hours worked.

We begin by discussing the log wage results from the CPS (Table 1.4, Panel A). The licensing coefficient is positive and statistically significant in all specifications. In the full sample without interacting licensing status with the immigrant dummy, we find a wage premium of 8.5%, which is somewhat low relative to the existing results in the literature, though it is higher than the results from Gittleman et al. (2018) and our own results from the SIPP.[26] The licensing premium is much higher for women (12.3%) than men (4.5%), a finding that has received little attention in the literature.[27]

The immigrant and license interaction terms are positive, statistically significant, and meaningfully large in all three samples: the wage premium for immigrants is 14.8% (7.8+7.0) for the full sample, while for natives it is only 7.8%, with the difference between these results significantly different at the 0.1% level. Considering the male and female samples separately, we find that immigrant men earn a much larger licensing premium than native men (3.7% versus 12.0%), while immigrant women also earn a higher premium than native women (11.8% versus 17.7%).

[26] See Kleiner and Krueger (2013), and Pizzola and Tabarrok (2017).

[27] Blair and Chung (2017) find evidence of a higher return to occupational licensing for women than men using the 2008 SIPP, which they argue is due to higher returns to human capital acquired with a license.

Table 1.4: OLS Regressions, Log Hourly Wage and Usual Hours Worked Per Week, CPS

Panel A: Log Hourly Wage

	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Immigrant	-0.179***	-0.186***	-0.175***	-0.181***	-0.183***	-0.190***
	(0.0098)	(0.0099)	(0.0137)	(0.0137)	(0.0138)	(0.0139)
License	0.085***	0.078***	0.045***	0.037***	0.123***	0.118***
	(0.0033)	(0.0033)	(0.0049)	(0.0050)	(0.0044)	(0.0045)
License x Imm		0.070***		0.083***		0.059***
		(0.0126)		(0.0192)		(0.0167)
Observations	212,184	212,184	107,483	107,483	104,701	104,701
R^2	0.479	0.479	0.484	0.484	0.461	0.461

Panel B: Usual Hours Worked Per Week

	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Immigrant	-0.595***	-0.627***	-0.718***	-0.670***	-0.292	-0.401
	(0.1619)	(0.1622)	(0.2229)	(0.2224)	(0.2314)	(0.2329)
License	1.074***	1.044***	1.642***	1.699***	0.649***	0.565***
	(0.0574)	(0.0594)	(0.0921)	(0.0962)	(0.0713)	(0.0733)
License x Imm		0.338		-0.641**		0.907***
		(0.2087)		(0.3218)		(0.2724)
Observations	212,184	212,184	107,483	107,483	104,701	104,701
R^2	0.382	0.382	0.289	0.289	0.438	0.438

Notes: Dependent variable is log of hourly wage in Panel A and usual hours worked per week in Panel B. All estimations include controls for age (as a third-order polynomial), state of residence, educational attainment (five categories), years since migration, years since migration squared, month in sample, month of the year, survey year, the number of times individual is observed, marital status, number of children, full time dummy variable, government worker dummy variable, union dummy variable, paid-by-the-hour dummy variable, and racial and ethnic dummy variables (Black, Asian, other, and Hispanic). Source: Current Population Survey, 2016-2019.
Standard errors in parentheses, and are clustered at the individual level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$, $^{***}p < 0.001$

It is unclear why immigrants appear to benefit more than natives from holding an occupational license. One possible explanation is that, because an occupational license is a costly investment, it serves as a signal, and the value of that signal is greater for immigrants than natives. US employers likely have less information about immigrants than they do about natives, (e.g., about the quality of their educational credentials), and thus a license is a stronger signal for immigrants than natives. Alternatively, an occupational license may signal a lower probability of return migration and thus a stronger attachment to the US labor force, which may make employers more likely to train immigrant workers with a license relative to immigrants without a license.

Due to the positive interaction term between immigrant status and licensing status, the wage gap between licensed natives and immigrants is lower than the wage gap between unlicensed natives and immigrants. For unlicensed workers, the native/immigrant wage gap is 18.6%; however, comparing native and immigrant licensed workers, this wage gap narrows significantly to only 11.6%. Thus, immigrants able to acquire an occupational license do relatively well in the labor market compared to native workers. This result mirrors Blair and Chung (2017), which finds that occupational licensing can help to narrow the black/white wage gap.

To estimate the effect of occupational licensing on wages in the SIPP (Table A.6, Panel A), we repeat a similar log wage regression as described in Equation (1.2) with the same control variables on the SIPP linear probability estimations described in the previous section. In the full sample, we find a return to holding a license of 6.5%. Interacting the license and immigrant controls, we find a positive coefficient, which is consistent with the CPS results: immigrants with a license earn 8.8% higher wages, while natives with a license earn 6.2% higher wages. The interaction term, however, is not statistically significant at conventional levels, so we cannot rule out that natives and immigrants receive the same wage premium from having a license. Nonetheless, adding immigrant language proficiency controls results in the immigrant-license interaction term to be negative. This result suggests that holding a license may partly serve to proxy for English proficiency, which is consistent with the large, positive returns to English language proficiency for immigrants found in the literature.[28]

[28]See Bleakley and Chin (2004), among many others, for evidence that English proficiency improves earnings.

Considering men and women separately, as in the CPS, we find a much larger licensing wage premium for women (11.0%) than for men (0.1%). In fact, we find no evidence of a licensing premium for men. Contrary to the wage results from the CPS, neither of the license and immigrant interaction terms are statistically significant at conventional levels, though both are positive. Adding English proficiency again results in the coefficient of the immigrant and licensing interaction term to become negative for both men and women, consistent with holding a license proxying for English ability.

Our wage results, using both datasets, identify a positive and statistically significant license premium. However, these estimates may not fully capture the relationship between licensing and worker earnings if hours worked is related to licensing status. Panel B of Tables 1.4 and A.6 repeats the log wage analysis from Panel A, with usual number of hours worked per week replacing log wages as the dependent variable.

In the CPS, we find that workers with a license work 1.1 more hours per week than those without a license. Interacting licensing status with immigrant status, we find a positive though statistically insignificant interaction term. Repeating these estimates separately for men and women, we find a much stronger (more positive) relationship between having a license and hours worked for men than women, though for both men and women the coefficient is positive and statistically significant. This larger effect of licensing on hours worked for men than women should, to some extent at least, offset the much larger wage premium for women than men found in Panel A. One possible explanation for the overall positive licensing status and hours worked relationship is that workers who would tend to work more hours (for some exogenous reason such as a relatively low preference for leisure) have additional incentive to acquire a license to boost their hourly wage.

The immigrant-license interaction term is large, negative, and statistically significant for men, which again would serve to reduce the overall earnings effect of having a license on immigrants versus natives. For women, the immigrant and license interaction term is positive and statistically significant at conventional levels, thus the larger wage premium for licensed immigrant women found in Panel A understates the difference in the effect of holding a license on earnings between immigrant and native women.

Turning to the SIPP hours worked results (Table A.6, Panel B), we find that as with the CPS result, there is a positive relationship between hours worked and having a license, and this positive relationship is also larger for men than women. Of note, the magnitude of the coefficient on licensing status is lower in the SIPP results than in the CPS results. The interaction terms between licensing status and immigrant status are negative, though they are statistically insignificant for men in all specifications, and only marginally significant for women. Nonetheless, the SIPP and CPS differ notably in the relationship between immigrant women who hold a license versus those who do not.

Overall, while there are some differences between the SIPP and the CPS results in terms of hours worked, we find evidence that licensed individuals work more hours per week than unlicensed workers. This seems to suggest that the wage premium from being licensed likely understates the earnings premium from holding a license.

1.4 Further Discussion and Conclusion

We use two sources of data-the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP)-to explore the differences in occupational licensing between natives and immigrants. Each dataset provides unique advantages, allowing us to paint a clearer picture of how occupational licensing differs between natives and immigrants than would be possible by using either dataset alone.

Though the CPS and SIPP differ in some key ways, where comparable our results are quite similar between the two datasets. We find that immigrants are significantly less likely to have an occupational license than natives; this gap is larger for men than for women and is especially large for the highest education level. The wage premium from having a license may not differ between natives and immigrants when controlling for English language ability, suggesting that though immigrants are less likely to have a license, they seem to benefit at least as much as natives from having one. Licensed workers tend to work more hours per week than otherwise similar unlicensed workers, so the wage premium understates the earnings premium.

Using the CPS, we find that the native/immigrant licensing gap declines with years since migration, consistent with immigrants assimilating toward natives. We also find large differences in licensing rates by region of origin; in particular, women from the Caribbean, Southeast Asia, and Africa have a higher probability of having a license than otherwise similar natives.

Using the SIPP, we find that a lack of English language proficiency lowers the probability than an immigrant has a license, even when controlling for other individual characteristics such as education level. Utilizing the richer set of occupational licensing questions available in the SIPP, we find no evidence to suggest that license characteristics differ between natives and immigrants, and thus we find no evidence that natives and immigrants are acquiring different types of licenses.

Our results suggest that occupational licensing disproportionately affects immigrants, especially male immigrants, those lacking English proficiency, and the most educated group. Indeed, insofar as occupational licensing helps to protect incumbent (largely native) workers in an occupation from competition, it is unsurprising that immigrants are particularly impacted. Results from both the SIPP and CPS indicate that among licensed workers, the native/immigrant wage gap is smaller than among unlicensed workers, though evidence from the SIPP suggests that this may be due in part to licensing status proxying for English language ability which is unmeasured in the CPS.

Given the substantial increases in both the prevalence of occupational licensing as well as the fraction of the US workforce that comprises immigrants, and given the apparent difficulty that immigrants have in acquiring occupational licenses, our results provide what we believe to be important insights into a largely underexplored aspect of the occupational licensing debate. Furthermore, our results suggest that the impacts of these licenses are heterogeneous in a number of important dimensions, including by education level and region of origin. Understanding how occupational licensing affects different groups of workers (e.g., men versus women and natives versus immigrants) is important in providing a clear picture of the impact of occupational licensing in its entirety.

One important characteristic of immigrant workers in the United States is that they are a more mobile group than natives, and thus help labor market efficiency by making locational choices based on wage differences across locations. This is especially true for newer immigrants. In the words of Borjas (2001), immigrants are "grease on the wheels of the labor market." Occupational licensing requirements, however, have the opposite effect: Johnson and Kleiner (2017) find evidence that between-state migration rates are significantly lower for workers in occupations with state-specific licensing exam requirements compared to workers in other occupations. Since occupational licensing appears to more strongly impact immigrants, licensing requirements may serve to diminish this important role that immigrants play in the economy, leading to lower labor market efficiency and greater interstate wage inequality. Put differently, if immigrants find it more difficult to acquire an occupational license (as our results suggest they do), then this otherwise highly mobile group of workers will be less able to respond to work opportunities across state lines if these opportunities are in licensed occupations.

Lastly, our results have important implications for potential changes to immigration policy, particularly movements toward more skill-based immigration.[29] Skill-based immigration would favor immigrants with high levels of education. Our results indicate that it is precisely this group that exhibits the largest licensing attainment gap with natives. Increasing the flow of immigrants from this education level may lead to substantial occupational mismatch for this group of immigrants if they face difficulty in acquiring licenses needed to work in their pre-migration occupations. As policymakers discuss the movement to a more skill-based immigration system, they should carefully consider the issue of occupational licensing for immigrants in order to avoid occupational mismatch, depressed earnings, and loss of economic efficiency, all of which would undercut the potential advantages of high-skilled immigration.

[29] Canada and Australia currently use a points-based immigration system that determines immigration eligibility partly based on criteria such as age, English (or French) language ability, and educational attainment. In the United States; the Reforming American Immigration for Strong Employment (RAISE) Act (S. 354, revised version S. 1720) introduced by Senators Tom Cotton and David Perdue in 2017 would introduce a points system for potential immigrants to the United States, also based in part on age, educational attainment, and English ability.

Chapter 2

Intergenerational Effects of Enhanced Sentencing

2.1 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 et al. (2015)). Due to this increase, there has been a corresponding increase in the number of children with incarcerated parents. Glaze and Maruschak (2008) report that 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 US 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 is associated with

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

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 mass incarceration in the United States most affects socially and economically disadvantaged children and families (Ewert et al. (2014) and Wakefield and Wildeman (2013)). Thus, children with incarcerated parents often suffer socio-structural disadvantages that may foster worse educational attainment and lower income. As a result, Noyes et al. (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 '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 examines the impact of sentence length. Identification comes from variation in timing and whether states implement Three Strikes policies. Between 1993 and 1996, 25 states and the federal government passed '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

[2]Additionally, The Pew Charitable Trusts (2010) report argues based on the law of transitivity that children are negatively impacted.

[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.

credited with contributing to mass incarceration in the United States.[5]

Using American Community Survey (ACS) data and a difference-in-differences method, we estimate changes to long-run outcomes for children. Since the criminal justice literature reports that some states used their laws much more often in practice than others, we estimate a separate treatment effect for high use states and low (or never) use states. Passing a law and implementing a law are two fundamentally different treatments, so we expect the effect is larger in states that actually used their law. We find that states that rarely or never used their statutes did not experience changes, but states that actually used their laws had children who moved from working part-time to working less than ten hours per week, yet had increased yearly earnings and household income. The results are entirely driven by males. Among males, there is decreased college attendance, a decrease in employment and shifts between part time work and working less than ten hours per week. We also find that males drive the increase in earnings and household income. We test for heterogeneous effects by race and find that Black males are less likely to be employed, but have higher earnings. However, the higher earnings are driven by Black males who attended college (regardless of graduation) as there is no increase among less educated Black males. Thus, there are hetergeneous impacts among Black males.

The rest of this paper is organized as follows. In Section 2.2 we provide an overview of Three Strikes laws, and Section 2.3 provides a discussion of related literature. Section 2.4 discusses the data used to estimate impacts on adult sentence lengths and children's long-run impacts. Section 2.5 describes the methodology. Section 2.6 discusses the results, first showing an increase in the probability an adult offender receives a sentence of 25 year or more and then showing the long-run impacts on children. Section 2.7 concludes and discusses the importance of our results for public policy.

[5] Zimring et al. (2001) describe Three Strikes in California as the most important effort to achieve an abrupt increase in criminal punishment in modern times.

2.2 Institutional Details

This paper extends the research on the intergenerational effects of parental incarceration by using Three Strikes law as exogenous variation. To accurately describe the impacts of Three Strikes laws on prisoner's sentence lengths it is important to discuss the defining features of these laws. 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 et al. (1997) posit that although these statutes share the same title, 'Three Strikes 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 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 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 B.1 provides more details on the nature of this sentence enhancing policy in each state as well as the year in which the law was adopted.

Following implementation, we expect the most dramatic changes to the criminal justice system to occur in California, 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. Two-thirds of California's strikers are imprisoned for non-violent offenses, much of which is driven by burglars (Zimring et al. (2001)).

In a review of Three Strikes policy on the US prison population Zhang and Vaughn (2009) highlight Dickey and Hollenhorst (1999) argument that the impact of Three Strikes law on prison and crime across the US had little national impact because few offenders outside of California, Florida and Georgia were ever sentenced under this provision. Additionally, Neal and Rick (2014) 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 were rarely used. 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.

2.3 Literature Review

Several studies investigate the effects of Three Strikes law on crime, with mixed results. For example, Kovandzic et al. (2004) 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 law. This is in contrast to an enhanced sentencing policy in the Netherlands which was found to have reduced crime (Vollaard (2012)).

Stolzenberg and D'Alessio (1997) find there was no reduction in serious crime or petty theft rates due to California's Three Strikes 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 Strikes states.

However, these papers did not focus specifically on individuals 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. Iyengar (2008) finds that California's Three Strikes law reduced criminal participation among those who would be eligible for their second and third strikable offense. 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 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 test whether there are changes in children's adult labor market outcomes. Given the variation across states in the severity and usage of the policy, we expect to find larger impacts in states that actually used their laws.

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

[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.

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, we build upon previous work related to the impacts of sentence lengths by showing they have long-run impacts on children.

Four recent or concurrent working papers seek 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, Dobbie et al. (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 et al. (2018c) 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 children 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 in terms of end of grade exams and behavioral measures from parental incarceration. Cho (2009) examines the impact of an incarcerated mother on a child's outcomes in Chicago by comparing outcomes for 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, Norris et al. (2018) use variation in judge harshness to look at spillover effects on siblings and children in Ohio. They find that parental incarceration decreases the child's likelihood of being incarcerated and increases the socioeconomic status of the neighborhood they live in.[7] We contribute to the literature by showing sentence length also matters.

Our research also relates to the literature that more broadly looks at the impact of spillovers related to incarceration and crime. Bhuller et al. (2018b) 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. 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.

Lastly, previous research suggests that the age at which a treatment occurs impacts the magnitude of the effect on children. Cunha and Heckman (2007) argue that early childhood investment is more productive and complementary with later investment. Knudsen et al. (2006) cites evidence from economics, neurobiology, and sociology to also show that some essential skills are developed in early childhood.

There is much empirical work to support the importance of early childhood. For example, Chetty et al. (2016a) study the Moving to Opportunity experiment and find that younger kids (those who moved before they were 13 years old) benefited from the program in terms of increased income and college attendance. The impacts for older children were slightly negative. Hoynes et al. (2016) use variation across time and counties in the implementation of the Food Stamps Program to look at whether exposed children had different incidences of metabolic syndrome and different levels of economic self-sufficiency. They find that the treatment effects become smaller with age at program introduction; thus, younger children experienced a significant decline in metabolic syndrome while older children did not. Bald et al. (2019) looks at the impact of removing children from neglectful homes using variation in child protective service harshness as an IV. They find there are improvements in test scores for

[7] Aizer and Dolye (2015) 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.

young girls, but not older girls or boys at any age. Thus, the impact of enhanced sentencing could also potentially vary by the age of the child when the policy was implemented.

2.4 Data

To test for an effect of this policy on sentence length across states, we use offender level data to examine changes in the probability that an adult offender receives a maximum sentence of 25 years or more in high usage strike states versus all other states. We use two different definitions of high usage states - the top three states of California, Georgia and Florida which are identified by the criminal justice literature, and states that used the law for at least 0.01 percent of total admissions in 1998 (previous group plus Maryland, Nevada, Washington, Virginia, North Dakota, and Vermont).

Data for this section of the analysis are 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 1974 and 2008 following conviction of a new offense (337,890 inmates from 43 states and the District of Colombia). Summary statistics can be found in Table B.2.

We also check to see whether Three Strikes impacts arrest rates to make sure our long-run impacts on children are driven by changes in sentence lengths and not changes in arrests rates. To do this, we use data from the FBI Uniform Crime Reporting (UCR) system, which 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 (2003)). In 1994, the National Archive of Criminal Justice Data 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. We therefore focus on the treatment effect in states that adopted Three Strikes in 1995 and 1996. All other treatment states were dropped from the analysis. Demographic variables characterizing the population 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 (Non-Hispanic Black, Non-Hispanic White, Hispanic, and 'other').[8]

To examine long-run impacts on children, we use data from the 2000 to 2017 American Community Survey (ACS), drawn from the Integrated Public Use Microdata series (see Ruggles et al. (2018)). Our sample includes individuals who are 27 years old. To get accurate estimates for labor market outcomes, the age we use in our analysis needs to be old enough so that individuals have had a sufficient time to complete schooling. Given that we have data from 2000, the age we choose must allow us to have enough birth cohorts prior to implementation in order to have a sufficiently sized control group. Similarly, if we pick too old of an age, there will not be enough kids who were young when the laws were passed to be able to precisely estimate treatment effects across age groups. Thus, we chose age 27.[9]

We use the state of birth question in the ACS to determine if the individual was raised in the $Used$, $Unused$ or control states. We assess the effect of exposure to enhance sentencing laws by comparing individuals born in Three Strikes states (treatment group) with those born in non-strike states (control group) using reports of labor market activity. In particular, we compare the treatment and control groups using labor force participation, employment status, the intensity of employment (based on hours worked), earnings and household income. While not the main focus of the paper, Appendix B also explore non-financial outcomes such as education attainment, teen pregnancy and family formation.

Following Arcidiacono et al. (2016), we classify workers based on reports of usual hours worked. Individuals are defined as part-time workers if they report working at least ten

[8]Data on the racial distribution within county over time are obtained from the Census Bureau. Specially, we collect race data from the US Intercensal County Population Data compiled by NBER.

[9]Comparing individuals at age 27 makes our results more comparable to Chyn (2018), which estimates impacts on children forced to move due to housing demolition.

hours per week but less than thirty five hours per week. Full-time employment is defined as working at least 35 hours per week. Working between zero and ten hours is classified as home production.

2.5 Methodology

Recent papers investigating the spillover effects of adult imprisonment on children's outcomes use judge leniency as an instrument for estimating the probability of incarceration. However, this instrumental variable is not well suited in an analysis of sentence length and the likelihood of receiving enhanced sentencing. This assertion is supported by research analyzing judge leniency pre and post the implementation of a system of binding Sentencing Guidelines. Specifically, Anderson et al. (1999) examine the impact of sentencing guidelines on judge leniency by comparing the average length of prison sentences of criminal defendants in federal district courts before and after implementation of Sentencing Guidelines in the 1980's. They conclude that inter-judge disparity in nominal sentencing is less pronounced in the Guidelines era, and that it fell to an average difference in sentence length of only 3.9 months. This suggests that Sentencing Guidelines have been successful in reducing inter-judge sentencing disparity and hence would not be a good source of exogenous variation in our post-1990 analysis. So, while it would be preferable to have individual level variation, we instead must take advantage of state level variation in implementation.

To identify the causal effect of Three Strikes on children's long-run outcomes, we use an event study framework which compares the probability an adult offender is sentenced to twenty five years or more. Specifically, we test whether the implementation of Three Strikes laws lead to children being more likely to be separated from adults for a larger fraction of childhood (relative to children born before implementation). After establishing an increase in sentence lengths among adults, we use a difference-in-differences model to identify the causal effect of exposure to enhanced sentencing laws (defined here as being raised in a state that implemented Three Strikes law) on children's long-run outcomes. In what follows we provide more details on the two parts of our analysis (adult impacts and child impacts).

We estimate an event study regression for the probability of receiving a sentence length of twenty five years or more by using yearly offender level data from 1989 and 2000. To achieve this, we use the following event study specification:

$$Y_{ist} = \sum_{k=-5}^{5} \gamma_k (S_{t+k}) + X'_{ist}\beta + \eta_s + \lambda_t + \epsilon_{ist} \tag{2.1}$$

where S_{t+k} is a series of dummy variables that captures the number of years before and after the implementation of Three Strikes laws 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. We test this for both definitions of high usage. 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 offenses, 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). Standard errors are clustered by state.

For the analysis of long-run impacts on children, we use a difference-in-differences approach with both definitions of high usage states (captured by the variable $Used$). Standard errors are again clustered at the state level. Specifically, we estimate the following model:

$$Y_{isb} = \alpha_1 * exposure_{sb} * Used_s + \alpha_2 * exposure_{sb} * Unused_s + X'_i\alpha_3 + \lambda_b + \eta_s + \theta_s b + \epsilon_{isb} \tag{2.2}$$

where Y_{isb} is the outcome of interest (different measures of labor supply, employment, annual earnings, and annual household income) for person i in state s and birth cohort b. $Exposure_{sb}$ is a dummy variable, which takes a value of one if an individual born in year b was legally a child when state s implemented Three Strikes laws, it takes a value of zero otherwise. Thus, the control is those just too old to be children when the laws went into place. $Used$ 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 use two different definitions of $Used$: (1) the top three states (California, Georgia and Florida) which is based on the criminal justice literature, and (2) states where usage exceeded 0.01 percent of all admissions in 1998. 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_i is a vector of individual characteristics such as race, ethnicity, and gender. The standard errors are clustered at the state level since that is the level at which the policy was implemented. We use logistic regression models to estimate the treatment effects for models with binary dependent variables (for example, being in the labor force, or being employed). While we use linear regressions for our earned income and household income variables, we use a multinomial logistic regression for categories of hours worked (full time versus part time versus specializing in home production).

2.6 Results

2.6.1 Sentence Lengths for Adults

Table 2.1 presents estimates from the event study analysis of the impact of Three Strikes laws in high usage states on the probability an offender receives a prison sentence of twenty five years or more. There is no apparent pattern in the probability of receiving a sentence of twenty five years or more prior to the implementation of Three Strikes laws. Table 2.1 shows that the lead coefficients are never significant while the lag coefficients are larger in magnitude and statistically significant. Hence the event study provides no evidence that the probability of receiving a sentence of twenty five years or more differs significantly between the high usage states and other states in the absence of the policy. Estimates in Column 1 of Table 2.1 suggest that the probability of receiving twenty five years or more increased significantly one year post implementation in California, Georgia and Florida (see Panel A of Table 2.1). The treatment effect remained significant and increased in magnitude up to five years after adoption and did not vary much when we expand the definition of high usage states in Panel B of Table 2.1.

Borusyak and Jaravel (2016) discuss identification problems in using unit fixed effects, time fixed effects and linear time trends. They propose that one restricts pre-trends such that one starts with a fully dynamic framework, and then drop any two terms corresponding to the pre-trend. Then run an 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 method is implemented in Column 2 of Table 2.1. Based on results from the F-test, we cannot reject the null hypothesis that the coefficients on the pre-periods are statistically the same. Specifically, for Panel A the F-test result is 0.6863, and for Panel B the result is 0.7512. These estimates provide further evidence that there are no significant pre-trends in sentence lengths.

Table 2.1: Comparison of States' Three Strikes Law

	Panel A: Top Three Used States: CA, GA, FL	
Treatment	Column 1	Column 2
S_{t-5}	0.021 (0.065)	0.031 (0.051)
S_{t-4}	-0.003 (0.037)	0.008 (0.026)
S_{t-3}	-0.018 (0.022)	omitted
S_{t-2}	-0.011 (0.012)	-0.003 (0.004)
S_t	0.014 (0.010)	0.021 (0.017)
S_{t+1}	0.043*** (0.012)	0.050* (0.019)
S_{t+2}	0.063*** (0.017)	0.070** (0.023)
S_{t+3}	0.074*** (0.020)	0.082** (0.026)
S_{t+4}	0.083** (0.024)	0.091** (0.030)
S_{t+5}	0.092** (0.029)	0.099** (0.033)
Number of Obs	1,629,662	1,572,048
	Panel B: State's Three Strikes use is >0.01 *of admissions in 1998*	
Treatment	Column 1	Column 2
S_{t-5}	0.020 (0.069)	0.031 (0.055)
S_{t-4}	-0.006 (0.038)	0.005 (0.026)
S_{t-3}	-0.020 (0.023)	omitted
S_{t-2}	-0.012 (0.012)	-0.004 (0.004)
S_t	0.016 (0.011)	0.023 (0.018)
S_{t+1}	0.046** (0.013)	0.053* (0.021)
S_{t+2}	0.064** (0.018)	0.070** (0.025)
S_{t+3}	0.074** (0.022)	0.080** (0.028)
S_{t+4}	0.083** (0.027)	0.090** (0.033)
S_{t+5}	0.091** (0.034)	0.093* (0.037)
Number of Obs	1,629,662	1,554,098

Notes: Dependent variable is binary, and equals one if the offender receives maximum sentence of 25 years or more, and zero otherwise. All linear probability models include controls, state fixed effects and year fixed effects and standard errors are clustered by state. Standard errors are in parentheses and * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Column 2 applies the Borusyak and Jaravel (2016) test, in which the sample decreases because we drop the observations from more than five years prior or more than five years after to create the full dynamic framework (this means you cannot aggregate time periods). For the top section (CA, GA, & FL) the F-test result is 0.6863, and for the bottom section (use is > 0.01 of admissions in 1998) the result is 0.7512. These results indicate no evidence of pre-trends.

To provide further evidence that the estimated effects on children are due to changes in sentence lengths, and are not being confounded with changes in arrest rates, we test for changes in arrest rates separately. We find no significant increase in arrest rates in the treatment states relative to rates in the control states. More details on the method and results can be found in Appendix A.

2.6.2 Impacts on Children

We now turn to a discussion of our main results, which focus on the causal effects on children's long-run outcomes due to exposure to enhanced sentencing laws. Our main outcomes of interest are labor force participation, employment, annual earnings and household income.

Before we estimate changes in labor supply on the intensive margin, we examine whether there are impacts on the extensive margin. For this analysis, we use logistic regressions to estimate Equation (2.2), and we present marginal effects in Table B.3 for ease of interpretation. Column 1 of Table B.3 provides the estimated difference in the probability of being a part of the labor force (the extensive margin), while Columns 2 and 3 of Table B.4 present the estimated difference in the probability of being self-employed, and the probability of being a part of the armed forces, respectively. Column 1 of Table B.3 shows no statistically significant difference in the probability of being in the labor force when we compare individuals born in high usage states relative to individuals born in states that did not adopt a Three Strikes law. Similarly, we find no significant difference when we test whether workers have different types of labor force participation (self-employed relative to salary-worker, or working in the armed forces relative to civilian employment) due to being raised in a state with a Three Strikes law (see Columns 2 and 3 of Table B.3, respectively).

Given the previous null results, we estimate changes in the labor supply by conditioning our sample on being a part of the labor force, being a salaried worker, and being part of the civilian population. First, we test for changes in the probability of being employed. Since this outcome variable is a binary indicator, we estimate logistic regressions using the different definitions for high usage states, and present marginal effects in Column 1, Panels A and

B of Table 2.2. Across definitions of high usage states, we find no statistically significant difference in the probability of being employed. Consequently, we further restrict our sample to being employed to see if workers shift between full-time work, part-time, work and home production (working for less than ten hours per week but still employed).

Table 2.2: Labor Market Outcomes: Standard Diff-in-Diff

	Panel A: Top Three Used States: CA, GA, FL				
Variable	Employed	Part-time	Home Production	Earnings	H.H. income
Used	-0.0043	-0.0133*	0.0027	0.2110***	0.2876**
	(0.0079)	(0.0073)	(0.0018)	(0.0804)	(0.1178)
Unused	0.0008	0.0001	0.0019	0.1169	0.0978
	(0.0058)	(0.0081)	(0.0024)	(0.0734)	(0.1323)
	Panel B: State's Three Strikes use is >0.01 *of admissions in 1998*				
Variable	Employed	Part-time	Home Production	Earnings	H.H. income
Used	0.0004	-0.0145*	0.0044	0.2319***	0.3798***
	(0.0066)	(0.0074)	(0.0026)	(0.0732)	(0.1437)
Unused	-0.0023	0.0042	0.0004	0.0795	-0.0171
	(0.0063)	(0.0080)	(0.0023)	(0.0743)	(0.1051)
state fixed effects	X	X	X	X	X
birth cohort fixed effects	X	X	X	X	X
state specific linear time trend	X	X	X	X	X
Observations	308,610	285,481	285,481	285,481	285,481

Notes: Entries in Column 1 are average marginal effects from a logit, while Columns 2 and 3 include average marginal effects from a multinomial logit. For the multinomial logit regression model, the base Outcome 2: full-time employment; Outcome 1: part-time employment (Part-time); Outcome 0: working less than 10 hours per week (Home Production). *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Results for full-time, part-time employment, and home production (working but less than 10 hours per week) as well as yearly earned income and yearly household income are conditional on being employed. Yearly earned income and yearly household income were divided by $10,000. All standard errors are clustered by state. Standard errors are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

To achieve this, we estimate a multinomial logistic regression with a trivariate dependent variable in which the baseline outcome is full-time employment, and our other outcomes are part-time employment, and home production (working for less than ten hours per week). Columns 2 and 3 of Table 2.2 display the average marginal effects. In particular, children raised in the treatment states are significantly less likely to report working part-time. When we use the most generous definition of high usage in Panel B, affected children are 1.5 percentage points less likely to report working part-time relative to those raised in the control states. No significant difference was identified when we compare workers in the treatment and control states using full-time employment, and the results for working less than ten hours per week are marginally significant (at the ten percent level).

Next, we test whether these changes in labor supply on the intensive margin (measured by hours worked) corresponds to changes in yearly earned income (wages and salary) and yearly household income. These results are displayed in Columns 4 and 5 of Table 2.2, respectively. Across our measures of high usage states ($Used$), there is a significant increase in yearly earned income and yearly household income. Given the switch from part-time to home production, an increase in yearly earned income is puzzling. Thus, we test for heterogeneous treatment effects across different groups of children (discussed in section 6.4).

While the focus of this paper is on labor market outcomes, Appendix B shows the effect of Three Strikes on teen pregnancy, number of children, marriage rates and educational attainment. We identify no significant difference when we compare adults raised in the treatment and control states using the probability of having a child before age 20 (i.e., teen pregnancy), number of children, the probability of being married, high school completion or college attendance across our definitions of high usage states (see Columns 1 to 5 of Table B.11). Appendix B has more details.

2.6.3 Placebo Tests

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 initial twenty-five control states (i.e., states that did not implement Three Strikes law), shift the actual treatment states to the donor pool and re-estimate Equation 2.2.

Specifically, we randomly select states from the initial control group to represent high usage states ($Used$), and states that rarely used Three Strikes laws, $Unused$. We focus on the most generous definition of "$Used$", so we assign at random, nine states to represent areas where Three Strikes law were enforced. The other sixteen states represent treatment states that rarely used these laws. The actual treatment states are considered the control group. The treatment is triggered based on being less than eighteen years old at policy implementation (i.e., in 1993, 1994 and 1995), coinciding with the actual treatment years.

We perform 5,000 replications and rank the actual estimated treatment effects (see Panel B of Table 2.2) among the effects from the placebo regressions. We test the estimated effect in high usage states, $Used$, for three long-run outcomes that we found evidence of impacts on: (1) the probability of working less than ten hours per week in Figure B.2a, (2) the probability of working part-time in Figure B.2b, and (3) annual earnings in Figure B.2c. If the estimated effects outlined in Table 2.2 are driven by strong heterogeneity in trends, the placebo estimates will often find an effect similar in magnitude to our actual treatment effect ($\hat{\alpha_1}$) from Equation (2.2). 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.

Figures B.2a, B.2b, and B.2c illustrate results from the placebo tests and provide supporting evidence that adults born in high usage states, and raised post-implementation are more likely to switch from part-time work to working less than ten hours per week relative to adults born in the control states. We also find evidence consistent with an increase in yearly earned income.

2.6.4 Heterogeneous Treatment Effects

To test for heterogeneous treatment effects by age, we separate children into three distinct age groups; *young*, *middle* and *old*. We used dummy variables to capture these stages of childhood and interact these terms with $Used$ to determine whether the impact of exposure to enhance sentencing due to Three Strikes was heterogeneous across different stages of childhood. Specifically, the regression we estimate is

$$\begin{aligned} Y_{isb} = \alpha_{11} * young_{sb} * Used_s + \alpha_{12} * middle_{sb} * Used_s + \alpha_{13} * old_{sb} * Used_s + \\ \alpha_2 * exposure_{sb} * Unused_s + X_i'\alpha_3 + \lambda_b + \eta_s + \theta_s b + \epsilon_{isb} \end{aligned} \tag{2.3}$$

where the dummy variable *young* takes a value one if a child born in year b was seven years or younger when the state in which they were raised adopted Three Strikes, zero otherwise. The dummy variable *middle* takes a value of one if a child was older than seven years but less than fourteen years when their state adopted Three Strikes law, zero otherwise. *Old* is also binary, set equal to one if a child was fourteen years to eighteen years when Three Strikes law was adopted, zero otherwise. Those above eighteen years old are not treated, consistent with our previous analyses. All other variables are identical to those presented in Equation 2.2. Standard errors are clustered at the state level.

Table B.4 shows the results by age at implementation. Panel B of Table 2.2 reveal that all age groups experience a significant decline in part-time work and a significant increase in specializing in home production (are employed, but work less than ten hours per week). However, the very young in the treatment state at exposure ($Used*young$) experience a larger decline in part-time employment and a larger increase in home production (see Columns 2 and 3 of Table B.4). This larger impact on younger children is consistent with Chetty et al. (2016b), Hoynes et al. (2016) and Bald et al. (2019). The increase in yearly earned income is limited to the old and middle age groups, although the increase in household income exists across all age groups (see Columns 4 and 5 of Table B.4, respectively).

The results separated by gender are shown in Tables B.5 and 2.3. Table B.5 shows that there are no significant impacts on females, across our variety of labor market outcomes and definitions of high usage states ($Used$). However, Table 2.3 shows that males are significantly impacted. Specifically, Column 1 of Table 2.3 shows that males born in treatment states are less likely to be employed relative to males born in control states. Conditional on being employed, these males are less likely to work part-time and more likely to specialize in home production.

Table 2.3: Male Only Sample

	Panel A: Top Three Used States: CA, GA, FL				
Variable	Employed	Part-time	Home Production	Earnings	H.H. Income
$Used$	-0.0226***	-0.0225**	0.0058	0.3465***	0.3004
	(0.0085)	(0.0098)	(0.0036)	(0.1362)	(0.3011)
$Unused$	-0.0082	-0.0029	0.0026	0.2494**	0.2196
	(0.0088)	(0.0129)	(0.0026)	(0.1217)	(0.1557)
	Panel B: State's Three Strikes use is > 0.01 *of admissions in 1998*				
Variable	Employed	Part-time	Home Production	Earnings	H.H. Income
$Used$	-0.0177**	-0.0239***	0.0059*	0.3674***	0.4264*
	(0.0085)	(.0096)	(0.0034)	(0.1299)	(0.2346)
$Unused$	-0.0094	0.0034	0.0019	0.2106*	0.0993
	(0.0094)	(0.0131)	(0.0026)	(0.1214)	(0.1324)
state fixed effects	X	X	X	X	X
birth cohort fixed effects	X	X	X	X	X
state specific linear time trend	X	X	X	X	X
Observations	153,752	141,314	141,314	141,314	141,314

Notes: Entries in Column 1 are average marginal effects from a logit, while Columns 2 and 3 include average marginal effects from a multinomial logit. For the multinomial logit regression model, the base Outcome 2: full-time employment; Outcome 1: part-time employment (Part-time); Outcome 0: working less than 10 hours per week (Home Production). $Used$ 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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Results for full-time, part-time employment, and home production (working but less than 10 hours per week) as well as yearly earned income and yearly household income are conditional on being employed. Yearly earned income and yearly household income were divided by \$10,000. All standard errors are clustered by state and are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

Though less likely to be employed, conditional on being employed, males raised in the treatment states earn approximately \$3,674 more income per year relative to males never exposed (see Column 4 of of Table 2.3). This estimate is statistically significant at the 1 percent level. Males raised in the treatment states also report higher household income, but only with the more generous definition of $Used$. Consequently, these results paint a nuanced picture about labor market outcomes. Some males are hurt by these policies, in that they experience less employment relative to males in the control group. Among those still employed, some workers raised in the treatment states are moving to fewer hours of employment. On the other hand, some males benefit from these policies in that conditional on being employed, they earn more than males in the control group. Thus, there appears to be heterogeneous effects among male children.

To further explore why there are negative impacts on male children, we separately estimate impacts on educational attainment for males. Table B.6 shows males are less likely to attend college, regardless of our definition of $Used$. Thus, the likely mechanism behind the reduction in being employed for males is the reduction in human capital accumulation. We do not find any consistent impact for females (see Appendix C for more details).

We next estimate whether there are heterogeneous impacts by race. It is well established that criminal justice policies disproportionately impact certain minorities (for example, see Carson (2018)). We separate our sample into Whites and Asians versus Blacks, Hispanics, and 'other' (largely more than one race) since we expect the impacts to be limited to Blacks, Hispanics and 'other'. Because we only identify statistically significant impacts among males, we focus on differences across race for our male only sample.[10] Results for White and Asian males are shown in Table B.7. The only significant impact for White and Asian males is a 1.9 percentage point decrease in being employed (see Column 1 of Table B.7). Results for Black, Hispanic or 'other' males are shown in Table B.8. This sub-sample is driving the increase in earnings (see Column 4 of Table B.8).

We next focus specifically on Black males given that this group is greatly impacted by criminal justice policies. Labor market results are shown in Table B.9. Among Black males there is a decrease in employment, an increase in earnings conditional on being employed

[10] We have also tested for differences across race for females, but results are consistently insignificant as there were for the aggregated only female sample.

and no change in household income conditional on being employed. Thus, some Black males are negatively impacted by Three Strikes laws due to reduced employment. However, those able to achieve employment, earn higher yearly income but experience no significant increase in household income. We further explore which Black males are earning more. Given the importance of educational attainment, we cut our sample to focus specifically on Black males who attended college. Results are shown in Table B.13 in Appendix D. For those who attend college (approximately 38.71% of the overall sample of Black males or 47.78% of the Black salaried civilian male workers) there is an increase in yearly earnings. Black males who attended college earn $14,734 more and have no decrease in employment. Black males who did not attend college do not have a statistically significant increase in earnings and are marginally more likely to report being unemployed (p-value of 0.062).

2.6.5 Multiple Hypothesis Testing

This paper has examined the impact of Three Strikes on numerous outcomes. Due to this, we now discuss the procedure we use to adjust for multiple hypotheses. Following Chyn (2018) we employ the Benjamini et al. (2006) method which is recommended in Anderson (2008). The Benjamini et al. (2006) method involves two stages, which improves upon previous methods by allowing for additional control of the false discovery rate. Anderson (2008) also reports this method has better power.

We focus on our more generous definition of used (displayed in Panel B of each table), which is our preferred specification. Our outcomes of interest are the labor market outcomes: probability of being employed, hours worked (home production and part-time coefficients), yearly earned income and yearly household income (shown in Table 2.2, Table B.5 and Table 2.3). We test the coefficients for our overall sample, female only sample and male only sample together (15 coefficients in total). Applying the Benjamini et al. (2006) method results in there still being an overall increase in yearly earned income and yearly household income, still no significant changes for females, and still a decrease in employment, decrease in part time work and increase in yearly earned income for males. Thus, our result of males being much more impacted is robust to adjusting for multiple hypothesis testing.

2.7 Conclusion

This paper shows that increases in adult incarceration rates, due to Three Strikes law, have a statistically significant impact on children at the time of implementation. This result complements previous and concurrent papers that examine the impact of changes in adult incarceration on the extensive margin (parents who are incarcerated versus those who are not). We show children raised in states with high usage Three Strikes law are exposed to a larger percent of adult offenders sentenced to twenty five years or more in prison, relative to children raised in the other states.

There are long-run impacts on children, but the results are only significant for males. Some males are worse off due to lower levels of employment in the treatment states. Some males are better off due to increased earnings conditional on being employed. Specifically, we find there are differential impacts among Black males. Among Black males who attended college there is no decrease in employment and a large increase in annual earnings ($14,734). If we instead focus on Black males who did not attend college, the estimated increase in earnings is not significant at the five percent level, and there is a marginally significant increase in unemployment (p-value of 0.062).

The impact of sentencing laws is important for public policy because the margin at which criminal justice reform has bipartisan support is sentence length and not whether we incarcerate people or not. For example, on December 21st, 2018, President Trump signed the First Step Act into law which made changes to the federal Three Strikes law by reducing sentence lengths for drug feloniesUnited States 115th Congress (2018). In his State of the Union speech, on February 5th, 2019, President Trump reported that "the First Step Act gives non-violent offenders the chance to reenter society as productive, law-abiding citizens. Now, states across the country are following our lead."[11] While the media discussed

[11] https://www.cnn.com/2019/02/05/politics/donald-trump-state-of-the-union-2019-transcript/index.html

implications for those being sentenced under the federal guidelines, less attention was paid to how sentencing policies affect the children of the incarcerated. Further discussions on this issue should also take into consideration the spillover effects of mandatory minimum sentencing policies.

Chapter 3

Understanding the Age–Dependent Impact of Paternal Incarceration

3.1 Introduction

Between 1991 and 2007, parents held in state and federal prisons increased by approximately 79% to 809,800 prisoners. Children of incarcerated parents increased by roughly the same amount (80%) to 1.7 million, accounting for 2.3% of the US resident population under 18 (Glaze and Maruschak (2008)). Growth in the number of adults incarcerated in the United States, and subsequently the number of US children with a parent in jail or prison, has prompted a body of research focused on estimating the effects of parental incarceration on the human capital development potential of their children. Seeing a father or mother arrested, visiting him or her in prison, and dealing with paternal or maternal absence, may traumatize children, as may the cycle of imprisonment and release that often follows (Billings (2017)). Additionally, lower financial contributions from the father or mother during and after their incarceration and the costs associated with having a family member incarcerated may diminish the financial resources available to children. Notwithstanding these potential negative effects, children may benefit from parental incarceration if it facilitates the removal of a harmful parent.

To better understand the connection between parental, or the more common, paternal incarceration and children's cognitive outcomes, it is critical to understand the dynamics of human capital formation and how this process may be affected by incarceration. Parents invest both material resources and their time in children's skills production (Price (2008)), and parent-child separation due to imprisonment reduces this potential parental investment. Children exposed to paternal or maternal incarceration may end up with lower levels of human capital and ultimately perform worse than their counterparts who are never exposed to parental incarceration. Existing studies on the link between parental incarceration and children's human capital outcomes show that affected children experience more grade retention (Turney and Haskins (2014)), lower reading and math test scores (Billings (2017)), and are less likely to complete college (Foster and John (2012)). Cunha and Heckman (2007) provide insight into the importance of family investment in children's skills formation. They show that child development is nonlinear in age (i.e., the effectiveness of parental investment varies across the different stages of childhood) and emphasize the importance of parental investment during early childhood.

Several studies on the effects of parental incarceration in the US focus on the impact of parent-child separation throughout childhood. However, they typically fail to account for age and the potential nonlinearity of age and human capital development.[1] Because of the multi-stage skills production process and the sensitivity of children's response by age, failure to account for changes in the effectiveness of parental investment (or its absence) may lead to imprecise estimates of the impact of parental incarceration on children's outcomes.

The current paper contributes to the literature by analyzing the link between paternal incarceration and children's short term and long term outcomes, while allowing for the responses to vary separately by children's age and gender.[2]

[1] The few exceptions include work by Cho (2010), which looks at the sensitivity of children's response to maternal incarceration across children ages 5-17 years and Henkhaus (2019) who compares the effect of parental incarceration on children exposed before birth or during childhood.

[2] Children respond differently to the absence of a mother relative to a father (Autor and Wasserman (2013)). By separating the analysis by gender, this paper allows for the assessment of the difference in the nonlinear development potential of girls and boys. Given that male inmates dominate the prison population, I focus on paternal incarceration, as this captures the experience of a more substantial proportion of the US economy.

I provide answers to the question: "Does the estimated effect of paternal incarceration on children's outcomes vary with age at exposure?" Given extensive research on the importance of early childhood environments, I hypothesize that paternal incarceration may differentially inhibit young relative to older children. In particular, I expect that the estimated association will be largest for children who are separated from their father for a large fraction of childhood (a greater dosage) or because they are exposed during a critical stage of development. Dosage levels may negatively affect children differently through differences in the separation length or frequency of incarceration spells. As a result, I examine whether the effect of paternal incarceration on children's outcomes vary with dosage. Because of data limitations, I only provide results where the dosage effect is measured using the frequency of incarceration spells.

I use data from the National Longitudinal Study of Adolescent to Adult Health (Add Health) to conduct the analyses. First, I estimate the effect of paternal incarceration on children's short term and long term outcomes. Second, I examine the timing effect by investigating how the relationship between paternal incarceration and children's human capital outcomes varies with age at exposure. Third, I assess how the estimated link differ across gender. Fourth, I test whether the frequency of incarceration spells matters. I make attempts to correct for potential bias in the estimation by controlling for parent or primary caregiver and child characteristics.

The Add Health data set contains information on paternal incarceration, and allows me to examine the effect of paternal incarceration occurring throughout childhood (from birth to age 18). The data set also contains several measures of educational and labor market performance. So I compare the treatment (youth exposed during childhood) and control (youths never exposed) groups using several measures of short-term human capital accumulation: cumulative high school GPA, the probability of having a high school diploma or GED, the probability of attending college, as well as long term outcomes such as adult earnings, full-time employment, and the probability of experiencing incarceration.

I find that paternal incarceration overall tends to have a negative effect on a number of children's short and long term outcomes. There is suggestive evidence that early exposure is more harmful for boys and relatively late exposure is more harmful for girls. However, I am not able to statistically reject the null hypothesis that the effect is the same across ages of exposure.

The remainder of this paper is organized as follows. Section 3.2 provides a brief review of the literature on parental incarceration and its potential effect on children at different stages of development. Section 3.3 describes the data and provides summary statistics. Section 3.4 describes the empirical strategy and discusses the results. Section 3.5 concludes.

3.2 Literature Review

In this section, I discuss the literature on how paternal incarceration may affect children's human capital outcomes by focusing on the importance of parental contribution to child development. I present a summary of both theoretical and empirical studies on the determinants of children's attainments and the importance of family investment. The theory of human capital formation reveals that parents invest both material resources and their time into raising children. Studies on the effects of imprisonment on family-life points to many pathways by which paternal incarceration may affect children's outcomes.[3] However, in this review, I focus on potential changes to parental time and material investment. In theory, the implications of parental incarceration for child well-being are ambiguous. While depriving the family of one potential earner and caregiver can have negative consequences, removing an emotionally and physically harmful person from the home can benefit children.[4]

3.2.1 Material Investment

The literature on human capital formation dates back to Becker (1964), where he shows that acquired skills can explain much of the earnings distributions not explained by innate

[3] Parental imprisonment may be result in stress, stigma, and trauma, it may change family structure since it elevates the risk of divorce and separation. Additionally, it may change the dynamics of family life and financial resources available to children.

[4] While my focus is on the economics literature, and I also include relevant studies from other social sciences.

ability. In this context, parental investment in educational goods for children is comparable to a firm's investments in capital equipment. The role of the child is assumed to be passive, and information available to parents is assumed to be perfect. Parental time investments are ignored, and investments at any stage of childhood are equally effective in producing adult skills. Related work by Becker and Tomes (1986) continue to focus on the financial investments of families in children's human capital accumulation and emphasize the importance of family income on the schooling and earnings of children. Later studies by Carneiro and Heckman (2002), Heckman (2006), Cunha et al. (2006), and Cunha and Heckman (2007) present a richer picture of family influence. However, instead of treating the family investment as equally effective at every instance, and childhood as a single period, these papers highlight that a multistage technology governs the human capital formation process. Each stage corresponds to a period in the life cycle of a child. Investment at each stage produces outputs at that stage, and skills produced at one stage augment the skills attained at later stages. Therefore, skills investment at different stages (consistent with different ages during childhood) bolster each other.

Cunha and Heckman (2008) define and measure "sensitive" and "critical" periods during child development. Sensitive periods refer to ages when parental investment is more productive, and a sensitive period becomes a critical one if the investment is only productive in a single period. They provide evidence of sensitive periods in the development of both cognitive and non-cognitive skills and conclude that the sensitive periods for investment in cognitive skills occurs earlier in the life cycle than the sensitive periods for investment in non-cognitive skills. If family investment is more productive at different stages of childhood, the effect of a reduction or absence of investment due to parental incarceration may also vary with a child's age.

3.2.2 Time Investment

Not only is financial support important and falls when parents are incarcerated, but children also lose parental time investment.[5] Numerous social scientists have theorized that how parents spend time with their young children influence their future academic success. Therefore parent-child separation and the subsequent fall in time spent as a caregiver may negatively impact the child's development. Zick et al. (2001) find a positive association between parental time spent reading or playing with children and their behavior and grades. Hill and Stafford (1980) examine time spent in housework as well as the labor market for parents in families of different social class and conclude that more educated women spend more time playing with children, helping with the teaching of children and in child-related travel. These kinds of childcare are especially crucial for cognitive development. Similarly, work by Ramey and Ramey (2010) using data from time diaries indicates that the childcare time gap between college-educated and less educated parents in the US doubled during the late 1990s, with college-educated mothers spending somewhere between four and six more hours per week caring for their children than their less-educated counterparts. College-educated parents not only spend more time with their children, but the quality is also higher since they are more inclined to spend more time reading to children and less time watching television (Hofferth and Sandberg (2001)). Consequently, the educational attainment of parents may influence family time investment in children's human capital formation.

3.2.3 Sources of Variation in Parental Investment

Cognitive development occurs over time, and the effectiveness of parental investments is influenced by the child's innate ability, when they occur, the child's birth order, and gender. It is commonly believed that birth order is an important determinant of success. Leibowitz (1974) highlights that first-born children and children from smaller families received more

[5] Correctional policies regarding visitation and phone use make it difficult for parents to stay in touch with their children. These facilities are often located in remote areas, often long distances from where children and caregivers live, making visitation extremely difficult for families with limited resources. Additionally, visitation hours are scheduled for set times each week rather than depending on would-be visitors' schedules (Kaplan & Sasser, 1996).

time with parents on average and perform better in school. Though high birth order children generally experience a household environment in which the parents are more mature, more experienced at parenting, and have more income; Price (2008) shows that a first-born child experiences more interactive time with their parents than second born children. Specifically, using data from the American Time Use Survey, he provides evidence that a first-born child receives approximately 20-30 more minutes of quality time each day with his or her parent than a second-born child of the same age from a similar family. Though parents spend less time with high birth order children, they may benefit from better quality time as parents become more efficient at specific tasks such that less time is needed to provide the same amount of care. Sibling interactions represent another possible mechanism through which birth order might be related to children's outcomes. For instance, older siblings could act as positive role models, their achievements adopted as goals and their failures serving as cautionary examples (Argys et al. (2006)). So it is essential to consider birth order when investigating parental time investment

The quantity of parental material and time investment may also be influenced by the child's gender. Although sons and daughters share the same household, historically, they have not had equal access to parental resources. The absence of stable fathers from children's lives has particularly significant adverse consequences for boys' psychological development and educational achievement (Autor and Wasserman (2013)). Bertrand and Pan (2013) postulate that boys raised without a biological father receive low levels of parental inputs and parental expectations, compared to girls raised in similar families. Single mothers interact differently with their sons and daughters. In particular, they spend an hour less per week with their sons than with their daughters. They also report feeling more emotionally distant from their sons and engage in disciplinary action such as spanking more frequently with their sons. These disparities in parenting are largely absent from dual parent homes.

On average across childhood, broken families are associated with lower levels and lower quality of parental inputs; boys' development, unlike girls, appears extremely responsive to such inputs (Bertrand and Pan (2013)). If boys are more responsive to parent inputs (or the absence thereof) and receive lower parental inputs when raised without a biological father than girls, father absence due to incarceration may affect boys differently than girls. As a result, analysis of the effect of father absence on children may yield different results when analyzed separately by gender.

3.2.4 Maternal versus Paternal Incarceration

The effect of parent-child separation due to incarceration may vary with parent's gender because separation from mother may affect children differently when compared with separation from father. Mothers and fathers also spend different lengths of time away from their children when incarcerated. On average, fathers serve between 80 and 103 months in prison, whereas mothers serve between 49 months and 66 months.[6] Mothers are more likely than fathers to report living with at least one child before incarceration and are more likely to report providing most of the daily care. So, it is unclear if children lose more parental investment when a father or mother is incarcerated. The effect of paternal incarceration may be less than the impact of maternal incarceration since a father is less likely to be living with and providing childcare before incarceration. However, children may lose larger amounts of monetary investment for longer periods when fathers are detained since the time served by males surpasses the average time served by females.

Children exposed to parental incarceration are more likely to be separated from their father than mother. Though the number of mothers in jail and prison (between the 1990s and early 2000s) grew at a faster rate than the number of incarcerated fathers during the 1990s, in absolute terms, the number of females detained is far less than the number of males. Glaze and Maruschak (2008) report that US prisons held approximately 744,200 fathers and 65,600 mothers at midyear 2007. Fathers in prison reported having 1,559,200 children, while mothers reported 147,400.

[6] The length of incarceration reflects the nature of the different offenses committed by males and females. Fathers are more likely than mothers to be in prison for violent crimes.

Causal Estimates of Parental Incarceration, Without Consideration of Child's Age

Significant work by sociologists, criminologists, and more recently by economists has been done to examine the link between parental incarceration and children's outcomes (Norris et al. (2018); Bhuller et al. (2018a)). The few causal papers exploit exogenous variation in incarceration using judge leniency, and broadly conclude that parental incarceration negatively affects children's outcomes. Though childhood development is a multistage process and the effectiveness of investment at each stage may vary, the literature focuses less on how the estimated results differ across age groups.[7]

The current paper contributes to the literature by providing a detailed comparison of the relationship between paternal incarceration and children's human capital outcomes in the US, while allowing for heterogeneity across age and gender.

3.3 Data

I use the first four waves of the publicly available National Longitudinal Study of Adolescent to Adult Health (Add Health) data to conduct my analyses. The Add Health survey began in 1994 with adolescents sampled in a stratified design from grades 7 to 12, and enrolled in one of 132 US schools, in 80 communities. Schools were selected first, and children in sampled schools were subsequently sampled.[8] School size varied from fewer than 100 students to more than 3,000 students. The study began with an in-school survey and then randomly sampled students from these schools to participate in an in-home survey. Over the next 15 years, subsequent in-home surveys were conducted in 1996 (Wave 2), over the period 2001 and 2002 (Wave 3), and between 2008 and 2009 (Wave 4).

A major attraction of the Add Health data for this research is the availability of information on youths exposed to paternal incarceration throughout childhood (between 0 and

[7]To my knowledge, only two papers, Arteaga (2018) and Dobbie et al. (2018), account for age in their discussion of parental incarceration and children's outcomes for Colombia and Sweden.

[8]See Harris (2013) for details on the sample design.

18 years), from a large number of US communities. In particular, I am able to compare short and long term outcomes of children exposed at different ages during childhood with the outcomes of relatively similar children who were never exposed.

Sample Selection

Questions regarding paternal incarceration were asked in Waves 3 and 4.[9] Wave 4 provides more details on paternal incarceration so, in order to create the baseline sample, I start with the 5,114 observations available from Wave 4.[10] Then I merge data from Waves 1, 2, and 3 to this initial sample, using unique identification numbers and keeping records from the earlier surveys only if they are also available in Wave 4. Wave 3 provides information on respondents' educational choice, while Waves 1 and 2 incorporate demographic data for respondents and their family. During Wave 1, the primary caregiver in each household completed a detailed survey which allows me to control for caregiver's socio-demographic characteristics when comparing children's human capital outcomes.

For the regression analysis, I keep only records of individuals with no missing values for the dependent and key independent variables (1,919 observations). In addition to dropping observations due to missing data, one observation is removed because of incorrect date of birth data. I drop observations for those who reported being exposed to paternal incarceration but did not include their age when father was first sent to jail or prison (395 respondents).[11] I also exclude observations for those who were older than eighteen years at their first exposure (39 children), since my focus is on disruptions or stressors occurring during childhood. If children were exposed to maternal incarceration (71 children), they are

[9]In addition to questions regarding biological parents experience with incarceration; Wave 4 provides information on whether a mother or father figure was incarcerated. This wave also provides information on an individual's age when his or her parent was first incarcerated, their age at parent's most recent release, as well as an individual's personal encounter with the criminal justice system. To minimize attrition, efforts were made to contact and conduct follow-up interviews with respondents while incarcerated.

[10]Though 15,701 respondents participated in the Wave 4 survey, only 5,114 records are available in the public use data file.

[11]Not knowing when a parent is incarcerated may indicate the type of parent-child relationship and may influence how children are affected by paternal incarceration. As a result, I re-estimate the main regression models without dropping this group of individuals to test whether their inclusion would change the results. Table C.10 displays these estimates and indicates that the main results are largely unchanged.

excluded from the estimation sample as well, given that children may respond differently to separation from their mother.[12] Therefore the initial sample of 5,114 observations shrinks by 2,425 to 2,689 observations.

Summary Statistics

Here I provide summary statistics for the estimation sample, separately discussing the characteristics of those exposed to paternal incarceration (exposed or the treatment group) and those who were never exposed (the control group). Each individual is assigned to the control group, never exposed to paternal incarceration, or assigned to one of four categories in the treatment group. The treatment group includes children exposed: (1) before birth, (2) during early childhood; between zero and five years, (3) during middle childhood; between six and twelve years, or (4) during late childhood; between thirteen and eighteen years. In this section, I also discuss how the dependent variables are defined, as well as how they compare across treatment and control groups. The effect of paternal incarceration in this paper is estimated by using two sets of outcome variables to compare children across the treatment and control groups. Initially, I compare children using short term human capital outcomes: cumulative GPA, receipt of a high school diploma, and college attendance. Then children across treatment and control groups are compared using long term outcomes: adult earnings, full-time employment, and criminal encounter (see summary statistics in Table 3.1).

Cumulative high school GPA captures the average of annual official GPA across all years of high school and is measured on a four-point scale. Information on overall cumulative GPA is reported in the Adolescent Health and Academic Achievement supplement conducted during Wave 3. These scores represent student academic performance in key subjects (mathematics, science, foreign language, English, history, social science, and physical education) as well as across non-core and non-academic courses (see Harris (2013)).

Table 3.1 shows the mean cumulative high school GPA for the full sample (2.62), as well as separately for the control group (not exposed) and the treatment group (exposed). As

[12]Results outlined in Table C.17 reveals that exposure to maternal incarceration has no significant adverse effects on children, while exposure to paternal incarceration is associated with significant negative effects.

expected, the average GPA for children exposed to paternal incarceration (2.28) is less than the average for those never exposed (2.69).

I use two dummy variables to indicate individual's educational choice based on reports of the highest level of educational attainment collected during Wave 3. The first dummy variable indicates receipt of a high school diploma and takes a value one if the respondent indicated that they have at least a high school diploma or GED. This indicator is set equal to zero otherwise. The second dummy variable is based on college attendance. Therefore, this binary variable takes a value of one if an individual reports attending college and zero otherwise. While 93.9% (19.3%) of those never exposed to paternal incarceration completed high school (earned a four-year college degree), only 82.7% (7.7%) of those exposed completed high school (completed college).Comparison of the human capital outcomes displayed in Table 3.1 reveals that there is a statistically significant difference between the educational attainment of the treatment and control groups.

Table 3.1: Summary Statistics: Outcome Variables

	All	Not Exposed	Exposed				Difference
			0-5 years	6-12 years	13-18 years	0-18 years	Not Exposed-Exposed (0-18 years)
Short-term Outcomes:							
Cumulative GPA	2.649	2.723	2.177	2.261	2.239	2.294	0.429***
	(0.829)	(0.785)	(0.876)	(0.849)	(0.834)	(0.864)	[0.047]
High School Diploma	0.913	0.939	0.797	0.774	0.736	0.827	0.112***
	(0.282)	(0.239)	(0.404)	(0.420)	(0.446)	(0.379)	[0.015]
College Attendance	0.600	0.649	0.384	0.387	0.269	0.437	0.212***
	(0.490)	(0.477)	(0.489)	(0.490)	(0.449)	(0.497)	[0.028]
Long-term Outcome:							
Adult Earnings (1000s)	38.15	39.80	28.33	29.34	23.62	28.95	10.85***
	(42.66)	(46.20)	(18.71)	(25.36)	(19.19)	(21.15)	[2.61]
Full-time Employment	0.861	0.860	0.832	0.864	0.753	0.830	0.030
	(0.346)	(0.347)	(0.376)	(0.344)	(0.436)	(0.376)	[0.021]
Criminal Engagement	0.127	0.103	0.267	0.265	0.200	0.235	-0.133***
	(0.333)	(0.303)	(0.444)	(0.444)	(0.405)	(0.425)	[0.019]
Observations	2,799	2,476	113	94	43	323	

Notes: Numbers are the mean of the variable. Standard deviations are in parentheses and standard errors are in square brackets. ***, ** and * indicate that the estimates are statistically significant at the 1%, 5% and 10% levels.
Source: National Longitudinal Study of Adolescent to Adult Health.

The measure of adult earnings was recorded between ages 24 and 32. Income generated before thirty may not be reflective of an individual's lifetime earning potential, but may be driven by labor market dynamics such as job search or schooling. Therefore, caution must be taken when interpreting the link between a shock to childhood development (due to paternal incarceration) and this measure of labor market performance. I utilize information on usual hours worked to I create an indicator for full-time employment. This indicator is binary, and equals one if the respondent reports working thirty five hours or more per week, zero otherwise. Using respondents' answer to the question "Have you ever spent time in a jail, prison, juvenile detention center or other correctional facility?", I create an indicator for criminal engagement. This binary variable takes a value one if the respondent spent time in jail or prison and a value zero otherwise. Assessment of unconditional means for these long term outcomes in Table 3.1 reveals that children exposed to paternal incarceration by age 18 earn less income, and are more likely to experience incarceration (hereinafter criminal engagement) relative to those never exposed.

For the rest of this section, I discuss and provide some intuition behind the explanatory variables. The key explanatory variable is a measure of exposure to paternal incarceration. I obtain information on exposure from individuals' responses to the following questions: "Has your biological father ever served time in jail or prison?" and "(Has/did) your [father figure] ever (spent/spend) time in jail or prison?". Since I am interested in how the effect of paternal incarceration varies with age at exposure, assignment to the treatment group is based on answering yes to these questions as well as a respondent's age when biological father or father figure first enters jail or prison. Consequently, my main explanatory variable is a categorical one. Approximately 11.5% of the sample was exposed to paternal incarceration because a biological father or a father figure spent time in jail or prison (see Column 1, Panel C of Table 3.2).[13]

[13] This estimate compares well with the estimate for the proportion of children exposed to parental incarceration, 11.7%, reported by Henkhaus (2019).

Table 3.2: Summary Statistics

	All	Exposed	Not Exposed
Panel A: Children Statistics			
Female	0.504	0.513	0.502
Race/ Ethnicity:			
White	0.731	0.592	0.749
Black	0.119	0.225	0.105
Other Race	0.150	0.183	0.146
Hispanic	0.116	0.170	0.109
Birth order	1.979	1.818	1.999
Birth weight (lbs)	7.007	6.738	7.040
Immigrant	0.038	0.023	0.040
Panel B: Primary Caregiver's Statistics			
Primary Caregiver's Education:			
Less than HH (PCG)	0.107	0.160	0.100
HH or GED (PCG)	0.312	0.427	0.298
Some college (PCG)	0.311	0.287	0.314
BA (PCG)	0.156	0.091	0.164
Post BA cert. (PCG)	0.114	0.035	0.124
Family income (1,000)	33.601	24.796	34.704
Immigrant (PCG)	0.082	0.055	0.085
Mother's Age at Birth	25.721	23.178	26.025
Panel C: Parental Incarceration			
Paternal Incarceration	0.115	1.000	0.000
Bio Dad's incarcerated (wave 3)	0.077	0.687	0.000
Bio Dad's incarcerated (wave 4)	0.117	1.000	0.000
Ages 0 to 5 at exp.	0.356	0.356	.
Ages 6 to 12 at exp.	0.259	0.259	.
Ages 13 to 18 at exp.	0.153	0.153	.
Ages 19 or older at exp.	0.132	0.132	.
Not yet born	0.099	0.099	.
Maternal Incarceration	0.022	0.084	0.014
Observations	2,799	2,476	323

Notes: Numbers are the mean of the variable. NH represents Non-Hispanic. PCG represents primary caregiver.
Source: National Longitudinal Study of Adolescent to Adult Health.

I include controls for child's and primary caregiver's characteristics. Summary statistics are displayed in Table 3.2.[14] For the focal child, I include controls for gender, race, ethnicity

[14] Tables C.2 and C.3 in the appendix provided additional descriptive statistics.

and immigration status (see Panel A of Table 3.2). A comparison of the treatment and control groups reveals that a larger fraction of the treatment group are Non-Whites, approximately 40.83%, relative to 25.13% of the control group who identify as Non-Whites. This pattern supports existing work by Ewert et al. (2014) which argues that incarceration primarily affects minority families since they view the penal expansion since the 1970s as a new type of state intervention in the lives of low-skill, disproportionately minority, men. It is important to account for birth order when analyzing children's outcomes given that the first born child generally performs better in school and spends more time, on average, with parents (see Price (2008)). According to Cesur and Rashad (2008) the relationship between birth weight and cognitive outcomes is quadratic in nature. While low birth weight is of primary concern, high birth weight should not be ignored and can also lead to adverse cognitive outcomes. Based on their assessment, I include birth weight as a second order polynomial.

I use information on primary caregiver's highest educational attainment and immigrant status as well as mother's age at birth to proxy for parents' characteristics (see summary statistics in Panel B of Table 3.2). Highly educated parents spend more time with their children than their less educated counterparts (Murnane et al. (1981)). Since the level and quality of parental time investment in children's human capital formation vary with parents educational attainment, I explicitly control for primary caregiver's level of education (see the distribution outlined in Table 3.2).[15] There is a positive link between educational attainment and income so I use primary caregiver's educational attainment as a proxy for household socioeconomic standards, instead of family income. Several papers have highlighted the positive link between family income and children human capital accumulation (see Leibowitz (1974), Becker and Tomes (1986), Heckman (2006), Cunha and Heckman (2008) among others). However, I do not use family income in my analysis because of concerns regarding endogeneity. The reported household income may be a consequence of paternal incarceration since it was measured in 1994 after some fathers had been convicted for the first time.[16] It is

[15]Primary caregiver's highest educational attainment was obtained from the "Parent instrument completed during Wave 1.

[16] Household income is based on a child's primary caregiver's answer to the question "About how much total income, before taxes did your family receive in 1994? Include your own income, the income of everyone else in your household, and income from welfare benefits, dividends, and all other sources."

important to note that because of the high-quality time investment outlined in the literature, and the wage premium associated with having a college degree, being raised by a highly educated primary caregiver may partly offset any potential shock the family experiences due to paternal incarceration.

Add Health includes data on the primary caregiver's age, largely biological mother, as well as information on the child's age. Using these two age variables, I compute mother's age at the birth of the focal child and include this variable as an additional measure of parenting style, which may influence parents' material and time investment in a child's skills production (and hence their potential loss if separated). The underlying assumption is that older, more educated mothers spend more time investing in children's human capital formation (Price (2008)). I also include a control for primary caregiver's immigration status.

3.4 Results

The summary statistics presented in Table 3.1 suggest that relative to the control group, children in the treatment group perform worse in school, earn lower income and are more likely to become incarcerated. In order to establish a clearer and more statistically robust assessment of the observed differences, I extend the standard model in Equation (3.1) used to investigate the association between paternal incarceration and children's outcomes by estimating Equations (3.2) and (3.3). These specifications allow for a comparison between children exposed to paternal incarceration at different ages during childhood with those never exposed.

3.4.1 Does the Estimated Effect of Paternal Incarceration on Children's Outcomes Vary With Age at Exposure?

The following standard model is used in studies investigating the effect of paternal incarceration on children's outcomes (see Henkhaus (2019), Foster and John (2012), and Cho

(2010)).

$$H_j = \alpha + \beta_1 X_j + \beta_{incar} Incar_j + \lambda_c + \gamma_s + \epsilon_j \tag{3.1}$$

here H_j represents an outcome variable for individual j, while X_j is a vector of controls. The variable of interest, $Incar$, is an indicator for father's absence due to incarceration; and takes a value one if individual j is ever exposed and zero otherwise. As a result, estimates of β_{incar} reflect differences between the treatment and control groups without consideration for age.

I estimate this standard model and report estimates in Column 1 of Tables 3.3, 3.4 and 3.5. My key extension to the literature is outlined in Equation (3.2). This specification extends the standard model by separately estimating the effect of paternal incarceration based on the child's age when this shock occurs, and allows me to statistically test whether the difference between the treatment and control groups varies with age at exposure.

$$\begin{aligned} H_j = \alpha + \beta_1 X_j + \beta_{incarB} IncarBeforeBirth_j + \beta_{incarE} IncarEarly_j \\ + \beta_{incarM} IncarMiddle_j + \beta_{incarL} IncarLate_j + \lambda_c + \gamma_s + \epsilon_j. \end{aligned} \tag{3.2}$$

I compare human capital outcomes, H, for children exposed at different ages relative to those never exposed. In the current study, H_j is the short term human capital outcome for individual j, i.e., either cumulative GPA, high school diploma or GED, college attendance, or long term outcome like adult earnings, full-time employment, and criminal engagement. Using three binary dummy variables, I identify three distinct periods in the first 18 years of a child's life when exposure to paternal incarceration could have differing impacts on a child's development. Specifically, $IncarEarly_j$ captures first exposure during early childhood, and takes a value one if individual j was exposed to paternal incarceration between zero and five years, and is assigned zero otherwise. $IncarMiddle_j$ takes a value one if individual j was first exposed between six and twelve years, zero otherwise; while $IncarLate_j$ takes a value one if individual j was first exposed between thirteen and eighteen years, zero otherwise. These three mutually exclusive groups coincide with the sequence of educational experience. Children treated between zero and five years have no formal educational arrangements or are enrolled in preschool; between six and twelve years, children are enrolled in elementary or middle school; while older children, between thirteen and eighteen years, are usually enrolled in high school. With these categories, I can test the effect of paternal incarceration occurring during early, middle and late childhood on children's outcomes. The fourth binary dummy variable used in the model, $IncarBeforeBirth_j$, takes a value one if individual j's father was incarcerated before their birth, zero otherwise.[17] Therefore individuals are placed into five mutually exclusive groups. Four treatment groups and a control group. Each individual exposed to paternal incarceration by age 18 (the treatment group) is included in exactly one group. That is, if their father or father figure was sent to jail or prison before their birth then, that individual is apart of the $IncarBeforeBirth_j$ group and excluded from the other three treatment groups, irrespective of whether their father or father figure was incarcerated during their childhood years.

X_j is a vector of child's and primary caregiver's characteristics as discussed in Section 3.3. I include controls for a child's gender, race and ethnicity dummy variables (black, other, and Hispanic), birth order, a dummy variable for immigrant status, birth weight and birth weight squared. I include controls for primary caregiver's highest educational attainment (a dummy variable with five categories), their immigrant status, and mother's age at birth. I also include cohort and school fixed effects, λ_c, γ_s, respectively, which allow for time-invariant differences across birth cohort, c and school, s.[18] I cluster all standard errors at the school level and weight all estimates using longitudinal sample weights.

I use ordinary least squares to estimate the regression models. For models with binary outcomes (such as high school diploma, college attendance, full-time employment, and criminal engagement), I re-estimate the results using logistic regression models, and report the marginal effects in Tables C.11, C.12, C.13, and C.14.

[17]When all four dummies variables are equal to zero, then the individual is part of the control group which was never exposed.

[18]School fixed effects has been used by researchers such as Murnane et al. (1981) to address the problem of missing data on school inputs, and with the assumption that children within the same school receive the same inputs. So by using school fixed effects, I am able to control for unobserved school-specific characteristics that may affect the likelihood of success in school. For example, schools with fewer resources provide lower quality education on average than is available to children who attend school with more resources.

Table 3.3: Educational Outcomes: High School Performance

	All		Male	Female
	(1)	(2)	(3)	(4)
	Panel A: Cumulative GPA			
$Incar$	-0.204**			
	(0.071)			
$IncarEarly$		-0.258**	-0.352**	-0.218*
		(0.111)	(0.151)	(0.123)
$IncarMiddle$		-0.179*	-0.106	-0.159
		(0.092)	(0.153)	(0.129)
$IncarLate$		-0.121	-0.056	-0.189
		(0.173)	(0.268)	(0.215)
$IncarBeforeBirth$		-0.186	-0.333	-0.000
		(0.147)	(0.243)	(0.191)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.336	0.336	0.322	0.363
$H_0 : \beta_{incarE} = \beta_{incarM} = \beta_{incarL} = \beta_{incarB}$				
F-test p-value		0.900	0.487	0.837
	Panel B: High School Diploma (LPM)			
$Incar$	-0.094**			
	(0.035)			
$IncarEarly$		-0.100**	-0.197**	-0.018
		(0.044)	(0.083)	(0.038)
$IncarMiddle$		-0.096*	-0.083	-0.061
		(0.051)	(0.084)	(0.057)
$IncarLate$		-0.121	0.002	-0.193
		(0.110)	(0.097)	(0.151)
$IncarBeforeBirth$		-0.036	-0.048	-0.018
		(0.052)	(0.104)	(0.054)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.183	0.183	0.261	0.279
$H_0 : \beta_{incarE} = \beta_{incarM} = \beta_{incarL} = \beta_{incarB}$				
F-test p-value		0.583	0.735	0.688

Notes: Dependent variables: Panel A, high school GPA and in Panel B, high school diploma or a GED. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. LPM represents linear probability model results. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

These results were largely consistent with those from the linear probability models.

The results from estimating Equations (3.2) using information for the full sample as well as separately for males and females are presented in Tables 3.3, 3.4, 3.5, and 3.6.[19] Tables 3.3, and 3.4 display results when children are compared using short term human capital outcomes, while Tables 3.5 and 3.6 present results when I compare children using long term outcomes.

Table 3.3 illustrates the estimated relationship between high school performance and paternal incarceration. Each panel presents the effect of paternal incarceration on a given outcome variable. Panel A presents the results when high school performance is measured using cumulative GPA and Panel B displays results when performances is captured by the probability of earning a high school diploma or GED. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. Column 1 illustrates the difference between children ever exposed (between birth and age eighteen or before birth) and those never exposed while controlling for child's and primary caregiver's characteristics (i.e., I present an estimate of β_{incar} from Equation (3.1)). In Column 2, I re-estimate the model with the full set of control but compare children treated at different stages during childhood with those never exposed (i.e., the main regression model). Columns 3 and 4 re-estimate Equation (3.2) using a sample of only males or females, respectively.

The association between paternal incarceration and children's educational outcomes: cumulative GPA, receipt of a high school diploma or GED, and college attendance are shown are similar and suggest worse educational outcomes for those exposed to paternal incarceration during childhood. More importantly, the results reveal that the very young at exposure (between zero and five years) experience a larger decline in educational attainment when compared with those never exposed.

Column 1, Panel A of Table 3.3 shows that exposure by age 18 reduces cumulative GPA by 0.204 points or 7.7% (0.204/2.649) relative to the mean score outlined in Table

[19] Appendix C includes more detailed results. I include a dummy variable for individuals with missing data.

3.1. When I decompose this estimate to assess how the relationship varies across age at exposure, I find that exposure during early, middle and late childhood are associated with worse child outcomes. However, only the effect of exposure during early and middle childhood are significantly different from zero. It is not surprising that no significant difference exists between youths exposed during late childhood and those never exposed, since their GPA may only be partly affected. Therefore, any negative effect of exposure to paternal incarceration may be offset by earlier scores.

In terms of magnitude, children exposed by age five, experience a larger loss in cumulative GPA relative to children exposed between ages six and twelve (see Columns 2, Panel A of Table 3.3). Specifically, exposure by age five is associated with 0.258 points lower GPA or 9.7% (0.258/2.649) less GPA relative to the mean score. Exposure during middle childhood reduces cumulative GPA by 0.179 points when compared with those never exposed to paternal incarceration. Though the decline for children exposed during early childhood is larger than the decline occurring during middle childhood, I am not able to reject the null hypothesis, at conventional levels, that the effect of exposure across age groups is identical (see p-value of 0.9 in Column 2).

For males, Column 3, Panel A of Table 3.3, the adverse effect of paternal incarceration seems to decline with age at exposure. Exposure by age five is associated with 0.352 points or 14.2% (0.352/2.481) lower GPA relative to the mean, and this difference is statistically significant at the five percent level. Girls exposed to paternal incarceration by age five earned 0.218 lower points, on average, which amounts to 7.6% (0.218/2.859) lower GPA relative to the mean score for girls. This result is only marginally significant. In percentage terms, the estimated decline in cumulative GPA that boys experience when exposed to paternal incarceration during early childhood is twice as large as the decline that girls experience. Though reflecting a negative association, no statistically significant difference is identified when I compare boys (girls) exposed after age five with boys (girls) never exposed to paternal incarceration. These results suggest that both boys and girls exposed during early childhood are adversely affected by paternal incarceration, and the decrease in cumulative GPA for boys is large.

A similar pattern is identified when I compare children based on the probability of receiving a high school diploma or GED (see Panel B of Table 3.3). This result is not surprising given the link between cumulative GPA and probability of earning a high school diploma or GED. Again, exposure during early, middle and late childhood seems to adversely affect high school performance, measured here as the probability of earning a high school diploma or GED. The largest statistically significant decline is identified among children exposed by five years.

When compared with the control group, Column 1, Panel B of Table 3.3 shows that after controlling for the child and primary caregiver characteristics, both boys and girls exposed to paternal incarceration during childhood (by age 18) are less likely to earn a high school diploma or a GED by 9.4 percentage points or 10.2% (9.4/92.6). However, when I consider exposure at different stages of childhood, those exposed by age five, experience a slightly larger decline in the probability of earning these credentials, 10 percentage points or 10.8% lower probability (see Column 2). This estimated difference is significantly different from zero at the five percent level of significance. Children exposed during middle childhood, age six to twelve, also experience a significantly lower probability of earning a high school diploma or GED, 9.6 percentage points or 10.4% (9.6/92.6) lower probability relative to children never exposed. However, this estimate is only marginally significant.

With regards to gender, when I consider only boys or only girls, the adverse link between paternal incarceration and the probability of acquiring a high school diploma or GED persists. Boys treated at different stages of childhood perform worse than boys in the control group. However, only the estimate associated with exposure by age five is significantly different from zero (see Column 3, Panel B of Table 3.3). In particular, boys treated by age five are 19.7 percentage points or 21.7% (19.7/90.85) less likely to earn a high school diploma, and this estimate is significantly different from zero. No significant difference is identified among girls.

When I compare children based on high school performance (using GPA or receipt of a high school diploma), no statistically significant difference exists between children whose father was incarcerated before their birth and those never exposed.[20] The insignificant effect on this treatment group was also highlighted by Henkhaus (2019), and suggests that the worse outcomes estimated for the very young are driven by direct exposure and not unobserved family qualities which may be common among those exposed during childhood, as well as those whose father was incarcerated before birth. For example parent-child interaction in the home which may result in emotional or physical abuse.

Table 3.4: Educational Outcome: College Attendance (LPM)

	All		Male	Female
	(1)	(2)	(3)	(4)
$Incar$	-0.105**			
	(0.042)			
$IncarEarly$		-0.106	-0.103	-0.157
		(0.075)	(0.071)	(0.136)
$IncarMiddle$		-0.090*	-0.141**	-0.055
		(0.048)	(0.068)	(0.081)
$IncarLate$		-0.156**	0.125	-0.290**
		(0.064)	(0.111)	(0.096)
$IncarBeforeBirth$		-0.071	-0.097	-0.100
		(0.100)	(0.132)	(0.184)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.292	0.292	0.357	0.311
$H_0 : \beta_{incarE} = \beta_{incarM} = \beta_{incarL} = \beta_{incarB}$				
F-test p-value		0.821	0.882	0.847

Notes: Dependent variable: college attendance. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. LPM represents linear probability model results. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

These results provide an interesting picture of the relationship between the timing of

[20] A similar result is identified when I compare these groups of children using cumulative GPA.

paternal incarceration and children's outcome. Exposure before age six is associated with lower cumulative GPA and a lower probability of earning a high school diploma or GED. The adverse effect on males exposed during early childhood is larger in magnitude than the adverse effect among females, and supports the claim of researchers like Bertrand and Pan (2013) who argue that boys are more affected by shocks to family structure.

Turning now to a discussion of college attendance and paternal incarceration. The structure of Table 3.4 resembles the structure of Table 3.3 but where the outcome of interest changes to the probability of attending college. Based on the results outlined in Table 3.4, children exposed to paternal incarceration throughout childhood are less likely to attend college. With the full set of controls (i.e., the standard model), the difference in the college attendance rate is -10.5 percentage points, which suggests that those exposed to paternal incarceration, before birth or during childhood, are 17.5% (10.5/60) less likely to attend a four-year college degree relative to those never exposed (see Column 1 of 3.4).

When I separate the treatment group by age, children exposed during middle and late childhood seem to experience a statistically significant decline in the probability of attending college. In particular, Column 2 shows that both boys and girls exposed between six and twelve years experience a 9.0 percentage points decline in the probability of attending college while those exposed between thirteen and eighteen years are 15.6 percentage points or 26% (15.6/60) less likely to attend college. Though the decline associated with the children exposed during middle childhood is significant at the 10% level. The lower probability associated with exposure during late childhood is large and significant at the five percent level. When I separate the sample by gender in Columns 3 and 4, I find that girls exposed during late childhood are significantly less likely to attend college when compared with girls never exposed (29 percentage points less likely). The estimate among girls is large and seems to suggest that though resilient to stressors occurring during early childhood, females may be less equipped to cope with shocks occurring in later childhood. Among boys, exposure during middle childhood results in a statistically significant decline in the probability of attending college (i.e., boys affected are 14.1 percentage points less likely to attend college, on average). A similar argument is outlined in Rudolph and Hammen

(1999). Together, the results for girls and boys suggest that estimates from the main model in Column 2 may be driven by heterogeneity across gender. Consistent with the results for high school performance, children whose fathers were incarcerated before their birth experience no statistically significant decrease in the likelihood of attending college relative to those never exposed.

The above discussion regarding the relationship between the timing of paternal incarceration and children's short term human capital outcomes suggests that both boys and girls are adversely affected by father absence. Among boys, the negative association seems to be largest when exposure occurs during early childhood (i.e., before age six). Though relatively resilient to father absence occurring during early childhood, the data shows that adolescent girls exposed to paternal incarceration are less likely to attend college.

Table 3.5 displays results from regression analyses used to test the relationship between paternal incarceration and long term outcomes, namely adult earnings and full-time employment. Its structure resembles that of Tables 3.3 and 3.4 with panels A and B displaying results when comparison is made using adult income and full-time employment, respectively. Column 1 Panel A of Table 3.5 reveals that a difference of 19.0 log points in adult earnings or 17.3% less income. Column 2 indicates that this negative difference is associated with exposure throughout childhood, since in terms of magnitude, children exposed to paternal incarceration during early, middle and late childhood experience approximately 21 log points less or 18.9% less annual income. However, only the decrease experienced by individuals exposed during early childhood is statistically significant at conventional levels.

Separation of the sample by gender reveals that females exposed during late childhood experience the largest decline in annual earnings, approximately 30.9 log points or 26.6% lower income relative to females never exposed. The smaller effects for females exposed during early and middle childhood are not significantly different from zero. Unlike females, males exposed by age five, are more adversely affected. Among males, exposure by age five is associated with 34.8 log points or 29.4% less annual earnings relative to boys never exposed. This estimate is statistically significant at the five percent level.

Table 3.5: Longer Term Outcomes

	All		Male	Female
	(1)	(2)	(3)	(4)
	Panel A: Adult earnings			
$Incar$	-0.190**			
	(0.061)			
$IncarEarly$		-0.211**	-0.348**	-0.104
		(0.092)	(0.170)	(0.105)
$IncarMiddle$		-0.205*	-0.174	-0.147
		(0.113)	(0.136)	(0.156)
$IncarLate$		-0.215*	-0.044	-0.309*
		(0.110)	(0.168)	(0.159)
$IncarBeforeBirth$		-0.050	-0.007	-0.180
		(0.160)	(0.242)	(0.280)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.199	0.199	0.230	0.233
$H_0 : \beta_{incarE} = \beta_{incarM} = \beta_{incarL} = \beta_{incarB}$				
F-test p-value		0.833	0.495	0.740
	Panel B: Full-time Employed (LPM)			
$Incar$	-0.050*			
	(0.029)			
$IncarEarly$		-0.057	-0.117**	-0.011
		(0.039)	(0.059)	(0.057)
$IncarMiddle$		-0.028	0.008	-0.088
		(0.041)	(0.048)	(0.070)
$IncarLate$		-0.119	-0.095	-0.146
		(0.111)	(0.121)	(0.152)
$IncarBeforeBirth$		0.011	-0.042	0.033
		(0.070)	(0.101)	(0.105)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.107	0.108	0.173	0.154
$H_0 : \beta_{incarE} = \beta_{incarM} = \beta_{incarL} = \beta_{incarB}$				
F-test p-value		0.674	0.286	0.616

Notes: Dependent variables: Panel A, log of adult income and Panel B, full-time employment. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. LPM represents linear probability model results. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Given mean annual earnings of \$45,916.31 for males, this suggests that exposure to paternal incarceration results in approximately \$13, 499.40 less income per year-on average-for these men relative to males never exposed. This loss in annual earnings is larger than the average loss of approximately \$8,773.99 estimated for females exposed during late childhood.[21] Conversely, no significant difference is identified among boys exposed during middle or late childhood and boys never exposed. In terms of statistically significant difference between children affect at different ages throughout childhood and those never exposed, results in Columns 3 and 4 of Tables 3.3, 3.4, 3.5 and 3.6 seem to suggest that paternal incarceration may affect adult income because exposure is associated with less human capital accumulation.

For boys, exposure while young (i.e, before age six) seem to have a large negative and statistically significant effect on usual hours worked. Column 1, Panel B of Table 3.5 indicates that for the full sample of exposure to paternal incarceration results in a 5.0 percentage points decline in the probability of working thirty five hours or more per week relative to those never exposed. This estimate is only marginally significant. When I explicitly control for age at first exposure, in Column 2, I find that exposure during early, middle or late childhood adversely affects children. However, these estimates are not statistically different from zero, and I am unable to reject the null hypothesis that they are identical, given a p-value of 0.833 from an F-test of no difference. When I further separate the sample by gender, boys exposed by age five are 11.7 percentage points less likely to work thirty five hours or more relative to boys never exposed. This estimate is statistically significant at the 5% level. The estimated relationship between boys exposed at a later age is smaller in magnitude and statistically insignificant. Column 4 reveals that among females, there is a negative association between exposure to paternal incarceration between 0 and 18 years, with the largest decline associated with exposure between thirteen and eighteen years. However, these estimates are all statistically insignificant. In sum, the estimated adverse association between exposure to paternal incarceration during childhood and usual hours worked, displayed in Column 1, seems to be influenced largely by boys exposed before age six and girls exposed

[21] The average annual earnings for females is \$32,984.93.

during late childhood.

Column 1, of Table 3.5 shows that for both boys and girls exposure to paternal incarceration increases the likelihood of experiencing incarceration as well, by 9.2 percentage points or 72% relative to the overall mean of 12.7%. In terms of magnitude the effect is largest among children exposed during early childhood and only marginally significant. Therefore not much can be said about the importance of the timing of paternal incarceration and criminal engagement with the current sample.

Though unobserved differences between the treatment and control groups prevent any causal discussions, the results provide evidence that paternal incarceration is adversely associated children short and long term outcomes and provides suggestive evidence that effect may be heterogeneous across age and gender. Particularly, the results indicate that for boys, exposure to paternal incarceration during early childhood (0-5 years) results in poorer high school performance and lower adult earnings when compared with boys never exposed. Among females, exposure during late childhood is associated with a lower probability of completing college and less adult earnings, relative to girls never exposed. For both boys and girls the estimated effect seems to be strongest among children exposed before age six, however, I am not able to reject the null hypothesis that the effect of paternal incarceration is identical across the different stages of childhood explored.

Table 3.6: Longer Term Outcomes

	All		Male	Female
	(1)	(2)	(3)	(4)
	Panel B: Criminal Engagement (LPM)			
$Incar$	0.092**			
	(0.042)			
$IncarEarly$		0.117*	0.184*	0.081
		(0.066)	(0.106)	(0.057)
$IncarMiddle$		0.084	0.124	0.039
		(0.053)	(0.097)	(0.040)
$IncarLate$		0.036	0.032	0.054
		(0.061)	(0.168)	(0.080)
$IncarBeforeBirth$		0.094	0.185	0.027
		(0.108)	(0.189)	(0.069)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.137	0.138	0.202	0.141
$H_0: \beta_{incarE} = \beta_{incarM} = \beta_{incarL} = \beta_{incarB}$				
F-test p-value		0.849	0.885	0.924

Notes: Dependent variable is criminal engagement. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. LPM represents linear probability model results. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

3.4.2 Is the Frequency of Incarceration Spells Important?

So far, the relationship between paternal incarceration and children's outcome has been restricted to discussions of the extensive margin: how children's outcomes change when a father is first incarcerated during early, middle or late childhood. However, the effect of paternal incarceration on children's outcomes is not only associated with the timing but also the length and frequency of incarceration spells (i.e., the dosage effect).

In what follows, I examine whether the effect of paternal incarceration on children's outcome vary with dosage or the frequency of incarceration spells. Exposure at a given stage of childhood may be associated with worse outcomes because of the effectiveness of investment at that stage (see Heckman (2006) for example) or because exposure persists for a large fraction of childhood. A higher dosage due to longer parent-child separation may be associated with several incarceration spells or exposure to longer sentences (discussed in Chapter 2). Here, I estimate the influence of dosage on children's outcomes using the frequency of incarceration spells, and test if children are differentially affected by exposure to one or more spells. To achieve this, I modify Equation (3.2) and estimate Equation (3.3). Due to data constraints, I am only able to exploit the number of incarceration spells, not the length of each spell. This information was obtained from responses to the question "How many times (has/did) your biological father (spent/spend) time in jail or prison?". The estimated equation is:

$$
\begin{aligned}
H_j = \alpha + \beta_1 X_j + \beta_{spell} Spell_j + \beta_{incarB} IncarBeforeBirth_j \\
+ \beta_{incarE} IncarEarly_j + \beta_{incarM} IncarMiddle_j \\
+ \beta_{incarL} IncarLate_j + \lambda_c + \gamma_s + \epsilon_j.
\end{aligned}
\tag{3.3}
$$

Here $Spell_j$ is a binary indicator that takes a value one if individual $j's$ father was incarcerated two or more times and zero if incarcerated once. As a result, β_{spell} provides information on whether exposure to more than one incarceration spell has a statistically different effect on children's outcomes relative to exposure to one incarceration spell. Frequent incarceration spells may prevent fathers from providing adequate childcare and material resources necessary for adequate child development. If this is the case then the estimate for β_{spell} will be negative and statistically different from zero. However, if the parenting quality and level of investment does not differ with dosage then β_{spell} may not be significantly different from zero, suggesting that dosage has no significant effect on children's outcomes.

If a respondent was exposed only once, I investigate whether the effect of this exposure varied with age using four mutually exclusive dummy variables to capture exposure at different stages of childhood. $IncarEarly_j$ captures exposure during early childhood, and takes a value one if individual j was exposed once to paternal incarceration between zero and five years, and is assigned zero otherwise. $IncarMiddle_j$ takes a value one if individual j was exposed only once between six and twelve years, zero otherwise; while $IncarLate_j$ takes a value one if individual j was exposed once between thirteen and eighteen years, zero otherwise. The fourth binary dummy variable used in the model, $IncarBeforeBirth_j$, takes a value one if individual j's father was incarcerated before their birth, zero otherwise.

Tables 3.7, C.15 and C.16 provide a summary of the dosage effect results. In each table, Column 1 reflects the difference in outcome for children exposed during childhood relative to those never exposed, with the full set of controls. Column 2 displays results when age at first exposure is controlled for, given that individuals are exposed to one incarceration spell. Columns 3 and 4 re-estimate the model outlined in Column 2 for a sample of only boys and only girls, respectively. In all models, I control for dosage (exposure to two or more incarceration spells). Overall, the results seem to suggest that exposure to two or more incarceration spells during childhood is associated with large negative effects. However, these estimates are not statistically significant. Table 3.7 illustrates results when individuals are compared using cumulative high school GPA and college attendance.

Panel A of Table 3.7 displays results when children are compared using high school GPA. Across columns, the estimated effects are predominantly negative. In terms of magnitude, children exposed to two or more incarceration spells experience a large decline in high school GPA. However, the coefficients are not statistically significant. Column 1 reveals that exposure to two or more incarceration spells by age 18 is associated with a 20.6 percentage points decline in high school GPA. This estimated effect is larger in magnitude than the decline of 6.9 percentage points decline that children exposed to one incarceration spell experience. Column 2 provides estimates of the link between high school GPA and paternal incarceration when age at exposure is accounted for, if an individual was exposed to only one spell. Relative to the estimates outlined in Panel A of Table 3.3, these results are smaller in magnitude.

Panel B of Table 3.7 illustrates comparisons using college attendance. Column 2 reveals that children exposed to two or more incarceration spells are 10.3 percentage points less likely to attend college relative to those never exposed. Similar to the results outlined in Table 3.4, among individuals whose father or father figure was sent to jail or prison once, exposure during late childhood has the largest impact on college attendance rate. In particular, children exposed between thirteen and eighteen years to one incarceration spell are 12.7 percentage points less likely to attend college relative to those never exposed. This estimate is marginally significant at the 10% level. A test of whether the estimated coefficients outlined in Column 2 are identical produced a p-value of 0.798. Therefore I am not able to reject the null hypothesis of equality, though the coefficient associated with first exposure during late childhood is at least twice the estimates associated with exposure during early and middle childhood. Additionally, this result suggests that there is insufficient evidence to conclude that exposure to more than one spell affects children differently than exposure to one spell during late childhood.

Among males, no significant difference is identified, though the point estimates had the expected negative signs. For girls, again exposure between thirteen and eighteen seems to result in the largest and statistically significant decline in college attendance rate. In particular, girls exposed during late childhood are 26.3 percentage point less likely to attend college relative to girls never exposed. In sum, though large in magnitude, the results outlined in Tables 3.7, C.15 and C.16 do not suggest that exposure to more than one incarceration spell has a significant adverse effect on children. However, caution must be taken when interpreting these results since I am not able to distinguish between five one-month spells in jail and one five-year spell in prison. As a result, information on time served is expected to provide a clearer picture.

Table 3.7: Educational Outcomes While Controlling for Dosage

	All		Male	Female
	(1)	(2)	(3)	(4)
	Panel A: Cumulative GPA			
Incar	-0.069			
	(0.131)			
Spell	-0.206	-0.197	-0.264	-0.238
	(0.150)	(0.152)	(0.210)	(0.199)
IncarEarly		-0.104	-0.173	-0.045
		(0.139)	(0.268)	(0.162)
IncarMiddle		-0.027	0.092	-0.012
		(0.154)	(0.243)	(0.195)
IncarLate		-0.009	0.032	-0.058
		(0.244)	(0.372)	(0.282)
IncarBeforeBirth		-0.084	-0.187	0.059
		(0.199)	(0.311)	(0.271)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.337	0.337	0.324	0.366
	Panel B: College Attendance			
Incar	-0.063			
	(0.048)			
Spell	-0.094	-0.103*	-0.117	-0.080
	(0.057)	(0.058)	(0.087)	(0.086)
IncarEarly		-0.053	-0.041	-0.113
		(0.081)	(0.075)	(0.153)
IncarMiddle		-0.037	-0.072	-0.019
		(0.055)	(0.089)	(0.089)
IncarLate		-0.127*	0.145	-0.263**
		(0.065)	(0.118)	(0.098)
IncarBeforeBirth		-0.045	-0.050	-0.095
		(0.103)	(0.142)	(0.182)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.300	0.294	0.358	0.313

Notes: Dependent variables: Panel A, cumulative high GPA and in Panel B, college attendance. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

3.4.3 Sensitivity Analysis

Though I am not able to exploit an exogenous change in paternal incarceration in this paper, I attempt to minimize selection bias by controlling for observable differences between individuals when comparing the treatment and control groups. To check whether unobservable differences are driving the results presented, I use placebo regressions, which look for "effects" where there should not (or could not) be a causal pathway. To estimate these placebo regressions I use the model outlined in Equation (3.2), but instead of comparing children treated by age eighteen, my treatment group includes individuals exposed as adults (after age eighteen). Panel A of Table 3.8 displays the results. Because the dependent variable in these regressions (high school GPA and High School completion) were measured before the independent variable (exposure after age eighteen), the independent variable could not possibly cause it. Therefore, no significant difference in the high school performance between the new treatment group, and those never exposed is expected.

Column 1, Panel A of Table 3.8 suggests that individuals exposed to paternal incarceration earn 0.013 points higher GPA relative to those never exposed, on average. Though Column 2 indicates that those exposed to paternal incarceration as adults are 2.1 percentage less likely to achieve a high school diploma or GED, the results are statistically insignificant. These estimates seem to suggest that the estimated relationship between paternal incarceration and high school performance is not spurious, since they reveal no significant difference where there should be none. That is, no significant difference was identified between the high school performance of individuals exposed to paternal incarceration as post high school and those never exposed. These results also provide suggestive evidence that the estimated adverse effects outlined in Table 3.3 are not driven by exposure to low quality home environment but by separation from one's father or father figure due to incarceration.

To further check the validity of the results, I compare high school outcomes (cumulative GPA and receipt of a high school diploma) among those who report having a father incarcerated outside of childhood (before birth or as an adult) with those exposed during childhood. Individuals exposed outside of childhood may perform worse than those never exposed because having a parent who is eventually incarcerated may be informative of whether that parent engaged in criminal activity. Criminal engagement may deplete the quality of the home environment in which a child was raised. On the other hand, children exposed during childhood may experience adverse outcomes relative to those never exposed because they are raised in low quality home environments or because of the negative shock associated with separation from parent due to incarceration. Therefore, if a significant difference is identified between individuals exposed during childhood and those exposed as adults, the difference may be driven by adversities associated with incarceration itself, since both groups are likely exposed to low quality home environments. I test this hypothesis by limiting the sample to only individuals who report exposure to paternal incarceration. Here the treatment group includes youths exposed between zero and eighteen years, but instead of those never exposed, the control group includes individuals exposed to paternal incarceration before birth or as adults (after age eighteen). The results are outlined in Panel B of Table 3.8.

Panel B of Table 3.8 shows that children exposed during childhood earn lower high school GPA and are less likely to complete high school relative to children exposed as adults. This provides additional evidence that the estimated effect may be driven by direct separation since both groups may be exposed to low quality home environments. However more analysis is needed to arrive at a definitive conclusion, though having the expected signs these results are statistically insignificant.

Table 3.8: Sensitivity Analysis

	Cum. GPA	High Sch. Diploma
	Panel A: Treatment Group exposed After Age 18	
Exposed	0.013	-0.021
	(0.110)	(0.058)
Child-specific controls	Yes	Yes
Caregiver-specific controls	Yes	Yes
School fixed effects	Yes	Yes
Cohort fixed effect	Yes	Yes
Observations	2,474	2,474
R^2	0.156	0.170
	Panel B: Sample Restricted to Only Youths Exposed	
$IncarEarly$	-0.316	-0.130
	(0.257)	(0.105)
$IncarMiddle$	-0.207	-0.150
	(0.240)	(0.106)
$IncarLate$	-0.239	-0.155
	(0.337)	(0.171)
Child-specific controls	Yes	Yes
Caregiver-specific controls	Yes	Yes
School fixed effects	Yes	Yes
Cohort fixed effects	Yes	Yes
Observations	327	327
R^2	0.586	0.587

Notes: Dependent variables are cumulative high GPA, and an indicator variable for the receipt of a high school diploma or GED. In Panel A, the treatment group was exposed to paternal incarceration after age eighteen, while the control group was never exposed. In Panel B, the treatment group are those exposed during childhood while the control group captures those exposed to paternal incarceration for the first time as adults. Control variables include child-specific controls such as gender (female), race, ethnicity, birth order, birth weight, and immigration status as well as primary caregiver's highest educational attainment (a proxy for SES), their immigration status and mother's age at birth. I also include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

3.5 Conclusion

My goal in this paper is to examine whether timing or age at exposure is essential when considering the effect of paternal incarceration on children's short and long term outcomes. Given that child development is not linear in age, and children of varying age cope differently with stress, it stands to reason that younger children may be more severely impacted because they tend to rely more heavily on family influence, or older children who model parents, may suffer a considerable loss when a father becomes incarcerated. I use data from the National Longitudinal Study of Adolescent to Adult Health to show that there exists negative relationship between exposure to paternal incarceration during children and children's short and long term outcomes. Additionally, I present suggestive evidence that children exposed before age six, primarily boys, perform worse in school and earn less income when compared with children never exposed. Children exposed during late childhood are also negatively affected by paternal incarceration. Notably, females exposed during late childhood are less likely to complete college, and receive lower income relative to females never exposed. These results confirm existing studies of a negative link between paternal incarceration and children's outcome, however more work is needed to precisely identify sources of heterogeneity, as I am not able to statistically reject the null hypothesis that the effect of exposure is identical across ages groups.

Although the analyses represent an important attempt to present a picture of how the effect of paternal incarceration varies with age at exposure, several limitations are noteworthy. First, it should be noted that the analysis does not differentiate between parents detained in jail and those serving time in prison. Jail and prison inmates may differ in several important ways, and children may respond differently to parents who spend a few weeks in a local jail relative to months or years in prison. I am not able to account for this potential difference with the current data, and so results must be considered in this context. Second, my analyses do not account for time served, which may have a direct effect on the child's location while parents are detained, as well as how much physical contact parents and children maintain.

Despite these limitations, this paper may be viewed as an important first step towards understanding the importance of gender and age at exposure when considering the effect of paternal incarcerations. The results outlined here may guide future causal studies, focusing on the effect of paternal incarceration across childhood. It may also guide public policy. Since the impact on children varies with age, and children exposed during early and late childhood may benefit more from interventions when a parent is incarcerated.

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Appendix A

Appendix: Chapter 1

Table A.1: Comparison of CPS and SIPP for Licensing Data

CPS	SIPP
• Includes birthplace • Includes years since migration • Large, ongoing sample • Longitudinal: licensing information asked twice, one year apart	• English proficiency recorded • Additional licensing characteristics, e.g., Was an exam required? • Longitudinal, but licensing only asked in one wave

Table A.2: Summary Statistics, Probability Sample, SIPP

	All		License		No License	
	Native mean	Imm. mean	Native mean	Imm. mean	Native mean	Imm. mean
Female	0.495	0.439				
Credentials:						
Credential	0.296	0.188	1.000	1.000	0.146	0.092
License	0.176	0.106	1.000	1.000	0.000	0.000
Certificate	0.121	0.082	0.000	0.000	0.146	0.092
Education:						
HS Dropout	0.035	0.217	0.010	0.073	0.041	0.234
HS Grad	0.233	0.235	0.113	0.095	0.258	0.251
Some Coll.	0.384	0.243	0.345	0.328	0.392	0.233
BA	0.230	0.191	0.282	0.277	0.219	0.180
GRAD	0.118	0.115	0.250	0.226	0.090	0.101
Race/Ethnicity:						
White	0.838	0.643	0.864	0.496	0.833	0.661
Black	0.111	0.099	0.090	0.170	0.116	0.091
Asian	0.014	0.217	0.016	0.302	0.014	0.207
Other	0.036	0.040	0.031	0.031	0.037	0.041
Hispanic	0.101	0.463	0.075	0.239	0.107	0.490
Other:						
Age	40.828	41.431	42.841	43.068	40.399	41.237
Married	0.565	0.675	0.660	0.699	0.545	0.672
# Children	0.739	1.081	0.795	0.915	0.728	1.100
Union	0.135	0.106	0.261	0.225	0.108	0.091
Pay by Hour	1.469	1.408	1.607	1.596	1.439	1.385
Govt. Worker	0.185	0.098	0.354	0.233	0.149	0.082
Service Worker	0.278	0.297	0.237	0.286	0.286	0.298
Imm. by English Ability:						
Very Well		0.604		0.836		0.577
Not Well		0.396		0.164		0.423
Observations	18,054	3,215	3,198	340	14,856	2,875

Notes: Numbers are the mean of the variable. Occupation controls are at the three-digit level. Source: 2008 Survey of Income and Program Participation.

Table A.3: Summary Statistics: License Requirements, SIPP

	All		Men		Women	
	Native mean	Imm. mean	Native mean	Imm. mean	Native mean	Imm. mean
Course or Training	0.942	0.940	0.923	0.904	0.956	0.962
Skills or Exam	0.922	0.918	0.921	0.910	0.922	0.922
Continuing Education	0.768	0.754	0.724	0.696	0.800	0.791
Reason for License:						
Work-related	0.990	0.992	0.989	0.993	0.990	0.992
Personal	0.020	0.010	0.021	0.014	0.020	0.007
Observations	3,198	340	1,282	130	1,916	210

Notes: Numbers are the mean of the variable. Source: 2008 Survey of Income and Program Participation.

Table A.4: Most Common Occupations of Licensed Workers

Panel A: Men

All		GRAD	
Natives	Immigrants	Natives	Immigrants
Truck, delivery, and tractor drivers (8.1)	Physicians (8.6)	Lawyers (16.1)	Physicians (26.0)
Primary school teachers (4.8)	Truck, delivery, and tractor drivers (7.0)	Physicians (12.6)	Subject instructors (HS/college) (7.0)
Managers and administrators, n.e.c. (4.2)	Registered nurses (4.6)	Primary school teachers (11.0)	Managers and administrators (5.0)
Police and detectives (4.0)	Managers and administrators, n.e.c. (3.8)	Secondary school teachers (8.4)	Primary school teachers (3.6)
Lawyers (4.0)	Nursing aides, orderlies, and attendants (3.3)	Managers in education (4.3)	Computer software developers (3.3)
Secondary school teachers (3.3)	Taxi cab drivers and chauffeurs (2.8)	Subject instructors (HS/college) (3.3)	Accountants and auditors (2.8)
Physicians (3.1)	Electricians (2.7)	Managers and administrators, n.e.c. (2.9)	Registered nurses (2.7)
Electricians (2.7)	Subject instructors (HS/college) (2.7)	Pharmacists (2.6)	Civil engineers (2.5)
Registered nurses (2.5)	Cooks, variously defined (2.2)	Registered nurses (2.0)	Lawyers (2.5)
fire fighters (2.2)	Computer software developers (2.1)	Accountants and auditors (1.9)	Computer systems analysts (2.5)

Panel B: Women

All		GRAD	
Natives	Immigrants	Natives	Immigrants
Registered nurses (16.7)	Registered nurses (19.6)	Primary school teachers (24.0)	Physicians (17.4)
Primary school teachers (14.3)	Nursing aides, orderlies, and attendants (17.1)	Registered nurses (8.0)	Registered nurses (11.8)
Nursing aides, orderlies, and attendants (5.9)	Hairdressers and cosmetologists (8.6)	Secondary school teachers (6.4)	Primary school teachers (11.0)
Secondary school teachers (3.5)	Primary school teachers (5.4)	Lawyers (6.4)	Subject instructors (HS/college) (5.6)
Licensed practical nurses (2.6)	Physicians (4.8)	Physicians (5.3)	Lawyers (4.3)
Hairdressers and cosmetologists (2.3)	Licensed practical nurses (3.1)	Vocational counselors (4.2)	Secondary school teachers (3.6)
Lawyers (2.1)	Health aides, except nursing (1.9)	Managers in education (3.8)	Pharmacists (3.5)
Health aides, except nursing (2.1)	Accountants and auditors (1.8)	Special education teachers (3.4)	Physical therapists (3.1)
Teachers , n.e.c. (2.0)	Physical therapists (1.7)	Social workers (3.2)	Accountants and auditors (3.0)
Kindergarten and early childhood teachers (1.9)	Subject instructors (HS/college) (1.7)	Subject instructors (HS/college) (2.9)	Dentists (2.3)

Notes: This table shows the most common occupations among licensed workers by group. Numbers in parentheses are the fraction of the licensed workers in that group that are in the given occupation. The first two columns (under "All") include all workers, while the final two columns (under "GRAD") only include workers with more than a bachelor's degree. Source: Current Population Survey, 2016-2019.

Table A.5: Linear Probability Models, Occupational License, SIPP

	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.053***	0.053***				
	(0.0051)	(0.0051)				
Immigrant	-0.048***	-0.071***	-0.056***	-0.074***	-0.036***	-0.065***
	(0.0081)	(0.0112)	(0.0102)	(0.0119)	(0.0125)	(0.0198)
Imm. By English Ability:						
Very Well		0.033***		0.027*		0.042*
		(0.0124)		(0.0138)		(0.0215)
Education:						
HS Grad	0.017**	0.011	0.022**	0.018*	0.015	0.007
	(0.0075)	(0.0077)	(0.0095)	(0.0094)	(0.0135)	(0.0148)
Some Coll.	0.083***	0.076***	0.070***	0.065***	0.111***	0.101***
	(0.0085)	(0.0085)	(0.0099)	(0.0101)	(0.0145)	(0.0155)
BA	0.129***	0.121***	0.065***	0.060***	0.206***	0.196***
	(0.0088)	(0.0093)	(0.0107)	(0.0109)	(0.0162)	(0.0178)
GRAD	0.262***	0.255***	0.183***	0.177***	0.353***	0.342***
	(0.0142)	(0.0143)	(0.0178)	(0.0178)	(0.0215)	(0.0222)
Mean	0.164	0.164	0.133	0.133	0.198	0.198
Observations	21,269	21,269	10,617	10,617	10,652	10,652
R^2	0.070	0.070	0.049	0.049	0.092	0.093

Notes: Dependent variable is binary, and equals one if the worker has an occupational license, and zero otherwise. All estimations include controls for age (as a third-order polynomial), state of residence, government worker dummy variable, service worker dummy variable, paid-by-the-hour dummy variable, union status, marital status, number of children, and racial and ethnic dummy variables (Black, Asian, other, and Hispanic). Omitted education group is high school dropout, and the omitted language group is immigrants who do not speak English very well. Row "Mean" shows the mean of the dependent variable. Source: 2008 Survey of Income and Program Participation.
Standard errors in parentheses, and are calculated using balanced repeated replication, with Fay's adjustment factor of 0.5 and 120 SIPP-provided replicate weights. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table A.6: OLS Regressions, Log Hourly Wage and Hours Worked Per Week, SIPP

Panel A: Log Hourly Wage

	All			Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Immigrant	-0.101***	-0.104***	-0.243***	-0.121***	-0.121***	-0.246***	-0.075***	-0.080***	-0.238***
	(0.0142)	(0.0144)	(0.0154)	(0.0192)	(0.0195)	(0.0205)	(0.0176)	(0.0179)	(0.0232)
License	0.065***	0.062***	0.065***	0.010	0.009	0.012	0.110***	0.107***	0.111***
	(0.0103)	(0.0110)	(0.0110)	(0.0138)	(0.0146)	(0.0145)	(0.0144)	(0.0154)	(0.0154)
License x Imm		0.026	-0.018		0.005	-0.037		0.026	-0.018
		(0.0346)	(0.0352)		(0.0547)	(0.0554)		(0.0403)	(0.0391)
Imm. By English Ability:									
Very Well			0.215***			0.199***			0.237***
			(0.0178)			(0.0284)			(0.0228)
Observations	21,269	21,269	21,269	10,617	10,617	10,617	10,652	10,652	10,652
R^2	0.436	0.436	0.440	0.443	0.443	0.447	0.419	0.419	0.424

Panel B: Hours Worked Per Week

	All			Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Immigrant	-0.525**	-0.433*	-0.420	-0.985***	-0.950***	-0.930**	-0.047	0.165	0.061
	(0.2259)	(0.2385)	(0.3146)	(0.2939)	(0.3047)	(0.3580)	(0.3246)	(0.3359)	(0.4782)
License	0.615***	0.703***	0.702***	1.022***	1.063***	1.063***	0.446*	0.607**	0.609**
	(0.1893)	(0.1970)	(0.1984)	(0.3077)	(0.3130)	(0.3141)	(0.2439)	(0.2621)	(0.2643)
License x Imm		-0.762	-0.758		-0.392	-0.385		-1.346	-1.374*
		(0.7026)	(0.7118)		(1.0471)	(1.0479)		(0.8153)	(0.8275)
Imm. By English Ability:									
Very Well			-0.020			-0.031			0.155
			(0.3693)			(0.3539)			(0.6349)
Observations	21,269	21,269	21,269	10,617	10,617	10,617	10,652	10,652	10,652
R^2	0.185	0.185	0.185	0.165	0.165	0.165	0.168	0.169	0.169

Notes: Dependent variable is log of hourly wage in Panel A and usual hours worked per week in Panel B. All estimations include controls for age (as a third-order polynomial), state of residence, educational attainment (five categories), government worker dummy variable, service worker dummy variable, paid-by-the-hour dummy variable, union status, marital status, number of children, and racial and ethnic dummy variables (Black, Asian, other, and Hispanic). Omitted education group is high school dropout, and the omitted language group is immigrants who do not speak English very well. Source: 2008 Survey of Income and Program Participation.

Standard errors in parentheses, and are calculated using balanced repeated replication, with Fay's adjustment factor of 0.5 and 120 SIPP-provided replicate weights. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, *** $p < 0.001$

Appendix B

Appendix: Chapter 2

Table B.1: Comparison of State Strikes Laws

State (Year)	Least number of strikes required to trigger enhanced sentencing	Types of Crime	Number imprisoned under Three Strikes law after 10 years
Alaska (1996)*			
Arkansas (1995)	Two	Violent Crime	5 (.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	NA
Louisiana (1994)	Three	Violent Crime	NA
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	NA
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%)

Notes: In paranthesis we represent the number of inmates sentenced under Three Strikes as a proportion of total admissions in 1998. Sources: Schiraldi, Colburn and Lotke (2004) and Dickey and Hollenhorst (1999). *There is a debate in the criminology literature about whether Alaska's law is considered a three strike law.

Table B.2: 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	0.047	0.035	0.219	0.058	0.084	0.059
	(0.213)	(0.184)	(0.414)	(0.234)	(0.278)	(0.237)
Male	0.916	0.896	0.945	0.919	0.932	0.907
	(0.277)	(0.305)	(0.227)	(0.273)	(0.251)	(0.290)
Age 18-24	0.320	0.274	0.377	0.339	0.405	0.363
	(0.467)	(0.446)	(0.485)	(0.473)	(0.491)	(0.481)
Age 25-34	0.415	0.370	0.385	0.350	0.380	0.346
	(0.493)	(0.483)	(0.487)	(0.477)	(0.485)	(0.476)
Age 35-44	0.197	0.262	0.172	0.230	0.162	0.218
	(0.398)	(0.440)	(0.378)	(0.421)	(0.369)	(0.413)
Age 45-54	0.051	0.0758	0.048	0.065	0.039	0.058
	(0.219)	(0.267)	(0.213)	(0.246)	(0.194)	(0.233)
Age 55 or older	0.017	0.018	0.017	0.017	0.013	0.014
	(0.128)	(0.134)	(0.131)	(0.129)	(0.114)	(0.118)
White	0.307	0.323	0.362	0.356	0.049	0.201
	(0.461)	(0.468)	(0.480)	(0.479)	(0.217)	(0.401)
Black	0.366	0.308	0.248	0.354	0.130	0.432
	(0.482)	(0.462)	(0.432)	(0.478)	(0.334)	(0.495)
Other Race	0.062	0.076	0.268	0.205	0.736	0.212
	(0.241)	(0.266)	(0.443)	(0.404)	(0.441)	(0.409)
Hispanic	0.266	0.294	0.122	0.085	0.084	0.155
	(0.442)	(0.455)	(0.328)	(0.279)	(0.277)	(0.362)
Violent Crime	0.338	0.301	0.515	0.399	0.351	0.277
	(0.473)	(0.459)	(0.50)	(0.490)	(0.477)	(0.447)
Property Crime	0.259	0.264	0.199	0.219	0.309	0.257
	(0.438)	(0.441)	(0.399)	(0.414)	(0.4n62)	(0.437)
Other Crime	0.403	0.435	0.286	0.382	0.340	0.466
	(0.491)	(0.496)	(0.452)	(0.486)	(0.474)	(0.499)
Long-run Outcomes	*7 or younger*	*8-13*	*7 or younger*	*8-13*	*7 or younger*	*8-13*
Employed:	0.874	0.888	0.888	0.885	0.899	0.895
	(0.332)	(0.316)	(0.316)	(0.319)	(0.302)	(0.306)
Full-time Worker:	0.588	0.623	0.611	0.696	0.720	0.706
	(0.492)	(0.485)	(0.488)	(0.460)	(0.449)	(0.456)
Part-time Worker:	0.368	0.332	0.341	0.277	0.253	0.266
	(0.482)	(0.471)	(0.474)	(0.448)	(0.435)	(0.442)
Home Production:	0.026	0.026	0.027	0.016	0.016	0.016
	(0.158)	(0.160)	(0.162)	(0.127)	(0.125)	(0.125)
Earnings (10,000):	$1.757	$1.831	$1.825	$2.158	$2.188	$2.172
	(2.442)	(2.372)	(2.414)	(2.452)	(2.383)	(2.464)
Household Income (10,000):	$7.199	$6.871	$6.850	$7.279	$6.840	$6.863
	(6.479)	(6.179)	(6.358)	(6.191)	(5.710)	(5.860)
High School Grad:	0.856	0.861	0.866	0.764	0.767	0.776
	(0.351)	(0.346)	(0.341)	(0.424)	(0.423)	(0.417)
College Attendance:	0.627	0.628	0.647	0.637	0.642	0.662
	(0.484)	(0.483)	(0.478)	(0.481)	(0.479)	(0.473)

Notes: The crime data represent averages over the sample period 1989 to 2000. Statistics have been computed using state population weights. For the long-run outcomes, 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 American Community Survey birth cohorts 1974 to 1990.

Table B.3: Type of Labor Force Participation: Standard Diff-in-Diff

	Panel A: Top Three Used States: CA, GA, FL		
Variable	In Labor Force	Self Employment	Armed Forces
Used	0.0044	0.0055	-0.0024
	(0.0053)	(0.0053)	(0.0029)
Unused	-0.0067	0.0111**	-0.0003
	(0.0074)	(0.0046)	(0.0023)
	Panel B: State's Three Strikes use is > 0.01 *of admissions in 1998*		
Variable	In Labor Force	Self Employment	Armed Forces
Used	0.0022	0.0071	-0.0035
	(0.0061)	(0.0049)	(0.0024)
Unused	-0.0072	0.0111**	0.0018
	(0.0079)	(0.0047)	(0.0023)
state fixed effects	X	X	X
birth cohort fixed effects	X	X	X
state specific linear time trend	X	X	X
Observations	399,721	399,721	399,721

Notes: *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Teen pregnancy is only measured for females. All standard errors are clustered by state. Standard errors are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

Table B.4: Labor Market Outcomes: Separate Age Effects

	Panel A: Top Three Used States: CA, GA, FL				
Variable	Employed	Part-time	Home Production	Earnings	H.H. Income
$Used * old$	0.0009	-0.0147**	0.0034*	0.1765*	0.3592***
	(0.0063)	(0.0064)	(0.0018)	(0.0958)	(0.1350)
$Used * middle$	0.0036	-0.0133	0.0049**	0.1437	0.6045***
	(0.0075)	(0.0101)	(0.0024)	(0.1423)	(0.1851)
$Used * young$	0.0150	-0.0162*	0.0062**	0.0798	0.6930***
	(0.0095)	(0.0095)	(0.0032)	(0.1802)	(0.2519)
$Unused$	0.0005	0.0002	0.0019	0.1184	0.0957
	(0.0058)	(0.0081)	(0.0025)	(0.0736)	(0.1324)
	Panel B: State's Three Strikes use is > 0.01 *of admissions in 1998*				
Variable	Employed	Part-time	Home Production	Earnings	H.H. Income
$Used * old$	0.0043	-0.0170**	0.0057**	0.2322***	0.4553***
	(0.0060)	(0.0072)	(0.0028)	(0.0832)	(0.1382)
$Used * middle$	0.0112	-0.0195**	0.0083***	0.2524**	0.7878***
	(0.0088)	(0.0095)	(0.0032)	(0.1185)	(0.1736)
$Used * young$	0.0188*	-0.0242**	0.0107***	0.2477	0.8667***
	(0.0104)	(0.0106)	(0.0041)	(0.1570)	(0.2140)
$Unused$	-0.0026	0.0044	0.0003	0.0798	-0.0187
	(0.0063)	(0.0080)	(0.0023)	(0.0748)	(0.1053)
state fixed effects	X	X	X	X	X
birth cohort fixed effects	X	X	X	X	X
state specific linear time trend	X	X	X	X	X
Observations	308,610	285,481	285,481	285,481	285,481

Notes: Entries in Column 1 are average marginal effects from a logit, while Columns 2 and 3 include average marginal effects from a multinomial logit. For the multinomial logit regression model, the base Outcome 2: full-time employment; Outcome 1: part-time employment (Part-time); Outcome 0: working less than 10 hours per week (Home Production). $Used$ 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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Results for full-time, part-time employment, and home production (working but less than 10 hours per week) as well as yearly earned income and yearly household income are conditional on being employed. Yearly earned income and yearly household income were divided by \$10,000. All standard errors are clustered by state. Standard errors are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

Table B.5: Female Only Sample

	Panel A: Top Three Used States: CA, GA, FL					
Variable	Employed	Full-time	Part-time	Home Production	Earnings	H.H. Income
Used	0.0122	-0.0052	0.0005	0.0707	0.2602	
	(0.0130)	(0.0090)	(0.0032)	(0.0579)	(0.2377)	
Unused	0.0096	0.0044	0.0010	-0.0287	-0.0295	
	(0.0081)	(0.0099)	(0.0040)	(0.0731)	(0.1914)	
	Panel B: State's Three Strikes use is > 00.01 *of admissions in 1998*					
Variable	Employed	Part-time	Home Production	Earnings	H.H. Income	
Used	0.0162	-0.0062	0.0030	0.0836	0.3267	
	(0.0114)	(0.0091)	(0.0044)	(0.0545)	(0.2388)	
Unused	0.0053	0.0072	-0.0015	-0.0602	-0.1434	
	(0.0076)	(0.0102)	(0.0037)	(0.0791)	(0.1783)	
state fixed effects	X	X	X	X	X	
birth cohort fixed effects	X	X	X	X	X	
state specific linear time trend	X	X	X	X	X	
Observations	154,858	144,167	144,167	144,167	144,167	

Notes: Entries in Column 1 are average marginal effects from a logit, while Columns 2 and 3 include average marginal effects from a multinomial logit. For the multinomial logit regression model, the base Outcome 2: full-time employment; Outcome 1: part-time employment (Part-time); Outcome 0: working less than 10 hours per week (Home Production). *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Results for full-time, part-time employment, and home production (working but less than 10 hours per week) as well as yearly earned income and yearly household income are conditional on being employed. Yearly earned income and yearly household income were divided by $10,000. All standard errors are clustered by state and are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

Table B.6: Male Only Sample: Non-Financial Outcomes

	Panel A: Top Three Used States: CA, GA, FL			
Variable	Number of Child	Married	High School	College
Used	0.0303*	-0.0233***	-0.0150*	-0.0270***
	(0.0164)	(0.0094)	(0.0081)	(0.0108)
Unused	0.0011	-0.0050	-0.0183**	-0.0185
	(0.0233)	(0.0141)	(0.0091)	(0.0137)
	Panel B: State's Three Strikes use is >0.01 *of admissions in 1998*			
Variable	Number of Child	Married	High School	College
Used	0.0186	-0.0075	-0.0189	-0.0305*
	(0.0183)	(0.0167)	(.0099)	(0.0133)
Unused	0.0043	-0.0139	-0.0157	-0.0137
	(0.0267)	(0.0124)	(0.0089)	(0.0138)
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	196,851	196,851	196,851	196,851

Notes: Teen pregnancy is only measured for females. *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). All standard errors are clustered by state. Standard errors are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

Table B.7: Males Only Sample (White and Asian)

	Panel A: Top Three Used States: CA, GA, FL				
Variable	High School	College	Employed	Earnings	H.H. Income
Used	-0.0084	-0.0206	-0.0229***	0.2046	0.2421
	(0.0124)	(0.0178)	(0.0078)	(0.1655)	(0.2452)
Unused	-0.0149	-0.0174	-0.0051	0.2110*	0.3423**
	(0.0104)	(0.0162)	(0.0074)	(0.1133)	(0.1603)
	Panel B: State's Three Strikes use is > 0.01 *of admissions in 1998*				
Variable	High School	College	Employed	Earnings	H.H. Income
Used	-0.0125	-0.0241	-0.0189*	0.2265	0.4131
	(0.0129)	(0.0186)	(0.0081)	(0.1360)	(0.2297)
Unused	-0.0135	-0.0142	-0.0043	0.1975	0.2486
	(0.0101)	(0.0162)	(0.0078)	(0.1184)	(0.1458)
state fixed effects	X	X	X	X	X
birth cohort fixed effects	X	X	X	X	X
state specific linear time trend	X	X	X	X	X
Observations	149,902	149,902	121,712	113,450	113,450

Notes: *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Results for full-time, part-time employment, and home production (working but less than 10 hours per week) as well as yearly earned income and yearly household income are conditional on being employed. Yearly earned income and yearly household income were divided by $10,000. All standard errors are clustered by state and are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

Table B.8: Males Only Sample (Blacks, Hispanic, and other)

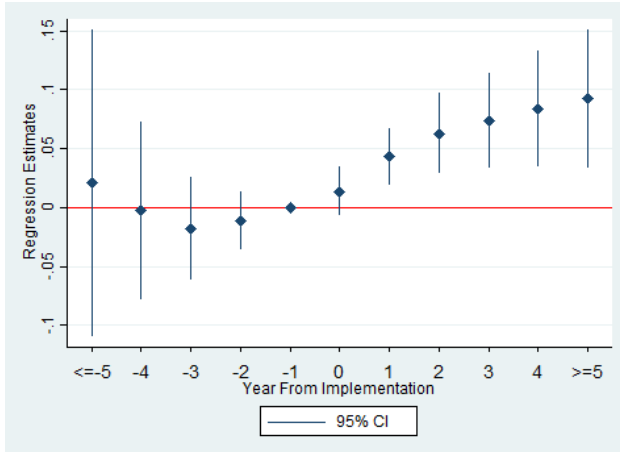
	Panel A: Top Three Used States: CA, GA, FL				
Variable	High School	College	Employed	Earnings	H.H. Income
Used	-0.0265	-0.0330	-0.0139	0.7319***	0.0614
	(0.0195)	(0.0272)	(0.0247)	(0.2482)	(0.5906)
Unused	-0.0299	-0.0249	-0.0151	0.4765	-0.3329
	(0.0288)	(0.0360)	(0.0269)	(0.3051)	(0.4025)
	Panel B: State's Three Strikes use is > 0.01 *of admissions in 1998*				
Variable	High School	College	Employed	Earnings	H.H. Income
Used	-0.0313	-0.0374	-0.0062	0.8220**	0.1850
	(0.0194)	(0.0258)	(0.0233)	(0.2903)	(0.5075)
Unused	-0.0244	-0.0177	-0.0234	0.3037	-0.5790
	(0.0331)	(0.04117)	(.0294)	(0.2581)	(0.3772)
state fixed effects	X	X	X	X	X
birth cohort fixed effects	X	X	X	X	X
state specific linear time trend	X	X	X	X	X
Observations	46,949	46,949	32,033	27,864	27,864

Notes: *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Results for full-time, part-time employment, and home production (working but less than 10 hours per week) as well as yearly earned income and yearly household income are conditional on being employed. Yearly earned income and yearly household income were divided by $10,000. All standard errors are clustered by state and are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

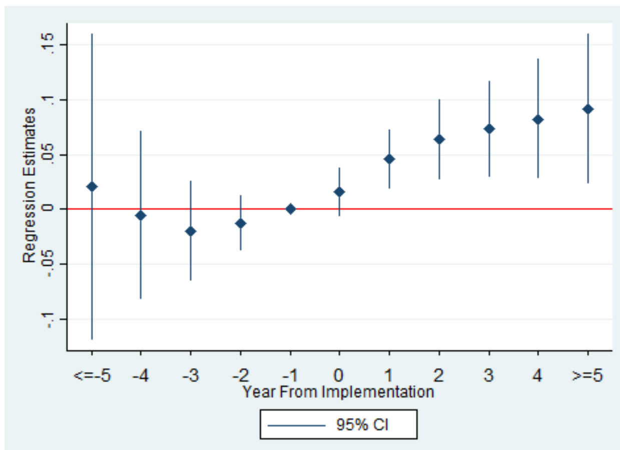
Table B.9: Males Only Sample (Blacks)

	Panel A: Top Three Used States: CA, GA, FL			
Variable	Labor Force Participation	Employed	Earnings	H.H. Income
Used	0.0244	-0.1162***	0.9279***	-0.3282
	(0.0489)	(0.0231)	(0.2670)	(1.1908)
Unused	-0.0331	-0.0297	0.4834*	-0.3564
	(0.0511)	(0.0400)	(0.2499)	(0.4400)
	Panel B: State's Three Strikes use is >0.01 *of admissions in 1998*			
Variable	Labor Force Participation	Employed	Earnings	H.H. Income
Used	0.0116	-0.0778**	0.9516***	-0.0796
	(0.0467)	(0.0375)	(0.2452)	(0.8550)
Unused	-.0340	-0.0335	0.3495	-0.5800
	(.0573)	(0.0453)	(0.2662)	(0.4661)
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	21,970	13,523	11,331	11,331

Notes: *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Employment is conditional on being part of the labor force given we found no differences in labor force participation in Three Strikes states. Results for yearly earned income and yearly household income are conditional on being employed and are dollar values divided by $10,000. All standard errors are clustered by state and are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

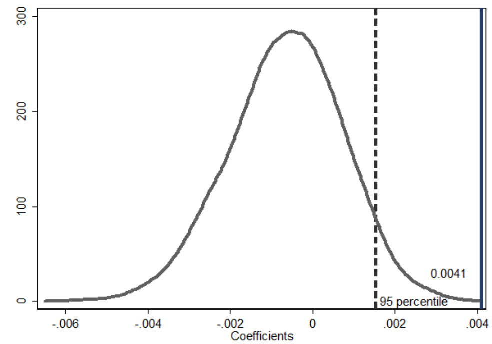


(a) Top Three Used States: CA, GA, FL

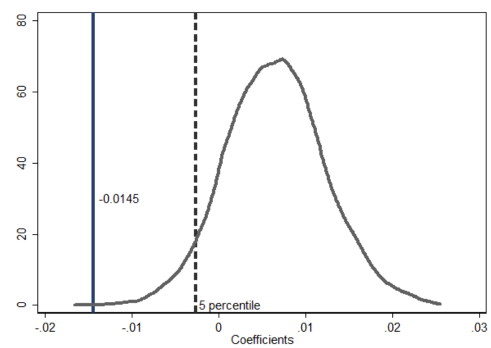


(b) State's three strike use is > .01 of admissions in 1998

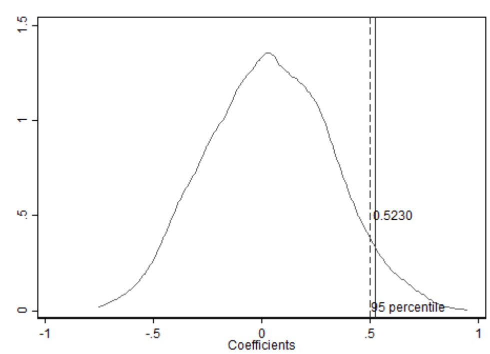
Figure B.1: Probability of Receiving Sentence Length of 25 Years or More
The figure plots coefficients from an event-study analysis. The reference period is one year before Three Strikes law was implemented Source: National Corrections Reporting Program, 1991-2015, Dataset 001: Term Records.



(a) Home Production: Distribution of Coefficients



(b) part-time Employment: Distribution of Coefficients



(c) Hourly Wage: Distribution of Coefficients

Figure B.2: Distribution of Coefficients

Figures B.2a, B.2b, and B.2c show the distribution of the coefficient on $Used_{st}$ (β_1) in 5,000 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 or upper 5% of the distribution. The solid line shows our actual estimate as reported in the bottom panel of Table 2.2.

B.1 Appendix: Robustness Check for Arrest Rates

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. To address this concern we compare arrest rates across the treated and control states by estimating the following model:

$$\begin{aligned} Y_{ct} = &+\beta_1 Used_{ct} + \beta_2 * Black_{ct} + \beta_3 Used_{ct} * Black_{ct} + \beta_4 Unused_{ct} \\ &+ X'_{ct}\gamma + \lambda_t + \eta_s + \theta_c t + \epsilon_{ct} \end{aligned} \tag{B.1}$$

where Y_{ct} is defined as log of adult arrests in county c at time t. $Used$ and $Unused$ are dummy variables, which follow the previously mentioned definitions. X_{ct} is a vector of demographic characteristics.

Table B.10 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. This suggests that arrest rates in the treatment states were not statistically different from the rates in control states. This result suggests the treatment effect of enhanced sentence lengths is not being confounded with changes in arrest rates.

Table B.10: Impact of Three Strikes Law Implementation on State Arrest

	State's Three Strikes use is > 0.01 *of admissions in 1998*
Variable	Arrest Rate
$Used$	-0.038
	(0.124)
$Unused$	0.077
	(0.074)
Black	0.130***
	(0.017)
$Used * Black$	0.040
	(0.022)
Hispanic	0.038
	(0.022)
Other race	0.029
	(0.020)
county fixed effects	X
year fixed effects	X
county specific linear time trend	X
Observations	11,972

Notes: The outcome variable is the log of adult arrest rate by county from 1994 to 1999. $Used$ 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, set equal to one for all other strike states and zero otherwise. The model incorporates 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 * p<0.05, ** p<0.01, *** p<0.001. The model includes county population weights. Since Texas always had a Three Strikes policy, observations from Texas are excluded from all models.

B.2 Appendix: Non-financial Outcomes

Table B.11 shows the impact of Three Strikes policies on non-financial outcomes. Namely, we estimate impacts on teen pregnancy among females, the number of children for both sexes, marital status for both sexes, and educational attainment for both sexes. Since all of these outcomes are dummy variables, we use logistic regressions and show marginal effects. The significant results are limited to when our definition of high usage only includes California, Georgia and Florida. In this case, there is a decline in having completed high school and having enrolled in college. In the bottom section of the table the coefficients are no longer significant at the five percent level.

Table B.11: Non-Financial Outcomes: Standard Diff-in-Diff

	Panel A: Top Three Used States: CA, GA, FL				
Variable	Teen pregnancy	Number of Child	Married	High School	College
Used	0.0093	-0.0083	-0.0127	-0.0131**	-0.0250***
	(0.0157)	(0.0223)	(0.0086)	(0.0058)	(0.0091)
Unused	0.0010	0.0230	0.0037	-0.0014	0.0017
	(0.0112)	(0.0222)	(0.0097)	(0.0078)	(0.0122)
	Panel B: State's Three Strikes use is > 0.01 *of admissions in 1998*				
Variable	Teen pregnancy	Number of Child	Married	High School	College
Used	0.01076	0.0028	-0.0023	-0.0139	-0.0229
	(0.0140)	(0.0212)	(0.0111)	(0.0091)	(0.0137)
Unused	-0.0018	0.0211	-0.0010	0.0019	0.0060
	(0.0117)	(0.0246)	(0.0102)	(0.0062)	(0.0102)
state fixed effects	X	X	X	X	X
birth cohort fixed effects	X	X	X	X	X
state specific linear time trend	X	X	X	X	X
Observations	202,870	399,721	399,721	399,721	399,721

Notes: *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic), except teen pregnancy, in which the sample is all female. All standard errors are clustered by state. Standard errors are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

B.3 Appendix: Non-financial Outcomes for Females

We also test for differences in non-financial outcomes for our female only sample. However, we do not find any evidence of effects due to Three Strikes. As with our labor market outcomes, males are more impacted.

Table B.12: Female Only Sample: Non-Financial Outcomes

	Panel A: Top Three Used States: CA, GA, FL			
Variable	Number of Child	Married	High School	College
Used	-0.0423	-0.0029	-0.0114*	-0.0226**
	(0.0321)	(0.0118)	(0.0065)	(0.0107)
Unused	0.0458	0.0119	0.0129	0.0179
	(0.0344)	(0.0113)	(0.0098)	(0.0152)
	Panel B: State's Three Strikes use is > 0.01 *of admissions in 1998*			
Variable	Number of Child	Married	High School	College
Used	-0.0083	0.0021	-0.0098	-0.0165
	(0.0414)	(0.01149)	(0.0107)	(0.0168)
Unused	0.0383	0.0112	0.0172**	0.0220
	(0.0337)	(0.0124)	(0.0084)	(0.0134)
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	202,870	202,870	202,870	202,870

Notes: Teen pregnancy is only measured for females. *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). All standard errors are clustered by state. Standard errors are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

B.4 Appendix: Black Males Who Attend College

In this appendix we focus on Black males who attend college, since labor market outcomes likely vary by human capital accumulation. We find better labor market outcomes for this sub-group of Black males.

Table B.13: Males Only Sample (Black and Attend College)

	Panel A: Top Three Used States: CA, GA, FL		
Variable	Employed	Earnings	H.H. Income
Used	-0.0718*	1.3554***	-0.2106
	(0.0419)	(0.4829)	(1.5569)
Unused	0.0385	0.8836	0.0737
	(0.0319)	(0.4709)	(0.7154)
	Panel B: State's Three Strikes use is > 0.01 *of admissions in 998*		
midrule *Variable*	Employed	Earnings	H.H. Income
Used	-0.0195	1.4734***	-0.0989
	(0.0494)	(0.4962)	(1.2308)
Unused	0.0224	0.6878	0.0514
	(0.0322)	(0.4572)	(0.6645)
state fixed effects	X	X	X
birth cohort fixed effects	X	X	X
state specific linear time trend	X	X	X
Observations	7,447	6,640	6,640

Notes: *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, and racial and ethnic dummy variables (Black, Asian, other and Hispanic). Results for full-time, part-time employment, and home production (working but less than 10 hours per week) as well as yearly earned income and yearly household income are conditional on being employed. Yearly earned income and yearly household income were divided by $10,000. All standard errors are clustered by state and are in parentheses * p<0.05, ** p<0.01, *** p<0.001. All models include person weights, which indicates how many persons in the US 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. Source: American Community Survey birth cohorts 1974 to 1990 (see Ruggles et al. (2018)).

Appendix C

Appendix: Chapter 3

Table C.1: Summary Statistics Measures of Parental Incarceration, Add Health Sample

	All	Exposed	Not Exposed
	mean	mean	mean
Timing:			
Paternal Incarceration	0.115	1.000	0.000
Bio Dad's incarcerated (wave 3)	0.077	0.687	0.000
Bio Dad's incarcerated (wave 4)	0.117	1.000	0.000
Ages 0 to 5 at exp.	0.356	0.356	.
Ages 6 to 12 at exp.	0.259	0.259	.
Ages 13 to 18 at exp.	0.153	0.153	.
Ages 19 or older at exp.	0.132	0.132	.
Not yet born	0.099	0.099	.
Maternal Incarceration	0.022	0.084	0.014
Dosage:			
Time Served by Father (yrs.)	1.027	1.027	.
Total No. of Incarceration Spell:			
Once	0.500	0.500	.
2-5 times	0.387	0.387	.
More than 5 times	0.114	0.114	.
Observations	2,799	323	2,476

Notes: Numbers are the mean of the variable. Summary Statistics are represented separately for those exposed to paternal incarceration at some point during their life (Exposed) and those never exposed (Not Exposed). Estimates are weighted using the cross-sectional weights.
Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.2: Summary Statistics: Youth Characteristics, Add Health Sample

	All	Exposed	Not Exposed
	mean	mean	mean
Female	0.504	0.513	0.502
Twins	0.020	0.035	0.019
Race/ Ethnicity:			
White	0.731	0.592	0.749
Black	0.119	0.225	0.105
Other Race	0.150	0.183	0.146
Hispanic	0.116	0.170	0.109
Language Spoken at Home:			
English	0.952	0.967	0.950
Spanish	0.033	0.031	0.033
Other	0.015	0.002	0.017
Age (Wave 1)	15.883	15.739	15.901
Age (Wave 2)	16.884	16.739	16.903
Age (Wave 3)	22.128	22.004	22.144
Age (Wave 4)	28.884	28.727	28.904
Enrolled in School (Wave 1)	0.988	0.976	0.990
Suspension	0.218	0.410	0.193
Birth order	1.979	1.818	1.999
Birthweight (lbs)	7.007	6.738	7.040
Sibling in sample	0.124	0.145	0.122
Immigrant	0.038	0.023	0.040
Current Grade:			
Grade 7	0.179	0.201	0.177
Grade 8	0.163	0.198	0.158
Grade 9	0.169	0.181	0.167
Grade 10	0.173	0.203	0.169
Grade 11	0.152	0.140	0.154
Grade 12	0.164	0.076	0.175
Highest level of Schools:			
Less than HH	0.088	0.209	0.072
HH or GED	0.317	0.409	0.305
Some college	0.435	0.301	0.452
BA	0.150	0.069	0.161
Post grad	0.010	0.012	0.010
Observations	2,799	323	2,476

Notes: Numbers are the mean of the variable. Summary Statistics are represented separately for those exposed to paternal incarceration at some point during their life (Exposed) and those never exposed (Not Exposed).
Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.3: Summary Statistics: Primary Caregiver and Biological Parents Outcomes, Add Health Sample

	All	Exposed	Not Exposed
	mean	mean	mean
Age (PCG)	41.843	39.848	42.091
Female (PCG)	0.918	0.938	0.915
Relation to Focal Child:			
Biological mother	0.902	0.877	0.905
Biological father	0.046	0.044	0.047
Mother's Age at Birth	25.721	23.178	26.025
Father living with child	0.770	0.551	0.799
Step parent	0.021	0.021	0.021
Other relative	0.018	0.036	0.015
Adoptive or Foster Parent	0.011	0.022	0.009
Non-relative	0.002	0.000	0.003
Marital Status:			
Married (PCG)	0.777	0.533	0.807
Single adult household (PCG)	0.037	0.102	0.029
Other Rel.(PCG)	0.185	0.366	0.163
Immigrant (PCG)	0.082	0.055	0.085
Primary Caregiver's Education:			
Less than HH (PCG)	0.107	0.160	0.100
HH or GED (PCG)	0.312	0.427	0.298
Some college (PCG)	0.311	0.287	0.314
BA (PCG)	0.156	0.091	0.164
Post BA cert. (PCG)	0.114	0.035	0.124
Biological Mother's Education:			
Less than HH (M)	0.113	0.175	0.105
HH or GED (M)	0.320	0.430	0.307
Some college (M)	0.297	0.252	0.303
BA (M)	0.160	0.107	0.166
Post BA cert. (M)	0.110	0.035	0.119
Household Income (Wave 1)	3.266	2.725	3.334
Easy access to drugs in home	0.026	0.067	0.021
Evidence of smoking in the household	0.193	0.321	0.177
Work outside the home (PCG)	0.759	0.715	0.765
Family income (1,000)	33.601	24.796	34.704
Income Assistance:			
Received Welfare	0.144	0.285	0.125
Received Foodstamp	0.080	0.242	0.060
Received SSI	0.043	0.062	0.040
Received AFDC	0.046	0.152	0.033
Received Worker's Comp.	0.054	0.059	0.054
Received Housing Assist.	0.022	0.076	0.016
Observations	2,799	323	2,476

Notes: Numbers are the mean of the variable. Summary Statistics are represented separately for those exposed to paternal incarceration at some point during their life (Exposed) and those never exposed (Not Exposed). Other Rel. (PCG) includes individuals who have been widowed, divorced or separated. Post BA cert. captures professional training beyond a BA. Family income includes the income of everyone in the household, and income from welfare benefits, dividends, and all other sources. Source: National Longitudinal Study of Adolescent to Adult Health.

C.1 Detailed Regression Results

Table C.4: Average High School Grade Point, Add Health

	All			Male	Female
	(1)	(2)	(3)	(4)	(5)
Incar	-0.204**				
	(0.071)				
Female	0.383**	0.382**	0.379**		
	(0.029)	(0.029)	(0.029)		
Black	-0.265**	-0.266**	-0.233**	-0.137	-0.339**
	(0.073)	(0.074)	(0.073)	(0.135)	(0.082)
Hispanic	-0.306**	-0.309**	-0.322**	-0.309**	-0.297*
	(0.104)	(0.105)	(0.107)	(0.139)	(0.160)
Birth order	-0.068**	-0.068**	-0.069**	-0.069**	-0.077**
	(0.015)	(0.015)	(0.016)	(0.030)	(0.022)
HH or GED (PCG)	0.292**	0.291**	0.236**	0.262**	0.248**
	(0.071)	(0.072)	(0.072)	(0.127)	(0.084)
Some College(PCG)	0.323**	0.322**	0.249**	0.257*	0.337**
	(0.073)	(0.073)	(0.072)	(0.135)	(0.095)
BA (PCG)	0.639**	0.636**	0.528**	0.613**	0.606**
	(0.071)	(0.072)	(0.073)	(0.136)	(0.092)
Post BA (PCG)	0.798**	0.796**	0.649**	0.900**	0.651**
	(0.076)	(0.076)	(0.076)	(0.144)	(0.095)
IncarEarly		-0.258**	-0.202*	-0.352**	-0.218*
		(0.111)	(0.113)	(0.151)	(0.123)
IncarMiddle		-0.179*	-0.127	-0.106	-0.159
		(0.092)	(0.093)	(0.153)	(0.129)
IncarLate		-0.121	-0.057	-0.056	-0.189
		(0.173)	(0.171)	(0.268)	(0.215)
IncarBeforeBirth		-0.186	-0.163	-0.333	-0.000
		(0.147)	(0.148)	(0.243)	(0.191)
Household Income			0.193**		
			(0.029)		
School fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes	Yes
Observations	2,689	2,689	2,689	1,249	1,440
R^2	0.336	0.336	0.352	0.322	0.363

Notes: The dependent variable is cumulative high GPA. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.5: High School Dropout (LPM), Add Health

	All			Male	Female
	(1)	(2)	(3)	(4)	(5)
Incar	-0.094**				
	(0.035)				
Female	0.050**	0.050**	0.050**		
	(0.014)	(0.014)	(0.014)		
Black	0.027	0.027	0.034	0.082*	-0.016
	(0.024)	(0.024)	(0.024)	(0.043)	(0.036)
Hispanic	-0.011	-0.010	-0.013	-0.004	0.012
	(0.032)	(0.032)	(0.032)	(0.053)	(0.053)
Birth order	-0.002	-0.002	-0.002	0.001	-0.005
	(0.006)	(0.006)	(0.006)	(0.013)	(0.009)
HH or GED (PCG)	0.126**	0.126**	0.116**	0.089*	0.134**
	(0.035)	(0.035)	(0.035)	(0.052)	(0.045)
Some College(PCG)	0.151**	0.152**	0.138**	0.117**	0.163**
	(0.035)	(0.035)	(0.036)	(0.054)	(0.045)
BA (PCG)	0.175**	0.176**	0.155**	0.151**	0.1668*
	(0.036)	(0.036)	(0.037)	(0.053)	(0.046)
Post BA (PCG)	0.194**	0.195**	0.167**	0.186**	0.187**
	(0.036)	(0.036)	(0.037)	(0.054)	(0.047)
IncarEarly		-0.100**	-0.091**	-0.197**	-0.018
		(0.044)	(0.045)	(0.083)	(0.038)
IncarMiddle		-0.096*	-0.088*	-0.083	-0.061
		(0.051)	(0.050)	(0.084)	(0.057)
IncarLate		-0.121	-0.110	0.002	-0.193
		(0.110)	(0.111)	(0.097)	(0.151)
IncarBeforeBirth		-0.036	-0.032	-0.048	-0.018
		(0.052)	(0.052)	(0.104)	(0.054)
Household Income			0.035**		
			(0.013)		
School fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes	Yes
Observations	2,689	2,689	2,689	1,249	1,440
R^2	0.183	0.183	0.188	0.261	0.279

Notes: The dependent variable is binary and takes the value one if the respondent has a high school diploma, and a value zero otherwise.The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.6: College Completion, Age Varying Effect (LPM), Add Health

	All			Male	Female
	(1)	(2)	(3)	(4)	(5)
Incar	-0.105**				
	(0.042)				
Female	0.144**	0.145**	0.144**		
	(0.021)	(0.021)	(0.021)		
Black	-0.005	-0.005	0.007	-0.033	0.020
	(0.038)	(0.038)	(0.038)	(0.055)	(0.050)
Hispanic	-0.221**	-0.221**	-0.225**	-0.208**	-0.221**
	(0.061)	(0.061)	(0.062)	(0.069)	(0.099)
Birth order	-0.037**	-0.037**	-0.037**	-0.028	-0.036**
	(0.012)	(0.012)	(0.012)	(0.019)	(0.013)
HH or GED (PCG)	0.136**	0.137**	0.117**	0.091*	0.182**
	(0.038)	(0.038)	(0.038)	(0.050)	(0.053)
Some College(PCG)	0.221**	0.221**	0.195**	0.190**	0.247**
	(0.038)	(0.038)	(0.040)	(0.059)	(0.059)
BA (PCG)	0.370**	0.371**	0.332**	0.379**	0.343**
	(0.040)	(0.041)	(0.042)	(0.062)	(0.059)
Post BA (PCG)	0.420**	0.421**	0.368**	0.417**	0.415**
	(0.048)	(0.048)	(0.051)	(0.077)	(0.058)
IncarEarly		-0.106	-0.085	-0.103	-0.157
		(0.075)	(0.078)	(0.071)	(0.136)
IncarMiddle		-0.090*	-0.070	-0.141**	-0.055
		(0.048)	(0.048)	(0.068)	(0.081)
IncarLate		-0.156**	-0.132**	0.125	-0.290**
		(0.064)	(0.064)	(0.111)	(0.096)
IncarBeforeBirth		-0.071	-0.061	-0.097	-0.100
		(0.100)	(0.100)	(0.132)	(0.184)
Household Income			0.071**		
			(0.019)		
School fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes	Yes
Observations	2,689	2,689	2,689	1,249	1,440
R^2	0.292	0.292	0.299	0.357	0.311

Notes: The dependent variable is binary, equal to one if the respondent completed at least a BA and zero otherwise. The treatment group includes children exposed to paternal incarceration by age 18. The results are reported for the full sample, as well as separately by gender. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.7: Adult Earnings, Age Varying Effect, Add Health

	All			Male	Female
	(1)	(2)	(3)	(4)	(5)
Incar	-0.190**				
	(0.061)				
Female	-0.328**	-0.328**	-0.130**		
	(0.033)	(0.034)	(0.018)		
Black	-0.192**	-0.193**	0.025	-0.255**	-0.068
	(0.063)	(0.063)	(0.031)	(0.110)	(0.085)
Hispanic	-0.020	-0.020	0.072	0.029	-0.056
	(0.112)	(0.112)	(0.045)	(0.161)	(0.171)
Birth order	0.007	0.007	0.008	0.067**	-0.047**
	(0.018)	(0.018)	(0.010)	(0.030)	(0.023)
HH or GED (PCG)	0.237**	0.238**	-0.014	0.283**	0.241**
	(0.064)	(0.064)	(0.032)	(0.106)	(0.090)
Some College(PCG)	0.225**	0.227**	-0.050	0.198*	0.252**
	(0.076)	(0.076)	(0.033)	(0.110)	(0.101)
BA (PCG)	0.356**	0.359**	-0.042	0.295**	0.428**
	(0.074)	(0.075)	(0.037)	(0.123)	(0.118)
Post BA (PCG)	0.449**	0.451**	-0.054	0.414**	0.504**
	(0.083)	(0.083)	(0.039)	(0.133)	(0.124)
IncarEarly		-0.211**	0.107*	-0.348**	-0.104
		(0.092)	(0.063)	(0.170)	(0.105)
IncarMiddle		-0.205*	0.075	-0.174	-0.147
		(0.113)	(0.051)	(0.136)	(0.156)
IncarLate		-0.215*	0.025	-0.044	-0.309*
		(0.110)	(0.060)	(0.168)	(0.159)
IncarBeforeBirth		-0.050	0.089	-0.007	-0.180
		(0.160)	(0.106)	(0.242)	(0.280)
Household Income			-0.033**		
			(0.015)		
School fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes	Yes
Observations	2,689	2,689	2,689	1,249	1,440
R^2	0.199	0.199	0.141	0.230	0.233

Notes: The dependent variable is real annual earnings measured in thousands of dollars. The results are reported for the full sample, as well as separately by gender. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.8: Full-time Employment, (LPM), Add Health

	All			Male	Female
	(1)	(2)	(3)	(4)	(5)
Incar	-0.050**				
	(0.029)				
Female	-0.093**	-0.092**	-0.092**		
	(0.018)	(0.017)	(0.017)		
Black	0.021	0.021	0.024	0.000	0.039
	(0.027)	(0.027)	(0.028)	(0.037)	(0.037)
Hispanic	0.124**	0.125**	0.124**	0.188**	0.039
	(0.053)	(0.053)	(0.052)	(0.081)	(0.065)
Birth order	0.001	0.001	0.001	0.006	-0.005
	(0.008)	(0.008)	(0.008)	(0.012)	(0.012)
HH or GED (PCG)	0.076**	0.076**	0.074**	0.056	0.136**
	(0.038)	(0.038)	(0.037)	(0.048)	(0.053)
Some College(PCG)	0.064*	0.065*	0.062*	0.019	0.117**
	(0.038)	(0.038)	(0.037)	(0.050)	(0.051)
BA (PCG)	0.055	0.056	0.050	-0.004	0.148**
	(0.043)	(0.043)	(0.041)	(0.054)	(0.065)
Post BA (PCG)	0.074*	0.075*	0.067	0.029	0.147**
	(0.045)	(0.045)	(0.043)	(0.055)	(0.072)
IncarEarly		-0.057	-0.056	-0.117**	-0.011
		(0.039)	(0.040)	(0.059)	(0.057)
IncarMiddle		-0.028	-0.027	0.008	-0.088
		(0.041)	(0.042)	(0.048)	(0.070)
IncarLate		-0.119	-0.117	-0.095	-0.146
		(0.111)	(0.112)	(0.121)	(0.152)
IncarBeforeBirth		0.011	0.010	-0.042	0.033
		(0.070)	(0.070)	(0.101)	(0.105)
Household Income			0.007		
			(0.014)		
School fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes	Yes
Observations	2,689	2,689	2,689	1,249	1,440
R^2	0.107	0.108	0.108	0.173	0.154

Notes: The dependent variable is binary, equal to one if the respondent report working thirty five hours or more per week, zero otherwise. The results are reported for the full sample, as well as separately by gender. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.9: Criminal Engagement, (LPM), Add Health

	All			Male	Female
	(1)	(2)	(3)	(4)	(5)
Incar	0.092**				
	(0.042)				
Female	-0.132**	-0.131**	-0.130**		
	(0.018)	(0.018)	(0.018)		
Black	0.029	0.030	0.025	0.020	0.022
	(0.031)	(0.031)	(0.031)	(0.062)	(0.028)
Hispanic	0.068	0.070	0.072	0.142*	0.041
	(0.045)	(0.045)	(0.045)	(0.075)	(0.041)
Birth order	0.007	0.007	0.008	0.006	0.010
	(0.010)	(0.010)	(0.010)	(0.013)	(0.011)
HH or GED (PCG)	-0.024	-0.024	-0.014	0.024	-0.086**
	(0.033)	(0.033)	(0.032)	(0.052)	(0.038)
Some College(PCG)	-0.063*	-0.063*	-0.050	-0.040	-0.084**
	(0.033)	(0.033)	(0.033)	(0.052)	(0.039)
BA (PCG)	-0.061	-0.060	-0.042	-0.031	-0.103**
	(0.037)	(0.037)	(0.037)	(0.061)	(0.040)
Post BA (PCG)	-0.079**	-0.078**	-0.054	-0.118*	-0.084*
	(0.038)	(0.038)	(0.039)	(0.061)	(0.045)
IncarEarly		0.117*	0.107*	0.184*	0.081
		(0.066)	(0.063)	(0.106)	(0.057)
IncarMiddle		0.084	0.075	0.124	0.039
		(0.053)	(0.051)	(0.097)	(0.040)
IncarLate		0.036	0.025	0.032	0.054
		(0.061)	(0.060)	(0.168)	(0.080)
IncarBeforeBirth		0.094	0.089	0.185	0.027
		(0.108)	(0.106)	(0.189)	(0.069)
Household Income			-0.033**		
			(0.015)		
School fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes	Yes
Observations	2,689	2,689	2,689	1,249	1,440
R^2	0.137	0.138	0.141	0.202	0.141

Notes: The dependent variable is binary, equal to one if the respondents went to jail or prison, zero otherwise. The results are reported for the full sample, as well as separately by gender. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.10: Sensitivity Analysis

	Cum. GPA (1)	High Sch. Diploma (2)	College. Attend. (3)	Adult Earnings (4)	Full-time Emp. (5)	Criminal Engagement (6)
	Incorporating Unknown Age When Father was Incarcerated					
IncarEarly	-0.268***	-0.098**	-0.105	-0.198**	-0.055	0.119*
	(0.110)	(0.044)	(0.074)	(0.094)	(0.040)	(0.065)
IncarMiddle	-0.173**	-0.096*	-0.093**	-0.205*	-0.024	0.078
	(0.091)	(0.050)	(0.048)	(0.114)	(0.041)	(0.053)
IncarLate	-0.114	-0.117	-0.155**	-0.216**	-0.114	0.034
	(0.160)	(0.105)	(0.067)	(0.108)	(0.107)	(0.057)
IncarBeforeBirth	-0.188	-0.038	-0.061	-0.031	0.022	0.076
	(0.144)	(0.049)	(0.098)	(0.155)	(0.069)	(0.110)
Age_unknown	-0.160***	-0.068**	-0.060	-0.052	0.040	0.139***
	(0.057)	(0.031)	(0.040)	(0.070)	(0.026)	(0.037)
Child-specific controls	Yes	Yes	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,904	2,904	2,904	2,904	2,904	2,904
R^2	0.331	0.265	0.285	0.188	0.098	0.322

Notes: The dependent variables are cumulative high GPA, and the log of adult income as well as indicator variables for the receipt of a high school diploma or GED, college attendance, full-time employment, and experiencing incarceration as an adult. Control variables include child-specific controls such as gender (female), race, ethnicity, birth order, birth weight, and immigration status as well as primary caregiver's highest educational attainment (a proxy for SES), their immigration status and mother's age at birth. I also include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

C.2 Marginal Effects

Table C.11: High School Dropout (Average Marginal Effects Following Logistic Regression), Add Health

	All		Male	Female
	(1)	(2)	(3)	(4)
$Incar$	-0.090***			
	(0.027)			
$IncarEarly$		-0.118**	-0.197**	-0.018
		(0.052)	(0.083)	(0.038)
$IncarMiddle$		-0.112*	-0.083	-0.061
		(0.059)	(0.084)	(0.057)
$IncarLate$		-0.131	0.002	-0.193
		(0.102)	(0.097)	(0.151)
$IncarBeforeBirth$		-0.018	-0.048	-0.018
		(0.065)	(0.104)	(0.054)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes
Observations	1,639	1,639	1,249	1,440

Notes: The dependent variable is binary and takes the value one if the respondent has a high school diploma, and a value zero otherwise. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.12: College Completion, Age Varying Effect (Average Marginal Effects Following Logistic Regression), Add Health

	All		Male	Female
	(1)	(2)	(3)	(4)
Incar	-0.0862			
	(0.037)			
IncarEarly		-0.093	-0.110	-0.121
		(0.065)	(0.067)	(0.119)
IncarMiddle		-0.070	-0.134*	-0.058
		(0.044)	(0.075)	(0.069)
IncarLate		-0.135**	0.147	-0.292**
		(0.069)	(0.102)	(0.122)
IncarBeforeBirth		-0.064	-0.122	-0.067
		(0.096)	(0.160)	(0.148)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes
Observations	2,668	2,668	1,149	1,344

Notes: The dependent variable is binary, equal to one if the respondent completed at least a BA and zero otherwise. The treatment group includes children exposed to paternal incarceration by age 18. The results are reported for the full sample, as well as separately by gender. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.13: Full-time Employment, (Average Marginal Effects Following Logistic Regression), Add Health

	All		Male	Female
	(1)	(2)	(3)	(4)
Incar	-0.063**			
	(0.030)			
IncarEarly		-0.088*	-0.243**	-0.030
		(0.050)	(0.105)	(0.081)
IncarMiddle		-0.042	0.005	-0.125
		(0.050)	(0.084)	(0.099)
IncarLate		-0.150	-0.177	-0.226
		(0.159)	(0.158)	(0.266)
IncarBeforeBirth		0.012	-0.044	0.094
		(0.083)	(0.123)	(0.135)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes
Observations	2,495	2,495	779	1,253

Notes: The dependent variable is binary, is equal to one if the respondents reports working thirty five hours or more per week, zero otherwise. The results are reported for the full sample, as well as separately by gender. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.14: Criminal Engagement, (Average Marginal Effects Following Logistic Regression), Add Health

	All		Male	Female
	(1)	(2)	(3)	(4)
Incar	0.074**			
	(0.030)			
IncarEarly		0.094*	0.177*	0.170
		(0.052)	(0.093)	(0.113)
IncarMiddle		0.082	0.137	0.091
		(0.051)	(0.105)	(0.094)
IncarLate		0.048	0.014	0.051
		(0.065)	(0.161)	(0.116)
IncarBeforeBirth		0.109	0.268	0.045
		(0.119)	(0.237)	(0.098)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effect	Yes	Yes	Yes	Yes
Observations	2,379	2,379	994	725

Notes: The dependent variable is binary, is equal to one if the respondents went to jail or prison, zero otherwise. The results are reported for the full sample, as well as separately by gender. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.15: Longer Term Outcomes While Controlling for Dosage

	All		Male	Female
	(1)	(2)	(3)	(4)
	Panel A: Adult Earnings			
Incar	-0.193**			
	(0.080)			
Spell	-0.007	0.008	-0.080	0.007
	(0.102)	(0.109)	(0.167)	(0.164)
IncarEarly		-0.208**	-0.377**	-0.102
		(0.095)	(0.175)	(0.111)
IncarMiddle		-0.203	-0.201	-0.145
		(0.128)	(0.159)	(0.174)
IncarLate		-0.209	-0.107	-0.305
		(0.149)	(0.221)	(0.196)
IncarBeforeBirth		-0.045	-0.048	-0.174
		(0.165)	(0.231)	(0.305)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.199	0.199	0.231	0.233
	Panel B: Full-time Employment			
Incar	-0.038			
	(0.033)			
Spell	0.029	0.025	-0.004	0.091
	(0.053)	(0.054)	(0.071)	(0.088)
IncarEarly		-0.049	-0.119*	0.014
		(0.040)	(0.062)	(0.054)
IncarMiddle		-0.020	0.007	-0.059
		(0.045)	(0.059)	(0.073)
IncarLate		-0.102	-0.098	-0.092
		(0.118)	(0.147)	(0.154)
IncarBeforeBirth		0.027	-0.044	0.102
		(0.078)	(0.110)	(0.115)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.107	0.108	0.173	0.155

Notes: Dependent variables: Panel A, adult annual earnings and in Panel B, full-time employment. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

Table C.16: Longer Term Outcomes While Controlling for Dosage

	All		Male	Female
	(1)	(2)	(3)	(4)
	Panel B: Criminal Engagement			
Incar	0.108**			
	(0.052)			
Spell	0.040	0.030	0.060	0.029
	(0.060)	(0.066)	(0.114)	(0.055)
IncarEarly		0.127*	0.207*	0.089
		(0.070)	(0.106)	(0.062)
IncarMiddle		0.094	0.144	0.048
		(0.057)	(0.104)	(0.045)
IncarLate		0.056	0.080	0.071
		(0.074)	(0.200)	(0.086)
IncarBeforeBirth		0.113	0.216	0.049
		(0.123)	(0.192)	(0.083)
Child-specific controls	Yes	Yes	Yes	Yes
Caregiver-specific controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Observations	2,689	2,689	1,249	1,440
R^2	0.137	0.138	0.203	0.142

Notes: Dependent variable is criminal engagement. The first two columns show results for the full sample, while the third and fourth columns outline results for males and females, respectively. All models include school and birth cohort fixed effects. Standard errors are clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.

C.3 Maternal versus Paternal Incarceration

Table C.17: Paternal, Maternal or Parental Incarceration, Add Health

	Cumulative GPA			Adult Earnings		
	(1)	(2)	(3)	(4)	(5)	(6)
Incar	-0.220***			-0.198**		
	(0.077)			(0.067)		
Maternal Incarceration		-0.236			-0.163	
		(0.142)			(0.165)	
Parental Incarceration			-0.237***			-0.199***
			(0.069)			(0.062)
Observations	2562	2,476	2,715	2,689	2,476	2,758
R^2	0.339	0.336	0.337	0.163	0.162	0.166

Notes: The dependent variable in Columns 1 to 3 is is cumulative high school GPA, while the dependent variable in Columns 4 to 6 is log of adult earnings. In each model we include controls for child-specific controls such as gender (female), race and ethnicity, birth order, birth weight, and immigration status as well as primary caregiver's highest educational attainment (a proxy for SES), their immigration status and mother's age at birth. I also include school and birth cohort fixed effects. Standard errors clustered at the school level. Standard errors are in parentheses and ***, ** and * indicate that the estimates are statistically significant at the 1%, 5%, and 10% levels. Source: National Longitudinal Study of Adolescent to Adult Health.