

MODERN AMERICAN INCARCERATION AND LABOR ECONOMICS

by

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To Phillips two, on either end
Of paths that I shall tread,
And third to whom I've yet to meet
On stranger paths ahead.

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ABSTRACT

Many individuals in the United States are incarcerated. The American incarceration rate and average sentence length have risen dramatically since the early 1980s. It is commonly hypothesized that mass incarceration has had various unintended consequences on individuals, households, and society at large. In this dissertation, I examine the effects of an individual's incarceration on several economic variables, including educational attainment, employment, and earnings. Over the course of three essays, I utilize the theoretical background and empirical methodology of contemporary labor economics to establish links between the experience of incarceration and generally negative subsequent outcomes. Each chapter draws on data from the 1979 cohort of the National Longitudinal Survey of Youth, which allow me to examine the varying life courses and behaviors of a subsample of individuals that are incarcerated at some point during adulthood.

The first chapter of this dissertation investigates the long-term effects of parental incarceration on children. I utilize detailed intergenerational data and a variety of empirical methods to provide evidence that individuals who report resident parental incarceration during childhood experience depressed levels of educational attainment and earnings as an adult. These effects appear to vary by parent and child gender. The second chapter is concerned with estimating the returns to education attained after an incarceration spell. I analyze longitudinal individual histories of incarceration, education, employment, and earnings for a sample of former prisoners using regression and

propensity score matching techniques. My results suggest that education has a positive effect on post-release labor supply and earnings, but this benefit is largely confined to the completion of four-year college degrees. The third and final chapter reevaluates the negative relationship between incarceration and earnings found in the current empirical literature. I extend this literature with a battery of quantile regression models. My results clarify incarceration's effect on subsequent low earnings and suggest that the incarceration wage penalty is smaller in magnitude for low-skill, low-earnings employment. In total, this dissertation extends the current understanding of incarceration's effects on individuals and households, particularly with respect to performance on the market for labor. Each essay also provides some insight into the effectiveness of American criminal justice policy.

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INTRODUCTION

Incarceration trends in the United States are anomalous among industrialized nations. Since the early 1980s, several changes in criminal justice policy have led to a dramatic increase in the incarcerated population. Such a massive demographic change could have an immense effect on individuals, households, and society as a whole. Social scientists and policy makers have maintained interest in calculating the costs and benefits of mass incarceration. Such examination of the explicit and implicit effects of incarceration has clear implications on policy concerning the sentencing, rehabilitation, and reentry of prisoners into mainstream society.

This dissertation is comprised of three essays that examine specific aspects of the American incarceration phenomenon. I draw on the theoretical background and methodology of labor economics to consider some of the effects that incarceration may have on American society. In each essay, I examine the event of incarceration at the individual level, particularly with respect to standard economic outcomes, including education, employment, and earnings. Using survey data drawn from the National Longitudinal Survey of Youth and a variety of econometric techniques, I attempt to further clarify the link between incarceration and subsequent performance on the market for labor.

The first chapter of this dissertation is entitled "Estimating the Economic Consequences of Parental Incarceration." Many people incarcerated in the United States are parents, and this may have negative effects on the social and educational development of their children. I evaluate this hypothesis empirically using intergenerational data drawn from the National Longitudinal Survey of Youth to estimate the effects of parental incarceration on a child's level of educational attainment and earnings as an adult. A battery of regression models are used to estimate parental incarceration's effects while controlling for parent and child gender, age at incarceration, incarceration

frequency, and pre-incarceration household residency. I find that the incarceration of parents, particularly mothers, is associated with lower levels of higher education and earnings. Daughters of incarcerated mothers appear to face higher educational penalties than sons, and sons appear to face higher wage penalties than daughters. My results suggest that for many American prisoners, the negative consequences of incarceration may spill over into the household and have lasting effects on children.

My attention turns next to a common method of prisoner rehabilitation: educational attainment. In the second chapter, entitled "Returns to Post-Incarceration Education for Former Prisoners," I estimate the returns to education for individuals who attain education after an incarceration spell. To accomplish this, I construct detailed incarceration, education, employment, and earnings histories drawn from the National Longitudinal Survey of Youth. Regression and propensity score matching results suggest a positive relationship between post-incarceration education and labor supply, especially for college completion. When decomposed into high school diploma and equivalency certification (such as the GED), the former appears to improve labor market outcomes. There is no evidence, however, that the GED benefits individuals in its own right. Mixed evidence is found for a post-incarceration college completion wage premium.

In the third and final chapter, "Incarceration and Earnings: Clarification from Quantile Regression," I bring a higher degree of understanding to the negative relationship between incarceration spells and subsequent earnings. The average prisoner differs from the average American in many observable ways, particularly with respect to education, employment, and earnings. Current research examining the incarceration wage penalty uses regression techniques that characterize such effects on the conditional mean wage. I employ quantile regression and recently-developed fixed effect quantile regression techniques to estimate the incarceration wage penalty across the conditional wage distribution, with particular attention given to lower, below-

median earnings quantiles for a sample drawn from the National Longitudinal Survey of Youth. I find that the current body of incarceration literature may be overstating the incarceration wage penalty for low-skill, low-earning former prisoners who may be only marginally attached to the market for labor.

CHAPTER I

Estimating the Economic Consequences of Parental Incarceration

1.1 Introduction

Incarceration has increased dramatically in the United States since the 1980s, with about 1.6 million individuals residing in jails and prisons in 2011 (Bureau of Justice Statistics [BJS], 2012). The American “prison boom” has been attributed in part to increases in sentencing frequency and length, especially for nonviolent crimes, particularly controlled substance violation (Western et al., 2001). Figure 1 shows that the incarcerated population has not only grown over time, but has grown as a segment of the national population, comprising about 0.5% of the United States population since 2006 (BJS, 2009). As such, the number of individuals directly affected by incarceration is growing steadily and researchers have shown increasing interest in estimating the cost of mass incarceration to society, including the socially optimal level of imprisonment (Levitt, 1996), the relationship between human capital investment and crime (Lochner & Moretti, 2004), and the effects of incarceration on the labor market (Grogger, 1995; Holzer, 2009; Kling, 2006; Needels, 1996; Western et al., 2001). In this paper, I examine the particular issue of the long-term effects of parental incarceration on children.

About 1 million of the aforementioned 1.6 million prisoners are identified as parents of minor children (Economic Mobility Project, 2010). If parental incarceration has a negative effect on a child’s educational attainment, this may result in lower wages as an adult. Incarceration may therefore be a negative shock to a child’s human

capital. Because the opportunity cost of committing a crime rises with the acquisition of mainstream human capital, the incarceration of a parent may increase the likelihood of his or her children committing crimes and therefore increase the likelihood of their own incarceration (Lochner & Moretti, 2004). Evidence of a negative parental incarceration effect may explain in part the theorized parent-child transmission of delinquency and crime.

Although most research suggests that parental incarceration has a negative effect on children, the question remains of interest because parental incarceration has a plausibly ambiguously effect. The removal and extended absence of a parent from the household may affect a child along economic, behavioral, and social dimensions, none of which are qualitatively unambiguous. A parent's incarceration may result in a negative shock to household income and may reduce investment in a child's human capital (Arditti et al., 2003; Geller et al., 2012; Wakefield & Uggen, 2010). Incarceration may also disrupt mainstream employment; upon release, a formerly incarcerated individual may face increased employment and income frictions (Geller et al., 2009). If society deems an individual necessary to incarcerate, however, he may be an inherent danger to the household. Removing a criminal parent from the household in such cases may potentially benefit a child immediately and into adulthood.

Male and female criminals (and prisoners) vary along dimensions other than gender, and these differences inform the interpretation of estimated incarceration effects. The majority of incarcerated individuals are male (BJS, 2012). Relative to incarcerated females, males are less educated, serve longer sentences, are more likely to recidivate, and are less likely to live with their families (U.S. Department of Justice, 1993). Males are more likely to be incarcerated for violent crimes; women are disproportionately incarcerated for nonviolent crimes, especially controlled substance violations and theft (Cho, 2009a). Paternal incarceration may result in material

hardship for the family (Schwartz-Soicher et al., 2011); maternal incarceration may be more likely to affect the stability of the household and the behavioral and educational development of children (Nichols & Loper, 2012; Sharp et al., 1997). By allowing the effects of incarceration to vary by the gender of the parent, I also compare the effect of incarceration and parental absence across varying criminal profiles.

This study estimates the effect of parental incarceration on a child's long-term economic outcomes, including educational attainment and adult earnings. Intergenerational data are drawn from the National Longitudinal Survey of Youth 1979 and its Child & Young Adult supplement, which identify the relationship between the survey respondent and incarcerated members of his or her household. Models interacting respondent gender and parental incarceration dummies allow the effect of incarceration to vary across the gender of both parent and child and control for an unusually large set of parent, child, and household characteristics. Results suggest that maternal incarceration is associated with decreased educational attainment and earnings. Incarceration's negative relationship with education is most apparent for higher education, especially in the decisions to start and complete a college education. Daughters of incarcerated mothers face more severe educational penalties than sons, and sons of incarcerated mothers face higher wage penalties than daughters. In contrast, little evidence is found that paternal incarceration is associated with negative outcomes even in basic regression specifications. In total, my findings suggest that parental incarceration is a significant and generally negative shock to a child's human capital and earnings, which may reduce the opportunity cost of crime and increase the intergenerational transmission of incarceration.

1.2 Previous Findings

Research examining the consequences of incarceration is comprised of qualitative and quantitative research across multiple disciplines, including sociology, developmental

psychology, and criminology, and a variety of data sets and methods. Recently, incarceration has become a topic of economic interest, with an emphasis on estimating the unintended economic consequences of the American prison boom. Next, I present an overview of literature relevant to the effects of parental incarceration.

The prison boom of the 1980s coincided with a heightened interest in the labor market outcomes of criminals and released prisoners. Most research finds that incarcerated individuals face lower wages and levels of employment after release (Holzer, 2009), although some studies suggest that labor market difficulties may only be correlated with pre-incarceration characteristics. Using administrative data comprised of releasees from California prisons, Grogger (1995) finds that an arrest has a modestly negative effect on employment and earnings that is short in duration for young men. Western et al. (2001) also suggests that incarceration may increase unemployment and reduce earnings for older, more skilled workers. Related studies strongly suggest that mass incarceration has reduced employment, wage mobility, and racial wage equality for black male releasees (Pettit & Western, 2004; Western, 2002; Western & Pettit, 2000; Western & Pettit, 2005).

Several papers on the intergenerational transmission of criminal tendencies suggest that antisocial, criminal, and violent tendencies may be transmitted from parent to child (Bornovalova et al., 2010; Hjalmarsson & Lindquist, 2012; Murray et al., 2007; Murray & Farrington, 2008; Thornberry et al., 2002) and that children of incarcerated parents are also more likely to commit acts of child delinquency and adult crime (Dallaire, 2007b; Murray & Farrington, 2005; Murray & Farrington, 2008). Increasingly severe prison sentences in the United States since the 1980s may have contributed to this cyclical nature of parent-child delinquency (Reed & Reed, 2010).

The incarceration of mothers is of interest as they are believed to play an integral role in the early development of children's personality and social skills (Cho, 2009a; Dallaire, 2007a; Johnston and Gabel, 1995), with some evidence suggesting

that maternal incarceration may increase the likelihood of children being convicted of crimes as adults (Huebner & Gustafson, 2007). Utilizing data from Chicago schools and Illinois prisons, Cho (2009a, 2009b, 2010) examines the effect of maternal incarceration on several educational outcomes. Contrary to a similar study by Poehlmann (2005), she finds little evidence that maternal incarceration is harmful to a child's cognitive development proxied by standardized exam scores (Cho, 2009a). In a related study, results suggest that maternal incarceration reduces the likelihood of grade retention (Cho, 2009b). In a third study, she finds that maternal incarceration has varying effects across child gender, sentence duration, and child-age timing of incarceration (Cho, 2010). These mixed results may arise from a focus on compulsory education; incarceration may be less likely to affect the attainment of secondary education if social institutions are in place to encourage completion. It is perhaps more likely that incarceration may affect the selection and completion of higher, non-compulsory levels of education; the majority of current research does not (or cannot) consider this issue, although a recent study using National Longitudinal Study of Adolescent Health links parental incarceration to reduced college completion (Foster & Hagan, 2012).

Considerable debate surrounds the "same-sex hypothesis" in divorce, that is, the belief that children fare better in the custody of their parent of the same sex (King, 1994; Peterson & Zill, 1986; Powell & Downey, 1997; Smith, 1968). Although evidence of such an effect is mixed, this notion may also suggest conversely negative effects on the development of children with incarcerated parents of the same sex. Because of this, I examine and compare both cases. The incarceration and absence of fathers from the household may have a significant effect on household stability, leading to a reduction in household income, financial contributions, child support payments, or divorce (Schwartz-Soicher et al., 2011; Turney & Wildeman, 2012). Additional evidence suggests that even after release, paternal monetary contributions may be reduced in

size and frequency (Geller et al., 2011). Recent studies link paternal incarceration with increased behavioral and development problems in children including physical aggression (Geller et al., 2009; Wildeman, 2010), attention problems (Geller et al., 2012), and mental development (Wakefield & Wildeman, 2011). Paternal incarceration may also have detrimental effects on the mental health of mothers, increasing the likelihood of major depressive episodes and life dissatisfaction (Wildeman et al., 2012), and may leave daughters vulnerable to abuse from other father figures (Foster & Hagan, 2007). Further, incarcerated fathers may feel increased stress and alienation from their household during an incarceration spell (Secret, 2012).

This study expands the current body of literature in several ways. Most importantly, I examine the long-term effects of parental incarceration on children by using adult outcomes such as completion of post-compulsory education and earnings. Models are specified such that the effects of incarceration are allowed to vary across both the gender of the parent and respondent child. Because the survey only considers parents who are members of the respondent child's household at the onset of incarceration, this study considers the specific case of parents that were household residents before incarceration. In addition, the intergenerational nature of the data allows for the inclusion of an extensive set of controls for parent and child characteristics. In total, these extensions yield a further exploration of the varying effects of parental incarceration on children.

1.3 Data and Methodology

1.3.1 National Longitudinal Survey of Youth 1979

I draw data for this study from the National Longitudinal Survey of Youth 1979 (NLSY79) and its Child & Young Adult supplement (Bureau of Labor Statistics [BLS], 2009a). In 1979, the NLSY began surveying a sample of 12,686 individuals

aged 14 to 21 annually over a range of topics including background characteristics and labor market outcomes. Since 1994, surveys have been administered to the remaining respondents biennially, and by the 2008 wave, 7,757 respondents remained in the sample. The Child & Young Adult survey (NLSY79CYA) began in 1986 as a biennial supplement to the NLSY79, surveying a sample of 4,971 children born to women of the NLSY79 cohort (BLS, 2011). By 2006, the data set had grown to contain 7,816 respondents (BLS, 2009b). Child surveys are conducted by mother- and self-reporting interviews on health, behavior, education, and household characteristics. Since 1994, children 15 or older enter the Young Adult category and participate in a more comprehensive interview in the style of the main survey (BLS, 1999). I link the data sets so that a CYA respondent is matched to his or her NLSY79 mother. Time-variant variables used in this study are derived from the 2008 waves of the NLSY79CYA and NLSY79; time-invariant variables are collected in a respondent's initial survey year.

There are several unique aspects of NLSY data that allow me to examine the effect of parental incarceration. The NLSY79 and the NLSY79CYA provide intergenerational and longitudinal data on two cohorts of related respondents from young childhood to adulthood. Both cohorts are interviewed on an unusually broad range of topics, including detailed household information and performance on standardized tests. The intergenerational nature of the linked data sets allows for the inclusion of a battery of parent-level controls, including youth and adult criminal activity. Controlling for a wide variety of pre-incarceration parental characteristics is likely to increase the validity of key parental incarceration variables as exogenous shocks to children and reduce the likelihood of endogeneity with parent or household characteristics. In addition, the intertemporal nature of parental incarceration and children's adult outcomes makes reverse causality extremely unlikely.

NLSY79CYA respondents are asked whether any adult members of their household had been sent to jail or prison in 2006 and 2008 and records the relationship between the respondent and household member and the respondent's age at first incarceration.¹ Respondents are then asked whether this household member was incarcerated more than once while living with the respondent. In the 2006 wave, respondents are asked whether any household members have been incarcerated while living with the respondent since his or her tenth birthday. In subsequent rounds, the survey asks if any household members have been incarcerated since the previous survey year. I combine the 2006 and 2008 rounds of parental incarceration variables to form my explanatory variables; a parental incarceration dummy will therefore equal one if a respondent experiences resident parental incarceration between his tenth birthday and the year he left the household (or 2008 if he still resides with his family). Table 1 summarizes the reported levels of household member incarceration for respondents of working age in 2008. About 13% of respondents in the working sample report that a household member has been sent to prison or jail since they were ten years of age.² About 1% of respondents report an incarcerated mother and almost 3% report incarcerated fathers. Consistent with national trends, male nuclear family members are much more likely to be incarcerated than their female counterparts. By construction, these statistics exclude "absentee fathers" (or any other non-resident relatives), which limits analysis to the case of resident parents but may also circumvent correlation between parental incarceration and outcomes that could arise in the case of absent parents.

¹"[Since date of last interview,/Since you were 10 years old,] has an adult member of your household (other than yourself), that is someone who was living in the same household as you at the time, been sent to jail or prison?" Choices include mother, father, stepmother, stepfather, brother, sister, maternal grandmother, maternal grandfather, paternal grandmother, paternal grandfather, step-grandmother, step-grandfather, spouse or partner, aunt, uncle, cousin, other relative, and other non-relative.

²This rate is likely inflated by characteristics of the NLSY, namely the interview of multiple siblings within a household and the oversampling of low income and at-risk individuals.

I sum the standard mathematics, reading recognition, and picture vocabulary scores of the Peabody Individual Achievement Test (PIAT) and percentile score on the Armed Forces Qualification Test (AFQT) to proxy for respondent and parental cognitive ability. The mean score is imputed for observations who do not report either score. My primary outcome variables for cross-sectional models are NLSY79CYA respondents' measures of education and earnings in 2008. Education is measured by reported highest grade completed. Income is drawn from data that report earnings and pay frequency (hourly, weekly, biweekly, monthly, or annually), which are converted to hourly values and logged. For log wage equations, outliers and likely miscodings in the top and bottom percent of the wage distribution are dropped. Respondents under the age of 22 are dropped from the working sample to exclude individuals too young to have completed college. The remaining working sample consists of 4,100 respondents who report over 537 instances of household member incarceration, including 165 parents; all respondents are matched to a mother in the NLSY79.

Table 2 presents selected variable means from the full working sample column (a) and subsamples split by parental incarceration (b) and (c). Column (d) reports the *t*-tested difference of means between subsamples. There is no wide variation in demographic characteristics except race: black respondents disproportionately report incarcerated parents. Parental cognition is lower for those respondents who report incarceration, however, there is no significant difference in respondent cognition over subsamples. Notably, average years of education and the likelihood of attending college are slightly higher for respondents who report incarcerated parents, although the likelihood of graduating is not appreciably different. Empirical modeling is needed to clarify the link between parental incarceration and children's outcomes.

1.3.2 Empirical Methodology

1.3.2.1 Linear Models

I initially estimate ordinary least squares (OLS) which take the form

$$y_i = \alpha_0 + \beta_1 Mother_i + \beta_2 Father_i + x_i' \beta + \varepsilon_i, \quad (1)$$

where y_i is the outcome variable for individual i in 2008 (highest grade completed or the natural logarithm of hourly wages), $Mother_i$ and $Father_i$ are dummies that equal one when individual i reports that the respective parent has been sent to prison or jail since the respondent's tenth birthday, and x_i' is a vector of controls. Respondent demographic controls include age, its square, sex (base category female), race (black and non-black/non-Hispanic, base category Hispanic), urban and Standard Metropolitan Statistical Area (SMSA) residency dummies, and regional fixed effects (northeast, north central, south, and west). Additional respondent controls include PIAT score, a dummy for an imputed PIAT score, a dummy for siblings in the household, and controls for parental presence in the household. Parental controls include AFQT score, age, race, highest grade completed, and dummies for residing in the maternal or paternal household as a youth. Controls drawn from the 1980 survey wave's illegal activity battery include dummies for being charged with an illegal activity as a youth or adult, a dummy for being sent to a youth correctional facility, and the number of times the respondent was sent. Controls drawn from the 1988 wave include a series of dummies for residing in a detention center, jail, or prison from age 12 to 18. Log wage models of earnings additionally control for highest grade completed, a grade retention dummy, the square of age, a measure of tenure at the respondent's current and primary job, and its square. Supplemental incarceration controls include the respondent's age at the earliest reported maternal

or paternal incarceration and the incarceration of non-parental household members. In initial specifications of education and earnings, I employ varying control schemes, first controlling for only respondent demographic variables and cumulatively adding additional respondent controls, parental controls, parent and child cognitive ability proxies, and supplemental incarceration variables.³ As only eight respondents report an incarceration of both parents, I do not include a covariate for such cases.

To explore the effects of parental incarceration for each combination of parent-child gender, I respecify (1) interacting the explanatory variables with respondent sex, allowing the effect of parental incarceration to vary across the gender of the respondent as well as the gender of the incarcerated parent. These models take the form

$$y_i = \alpha_0 + \beta_1 Mother_i * male_i + \beta_2 Mother_i * (1 - male_i) + \beta_3 Father_i * male_i + \beta_4 Father_i * (1 - male_i) + x_i' \beta_5 + \varepsilon_i, \quad (2)$$

where y_i is again a measure of adult earnings or education, $male_i$ is a dummy that equals one when respondent i is male, and x_i' is the same vector of controls.

Because there may be inherent differences between parents who are incarcerated once versus those incarcerated multiple times, I control for parental recidivism (or incarceration “dosage”) with dummies that equal one when the respondent reports that the incarcerated household member has been incarcerated more than once during the respondent’s time in the household. Although this survey instrument lacks detail with respect to individuals incarcerated more than twice, it allows models to estimate the effect of single incarceration on a child’s outcome more accurately by controlling for incarceration frequency.

³This strategy is discussed by Altonji et al. (2005) and implemented by Angrist and Krueger (1999), Bronars and Grogger (1994), Cameron and Taber (2004), and Ruhm (1997).

1.3.2.2 Nonlinear Models

Because parental incarceration is unlikely to have a linear effect across the education distribution, nonlinear models are also specified. I estimate ordered logistic regressions in the form of (1) and (2), where education is given as

$$y_i = \begin{cases} 1 & \text{if } hgc_i < 12 \\ 2 & \text{if } hgc_i = 12 \\ 3 & \text{if } 12 < hgc_i < 16 \\ 4 & \text{if } hgc_i \geq 16 \end{cases} \quad (3)$$

for an individual i with a reported highest grade completed hgc_i . This scale corresponds to an individual having (i) no high school diploma, (ii) a high school diploma or GED, (iii) some college or a two-year degree, and (iv) a four-year college or higher degree. All respondent, parental, and incarceration controls present in (1) and (2) are included and marginal effects are reported for each value of y_i .

1.4 Results

1.4.1 Education

Tables 3 and 4 present least squares estimates of parental incarceration's effect on educational attainment. Columns (a) through (e) of both tables report estimates under various control schemes, cumulatively including (a) respondent demographics, (b) additional respondent household characteristics, (c) parental background and criminal characteristics, (d) PIAT and AFQT scores, and (e) parental recidivism controls. Table 3 reports estimates for specification (1); under control scheme (a), maternal incarceration is estimated to negatively affect education. All subsequent control schemes, however, render the maternal incarceration variable statistically insignificant. When

incarceration effects are allowed to vary by child gender in specification (2), however, distinct differences between sons and daughters emerge. As shown in Table 4, the negative effects of maternal incarceration are associated primarily with daughters. Estimates of the maternal incarceration penalty on daughters are relatively stable across all control schemes, ranging from about 1.2 to 1.7 fewer years of education. This contrasts with smaller, positive, and insignificant coefficients for sons of incarcerated mothers. These results suggest that important variations in the effect of incarceration are found in (2) that are undetected in (1). Including a progressively large number of respondent and parent covariates across columns (a) through (d) demonstrates that estimated incarceration effects are largely unaffected, even after including AFQT and PIAT scores as controls. This suggests that heterogeneity in parents and respondents is unlikely to strongly bias the explanatory variables. As reverse causality between parental incarceration and children's outcomes is unlikely, these results are suggestive of incarceration's relatively exogenous impact on respondents. Controlling for incarceration frequency in column (e) slightly decreases the severity of the education penalty for daughters of incarcerated mothers, although neither parental recidivism variable is statistically significant. Across both specifications, the incarceration of fathers does not have a significant effect on educational attainment.⁴

Tables 5 and 6 report the marginal effects of parental incarceration from the ordered logit specification of (1) and (2). As OLS estimates of education are largely stable across varying control schemes, corresponding nonlinear models control for the full set of child and parent variables, alternatively excluding and including controls for maternal and paternal recidivism in the manner of column (e) of Table 4. Table 5 shows that the marginal effects of the child gender-invariant specification (1)

⁴For these and subsequent models, analogous specifications of general parent absence are estimated. A moderate parental absence spell of one to five years is not estimated to have a significant impact on children's outcomes in models of education and earnings, controlling for a respondent's age at first maternal and paternal absence.

are largely inconclusive, with only the third and fourth columns suggesting a small increase in the likelihood of high school completion. Table 6 shows again that specification (2) allows distinct incarceration effects to emerge for sons and daughters. Maternal incarceration is estimated to increase a daughter's likelihood of having an incomplete high school education by about 27 percentage points, although this estimate is rendered insignificant by the inclusion of recidivism controls in the second column. For sons, maternal incarceration is estimated to decrease the likelihood of having an incomplete high school education by 12 percentage points; controlling for incarceration dosage renders this estimate insignificant. Columns three and four of Table 6 are inconclusive on incarceration's effect on high school completion. Columns five and six indicate that maternal incarceration reduces a daughter's likelihood of having some college education by about 16 percentage points without controlling for recidivism. Maternal incarceration is also estimated to increase the likelihood of sons having an incomplete college education by about 10 percentage points. In column six, maternal recidivism is estimated to increase the likelihood of a child having an incomplete education at the 0.10 level. The final two columns of Table 6 indicate that maternal incarceration reduces a daughter's likelihood of completing a college or higher degree by about five to six percentage points across recidivism control schemes. When recidivism controls are not included, maternal incarceration appears to increase the likelihood of college completion for sons, although this result is only significant at the 0.10 level. As in (1), the ordered logit specification of (2) does not suggest that paternal incarceration has a significant effect of children's educational outcomes. In addition, only one parental recidivism variable is found to have a statistically significant effect on children's educational outcomes, although their inclusion reduces the magnitude of the main effect.

For both linear and nonlinear specifications of education, several key results emerge. First, the incarceration of mothers has a significant effect on children's ed-

educational attainment; in comparison, there is little evidence that the incarceration of fathers influences educational outcomes. The latter finding contrasts with several recent studies on paternal incarceration. Second, the effects of maternal incarceration vary by child gender, and models that interact incarceration variables with child gender report estimates that differ in sign, magnitude, and significance between sons and daughters. Findings suggest that maternal incarceration is associated with decreased educational attainment, particularly for post-compulsory levels of schooling. This negative relationship is most pronounced for the same-sex pairing of incarcerated mothers and daughters.

1.4.2 Earnings

My analysis turns next to parental incarceration's effects on the adult earnings of children. Table 7 and 8 reports OLS estimates for log wage specifications of (1) and (2). Columns (a) through (e) report estimates under the various controls schemes used in specifications of education, with the addition of controls for respondents' highest grade completed, tenure, and its square in (b) and subsequent columns. Table 7 presents results in which the effect of parental incarceration does not vary by child gender. As in models of education, the incarceration of mothers has the most consistently significant effect on children. Column (a) suggests that children of incarcerated mothers face a 23% wage penalty. Columns (b) through (d) show that the maternal incarceration coefficient is relatively stable across controls schemes, with an estimated wage penalty of about 17% when recidivism is not controlled in column (d). Again, the stability of the estimated effect of incarceration under these cumulative control schemes is suggestive of parental incarceration's exogenous effect on children's outcomes. The inclusion of incarceration dosage controls in column (e) increases the estimated wage penalty of maternal incarceration to about 18.5%. As in previous

education models, no parental recidivism coefficients have a statistically significant effect on wages.

Again, when the effects of incarceration are allowed to vary by both parent and child gender, distinct differences emerge for sons and daughters. Table 8 presents estimates of earnings for specification (2) and further illustrates the partitioning of parental incarceration effects into distinct parent-child combinations. Under initial control scheme (a), daughters of incarcerated mothers face a wage penalty of about 17% and sons of incarcerated mothers face a substantially larger penalty of 32%. The subsequent addition of controls across columns (b) through (e), however, largely renders maternal incarceration's effect on daughters statistically insignificant. In contrast, the maternal incarceration wage penalty faced by sons is significant across control schemes. Columns (b) through (d) show that the estimated wage penalty is relatively stable with the inclusion an exhaustive set of controls, ranging from 22-23%. When parental recidivism controls are included in column (e), a wage penalty of about 24% is found.

Results suggest that parental incarceration is associated with decreased earnings. As with models of educational attainment, the effects of incarceration vary in magnitude across parent and child gender, and preferred specification (2) illustrates the variation in wage penalties for children. Parental incarceration's impact on earnings is consistently negative across models that include a variety of respondent and parent controls. As in models of educational attainment, the incarceration of mothers appears to have a significant effect on children's outcomes; in contrast, the incarceration of fathers does not. Results suggest that maternal incarceration has substantial effects on the adult earnings of children, especially sons.

1.5 Respecifications and Related Outcomes

1.5.1 Nonlinear Education Threshold Models

Substantial evidence indicates that certain thresholds of education (such as high school or college completion) may have pronounced returns (Belman & Heywood, 1991; Hungerford & Solon, 1987; Jaeger & Page, 1996). To supplement previous models and clarify the link between incarceration and education, I examine the effect of parental incarceration on education through a series of logistic regressions, with dependent variables representing crucial educational decisions: continuing to college and completing a college or higher degree.⁵ These education threshold dummies are derived from the measure of education used in linear specifications of (1) and (2), and each logit contains the comprehensive vector of controls x'_{it} used in the linear models of education, alternatively excluding and including parental recidivism.

Table 9 presents marginal effects from logit models of (1). The first two columns indicate that parental incarceration has no effect on the likelihood of entering college. When recidivism controls are included in the fourth column, however, the likelihood of college completion is reduced by about three percentage points. When models are estimated in specification (2), these effects vary widely. Marginal effects in Table 10 indicate that maternal incarceration reduces the likelihood of a daughter entering college by about 24 to 26.5 percentage points across recidivism control schemes. Maternal incarceration also appears to increase the likelihood of sons attaining more than a high school education by 23 to 40 percentage points. Although maternal incarceration appears to increase the likelihood of sons attending college, columns three and four of Table 10 do not suggest that maternal incarceration increases the likelihood of completing college. These models support previous

⁵Outcomes are dummies that equal one when $hgc_{it} > 12$ (continuing to college), and $hgc_{it} \geq 16$ (college degree or higher).

results that suggest sons of incarcerated parents are more likely to begin higher education than otherwise comparable individuals but are not necessarily more likely to complete it. The negative effects on college completion found in specification (1) are largely partitioned to females in (2). Maternal incarceration reduces a daughter’s likelihood of completing college or a higher degree by about four percentage points across recidivism schemes and paternal incarceration reduces the likelihood of completion by about three percentage points, although this estimate is not significant when incarceration dosage is controlled. These models of educational thresholds are consistent with the linear and ordinal models of education, and further clarify the link between parental incarceration and the educational attainment of children. In total, these results suggest that incarceration most significantly affects the attainment of non-compulsory and higher education.

1.5.2 Education as an Intermediate Outcome

In previous models, incarceration directly affects the intermediate outcome of education, which in turn enters wage equations as a control. To estimate the “gross effect” of incarceration, I reestimate a log wage specification of (2) without education controls in the manner of Burgess et al. (2001). Table 11 reports coefficients from OLS estimates of (1) omitting the education covariate, excluding and including controls for parental recidivism. Columns one and two estimate maternal incarceration wage penalties of about 17-18%, less severe than wage penalties estimated in analogous models controlling for education. Table 12 reports estimates from log wage specifications of (2). As in previous log wage models, this specification allows several distinct results to emerge. Estimates reported in column one of Table 12 suggest that maternal incarceration results in a wage penalty of about 16% for daughters, about three percentage points more severe than the analogous estimate controlling for education. When parental recidivism controls are included, this penalty increases to

about 18.5%. For sons, maternal incarceration is estimated to induce a wage penalty of about 22% when recidivism controls are excluded. When parental recidivism controls are included in the second column, however, maternal incarceration is estimated to induce a wage penalty of 23%. In contrast to estimates for daughters of incarcerated mothers, omitting educational attainment as a control seems to lessen the severity of incarceration's effect on earnings for sons. Findings suggest that these incarceration effects may affect earnings models directly as well as indirectly through educational attainment. These mixed results provide further evidence of the distinct differences in parental incarceration's effect on sons and daughters and further suggest that the relationship between parental incarceration and children's outcomes varies across parent and child gender.

1.6 Discussion and Conclusion

Incarceration may be a life-altering event for both a prisoner and his or her children. I present evidence that parental incarceration may significantly affect children as they enter adulthood and the labor market. Results suggest that parental incarceration has varying effects across parent and child gender, as well as across outcomes of education and earnings. Because of the difference in effects on sons and daughters, which can vary in sign, size, and significance, models that allow estimation across child gender are essential to clarify the link between parental incarceration and later outcomes. Maternal incarceration is most consistently associated with negative outcomes for children across specifications and control strategies. In contrast, paternal incarceration is not conclusively associated with negative outcomes across all models, even when controlling for pre-incarceration household residence. The incarceration of mothers is associated with decreased educational attainment for daughters; nonlinear models find that this most pronounced for higher levels of education, particularly for starting and completing a college degree. Because education beyond the secondary

level is non-compulsory, these findings may suggest maternal incarceration and absence may lead to a child's ignorance of or ambivalence toward higher education, or inhibit a child's ability to finance a college education. Estimates for sons of incarcerated mothers suggest that incarceration may increase the likelihood of starting college, although there is no evidence that incarceration increases the likelihood of completion. Estimates of these models are generally insensitive to the progressive inclusion of a set of individual and parental controls; this strategy minimizes potential unobserved heterogeneity in incarcerated parents and respondent children, suggesting that parental incarceration is an exogenous shock to children's outcomes. The intertemporality of parental incarceration experienced as a child and subsequent adult outcomes also suggests that reverse causality is unlikely in specifications of education and wages.

Results from models of earnings suggest that maternal incarceration also has potentially negative effects on the adult earnings of children, particularly sons. Estimates suggest that for sons of incarcerated mothers, there are severe wage penalties with an upper bound of 22%. Log wage models omitting education covariates demonstrate that maternal incarceration may also negatively affect wages of daughters through its impact on the intermediate outcome of educational attainment. In total, incarceration seems to affect children through a channel outside of mainstream human capital acquisition in addition to its direct effect on education. These effects may perhaps take the form of shocks to a child's non-cognitive social capital or distortion of a child's perception of social norms, which may reduce his or her desire to seek mainstream employment or the likelihood of finding and maintaining employment. My findings suggest a strong link between the incarceration of a parent and negative labor outcomes for his or her children, which implies that the opportunity cost of criminal activity is lower for children of incarcerated parents. These findings point toward another channel through which anti-social and criminal activity may be transmitted from parent to child.

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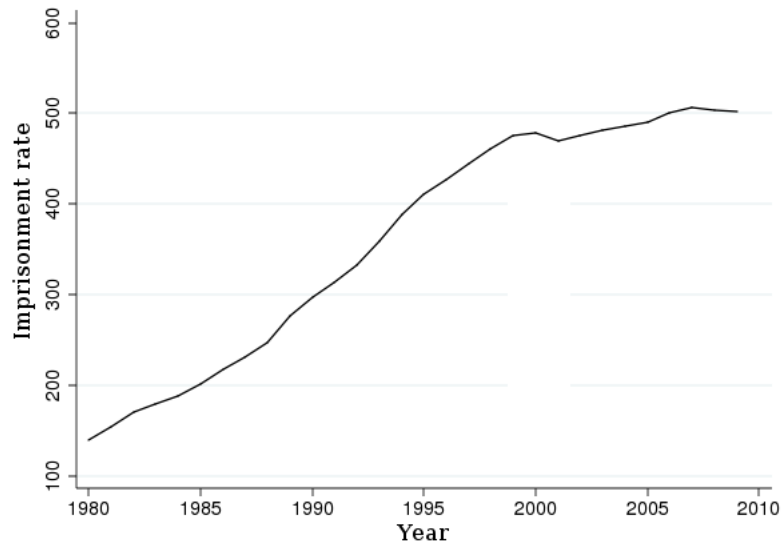
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FIGURES AND TABLES

Figure 1. U.S. imprisonment rate, 1980-2009.

Notes: Number of prisoners under state and federal jurisdiction per 100,000 population. Adapted from the Bureau of Justice Statistics “Correctional Population in the United States, 2009” data collection.

Table 1. Reported incarcerated household members, 2006 and 2008.

Relation to respondent	Percentage	
Any household member	13.10	[0.34]
Mother	1.44	[0.12]
Father	2.73	[0.16]
Stepfather	1.00	[0.10]
Brother	3.83	[0.19]
Sister	0.32	[0.06]
Maternal grandfather	0.01	[0.03]
Spouse or partner	1.39	[0.12]
Aunt	0.12	[0.03]
Uncle	1.22	[0.11]
Cousin	0.78	[0.09]
Other relative	0.54	[0.07]
Non-relative	1.39	[0.12]
<i>N</i>	4,100	

Notes: Standard deviations are in brackets. The sample describes respondents 22 or older who are matched to a respondent parent. No respondents report an incarcerated stepmother, grandmother, paternal grandfather, or step-grandparent.

Table 2. Selected sample means, respondents and parents.

Variable	(a) Full sample ($N = 4, 100$)	(b) No incar. parents ($N = 3, 935$)	(c) Incar. parents ($N = 165$)	(d) Mean difference (b)-(c)
Age	27.35 [3.15]	26.56 [3.18]	26.73 [3.24]	-0.1443 (0.2287)
Years of education	11.12 [3.60]	11.07 [3.80]	12.16 [2.51]	-1.0841*** (0.2988)
Some college dummy	0.33 [0.48]	0.33 [0.47]	0.42 [0.04]	-0.0876** (0.0375)
College graduate dummy	0.10 [0.33]	0.10 [0.30]	0.08 [0.02]	0.0140 (0.0234)
PIAT composite score	288.34 [37.47]	289.84 [32.37]	286.29 [38.93]	3.5626 (2.3413)
Male dummy	0.51 [0.50]	0.51 [0.50]	0.47 [0.50]	0.0413 (0.0359)
Black dummy	0.36 [0.48]	0.30 [0.46]	0.38 [0.49]	-0.0770** (0.0331)
White dummy	0.43 [0.50]	0.51 [0.50]	0.40 [0.49]	0.1127*** (0.0359)
Age (mother)	47.43 [2.16]	47.44 [2.18]	47.06 [2.28]	0.3838** (0.1570)
Years of education (mother)	12.20 [2.68]	12.28 [2.63]	12.33 [2.14]	-0.0409 (0.1876)
AFQT percentile (mother)	28.90 [23.59]	32.94 [24.47]	26.78 [22.36]	6.1600*** (1.8231)

Notes: Standard deviations are in brackets. Standard errors are in parentheses. Time-variant data are drawn from the 2008 waves of the NLSY79 and NLSY79CYA. The sample describes respondents 22 or older who are matched to a respondent parent. Reported means describe the unweighted sample.

Table 3. OLS estimates from education model (1).

	(a)	(b)	(c)	(d)	(e)
<hr/> Key variables <hr/>					
Mother	-0.8635** (0.3954)	-0.5968 (0.5019)	-0.5887 (0.4540)	-0.5401 (0.4470)	-0.6756 (0.4744)
Father	-0.1066 (0.2669)	-0.0621 (0.3253)	-0.0934 (0.3104)	-0.0011 (0.2930)	0.0519 (0.3157)
Recidivism (Mother)					0.9550 (0.8622)
Recidivism (Father)					-0.2047 (0.6620)
<hr/> Controls <hr/>					
Demographics	X	X	X	X	X
Respondent/household		X	X	X	X
Parental characteristics			X	X	X
PIAT/AFQT scores				X	X
Recidivism					X
R^2	0.1310	0.2767	0.3017	0.3409	0.3412

Notes: $N = 3,187$. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted.

Table 4. OLS estimates from education model (2).

Key variables	(a)	(b)	(c)	(d)	(e)
Mother/daughter	-1.6582*** (0.4934)	-1.2562* (0.6587)	-1.3660** (0.5695)	-1.4929*** (0.5668)	-1.3475** (0.6576)
Mother/son	-0.0324 (0.5637)	0.1971 (0.6681)	0.3253 (0.6298)	0.5979 (0.5525)	0.1370 (0.5527)
Father/daughter	0.1218 (0.3519)	0.1841 (0.3684)	0.1712 (0.3420)	0.1476 (0.3348)	0.1831 (0.4253)
Father/son	-0.3014 (0.3939)	-0.3109 (0.4437)	-0.3572 (0.4406)	-0.1506 (0.4147)	-0.1120 (0.4609)
Recidivism (Mother)					0.9576 (0.8696)
Recidivism (Father)					-0.1852 (0.6488)
Controls					
Demographics	X	X	X	X	X
Respondent/household		X	X	X	X
Parental characteristics			X	X	X
PIAT/AFQT scores				X	X
Recidivism					X
R^2	0.1325	0.2789	0.3041	0.3437	0.3425

Notes: $N = 3,187$. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted.

Table 5. Marginal effects from educational attainment ordered logit (1).

Key variables	$hgc_i < 12$	$hgc_i = 12$	$12 < hgc_i < 16$	$hgc_i \geq 16$
Mother	0.0473 (0.0889)	0.0722 (0.0992)	0.0056* (0.0031)	0.0052 (0.0060)
Father	0.0307 (0.0528)	0.0553 (0.0584)	0.0047 (0.0051)	0.0057*** (0.0020)
Recidivism (Mother)	-0.1585 (0.1146)	-0.1154 (0.1665)	-0.0360 (0.0639)	-0.0533 (0.0670)
Recidivism (Father)	-0.0314 (0.1005)	-0.0091 (0.0371)	-0.0239 (0.0396)	-0.0417 (0.0412)
			-0.0169 (0.0276)	-0.0116 (0.0182)
			0.1345 (0.0673)	0.1394 (0.2140)
			0.0260 (0.0860)	0.0144 (0.0516)

Notes: $N = 3,187$. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted.

Table 6. Marginal effects from educational attainment ordered logit (2).

Key variables	$hgc_i < 12$	$hgc_i = 12$	$12 < hgc_i < 16$	$hgc_i \geq 16$
Mother/daughter	0.2702** (0.1351)	-0.0524 (0.0632)	-0.1596*** (0.0573)	-0.0581*** (0.0157)
Mother/son	-0.1235** (0.0440)	-0.0714 (0.0592)	0.1091** (0.0493)	0.0858 (0.0663)
Father/daughter	-0.0115 (0.0495)	-0.0028 (0.0132)	0.0094 (0.0408)	0.0049 (0.0218)
Father/son	0.0957 (0.0886)	0.0030 (0.0114)	-0.0689 (0.0568)	-0.0297 (0.0210)
Recidivism (Mother)	-0.1567 (0.1158)	-0.1134 (0.1668)	0.1341* (0.0712)	0.1360 (0.2116)
Recidivism (Father)	-0.0306 (0.0996)	-0.0089 (0.0365)	0.0255 (0.0854)	0.0140 (0.0507)

Notes: $N = 3, 187$. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted.

Table 7. OLS estimates from earnings model (1).

Key variables	(a)	(b)	(c)	(d)	(e)
Mother	-0.2339*** (0.0624)	-0.1758*** (0.0618)	-0.1638*** (0.0620)	-0.17456** (0.0636)	-0.1855*** (0.0642)
Father	-0.0650 (0.0605)	-0.0975 (0.0780)	-0.0697 (0.0639)	-0.0729 (0.0636)	-0.1321 (0.0933)
Recidivism (Mother)					0.0435 (0.1696)
Recidivism (Father)					0.1484 (0.1145)
Controls					
Demographics	X	X	X	X	X
Respondent/household		X	X	X	X
Parental characteristics			X	X	X
PIAT/AFQT scores				X	X
Recidivism					X
R^2	0.1280	0.2414	0.2445	0.2528	0.2534

Notes: $N = 2,967$. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted.

Table 8. OLS estimates from earnings model (2).

Key variables	(a)	(b)	(c)	(d)	(e)
Mother/daughter	-0.1686* (0.0867)	-0.1346 (0.0824)	-0.1138 (0.0758)	-0.1297* (0.0730)	-0.1420 (0.0876)
Mother/son	-0.3211*** (0.0868)	-0.2236** (0.0888)	-0.2286** (0.0979)	-0.2351** (0.1048)	-0.2373** (0.0927)
Father/daughter	-0.0185 (0.0694)	-0.0492 (0.0851)	-0.0282 (0.0763)	-0.0365 (0.0755)	-0.0941 (0.0962)
Father/son	-0.1158 (0.0991)	-0.1438 (0.1124)	-0.1143 (0.1010)	-0.1121 (0.1003)	-0.1766 (0.1276)
Recidivism (Mother)					0.0278 (0.1888)
Recidivism (Father)					0.1530 (0.1151)
Controls					
Demographics	X	X	X	X	X
Respondent/household		X	X	X	X
Parental characteristics			X	X	X
PIAT/AFQT scores				X	X
Recidivism					X
R^2	0.1285	0.2426	0.2458	0.2541	0.2547

Notes: $N = 2,967$. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted.

Table 9. Marginal effects from education logit (1).

Key variables	$Pr(college)$		$Pr(degree)$	
Mother	0.0332 (0.1394)	-0.0622 (0.1423)	-0.0324 (0.0226)	-0.0228 (0.0321)
Father	0.0481 (0.0684)	0.0202 (0.0869)	-0.0200 (0.0147)	-0.0326*** (0.0105)
Recidivism (Mother)		0.2184 (0.2309)		
Recidivism (Father)		0.0842 (0.1005)		0.0140 (0.0392)

Notes: $N = 3,187$. $Pr(college) \equiv hgc_i > 12$ dummy; $Pr(degree) \equiv hgc_i \geq 16$ dummy. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted. Maternal recidivism controls are omitted in degree completion logits due to data limitations.

Table 10. Marginal effects from education logit (2).

Key variables	$Pr(college)$		$Pr(degree)$	
Mother/daughter	-0.2656*** (0.0979)	-0.2440** (0.1208)	-0.0393*** (0.0129)	-0.0418** (0.0166)
Mother/son	0.4033*** (0.1028)	0.2286* (0.1323)	0.0025 (0.0607)	0.0237 (0.0817)
Father/daughter	0.0850 (0.0923)	0.1855 (0.1515)	-0.0252* (0.0137)	-0.0256 (0.0160)
Father/son	0.0226 (0.1043)	-0.1287 (0.0944)	-0.0058 (0.0297)	-0.0416 (0.0142)
Recidivism (Mother)		0.2307 (0.2259)		
Recidivism (Father)		0.0889 (0.1001)		0.0173 (0.0392)

Notes: $N = 3,187$. $Pr(college) \equiv hgc_i > 12$ dummy; $Pr(degree) \equiv hgc_i \geq 16$ dummy.
 * Significant at the 0.01 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted. Maternal recidivism controls are omitted from degree completion logits due to data limitations.

Table 11. OLS estimates from earnings model (1), education control omitted.

Key variables		
Mother	-0.1728*** (0.0654)	-0.1839*** (0.0666)
Father	-0.0687 (0.0619)	-0.1288 (0.0899)
Recidivism (Mother)		0.0449 (0.1736)
Recidivism (Father)		0.1506 (0.1115)
R^2	0.2307	0.2313

Notes: $N = 2,967$. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted.

Table 12. OLS estimates from earnings model (2), education control omitted.

Key variables		
Mother/daughter	-0.1609** (0.0776)	-0.1854* (0.0952)
Mother/son	-0.2194** (0.1109)	-0.2300** (0.0945)
Father/daughter	-0.236 (0.0745)	-0.0869 (0.0927)
Father/son	-0.1222 (0.0960)	-0.1933 (0.1228)
Recidivism (Mother)		0.0698 (0.1984)
Recidivism (Father)		0.1695 (0.1111)
R^2	0.2378	0.2386

Notes: $N = 2,967$. * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. Results are weighted.

CHAPTER II

Returns to Post-Incarceration Education for Former Prisoners

2.1 Introduction

There has been a dramatic increase in the number of Americans incarcerated since the early 1980s, resulting in a prison population of 1.6 million by 2011 (Bureau of Justice Statistics [BJS], 2012). In 2001, about 600,000 prisoners were readmitted into mainstream society; by 2009, this number was about 730,000 (Austin, 2001; BJS, 2012; West et al., 2011). Over the past three decades, federal policies have effectively lengthened the severity of sentencing, including longer sentences for non-violent crimes, truth-in-sentencing laws, determinate sentencing, and mandatory minimum sentencing (Austin, 2001; Seiter & Kadela, 2003), coupled with a decrease in parole and probation eligibility (Petersilia, 1999). The most recent national study estimates that 67.5% of releasees are rearrested and 51.8% are reincarcerated within three years of release (Langan & Levin, 2002). Among the explanations is the increased difficulty finding and maintaining employment after an incarceration spell. Current research suggests that ex-offenders face increased labor market difficulties stemming from incarceration, including depreciation of human and social capital (Apel & Sweeten, 2010a; Huebner, 2005), social networks (Davies & Tanner, 2003; Western et al., 2001), and mental health (Petersilia, 1999). Employment after incarceration, however, is believed to reduce the likelihood that releasees commit crime and are resentenced (Becker, 1968). This theoretical path to recidivism is particularly likely for criminals who supplement or substitute legal income for income gained from criminal activity.

In an attempt to address this problem, private, state, and federal prisons have implemented various programs to rehabilitate prisoners and releasees by streamlining their reentry into society: between 2010 and 2012, about \$183 million were awarded to prisoner reentry programs, a marked increase from \$100 million allocated between 2001 and 2004 (Petersilia, 2004; Tucker, 2012). About half of inmates participate in various reentry programs, including education, vocational and technical training, substance abuse treatment, and psychological counseling (Austin, 2001; Harlow, 2003; Lynch & Sabol, 2001). This paper investigates the effect of post-incarceration education on employment, earnings, and recidivism in an attempt to evaluate the effectiveness of education as rehabilitation.

Prisoners and releasees are encouraged to attain education because of the well-established relationship between educational investment and labor market success. In turn, higher levels of human capital and earnings are believed to increase the opportunity cost of illegal activity, decreasing crime (Henry & Jacobs, 2007; Lochner & Moretti, 2004). Steady employment is also seen as a signal of successful rehabilitation into society (Solomon et al., 2004). Studies also suggest that the responsibilities, regularity, and structure of employment help reassimilation into the family and community (Laub & Sampson, 2001; Maruna, 2001). If post-incarceration education does not result in appreciable labor market benefits for former prisoners in comparison to those who do not attain education, however, this may imply that education is limited as a rehabilitative tool.

In addition to its detrimental effects on labor supply, there are potential links between incarceration and labor demand. Releasees often face explicit barriers to employment after release including increased difficulty or prohibition from obtaining licensing in certain industries (Henry, 2008). In addition, many states require former prisoners to indicate their ex-offender status during the job application process (Love, 2006). If prior incarceration is known to employers, it may signal negative

characteristics such as unreliability or the potential of crime in the workplace. In addition to augmenting human capital, post-incarceration education may serve as a counter-signal, suggesting a releasee's willingness to reform, persistence, and aptitude. Positive returns to post-incarceration education could therefore mitigate the stigma of incarceration on the labor market.

There are several potential reasons, however, why educational investment after incarceration may not result in appreciable gains. Incarceration may be psychologically strenuous and damaging, making education more difficult to attain. Research suggests that releasees may face difficulty reentering society, and the negative mental, social, and psychological characteristics of prisoners (prior to and influenced by incarceration) may increase the difficulty of attaining education (Petersilia, 1999; Solomon et al., 2004; Western et al., 2001). In addition, as prisoners possess below-average levels of education before incarceration, they may be more likely to sort into low-skill, low-pay employment (Solomon et al., 2004). In such cases, education may be less relevant to potential employment available to releasees. This may suggest that education is less effective at increasing employment and earnings for individuals who have experienced an incarceration spell.

I estimate the returns to education attained after incarceration to evaluate the effectiveness of education on improving labor market performance and decreasing recidivism. Data are drawn from the National Longitudinal Survey of Youth 1979, which identify incarceration spells for a sample of respondents over three decades. I identify incarceration spells and subsequent investment in education, including high school and college completion. Regression and propensity score matching models examine reported wages, weeks of the year employed, unemployed, and out of the labor force, and the likelihood of recidivism. In comparison to releasees that do not attain education after a sentence, post-incarceration education increases a respondent's employment, decreases unemployment, and increases labor force participation. The

magnitude of this benefit, however, is directly related to the level of education attained, with college attendance and completion yielding the largest benefits. Mixed evidence is found for a relationship between post-incarceration college completion and increased wages. When models are refined to distinguish between traditional high school completion and equivalency certification (specifically, the General Equivalency Diploma), the former is associated with increased employment and decreased unemployment. Equivalency certification does not seem to directly improve labor market performance. In total, this study provides evidence that higher levels of post-incarceration educational investment may have a limited but positive effect on some aspects of labor market performance.

2.2 Previous Findings

Mass incarceration and its effect on labor market performance has become a topic of interest in current economic research. It is well established in descriptive and empirical studies that prisoners are more likely to possess lower levels of education, employment, and household income before incarceration (Freeman, 1996; Holzer et al., 2004). In 1997, about 40% of the incarcerated population did not hold a high school diploma or GED, well above the national average of 18% (Solomon et al., 2004). Most empirical studies therefore attempt to disentangle the effect of incarceration on labor outcomes from an individual's prior wage- and employment-depressing characteristics, with most research suggesting a negative relationship (Holzer, 2009). Early studies by Freeman (1991) and Grogger (1995) examining earnings and employment data for young adults suggest a severe, negative incarceration effect. Later studies exploiting the growth of longitudinal survey data have refined and largely corroborated these findings. Negative relationships between employment, earnings, and earnings growth have been established utilizing the Fragile Families and Child Wellbeing Survey (Geller et al., 2006) and the 1979 and 1997 cohorts of the National

Longitudinal Survey of Youth (Apel & Sweeten, 2010a; Western, 2002). Studies utilizing state prisoner and releasee surveys find similar negative effects on post-release earnings and employment (Grogger, 1995; Needels, 1996). Related studies of recidivism suggest that stable employment and wages are negatively related to individual arrest rates (Bernstein & Houston, 2000; Sampson & Laub, 1993; Western & Pettit, 2000; Western & Pettit, 2005)).

Over the previous half century, American prisoner rehabilitation has had an institutional focus, in which recidivism is theoretically reduced by influencing access to housing, education, and employment (Petersilia, 2004). In the early 1960s, the correctional system began to implement education and training as a prisoner reentry program (Tyler & Kling, 2007). After decades of emphasizing rehabilitative methods, however, correctional systems have shifted toward methods of deterrence, with a reduced emphasis on preparing prisoners for release. This shift was in part supported by early research, most notably by Martinson (1974), that concluded that no prisoner reentry programs effectively reduce recidivism. Subsequent criticism by Gendreau and Ross (1979), however, left the question of program effectiveness open to debate. Although the link between education and decreased likelihood of crime is well-established in the economic literature (Lochner, 2004; Lochner & Moretti, 2004; Machin et al., 2011), research on the link between post-incarceration education and crime has largely concerned program effectiveness. MacKenzie and Hickman (2006) examine 184 correctional programs that employ treatment and control groups, concluding that vocational training and basic adult education appear to reduce recidivism. Studies by Adams et al. (1994) and Vito and Tewksbury (1999) suggest that prisoner education programs raise scores on educational achievement tests but do not conclusively decrease recidivism.

A variety of reentry programs are currently offered in private, state, and federal prisons including counseling, vocational training, and education (Seiter & Kadela,

2003; Solomon et al., 2004). Additionally, intermediary agencies offer services such as job placement, job coaching, and assistance acquiring Social Security cards (Holzer et al., 2004). Education programs vary in scope, level of education conferred, and their occurrence during or after a sentence, and include GED preparation and completion, high school diploma completion, English competency classes, and college credit toward two- and four-year degrees (Solomon et al., 2004; Tyler & Kling, 2007). Access to these programs, however, has been reduced over the previous decade, most notably by the Violent Crime Act of 1994, which ended Pell Grant eligibility for prisoners (Gorgol & Sponsler, 2011). Examining the returns to education for former prisoners may clarify the link between institution-sponsored prisoner education, subsequent labor market outcomes, and recidivism.

This study extends the current incarceration literature in several ways. First, I examine the effect of education attained after an incarceration spell, within and without formal reentry programs. The unique nature of the National Longitudinal Survey allows me to construct annual and biennial incarceration, education, and employment histories for a sample of Americans that entered adulthood during the prison boom of the 1980s. In addition, an unusually rich set of survey variables allows me to distinguish the timing and level of post-incarceration education and control for a wide variety of demographic, economic, and behavioral characteristics. This study clarifies the relationship between education and employment after incarceration and considers the various levels of complete and incomplete education a former prisoner may attain.

2.3 Data and Methodology

2.3.1 National Longitudinal Survey of Youth 1979

I draw data for this study from the 1979 cohort of the National Longitudinal Survey of Youth (Bureau of Labor Statistics [BLS], 2009). The survey interviews a sample of 12,868 youth and young adults interviewed throughout their life course. The National Longitudinal Survey of Youth 1979 (NLSY79) began surveying individuals aged 14 to 21 in 1979 over an especially wide variety of demographic, household, and individual characteristics. Subsequent survey waves were conducted annually through 1994 and biennially thereafter (BLS, 2009). By the 2010 survey, 7,565 respondents remained in the sample.

The longitudinal nature of the NLSY79 provides a rich battery of questions over a range of time during which most respondents enter the labor market and some are incarcerated. Importantly, the survey attempts to interview incarcerated respondents on location in jail or prison when permitted (BLS, 2011). In each survey year, respondents report their residence type and can indicate residence in an incarceration facility. Combined with post-release interviews, this provides a unique opportunity to trace the life course of individuals that are incarcerated, released, and subsequently face the decision to enter the labor force. For my analysis, I identify the earliest survey wave in which respondents report adult incarceration and the earliest subsequent wave in which incarcerated respondents do not report incarceration, which constitutes an incarceration spell. In total, 442 respondents in the working sample report adult prison residence in at least one survey year.¹ These respondents are an average age of 28 and spend an average of 3.78 years incarcerated. About 48% of respondents report non-consecutive incarceration spells which I assume represents recidivism.

¹Individuals who have been incarcerated comprise about 5.8% of the working sample, as compared to the national average of about 0.5% (BJS, 2009). This is likely due to intentional oversampling in the National Longitudinal Surveys.

I draw time-variant variables from the 2010 wave of the NLSY79 and time-invariant variables from their earliest occurrence in the survey. Using the incarceration history data, I am able to identify any education prior to and after a respondent's earliest reported incarceration. Post-incarceration education forms the basis of my explanatory variables, and pre-incarceration education is retained as a covariate for subsequent models.² I use the reported number of weeks of the year employed, unemployed, not in the labor force, and the natural logarithm of reported hourly wages as my primary dependent variables. In addition, I create a recidivism dummy that equals one for respondents that report incarceration in multiple, nonconsecutive survey waves. I use respondents' 1980 percentile score on the Armed Forces Qualification Test (AFQT) as a proxy of innate cognitive ability; to preserve sample size, the mean score is imputed for respondents who do not report a score. I use the reported number of years worked at a respondent's primary job as a measure of job tenure.

In addition to the aforementioned variables, I construct several variables pertaining to criminal activity as a young adult, drawn from the 1980 battery of criminal activity questions. I condense several of these variables into dummies for (a) the underage use or sale of alcohol, marijuana/hashish, or other hard drugs and/or chemicals, (b) shoplifting, property theft, automobile theft, or knowingly selling stolen goods, (c) destruction of property, using force to obtain property, threatening to attack someone, and (d) attacking someone with the intent to injure or kill. In addition, I include several variables concerning experience with the criminal justice system: the earliest age and number of times they have been stopped by police, charged with an illegal activity as a youth or adult, convicted with an illegal activity, or sent to a correctional institution. These criminal activity variables are vital in subsequent empirical analysis, especially as balancing covariates in matching techniques.

²Essentially, respondent education is decomposed such that $Edu_{i,2010} = Edu_i^{pre-incar.} + Edu_i^{post-incar.}$. For respondents who never report an incarceration spell post-incarceration education is zero such that $Edu_{i,2010} = Edu_i^{pre-incar.}$.

Table 1 presents selected means for the working sample of 7,565 respondents. Mean labor, education, and cognition variables are reported for the subsample that never reports an incarceration spell and the subsample that reports one or more incarceration spells. Column (c) reports *t*-tested differences of means between subsamples. Consistent with prior descriptive research, incarcerated individuals report lower wages, labor supply and participation, education, and AFQT scores than individuals who are never incarcerated. The average respondent who has never been incarcerated spends 13 more weeks employed, three fewer weeks unemployed, and eight fewer weeks out of the labor force than the average respondent that has been incarcerated.

Table 2 further examines the incarcerated subsample and presents sample means for the subsample of incarcerated individuals split by post-incarceration education status. About 57% of respondents who experience incarceration do not acquire more education after release. In contrast to Table 1, however, the results of *t*-tests in column (c) suggest a less clear statistical link between post-incarceration education and improved labor and human capital characteristics. There is no significant difference in means between subsamples for reported hourly wages, labor supply, or AFQT percentile. The lack of a significant difference in AFQT scores suggests that gaps in academic aptitude or cognition are not driving selection into education after incarceration.

2.3.2 Empirical Methodology

2.3.2.1 Regression Models

I estimate ordinary least squares (OLS) and probit models of the form

$$y_i = \alpha_0 + \beta_1 Education_i^{post-incar.} + x_i' \beta + \varepsilon_i, \quad (1)$$

where y_i is an individual i 's 2010 outcome (log wages, weeks employed, weeks unemployed, or weeks out labor force), and $Education_i^{post-incar.}$ is the explanatory post-incarceration education variable. I alternatively specify this variable as (a) the number of years of post-incarceration education, (b) a dummy that equals one if the individual attains any level of post-incarceration education, (c) a dummy for completion of high school diploma or more (either traditional completion of a high school curriculum or GED), (d) a dummy for completing any level of education after high school, including college, and (e) a dummy for completion of a four year college degree or higher. Controls include age, its square, gender, race (non-white/non-Hispanic and black, base category Hispanic), region residence (northeast, north central, south, and west), urban and SMSA residence, marital status, number of children, household size, the aforementioned criminal activity variables, and AFQT score. Incarceration and criminal activity controls include a dummy for experiencing incarceration in any year, total number of years in jail, age at first incarceration, age at first release, and the previously mentioned criminal activity and criminal justice variables. Wage equations also control for years of education and job tenure. In my log wage and employment specifications, I use a variety of control schemes, initially controlling for only respondent demographics, and progressively including the household, crime, and cognition control blocks.³ I initially estimate (1) using the full sample and reestimate using the subsample of respondents that report at least one incarceration spell.

2.3.2.2 Propensity Score Matching

My empirical methodology turns next to propensity score matching (PSM). Because post-incarceration education is unlikely to be randomly assigned, issues of selection may arise when evaluating the effect of education on post-release outcomes. It is possible that releasees who attain education after incarceration may systematically

³This strategy is discussed and implemented by Altonji et al. (2005), Angrist and Krueger (1999), and Ruhm (1997).

vary observably from those who do not; an individual may possess higher levels of cognition, aptitude, or other unobserved characteristics that may simultaneously influence his likelihood of attaining post-incarceration education and employment. PSM allows comparison of similar ex-prisoners that vary only in their decision to obtain education after incarceration. Several earlier papers rely on the comparison of incarcerated individuals’ outcomes to a constructed comparison sample of non-incarcerated individuals, but as matching techniques have become more commonplace they have supplanted this method in many recent studies (Apel & Sweeten, 2010a, Apel & Sweeten, 2010b; Geller et al., 2006; Grogger, 1995; Western, 2002).

As detailed by Rosenbaum and Rubin (1983), the propensity score $p(z_i)$ is the likelihood of an individual i attaining (or receiving the “treatment” of) post-incarceration education conditional on a vector of pre-incarceration characteristics z_i such that

$$p(z_i) = Pr(\text{Education} = 1 \mid z_i). \quad (2)$$

This first stage result is obtained from a probit in which the dependent variable is a dummy that equals one when an individual receives the treatment conditioned on a control vector z_i . The treatment is alternatively specified as (a) attaining any level of post-incarceration education, (b) completing at least high school, (c) attaining any level of college education, and (d) completing a college degree or higher. Treatment and control groups are balanced on pre-treatment characteristics and matched by propensity score (Imbens & Wooldridge, 2009). I utilize kernel and stratification matching algorithm methods as a check of estimate robustness.⁴ Both matching algorithms estimate the average effect of the treatment on the treated (ATT), interpreted as the average difference in outcomes between treated and untreated observations with similar propensity scores. Here, PSM compares individuals that have similar charac-

⁴Caliendo and Kopeinig (2008) provide a overview of these and other matching algorithms.

teristics (including incarceration history) and differ only in their post-incarceration education status. The pre-treatment vector z_i is comprised of covariates that plausibly influence the outcome and the likelihood of receiving the treatment, including age, gender, race, AFQT percentile, the criminal activity and criminal justice variables, age at first reported incarceration, age at first release, and total number of years incarcerated.

2.4 Results

2.4.1 Regression Results

Table 3 presents estimates of the returns to post-incarceration education across the subsample that reports at least one incarceration spell. Columns (a)-(d) progressively include blocks of demographic, household, criminal activity, and cognition controls. Point estimates are largely unchanged across columns. Column (a) suggests that the return to a year of post-incarceration education is a wage premium of about 9%; the inclusion of additional controls over columns (b)-(d) reduces this estimate to about 7%. When the explanatory variable is a dummy for any level of post-incarceration education, the return to education is between 18-22%. Subsequent specifications of the explanatory variable distinguishing between different levels of education do not report significant wage effects. The lack of significant effects for specific levels of education suggests that selection into education due to unobserved characteristics may be driving the observed wage premiums. The bottom panel of Table 3 suggests that attending some level of college may increase employment by about five weeks, although this result is not apparent across all control schemes.

Table 4 presents estimates of (1) across all outcomes and explanatory variables, controlling for the comprehensive vector of variables used in column (d) of Table 3. The results for the other outcomes expand upon those reported for earnings

and employment. College education after an incarceration spell appears to have an appreciable effect on outcomes: attending some college increases labor market participation by about nine weeks annually, and college completion is estimated to reduce annual unemployment by about 3.5 weeks.

Table 5 presents estimates of (1) across the subsample of respondents that report at least one incarceration spell, retaining the comprehensive control vector x'_i . In this specification, only the completion of college or a higher degree appears to appreciably influence labor market outcomes. College-educated releasees experience a wage penalty of about 44% in comparison to those releasees that do not receive a college degree. Results also suggest that college-educated releasees spend about five fewer weeks of the year unemployed. In total, regression results point toward the positive albeit dampened effects of post-incarceration education. Higher education appears to have the largest benefits to former prisoners, although these regression specifications are unable to address the potential problem of non-random selection into post-incarceration education.

2.4.2 Matching Results

Table 6 presents results from PSM models utilizing kernel and stratification matching algorithms. The average treatment effect on the treated (ATT) is reported for each “treatment” category of post-incarceration education. The first two columns report education treatment effects on wages and suggest that college completion results in wage premiums of 36-40%. Acquiring some level of post-high school education results in a smaller wage premium of 19-24%. Columns three and four provide new evidence that post-incarceration education increases employment with college attendance increasing the annual number of weeks worked by five to seven weeks and college completion increasing the annual number of weeks employment by eight to nine weeks across matching algorithms.

The fifth and sixth columns suggest that college completion is associated with a decrease in unemployment of about six weeks annually. In contrast with the aforementioned outcomes, the college attendance treatment does not have a significant effect on unemployment. The final two columns suggest that individuals that receive any level of college education spent four fewer weeks out of the labor force annually. In all specifications, estimated ATT are internally consistent across matching algorithms. For this final outcome variable, the lack of a significant return to “completed” education (at the high school or college level) may be explained in part in several ways. First, matching techniques employed may not fully address non-random selection into education. Second, there may be a correlation between college completion and non-participation in the labor force in the case of illegal income earners: individuals that subsist primarily on income earned from crime may be incentivized to attain education, perhaps in an attempt to impress parole boards or probationary authorities. Another possibility is that high school equivalency certifications such as the GED function as negative or weak signals to potential employers of releasees and are biasing composite high school treatment effects. As this final hypothesis is easily tested, I examined it further in this study.

In total, results suggest a link between post-incarceration education and improved labor outcomes, although these benefits are largely confined to respondents who attain non-compulsory levels of education, particularly a college degree. Regression results controlling for a wide battery of demographic, household, and criminal variables suggest that college education almost solely predicts decreased unemployment and increased labor force participation. There is also some evidence that the attainment of some college reduces a respondent’s number of weeks spent out of the labor force. A preliminary examination of post-incarceration education’s effect on recidivism is largely inconclusive. Matching results largely support and expand the

results of corresponding regression models, and estimated treatment effects are consistent in sign, size, and significance across matching algorithms.

2.5 Respecification of High School Completion

I now consider the question of post-incarceration education at the high school level. Many prisoner education programs involve preparation and administration of the General Equivalency Diploma, a test designed to certify an individual's academic competency at the high school level. Studies on the GED, however, suggest that the certification has limited benefits on the labor market and in some cases can be detrimental (Cameron & Heckman, 1993; Heckman et al., 2000; Heckman, 2010). Individuals who earn the GED during or after a sentence may therefore not find the certification helpful in securing employment.

NLSY79 respondents that have completed high school are asked to specify whether this education was attained by the traditional receipt of a high school diploma or by completion of the GED. The preceding high school completion variable equals one when respondents report the completion of a high school diploma or GED after their earliest reported incarceration spell. I reestimate regression and matching models distinguishing between completion types, utilizing GED completion and high school diploma completion dummies as explanatory variables.⁵

Table 7 presents OLS and probit results for GED and high school diploma completion. These results suggest that high school diploma completion increases annual employment by about 18 weeks and decreases unemployment by about seven weeks in comparison to those releasees who do not attain a post-incarceration high school diploma. In contrast, the GED appears to have no appreciable effect on any outcomes;

⁵Some respondents report holding both a GED and a high school diploma. I count these respondents in the diploma group only.

estimates are small and statistically insignificant, suggesting that the composite high school completion variable in previous models is not the preferred specification.

Table 8 presents results from PSM models for both post-incarceration high school completion treatments. Again, these findings mirror and expand on those presented earlier. Neither high school completion treatment is associated with a wage premium, consistent with the general high school ATT reported in Table 5. Columns three and four suggest that across matching algorithms, diploma receipt increases a respondent's employment between eight and nine weeks and decreases unemployment by about four weeks, again paralleling similar employment premiums for college completion. GED completion is not associated with benefits in any specification. There is no clear link between GED or diploma completion and labor force participation, consistent with the general high school completion ATT reported in Table 5. These respecifications echo the notion posited by Heckman et al. (2000) that the GED may function as a negative signal to potential employers and may not appreciably benefit ex-prisoners reentry into the mainstream labor market.

The respecification of high school completion to distinguish between traditional high school completion and high school equivalency demonstrates marked differences between education types. High school diploma completion results in significant returns, although it is unclear whether the typical prisoner or releasee has access to traditional high school completion. GED completion is not associated with labor market benefits and may negatively affect releasees in the case of labor force participation. This result may have some implication on programs geared toward GED completion during or after incarceration. As suggested elsewhere, the GED may derive its most apparent benefits from its use as a bridge to higher education. These results point toward a similar story for the narrower case of former prisoners.

2.6 Discussion and Conclusion

In this study, I present empirical evidence of the varying benefits of education on labor market performance after incarceration. Regression and matching methods present results that are largely consistent and suggest a positive link between post-incarceration education and subsequent employment, unemployment, and labor force participation. Mixed evidence is found for a post-incarceration education wage premium. These effects are found largely for college attendance and completion, suggesting that programs that encourage college education during and after an incarceration spell may be most likely to benefit releasees on the labor market. I find that GED completion does not appreciably improve labor outcomes in its own right and may function as a weak signal of employer characteristics to employers, a view consistent with the existing literature. The same literature suggests that for the general population, the GED's benefit is largely derived from its use as a transition to higher education. The completion of a high school diploma is shown to benefit releasees, although because of age eligibility for high school completion, it is unclear whether this is a viable option for the average prisoner or releasee.

Incarceration has become a significant part of the life cycle for an increasing number of Americans since the 1980s, and a wide body of research suggests that incarceration spells negatively affect earnings and employment after release. Many theories of prisoner rehabilitation and reentry into mainstream society link these labor market difficulties to the likelihood of resuming criminal activities after release. The motivation behind prisoner and releasee education is that educational attainment should ease an individual's transition into the mainstream labor market and decrease the likelihood of recidivism. My findings suggest those who shape prisoner reentry programs should strongly encourage and incentivize prisoners and releasees to attain higher education and emphasize the GED as transitional rather than terminal. In

addition, policies that inhibit prisoner access to higher education, such as Pell Grant ineligibility, may be unintentionally hindering rehabilitation of releasees.

This study presents evidence that college education may help former prisoners attain higher levels of employment and earnings; post-incarceration education appears to best serve releasees as a step toward consistent employment and decreased job search time. Educational attainment that does not result in the completion of a diploma or degree, however, does not seem to appreciably improve labor outcomes. This indicates that completion of a course of education may signal to employers that a releasee is progressing toward rehabilitation and reassimilation into mainstream society. My methodology, particularly my use of matching techniques, indicates that selection into post-incarceration education is not largely driving estimated labor market benefits.

My findings also suggest that for formerly incarcerated individuals, the positive link between education, labor supply, and earnings is somewhat weakened. There are several potential explanations for this apparent erosion of returns to education for former prisoners. First, for some releasees, the resumption of crime may be more attractive than seeking and maintaining legal employment, even when education makes employment more attainable and lucrative. At the institutional level, the education available to prisoners and releasees may be inferior compared to mainstream education; evidence of this has been found for the GED, which is currently offered as a part of many reentry programs. A final potential explanation is that for many employers, the negative signal of previous incarceration far outweighs any positive signals conveyed by education. Because criminal history reporting is allowed and required in many industries, employers can easily screen applicants based on criminal activity and incarceration. Post-incarceration education, particularly for the levels of education that releasees are most likely to attain, may therefore have limited benefits to earnings and employment. As many of the prisoners released each year possess below-

average levels of education and training, and a limited number attain education after an incarceration spell, my results suggest reentry programs emphasizing education may widely vary in their effect on labor market performance, and by extension, the likelihood of crime and recidivism.

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TABLES

Table 1. Selected sample means by incarceration status, 2010.

Variable	(a)		(b)		(c)	
	Never incarcerated	Ever incarcerated	Mean	N	Mean	N
Highest grade completed	13.67 [2.64]	7,123	11.92 [1.85]	442	1.7451*** (0.1276)	7,565
Log wages	2.88 [0.72]	5,783	2.34 [0.73]	272	0.5379*** (0.0447)	6,055
Weeks employed	38.92 [20.79]	7,123	25.32 [24.02]	442	13.5965*** (1.0289)	7,565
Weeks unemployed	3.30 [10.91]	7,123	7.02 [15.52]	442	-3.7199*** (0.5506)	7,565
Weeks not in labor force	8.82 [18.31]	7,123	17.11 [23.04]	442	-8.2903*** (0.9129)	7,565
AFQT percentile (1980)	39.97 [0.33]	7,123	18.57 [0.86]	442	21.4017*** (1.3504)	7,565

Notes: * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Variables are drawn from the 2010 wave of the NLSY79 unless otherwise noted. Standard deviations are in brackets. Standard errors are in parentheses.

Table 2. Selected sample means by post-incarceration education, 2010.

Variable	(a)		(b)		(c)	
	Mean	<i>N</i>	Mean	<i>N</i>	Mean difference (a)-(b)	<i>N</i>
Highest grade completed	11.68 [1.49]	253	12.24 [2.21]	189	-0.5596*** (0.1762)	442
Log wages	2.30 [0.77]	159	2.40 [0.66]	113	-0.1047 (0.0895)	272
Weeks employed	25.23 [23.98]	253	25.44 [24.13]	189	-0.2060 (2.3117)	442
Weeks unemployed	7.33 [15.87]	253	6.60 [15.08]	189	0.7302 (1.4937)	442
Weeks not in labor force	17.30 [23.24]	253	16.86 [22.84]	189	0.4433 (2.2178)	442
AFQT percentile (1980)	18.71 [18.55]	253	18.39 [17.65]	189	0.3266 (1.7468)	442

Notes: * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Sample describes all NLSY79 respondents that report an incarceration spell. Variables are drawn from 2010 wave of the NLSY79 unless otherwise noted. Standard deviations are in brackets. Standard errors are in parentheses.

Table 3. Post-incarceration education, OLS wage estimates.

	(a)	(b)	(c)	(d)
	Log wages			
Explanatory variable (post-incarceration)				
Years of education	0.0917*** (0.0331)	0.0876*** (0.0318)	0.0826** (0.0322)	0.0729** (0.0318)
Any education dummy	0.2210*** (0.0805)	0.2180*** (0.0774)	0.2070*** (0.0780)	0.1786** (0.0771)
High school dummy	0.1381 (0.1112)	0.1629 (0.1069)	0.1395 (0.1079)	0.1472 (0.1066)
Some college dummy	0.0592 (0.1125)	0.0048 (0.1082)	-0.0016 (0.1090)	-0.0284 (0.1077)
College degree dummy	0.2027 (0.1726)	0.1185 (0.1662)	0.1241 (0.1658)	0.1005 (0.1650)
	Weeks employed			
Years of education	1.2321 (0.8071)	0.9270 (0.8001)	1.0709 (0.8232)	0.8715 (0.8223)
Any education dummy	1.0771 (3.0250)	0.3242 (2.9950)	0.7269 (3.0592)	0.2339 (3.0289)
High school dummy	4.1286 (4.2348)	3.7432 (4.1967)	4.3816 (4.1249)	4.5991 (4.1340)
Some college dummy	5.5383** (2.8164)	4.4396 (2.7948)	4.7782* (2.8133)	4.4638 (2.8081)
College degree dummy	6.3407 (4.2830)	5.2251 (4.2504)	5.0468 (4.2628)	4.4225 (4.2540)
Controls				
Demographics	X	X	X	X
Respondent/household		X	X	X
Criminal history			X	X
AFQT percentile				X
N	5,981	5,981	5,981	5,981

Notes: * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Robust standard errors are in parentheses. High school completion is defined as earning a high school diploma or GED. Results are weighted.

Table 4. Post-incarceration education, OLS/probit estimates.

Explanatory variable (post-incarceration)	Log wages		Weeks employed	Weeks unemployed	Weeks not in labor force	Recidivism dummy
Years of education	0.0729** (0.0318)	0.8715 (0.8223)	-0.6657 (0.5453)	-0.9881 (1.1106)	0.0075 (0.0294)	
Any education dummy	0.1786** (0.0771)	0.2339 (3.0289)	-1.3023 (1.7289)	-0.6789 (2.8852)	0.0321 (0.0637)	
High school dummy	0.1472 (0.1066)	4.5991 (4.1340)	-0.8517 (2.0756)	-0.2270 (3.9113)	0.0405 (0.0999)	
Some college dummy	-0.0284 (0.1077)	4.4638 (2.8081)	1.9189 (2.7579)	-8.6376*** (3.2620)	0.0992 (0.1093)	
College degree dummy	0.1005 (0.1650)	4.4225 (4.2540)	-3.5317** (1.7202)	4.0743 (7.4723)	0.1696 (0.1364)	
<i>N</i>	5,981	7,476	7,476	7,476	7,476	7,476

Notes: * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. All models include the comprehensive control vector described in the text. Robust standard errors are in parentheses. High school completion is defined as earning a diploma or GED. Results are weighted.

Table 5. Post-incarceration education, incarcerated subsample OLS estimates.

Explanatory variable (post-incarceration)	Log wages			Recidivism dummy	
	Weeks employed	Weeks unemployed	Weeks not in labor force		
Years of education	-0.0172 (0.0446)	1.1829 (1.2063)	-0.2910 (0.5520)	-1.4034 (1.0609)	0.0092 (0.0319)
Any education dummy	-0.0238 (0.1245)	-0.2481 (3.0371)	-0.2238 (1.8466)	-0.9580 (2.7767)	0.0453 (0.0683)
High school dummy	-0.1480 (0.1815)	6.5907 (4.3000)	0.2988 (2.0044)	-2.7058 (4.2478)	0.0361 (0.1073)
Some college dummy	0.0400 (0.1413)	-1.8468 (4.3697)	2.2556 (2.7346)	-5.2198 (3.5858)	0.1186 (0.1236)
College degree dummy	0.4407** (0.2111)	-1.8806 (7.2421)	-5.0954** (2.2227)	9.4136 (7.3064)	0.1875 (0.1410)
<i>N</i>	269	438	438	438	438

Notes: * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. All models include the comprehensive control vector described in the text. Robust standard errors are in parentheses. High school completion is defined as earning a diploma or GED. Results are weighted.

Table 6. Post-incarceration education, PSM estimates (ATT).

Treatment	Log wages						Weeks employed			Weeks unemployed			Weeks NLF			
	Kernel		Strat.		Kernel		Strat.		Kernel		Strat.		Kernel		Strat.	
Any education	0.1020 (0.0082)	0.1010 (0.0710)	0.3750 (2.0069)	0.5780 (2.2180)	-1.2620 (1.4420)	-1.7280 (1.4030)	-0.4310 (1.5700)	0.6580 (1.6580)								
High school	0.0100 (0.1190)	-0.0330 (0.1180)	2.7640 (2.9630)	2.33150 (3.0510)	-1.3190 (2.0890)	-1.3730 (2.4100)	0.5990 (2.1790)	1.2030 (2.1820)								
Some college	0.1920** (0.0960)	0.2440*** (0.0870)	5.3970*** (2.3420)	7.2840*** (2.6280)	-2.5310 (1.9820)	-2.8950 (2.0100)	-4.142*** (1.1580)	-4.3830*** (1.2120)								
College degree	0.4000*** (0.1280)	0.3620*** (0.1000)	8.3620** (4.1990)	9.0040** (3.6970)	-5.7660*** (2.1980)	-6.9370** (2.9730)	-0.1850 (3.4220)	0.3870 (3.3690)								

Notes: * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The propensity score is estimated by a probit ($N = 7,565$) and matched by kernel and stratification algorithms. Bootstrapped standard errors (100 replications) are in parentheses. High school completion is defined as earning a diploma or GED. First stage results are weighted.

Table 7. High school completion split, OLS/probit estimates.

Explanatory variable (post-incarceration)	Log wages		Weeks employed		Weeks unemployed		Weeks not in labor force		Recidivism dummy	
GED dummy	-0.0875 (0.2869)	-1.1588 (4.6890)	0.8595 (2.6377)	3.7270 (4.6305)	-0.0015 (0.1169)					
HS Diploma dummy	0.0156 (0.1970)	18.3342*** (6.2203)	-7.0575*** (1.3688)	-8.0798 (5.8792)	0.0127 (0.1739)					
<i>N</i>	5,981	7,476	7,476	7,476	7,476					

Notes: * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. All models include the comprehensive control vector described in the text. Robust standard errors are in parentheses. Results are weighted.

Table 8. High school education split, PSM estimates.

Treatment	Log wages		Weeks employed		Weeks unemployed		Weeks NLF	
	Kernel	Strat.	Kernel	Strat.	Kernel	Strat.	Kernel	Strat.
GED	-0.0180 (0.1180)	-0.0030 (0.1150)	-0.1180 (3.2750)	-0.6070 (3.1230)	-0.9080 (2.7099)	-0.9570 (1.9830)	1.4600 (2.4830)	1.1260 (2.3370)
Diploma	0.1510 (0.1150)	0.0920 (0.1180)	8.1500*** (2.5580)	9.2660*** (3.2620)	-3.5540** (1.5960)	-4.3010** (1.6000)	-2.5600 (1.9960)	-2.3220 (2.2440)

Notes: * Significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. The propensity score is estimated by a probit ($N = 7,565$) and matched by kernel and stratification algorithms. Bootstrapped standard errors (100 replications) are in parentheses. First stage results are weighted.

CHAPTER III

Incarceration and Earnings: Clarification from Quantile Regression

3.1 Introduction

A substantial increase in U.S. incarceration since the 1980s has brought with it an increased interest in its determinants, benefits, and costs. While the explicit costs of maintaining an increased prison population¹ are relatively easy to calculate, determining the implicit costs of incarceration to society is less straightforward. Researchers across the social sciences have attempted to characterize the total effect of the incarceration boom on a variety of quantifiable outcomes, including the likelihood of recidivism (Holzer, 2009; Langan & Levin, 2002), family structure and offspring (Geller et al., 2009; Geller et al., 2012; Wakefield & Uggen, 2010), and labor market measures such as earnings and employment (Apel & Sweeten, 2010; Kling, 2006; Pettit & Lyons, 2009; Raphael, 2007; Waldfogel, 1994; Western, 2002). As many administrative and survey data sets collect individual-level earnings data, have some longitudinal structure, and are large enough that a substantial fraction of respondents become incarcerated, inquiries into the incarceration-induced wage penalty have been popular and useful in explaining a portion of the implicit costs of incarceration.

Descriptive analysis almost unanimously suggests that the average prisoner is not the average American. Examination of a variety of data sets shows that individuals who experience incarceration are typically disadvantaged in areas identified as determinants of labor market success: they score lower on standardized tests, are less

¹Such costs may include housing, feeding, and providing medical care for prisoners as well as personnel and facility upkeep.

educated, are less likely to be employed, and earn less than those who are incarcerated (Bound & Freeman, 1992; Holzer, 2009; Raphael, 2010; Waldfogel, 1994). Incarceration has become a major component of the life cycle of a growing number of low-skill, minority youth who are often only marginally attached to the mainstream labor market (Western et al., 2001). Compounding this gap in characteristics is a growing body of research that largely suggests a causal link between incarceration and subsequent labor market difficulties, even when observable and unobservable wage- and employment-depressing characteristics are properly controlled (Apel & Sweeten, 2010; Geller et al., 2006; Grogger, 1992; Sabol, 2007; Western, 2002).

Because evidence of an incarceration-induced wage penalty could have potential implications on criminal justice policy, it is important to model incarceration's effect on earnings as realistically as possible. The current literature relies primarily on least squares techniques, which estimate the conditional mean of an outcome variable. These equations typically model the log of wages or earnings as a function of previous incarceration and other observables, with the incarceration coefficient interpreted as the difference in the average wage between those who report a previous incarceration and those who do not. While such models shed general insight onto the link between incarceration and earnings, they may be limited in their applicability. First, if incarceration is correlated with unobserved characteristics, least squares estimates of the incarceration coefficient will be biased. This is more likely if there is systematic variation between the treatment group (those who incarcerated) and the control group (those who are not). Second, the focus of least squares techniques on the mean wage may limit their relevance to the typical releasee earning a below-average wage. I address the first issue by exploiting panel data to estimate individual-level fixed effects and utilizing a working panel of individuals who are incarcerated or at risk of incarceration. The second issue can be addressed with quantile regression techniques. Examining incarceration's effects across the wage distribution, with par-

ticular emphasis on the low end, could help clarify the magnitude of the incarceration wage penalty at wage levels that the population of interest are more likely to receive. Controlling for unobserved, time-invariant heterogeneity within the same models may reduce potential endogeneity and increase their veracity.

The relationship between incarceration and earnings may potentially be clarified by quantile regression techniques. That former prisoners are likely to earn below-average wages is hardly a controversial statement, regardless of whether this is causal or due to correlation with unobserved characteristics. Simply modeling the conditional mean wage as a response to incarceration may therefore not represent the question at hand as accurately as possible. In addition, there may be further reason to believe that in addition to adding flexibility to the incarceration-wage relationship, quantile regression may uncover information about the particular labor markets available to releasees. The market for releasee labor is likely to differ from the broader labor market for several reasons. For example, studies by Caspi et al. (1998) and Sullivan (1989) present evidence that former prisoners sort into secondary or spot markets, in part due to legal prohibitions against hiring convicts. It is possible that a low-pay market with low earnings growth potential may emerge to employ individuals with lower human capital and lower likelihood of employment stability (Nagin & Waldfogel, 1998; Western & Beckett, 1999). In such a casual or transient labor market, incarceration may have less stigma than it would on the broader market. Low-skill, low-pay employment may also require lower levels of general and firm-specific human capital; this suggests that incarceration's depreciating effect on human capital may be reduced or unapparent for low earners. Quantile regression techniques are particularly suited to examine the hypothesis that incarceration is less detrimental to earners of lower wages.

In this study, I attempt to clarify the link between incarceration and wages by using quantile regression techniques and data drawn from the National Longitudinal

Survey of Youth 1979. Quantile regression models of wages allow me to trace the effect of incarceration covariates across the entire conditional distribution, paying particular attention to incarceration's effect on lower quantiles of the wage distribution. Additional models of wages and labor supply examine the effects of incarceration in the periods after an incarceration spell. The rich nature of the National Longitudinal Survey allows me to control for a wide variety of observable characteristics, and an individual-specific fixed effect quantile regression model allows me to control for time-invariant unobserved heterogeneity. I find that while incarceration has an appreciably negative effect at most key points of the wage distribution, wage penalties for lower quantiles are significantly smaller, indicating that mean regression techniques overstate the incarceration wage penalty. These results suggest that for the lower-paying, lower-skill jobs that former prisoners are most likely to have, an incarceration spell has a less severe effect on earnings than has been previously estimated, and these penalties diminish in the years after release. Additional models of wage growth over the employee life cycle suggest that former prisoners are less likely to have access to employment with age-graded pay after incarceration. In total, this study clarifies the established empirical link between incarceration and earnings by providing a more comprehensive analysis of the effect of incarceration on low-skill, low-paying employment.

3.2 Previous Findings

The majority of empirical studies link incarceration spells to negative labor market outcomes, including decreased employment and earnings (Holzer, 2009; Western et al., 2001). Most studies interpret these wage penalties as partially due to prior statistical differences between prisoners and the general population and partially induced by incarceration. Some of the hypothesized reasons for the latter effect include the stigma associated with hiring a convict (Holzer, 1999; Western et al., 2001), depreci-

ation of human capital (Raphael, 2010; Waldfogel, 1994), disruption of employment (Laub & Sampson, 1993; Lyons & Pettit, 2011; Sampson & Laub, 1997), prohibition from certain jobs and industries (Nagin & Waldfogel, 1998; Western, 2000), and deterioration of mainstream social networks or familial ties (Holzer, 2009; Holzer et al., 2004).

Table 1 presents a chronological summary of literature relevant to the incarceration wage penalty. Across a variety of data sources and methodologies, this body of research almost uniformly points toward a significant wage penalty that is reasonably attributed to incarceration. Studies using administrative data linking prison and earnings records across several states² find large earnings or wage penalties ranging from 5-30% (Grogger, 1995; Kling, 1999; Kling, 2006; Lyons & Pettit, 2011; Needels, 1996; Pettit & Lyons, 2007; Pettit & Lyons, 2009; Waldfogel, 1994). The most apparent limitation of the administrative incarceration data is the nature of the panels. The data used in Waldfogel (1994) observes prisoners only twice: during and immediately after a sentence is served. Grogger (1995) and Kling (2006) also have a limited range of pre- and post-incarceration survey waves. As such, it is more difficult to track the long-term effects of an incarceration spell on earnings. In addition, if incarceration's effect of earnings diminishes over a longer period of time, studies with limited longitudinal data may not fully characterize the long-run incarceration-earnings profile. Studies using longitudinal survey data including the Fragile Families and Child Wellbeing Survey and the National Longitudinal Survey of Youth estimate wage penalties between seven and 26% (Geller et al., 2006; Raphael, 2007; Western, 2002). The Fragile Family panel used in Geller et al. (2006) is by construction a sample of families more likely to have contact with the criminal justice system, but its structure (a baseline survey year with one year and three year follow-up waves)

²California (Grogger, 1995; Kling, 1999; Kling, 2006), Florida (Kling, 2006), Georgia (Needels, 1996), and Washington (Lyons & Pettit, 2011; Pettit & Lyons, 2007; Pettit & Lyons, 2009). Waldfogel (1994) draws on records from the Administrative Office of the U.S. Courts (AO).

limits its examination of incarceration and earnings over an extended period of time, similar to the studies using administrative data. Both the NLSY79 and the Fragile Family panels offer a broader range of demographic, household, and criminal controls than the administrative studies. Across data sets, however, most of these studies rely on ordinary least squares (OLS) techniques, although many exploit panel data to estimate individual-level fixed effects models. Other techniques used include random effects models (Waldfoegel, 1994) and propensity score matching (Geller et al., 2006), which do not address selection on unobservables. Studies using survey data with multiple pre- and post-incarceration waves have the added advantage of estimating the effect of historical incarceration spells while controlling for current incarceration status, as well as exploiting panel techniques, strategies I will utilize in my empirical analysis.

Quantile regression (QR) techniques have been applied to a variety of empirical topics and are best suited for situations in which the effect of a covariate on an outcome may vary across the distribution of the outcome (Buchinsky, 1998b; Koenker & Hallock, 2001). Quantile regression models have been used to examine topics such as infant birth weight and height (Wei et al., 2005), standardized tests scores (Eide & Showalter, 1998), and earnings growth (Buchinsky, 1998a). Quantile wage regressions have also been used to fully detail the returns to standard wage determinants such as education and experience for low-, median-, and high-wage jobs (Buchinsky, 1994; Buchinsky, 1998b). Because the individuals of interest and their labor market performance are non-representative of the average American experience, quantile techniques lend themselves naturally to such an examination. In addition, recent developments in the application of QR to longitudinal data by Koenker (2004) allows my study to better characterize the incarceration-earnings relationship by correcting for unobserved, time-invariant heterogeneity.

3.3 Data and Methodology

3.3.1 National Longitudinal Survey of Youth 1979

This study draws data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79). The NLSY79 is an ongoing survey of Americans aged 14 to 21 upon their initial interview in 1979 (Bureau of Labor Statistics [BLS], 2009). The initial survey interviewed a cohort of 12,868 individuals on a wide variety of demographic and behavioral topics, including education, fertility, and labor market performance. Follow-up interviews were conducted annually until 1994 and biennially thereafter. The 2008 wave contains 7,734 members of the original cohort. Because of the panel nature of the survey, the NLSY79 offers a rare opportunity to follow a sample of individuals throughout adulthood, during which many report entering and leaving a state of incarceration (BLS, 2011). This allows me to compare individuals who are observably similar apart from incarceration status.

I construct a panel drawn from the NLSY79 from 1983 to 2008.³ The outcome variable of interest is the natural logarithm of an individual's reported hourly wages in the previous year at his or her primary job adjusted to 2007 dollars. Residence at the time of survey is recorded for all respondents in all survey years; respondents can indicate their residence in a jail or prison. Figure 1 illustrates the frequency of consecutive reported periods of incarceration in the NLSY. Nearly half of incarcerated respondents do not report prison residency in multiple consecutive survey waves, and about 20% of respondents are surveyed in prison in two consecutive survey years. The remaining portion of incarcerated respondents report incarceration in multiple consecutive survey waves, with an upper bound of a single respondent reporting prison residency in 15 consecutive survey waves. Although the survey does not observe any brief incarceration spells between surveys (or, after 1994, during non-survey years),

³Most respondents are 18 by 1983. The 2008 wave is used to capture 2007 wage data with the intention of circumventing any effects of the subsequent recession.

it allows for the combination of incarceration data with a rich set of pre- and post-incarceration variables. In addition, the longitudinal incarceration data allow me to distinguish between prior and current incarceration and control for the latter. This prevents estimates of incarceration's effect on wages being overstated by respondents who are currently incarcerated and report low or no wages in that year. This working sample consists of 7,885 individuals surveyed over 19 survey waves, 580 of which report residence in a jail or prison at least once.

Because of the aforementioned plausible differences in respondents across incarceration status, I construct a subsample of respondents that includes only individuals with a history of incarceration and respondents who do not report incarceration but are identified as at risk of incarceration. The control group of this subsample serves as a more accurate comparison population than the full sample (Ramakers et al., 2012; Western, 2002). The at-risk subsample is comprised of respondents who either (a) report incarceration in any survey wave, (b) report contact with the criminal justice system that did not culminate in sentencing and incarceration (including being stopped by police, charged with a crime, or going to court), or (c) report committing severe crime as a youth or young adult. The at-risk subsample consists of 2,327 individuals and contains the same 580 respondents who report jail or prison residence. Table 2 presents selected means for the full and at-risk working panels and are split by incarceration history in the second and third columns. A similar descriptive story emerges in both panels: respondents who report any history of incarceration also report lower levels of earnings, education, job tenure, and cognition, as proxied by performance on the Armed Forces Qualification Test (AFQT). The incarceration group is disproportionately male and black, mirroring what is known about U.S. incarceration trends. The only notable differences in means between the full and at-risk comparison groups are that of AFQT score and gender; at-risk individuals who never report incarceration are more likely to be male and score slightly lower on the AFQT

than their never-incarcerated counterparts from the full sample. Although there are few apparent differences between each working sample, I retain both for my empirical analysis. As an additional descriptive look at the incarcerated subsample, Figure 2 plots the unadjusted reported wage for individuals that have been or will be incarcerated in the periods leading up to and following an incarceration spell. Reported wages appear to fall in the periods leading up to incarceration, reaching a low point immediately after release and steadily approaching pre-incarceration levels over the next six survey periods.

3.3.2 Empirical Methodology

I specify a model of wages

$$\ln(w_{it}) = \alpha_0 + \beta_1 \text{Prior}_{it} + \beta_2 \text{Current}_{it} + x'_{it}\beta + \varepsilon_{it}, \quad (1)$$

where the outcome is the natural logarithm of individual i 's reported wage in time t , Prior_{it} is the key dummy variable that equals one when an individual i reports incarceration in period $t - 1$ or before, and Current_{it} is a dummy that equals one if an individual reports incarceration in period t . I subsequently respecify (1) in the form

$$\ln(w_{it}) = \alpha_0 + \sum_{j=0}^6 \beta_j \text{Incarceration}_{it-j} + \sum_{k=1}^3 \delta_k \text{Incarceration}_{it+k} + x'_{it}\beta + \varepsilon_{it}, \quad (2)$$

which includes dummy vectors $\sum_{j=0}^6 \text{Incarceration}_{it-j}$ and $\sum_{k=1}^3 \text{Incarceration}_{it+k}$ indicating whether an individual was incarcerated in j periods before or k periods after t . This allows the construction of a wage trajectory prior to and after an incarceration spell.⁴ In both equations, the control vector x'_{it} contains sex, race (black, Hispanic, non-white/non-Hispanic dummies), age, its square, years of education, mar-

⁴This method is adapted from Fernández-Kranz et al. (2013), who examine the wages of Spanish mothers prior to and after birth.

ital status, number of children, number of household members, region of residence (northeast, north central, south, west dummies), urban and SMSA residence, number of years at primary job, and AFQT percentile. Additional criminal activity controls include the number of reported survey waves in jail or prison, age at earliest reported incarceration, the reported number of times an individual has been stopped by police, charged with a crime, or been sent to a juvenile correctional facility. Additional criminal activity dummies include (a) the use or sale of marijuana/hashish or other hard drugs and/or chemicals, (b) shoplifting, property theft, automobile theft, or knowingly selling stolen goods, and (c) destruction of property, using force to obtain property, threatening to attack someone, or attacking someone with the intent to injure or kill.⁵ Models are estimated over the at-risk subsample and the full working sample.

In (1) and (2), coefficients β_1 and β_j represent the wage penalty associated with prior incarceration, controlling for x'_{it} and current incarceration status. If incarceration is correlated with unobserved characteristics, however, ordinary least squares (OLS) estimates of β_1 and β_j will be biased. I address potential endogeneity in several ways. First, I exploit the panel nature of the data to estimate individual-level fixed effects (FE) for (1) and (2) to control for time-invariant, unobserved heterogeneity. Second, the coefficient vector δ_k in (2) functions as a form of falsification test: as earnings are unlikely to be affected by future incarceration, significant estimates of these lead dummies may be evidence of endogeneity. I present estimates of (2) with and without incarceration leads. Finally, my use of a subsample containing only individuals who are incarcerated at some point and individuals at risk for incarceration may minimize observed and unobserved differences between treatment and control groups.

⁵These criminal activity variables are not included for models estimated for the at-risk panel due to collinearity.

My methodology turns next to quantile regression techniques. As first proposed by Koenker and Bassett (1978), the quantile regression (QR) specification takes the form of (1) for quantiles of interest such that

$$\ln(w_{it}) = \alpha_{\tau 0} + \beta_{1\tau} \text{Prior}_{it} + \beta_{2\tau} \text{Current}_{it} + x'_{it} \beta_{\tau} + \varepsilon_{\tau it},$$

with

$$\text{Quantile}_{\tau}(\ln(w_{it})|x_{it}) = \alpha_{\tau 0} + \beta_{1\tau} \text{Prior}_{it} + \beta_{2\tau} \text{Current}_{it} + x'_{it} \beta_{\tau} \quad (3)$$

denoting the τ th conditional wage quantile of interest. In this study, I examine key quantiles such that $\tau = \{0.05, 0.25, 0.50, 0.75, 0.95\}$, corresponding to the 5th, 25th, median, 75th, and 95th wage quantiles. Specifications of (3) and are estimated simultaneously for all quantiles of interest, and bootstrapped standard errors are calculated (Koenker & Hallock, 2001; Hao & Naiman, 2007). An individual-level fixed effects specification of (3) is also estimated using the fixed effects quantile regression (FEQR) method proposed by Koenker (2004).

3.4 Results

Table 3 presents mean and quantile regression estimates of (1) and (3) for both the at-risk and full samples. Column one reports OLS estimates of prior incarceration coefficients and suggests that conditional on current incarceration status, historical incarceration is associated with a wage penalty of 10-12%, approximately the same size as the wage premium for an additional year of education. The estimated wage penalty fits within the range of estimates found in the current literature, although it is slightly smaller than most. This could be in part due to the inclusion of AFQT as a control for cognition.

Columns two through six report quantile regression estimates for the 5th, 25th, median, 75th, and 95th quantiles. The first notable result is that the previous mean

wage penalty seems to overstate prior incarceration’s effect on most of the quantiles of interest. For the at-risk sample, prior incarceration’s effect on wages increases across the wage distribution. The fifth quantile of wages is not significantly different for individuals who report incarceration. However, a wage penalty of 7% is found for the 25th quantile and a wage penalty of 8% is found for the median and 75th quantile. Notably, the 95th quantile wage penalty is 15%, which OLS underpredicts. In total, tracing the coefficient across the conditional wage distribution suggests that incarceration’s effect on an individual’s earnings increases with earnings. Results from the full working sample follow a similar although more pronounced path and suggest lower wage penalties of 8% for the 5th and 25th quantiles that increase to 10% for the median and 75th quantile. As in the at-risk sample, the 95th quantile wage penalty is the largest in magnitude (18%), nearly the same size as the return to two years of education. As the full sample control group contains predominantly non-criminal individuals, however, the results from the at-risk subsample are more likely to represent the true effect of incarceration.

Table 4 presents estimates from individual-level fixed effects models of mean specification (1) and the quantile specification (3). When unobserved heterogeneity is controlled, estimated incarceration penalties generally appear larger in magnitude. Estimated coefficients in column one associate an incarceration history with a wage penalty of about 30-34% across both the full and the at-risk panels. The fixed effect quantile regression estimates suggest that the effect of incarceration is overstated in the mean regression estimated for all of the conditional quantiles of interest. As in the pooled models, the incarceration penalty generally increases across the wage distribution, having its smallest effect (15%) at the 5th quantile and having its largest effect (22%) at the 95th quantile. The median regression estimate of about 15% is similar in magnitude to the 5th quantile estimate, but it is significantly different from the standard fixed effect specification. The full sample results present similar,

although larger, effects: a wage penalty of 18-20% is estimated for all conditional quantiles of interest with the exception of a 25% penalty at the 95th quantile.

Tables 5 and 6 present estimated incarceration-wage trajectories in the form of (2) with and without controls for lead periods into incarceration. It is notable that incarceration leads (periods $k = \{1, 2, 3\}$) are statistically insignificant in all specifications and subsamples. This suggests that results are not biased in such a way that incarceration has a statistically significant effect on wages prior to incarceration. Table 5 presents OLS estimates of (2) and suggests that immediately after an incarceration spell, there is an observed wage penalty of about 30% that decreases to 8-10% in the next period and largely dissipates in subsequent periods. Table 6 presents analogous fixed effect estimates of (2). As in the previous results, fixed effect point estimates are larger in magnitude than corresponding pooled OLS estimates, but point toward the same general wage trajectory: an immediate, severe decrease in earnings followed by gradual recovery in subsequent periods.⁶ Figure 1 presents the results reported in Tables 5 and 6 graphically, along with corresponding 95% confidence intervals. Each plot clearly illustrates the initial observed dip in wages after release and its gradual recovery.

3.5 Respecifications and Related Outcomes

3.5.1 Wage Growth and Incarceration

In addition to providing detailed labor market information prior to and after incarceration, the panel nature of the data allows for the examination of wage growth over time. Research suggests that job market failure as a young adult is predictive of poor performance throughout working life, and that absence from employment can stunt long-term wage growth (Caspi et al., 1998; Ramakers et al., 2012). Labor dis-

⁶Taking this into consideration, I reestimate (1) and (3) with prior incarceration restricted to include only spells in the previous six survey waves. Estimates are not presented here, but are largely unchanged.

continuity makes firm-specific human capital investment less likely and may decrease the likelihood that an individual enters a “career” occupation with lifetime earnings growth potential (Nagin & Waldfogel, 1998; Western, 2000; Western et al., 2001). Incarceration during young adulthood has been shown to negatively affect long-term labor performance (Laub & Sampson, 1993). Discontinuity in employment due to incarceration may therefore inhibit earnings growth over time. Following a specification of wage growth by Western (2002), I allow the returns to age to vary by incarceration history by including an interaction of the key prior incarceration variable with the log of age in specifications of (1) and (3). In addition, I replace age and its square in the control vector with log age. This allows the age effect to be nonlinear without requiring two incarceration interaction terms.

Table 7 reports the estimated incarceration wage penalty and the returns to age across incarceration status. This coefficient will be negative if former prisoners cannot find access to employment with potential for earnings growth over time. The OLS estimate indicates that age is not associated with increased wages for the formerly incarcerated. Across quantile estimates, the age penalty for incarcerated individuals is most severe at the extreme (5th and 95th) quantiles. However, when each interaction effect coefficient is combined with its corresponding main effect age effect coefficient, a clear trend emerges, with lower wage quantiles (5th and 25th) being associated with negative total effects, and the median, 75th, and 95th quantile estimates being associated with a net positive (albeit reduced) age effect. Results from the full sample are larger in magnitude than those from the at-risk subsample but are consistent in sign and pattern across quantiles. These results suggest that incarceration history is associated with a lack of access to age-graded employment, and that incarceration has an extreme stunting effect on course wage trajectory, especially for low-skill, low-wage jobs.

Table 8 presents corresponding fixed effects results for (1) and (3). When time-invariant heterogeneity is controlled, the estimated coefficients of interest are similar in size and shape across the wage distribution, although the total effects are slightly larger in absolute terms than the pooled regression estimates. In total, these models suggest that the return to age increases across wage quantiles. This is consistent with scenarios in which the highest-paying jobs are those with age-graded payscales. Lower quantiles of earnings are associated with diminished value placed on age or employment duration. For the lower wage quantiles, the age wage penalty is more severe than estimated in analogous mean regression models. These results show that the potential for earnings growth over their employment life cycle may be extremely diminished for former prisoners in low-pay jobs. In the context of this study, a history of incarceration appears to have a punctuating, negative effect on lifetime earnings potential.

3.5.2 Labor Supply and Incarceration

My analysis turns next to incarceration's effect on labor supply, particularly employment and labor force participation. It is likely that an incarceration spell may decrease time spent employment and increase time spent out of the labor force for the same reasons incarceration appears to decrease wages. As models of incarceration and earnings are only estimated for those individuals who report wages from employment, they may not capture the full extent of incarceration's effects. To examine this, I model annual numbers of weeks employed and annual numbers of weeks not in the labor force in the form of (2), omitting the job tenure variable from the control vector. Models are estimated with and without the lead vector $\sum_{k=1}^3 Incarceration_{it+k}$ for both the at-risk and full samples and are also presented in graphical form.

Tables 9 and 10 present incarceration-employment trajectories in the form of (2). These specifications provide additional evidence of the negative effects of an incarceration spell, and indicate that across pooled OLS and fixed effects specifications, in the period immediately following release, the formerly incarcerated spend about 14-15 fewer weeks employed annually. Incarceration's effect diminishes over the next two observed periods for both OLS and fixed effect models and is largely insignificant in the subsequent survey periods. In contrast with analogous wage models, however, the coefficients for the incarceration lead vector are significant, but small in magnitude. This could be attributed to in part to endogeneity or to possible shifting from legal to illegal forms of employment in the survey periods leading up to incarceration. Figure 4 displays the contents of Tables 9 and 10 graphically and illustrates the substantial shock to employment upon release and its gradual recovery over six survey waves.

Tables 11 and 12 present estimates of (2) for the annual number of weeks spent out of the labor force. As the formerly incarcerated are often only marginally attached to the market for labor, any of the aforementioned wage- and employment-depressing phenomena may also cause an individual to reduce or forgo the search for a job and unemployment. Across both pooled and fixed effects specifications, incarceration spells appear to increase annual time spent out of the labor force by about 15-16 weeks in the period immediately following release. As in employment models, these effects are statistically significant but diminish in subsequent survey waves. Across all models and samples, incarceration does not appear to significantly affect labor force participation four survey periods after release. Again, however, the leading coefficients are significant, albeit small in magnitude, suggesting potential bias in specifications of (2). Figure 5 illustrates the point estimates summarized in Tables 11 and 12 and clearly shows the large initial shock and eventual recovery. In total, models of employment and labor force participation provide a largely symmetrical perspective

on incarceration's effect on labor supply. The negative shock to employment and labor force participation is similar in magnitude and both outcomes exhibit a similar trajectory in the periods after release. These findings suggest that in addition to potential wage penalties, the former prisoner may face a large initial employment penalty. For marginally skilled releasees, this could make the resumption of criminal activity more appealing in the short run.

3.6 Discussion and Conclusion

This study presents a clarification of the relationship between an individual's incarceration and subsequent earnings. The results of several quantile regression models suggest that incarceration's effect on future earnings may be less severe for many individuals than mean regression techniques suggest. I find that incarceration wage penalties are less severe at the below-median earnings typically associated with low-skill or secondary market employment, but are in some cases still approximately equal in magnitude to wage premiums for an additional one to two years of education. In addition, wage depression associated with incarceration appears to diminish in the subsequent periods following release. I also find additional evidence that former prisoners in lower wage quantiles are increasingly unable to find employment with age-graded pay, and that incarceration has a large initial effect on employment and labor force participation. Incarceration appears to have a severe stunting effect on lifetime earnings in addition to its effect immediately after release. These patterns in incarceration and earnings are still apparent when unobserved heterogeneity in controlled in mean and quantile regressions.

These results shed new light on what is understood about the labor market consequences of incarceration and appear to confirm that many former prisoners are confined to forms of employment that have low potential for earnings growth and

career development over time. I present initial evidence in this study, however, that the incarceration wage penalty is lower than previously estimated and that it diminishes over time. This could imply that many former prisoners are only marginally attached to the labor market and may participate in transitory or temporary forms of employment. Such types of employment may be less associated with several of the hypothesized mechanisms of incarceration wage penalties; low-earning jobs may be less sensitive to the depreciation of human or social capital, or they may place less stigma on convict status. This study points toward further avenues of research utilizing quantile regression with administrative prison data, which offers advantages in sample construction and crime/sentence-specific controls. As this data type is predominant in the current literature and results in the highest estimated wage penalties, such an analysis would be a simple yet worthwhile investigation.

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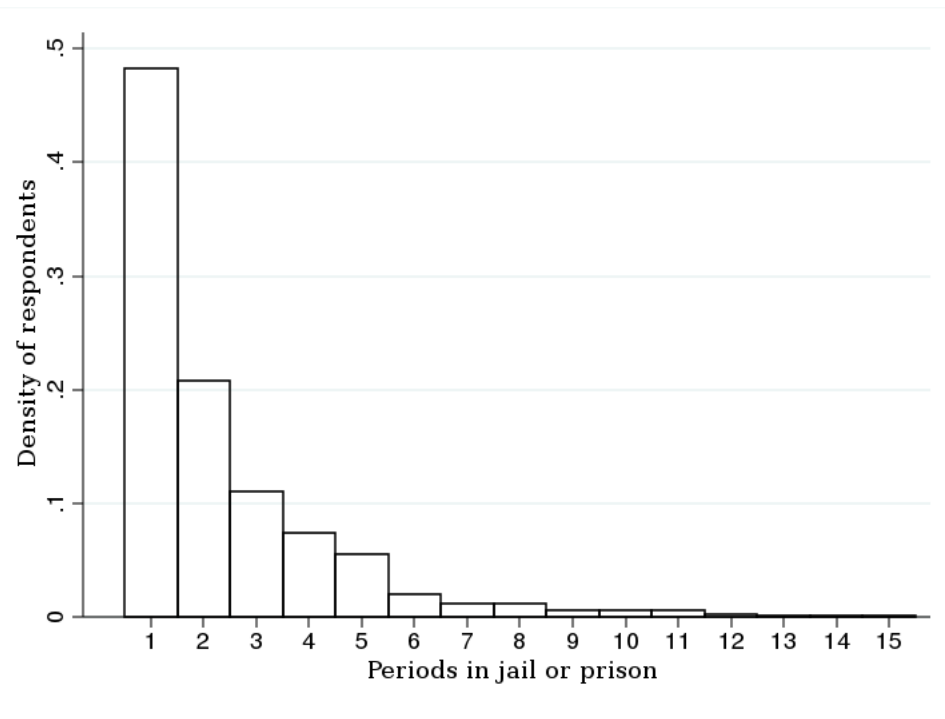
TABLES AND FIGURES

Table 1. Previous wage penalty estimates.

Study	Key variable(s)	Earnings penalty	Data	Method
Waldfogel, 1994	Prior conviction	16-28% real income	Administrative (AO)	FE/RE
Grogger, 1995	Prior incarceration	11-30% quarterly	Admin. (CA)	FE
Needels, 1996	Prison releasee status	23-29% annual	Admin. (GA)	2-step
Kling, 1999	Prior incar., length	10-30% quarterly	Admin. (CA)	-
Western, 2002	Current, prior incar.	7-24% hourly wages	NLSY79	FE
Geller et al., 2006	Prior incarceration	14-26% hourly wages	Fragile Families Survey	PSM
Kling, 2006	Incarceration length	0% quarterly	Admin. (CA/FL)	2SLS
Pettit & Lyons, 2007	Prior, quarters incar.	3-6% hourly wages	Admin. (WA)	FE
Raphael, 2007	Current, prior incar.	0-22% hourly wages	NLSY79	FE
Pettit & Lyons, 2009	Prior, quarters incar.	5-7% hourly wages	Admin. (WA)	FE
Lyons & Pettit, 2011	Prior incarceration	21% wage growth	Admin. (WA)	FE

Notes: The final column reports methodology used in addition to ordinary least squares: fixed effects, random effects (RE), Heckman correction (2-step), propensity score matching (PSM), and two-stage least squares (2SLS).

Figure 1. Distribution of NLSY incarceration lengths.



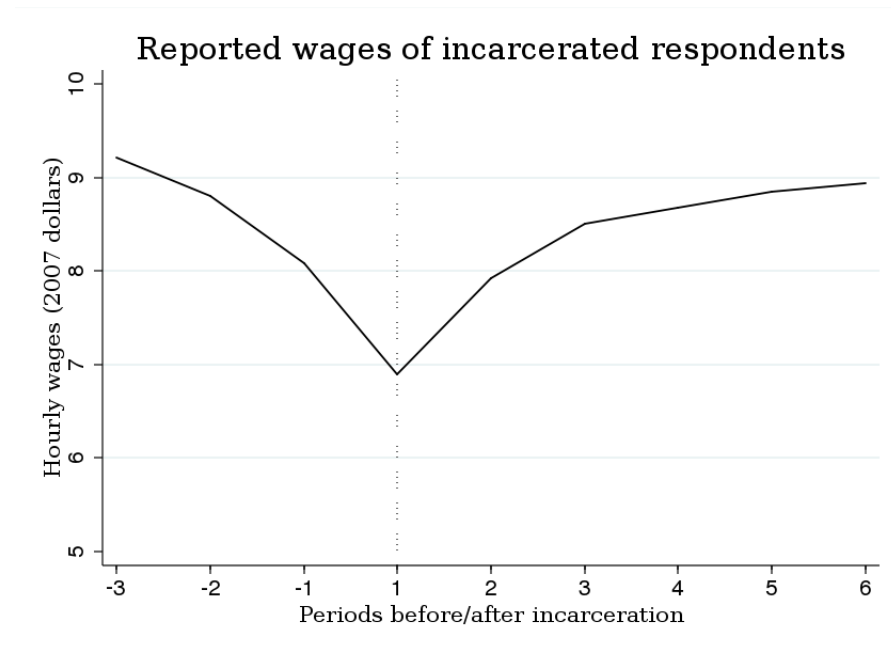
Notes: Incarceration lengths represent consecutive survey waves in which the respondent reports jail or prison residency.

Table 2. Selected sample means.

Variable	Total		Never incar.		Incarcerated	
<i>At-risk subsample</i>						
Log wage	2.57	[0.69]	2.61	[0.66]	2.26	[0.83]
Years of education	12.06	[2.33]	12.19	[2.36]	11.07	[1.84]
Job tenure (years)	1.56	[1.67]	1.67	[1.72]	0.78	[0.81]
AFQT percentile	35.35	[26.71]	37.49	[26.93]	18.97	[17.87]
Male dummy	0.79	[0.41]	0.77	[0.42]	0.93	[0.26]
Black dummy	0.27	[0.45]	0.24	[0.43]	0.53	[0.50]
White dummy	0.54	[0.50]	0.57	[0.49]	0.27	[0.44]
Age	31.31	[7.60]	30.91	[7.53]	34.36	[7.41]
$N_{At-risk}$	35,653		31,526		4,127	
<i>Full sample</i>						
Log wage	2.59	[0.70]	2.60	[0.69]	2.26	[0.83]
Years of education	12.89	[2.51]	12.95	[2.50]	11.07	[1.84]
Job tenure (years)	1.82	[1.83]	1.86	[1.84]	0.78	[0.81]
AFQT percentile	41.17	[27.98]	41.82	[27.96]	18.97	[17.87]
Male dummy	0.52	[0.50]	0.51	[0.50]	0.93	[0.26]
Black dummy	0.27	[0.44]	0.26	[0.44]	0.53	[0.50]
White dummy	0.56	[0.50]	0.57	[0.50]	0.27	[0.44]
Age	31.48	[7.64]	31.40	[7.63]	34.36	[7.41]
N_{Full}	146,453		142,326		4,127	

Notes: Standard deviations are in brackets.

Figure 2. Reported pre- and post-incarceration wages.



Notes: Hourly wages represent unadjusted reported wages prior to and after a period of jail or prison residency.

Table 3. Incarceration wage penalties, OLS/QR estimates.

Key variables	OLS (mean)	5th	25th	50th	75th	95th
	<i>At-risk subsample (N = 35, 653)</i>					
Prior	-0.0969*** (0.0142)	-0.0376 (0.0337)	-0.0735*** (0.0120)	-0.0798*** (0.0119)	-0.0820*** (0.0134)	-0.1373*** (0.0340)
	<i>Full sample (N = 146, 453)</i>					
Prior	-0.1149*** (0.0140)	-0.0487 (0.0383)	-0.0762*** (0.0113)	-0.0992*** (0.0119)	-0.1109*** (0.0123)	-0.1686*** (0.0284)

Notes: * Significant at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses and are bootstrapped for quantile regression estimates (100 replications).

Table 4. Incarceration wage penalties, FE/FEQR estimates.

Key variables	FE (mean)	5th	25th	50th	75th	95th
	<i>At-risk subsample (N = 35, 653)</i>					
Prior	-0.2864*** (0.0180)	-0.1391*** (0.0480)	-0.1578*** (0.0194)	-0.1554*** (0.0175)	-0.1579*** (0.0187)	-0.2127*** (0.0459)
	<i>Full sample (N = 146, 453)</i>					
Prior	-0.3268*** (0.0171)	-0.1600*** (0.0535)	-0.1902*** (0.0207)	-0.1833*** (0.0163)	-0.1833*** (0.0163)	-0.2484*** (0.0445)

Notes: * Significant at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses and are bootstrapped for quantile regression estimates (100 replications).

Table 5. Incarceration-wage trajectories (2), pooled OLS estimates.

	$k = 3$	$k = 2$	$k = 1$	$j = 1$	$j = 2$	$j = 3$	$j = 4$	$j = 5$	$j = 6$
<i>At-risk subsample (N = 31, 102)</i>									
Incarceration				-0.3062*** (0.0250)	-0.0811*** (0.0240)	-0.0272 (0.0241)	-0.0330 (0.0247)	-0.0365 (0.0255)	-0.0458* (0.0258)
Incarceration	0.0219 (0.0237)	-0.0334 (0.0238)	-0.0352 (0.0239)	-0.3036*** (0.0250)	-0.1042*** (0.0242)	0.0033 (0.0249)	-0.0356 (0.0258)	-0.0257 (0.0271)	-0.0475* (0.0269)
<i>Full sample (N = 128, 414)</i>									
Incarceration				-0.3100*** (0.0254)	-0.0853*** (0.0245)	-0.0326 (0.0245)	-0.3840 (0.0252)	-0.0458* (0.0260)	-0.0560** (0.0258)
Incarceration	0.0254 (0.0231)	-0.0309 (0.0231)	-0.0339 (0.0233)	-0.3082*** (0.0243)	-0.1102*** (0.0236)	-0.0023 (0.0242)	-0.0412 (0.0251)	-0.0341 (0.0264)	-0.0566** (0.0262)

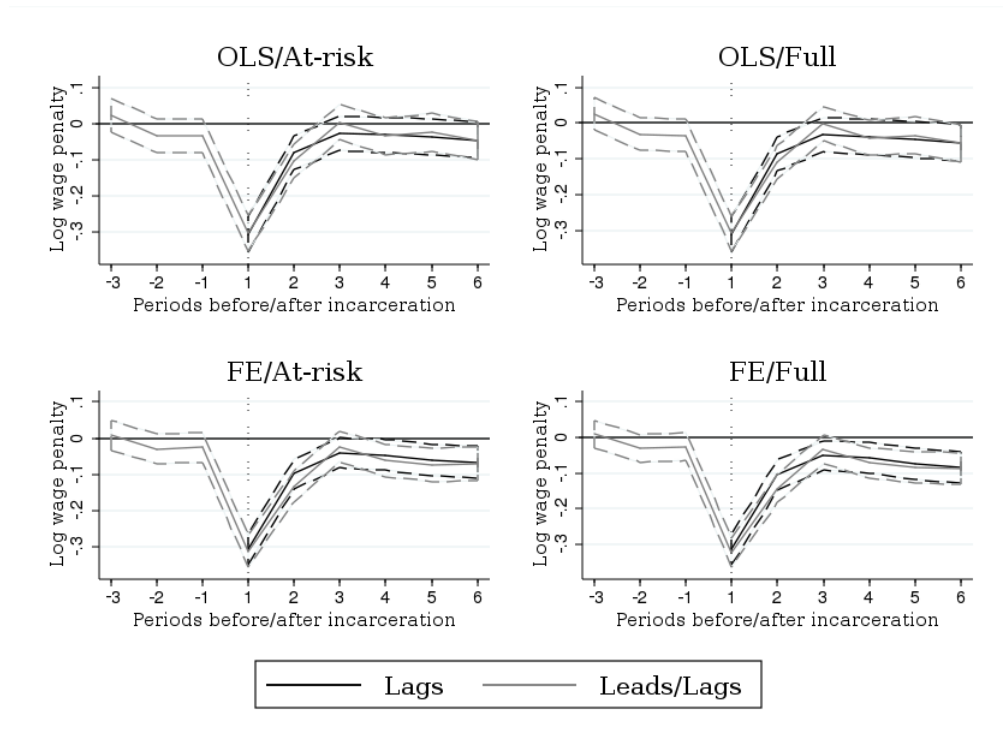
Notes: * Significant at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses. Positive j represent periods since incarceration; k represent periods prior to incarceration.

Table 6. Incarceration-wage trajectories (2), fixed effects estimates.

	$k = 3$	$k = 2$	$k = 1$	$j = 1$	$j = 2$	$j = 3$	$j = 4$	$j = 5$	$j = 6$
	<i>At-risk subsample (N = 31, 102)</i>								
Incar.				-0.3054*** (0.0221)	-0.0984*** (0.0213)	-0.0405* (0.0212)	-0.0459** (0.0218)	-0.0600** (0.0224)	-0.0666*** (0.0270)
Incar.	0.0080 (0.0208)	-0.0294 (0.0208)	-0.0252 (0.0210)	-0.3113*** (0.0219)	-0.1331*** (0.0214)	-0.0229 (0.0218)	-0.0616** (0.0225)	-0.0730*** (0.0236)	-0.0708*** (0.0240)
	<i>Full sample (N = 128, 414)</i>								
Incar.				-0.3129*** (0.0223)	-0.1055*** (0.0215)	-0.0503** (0.0213)	-0.0574** (0.0219)	-0.0730*** (0.0226)	-0.0843*** (0.0228)
Incar.	0.0100 (0.0197)	-0.0302 (0.1968)	-0.0262 (0.0199)	-0.3220*** (0.0208)	-0.1435*** (0.0202)	-0.0325 (0.0206)	-0.0713*** (0.0213)	-0.0837*** (0.0224)	-0.0864*** (0.0227)

Notes: * Significant at at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses. Positive j represent periods since incarceration; k represent periods prior to incarceration.

Figure 3. Incarceration-wage trajectories (2).



Notes: Dashes represent corresponding 95% confidence intervals. Positive periods represent j periods since incarceration and negative periods represent k periods prior to incarceration in the form of (2).

Table 7. Incarceration wage growth penalties, OLS/QR estimates.

Key variables	OLS (mean)	5th	25th	50th	75th	95th
<i>At-risk subsample (N = 35, 653)</i>						
Prior	1.6698*** (0.1609)	2.3210*** (0.4656)	0.9860*** (0.1468)	0.7650*** (0.1277)	0.7723*** (0.1695)	1.8107*** (0.4074)
Prior*ln(age)	-0.5046*** (0.0460)	-0.6953*** (0.1418)	-0.3049*** (0.0432)	-0.2421*** (0.0373)	-0.2446*** (0.0401)	-0.5698*** (0.1151)
ln(age)	0.2432*** (0.0177)	-0.1149*** (0.0364)	0.2206*** (0.0158)	0.2975*** (0.0151)	0.3867*** (0.0176)	0.7050*** (0.0422)
<i>Full sample (N = 146, 453)</i>						
Prior	1.8560*** (0.1564)	2.9395*** (0.5515)	1.1410*** (0.1537)	1.0090*** (0.1323)	1.1143*** (0.1546)	1.9978*** (0.3258)
Prior*ln(age)	-0.5598*** (0.0445)	-0.8698*** (0.1669)	-0.3493*** (0.0447)	-0.3148*** (0.0381)	-0.3492*** (0.0440)	-0.6305*** (0.0881)
ln(age)	0.2677*** (0.0086)	-0.0678*** (0.0237)	0.2316*** (0.0077)	0.3298*** (0.0072)	0.4579*** (0.0082)	0.7281*** (0.0164)

Notes: * Significant at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses and are bootstrapped for quantile regression estimates (100 replications).

Table 8. Incarceration wage growth penalties, FE/FEQR estimates.

Key variables	FE (mean)	5th	25th	50th	75th	95th
<i>At-risk subsample (N = 35, 653)</i>						
Prior	1.7029*** (0.1712)	3.2585*** (0.7349)	1.3385*** (0.2280)	1.0738*** (0.1777)	0.9079*** (0.1900)	0.9602** (0.4503)
Prior*ln(age)	-0.5559*** (0.0478)	-0.9927*** (0.2259)	-0.4229*** (0.0652)	-0.3441*** (0.0511)	-0.2999*** (0.0545)	-0.3409*** (0.1292)
ln(age)	0.4900** (0.0217)	0.1593*** (0.0580)	0.4444*** (0.0245)	0.4496*** (0.0215)	0.4768*** (0.0241)	0.6953*** (0.0559)
<i>Full sample (N = 146, 453)</i>						
Prior	1.8132*** (0.1668)	3.6719*** (0.7497)	1.4526*** (0.2087)	1.2776*** (0.1540)	1.0695*** (0.1613)	1.1272** (0.5017)
Prior*ln(age)	-0.5919*** (0.0462)	-1.1198*** (0.2298)	-0.4614*** (0.0599)	-0.4080*** (0.0442)	-0.3532*** (0.0465)	-0.3884*** (0.1425)
ln(age)	0.5113*** (0.0106)	0.2376*** (0.0308)	0.4857*** (0.0123)	0.5074*** (0.0113)	0.5423*** (0.0123)	0.7013*** (0.0222)

Notes: * Significant at at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses and are bootstrapped for quantile regression estimates (100 replications).

Table 9. Incarceration-employment trajectories (2), pooled OLS estimates.

	$k = 3$	$k = 2$	$k = 1$	$j = 1$	$j = 2$	$j = 3$	$j = 4$	$j = 5$	$j = 6$
	<i>At-risk subsample (N = 31, 102)</i>								
Incarceration				-13.7379*** (0.5473)	-2.9639*** (0.5500)	-0.9883* (0.5511)	0.7071 (0.5660)	0.9527 (0.5836)	0.7301 (0.5806)
Incarceration	-2.6201*** (0.5784)	-1.3212** (0.5802)	-2.0876*** (0.5847)	-14.5971*** (0.6098)	-3.5539*** (0.5925)	-1.2304** (0.6073)	0.3387 (0.6302)	0.8689 (0.6614)	0.2928 (0.6579)
	<i>Full sample (N = 128, 414)</i>								
Incarceration				-13.9800*** (0.5428)	-3.1310*** (0.5225)	-1.0680** (0.5237)	0.6219 (0.5378)	0.8744 (0.5543)	0.6375 (0.5510)
Incarceration	-2.7672*** (0.5547)	-1.4239** (0.5564)	-2.2578*** (0.5607)	-14.8805*** (0.5846)	-3.7544*** (0.5681)	-1.3419** (0.5824)	0.2749 (0.6043)	0.7570 (0.6341)	0.1323 (0.6301)

Notes: * Significant at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses. Positive j represent periods since incarceration; k represent periods prior to incarceration.

Table 10. Incarceration-employment trajectories (2), fixed effects estimates.

	$k = 3$	$k = 2$	$k = 1$	$j = 1$	$j = 2$	$j = 3$	$j = 4$	$j = 5$	$j = 6$
	<i>At-risk subsample (N = 31, 102)</i>								
Incar.				-13.5285*** (0.5310)	-2.1896*** (0.5119)	-0.1516 (0.5087)	0.9021* (0.5227)	1.0766** (0.5385)	0.8716 (0.5445)
Incar.	-2.8031*** (0.5342)	-1.3863** (0.5330)	-1.3976** (0.5390)	-14.5729*** (0.5628)	-2.6901*** (0.5484)	-0.4377 (0.5584)	0.6477 (0.5778)	0.9332 (0.6059)	0.7817 (0.6164)
	<i>Full sample (N = 128, 414)</i>								
Incar.				-13.9800*** (0.5423)	-3.1310*** (0.5225)	-1.0680** (0.5237)	0.6219 (0.5378)	0.8744 (0.5543)	0.6375 (0.5510)
Incar.	-2.8838*** (0.5081)	-1.5002*** (0.5069)	-1.5510*** (0.5125)	-14.8804*** (0.5347)	-2.9470*** (0.5212)	-0.6442 (0.5311)	0.4487 (0.5495)	0.6958 (0.5760)	0.4525 (0.5853)

Notes: * Significant at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses. Positive j represent periods since incarceration; k represent periods prior to incarceration.

Table 11. Incarceration-labor force participation trajectories (2), pooled OLS estimates.

	$k = 3$	$k = 2$	$k = 1$	$j = 1$	$j = 2$	$j = 3$	$j = 4$	$j = 5$	$j = 6$
<i>At-risk subsample (N = 31, 102)</i>									
Incar.				15.1776*** (0.4660)	3.0204*** (0.4505)	1.3572*** (0.4511)	-0.4716 (0.4616)	-0.1818 (0.4781)	-0.5155 (0.4776)
Incar.	1.8766*** (0.4768)	1.1980** (0.4786)	1.7236*** (0.4832)	16.3302*** (0.5017)	3.4081*** (0.4881)	1.5511*** (0.5005)	-0.3693 (0.5177)	0.999 (0.5451)	-0.5088 (0.5452)
<i>Full sample (N = 128, 414)</i>									
Incar.				15.4098*** (0.4525)	3.1536*** (0.4376)	1.4734*** (0.4384)	-0.3462 (0.4484)	-0.0254 (0.4644)	-0.3041 (0.4634)
Incar.	1.9576*** (0.4688)	1.2979** (0.4705)	1.8562*** (0.4751)	16.6205*** (0.4930)	3.5912*** (0.4798)	1.6848*** (0.4921)	-0.2612 (0.5090)	0.2685 (0.5358)	-0.2478 (0.5353)

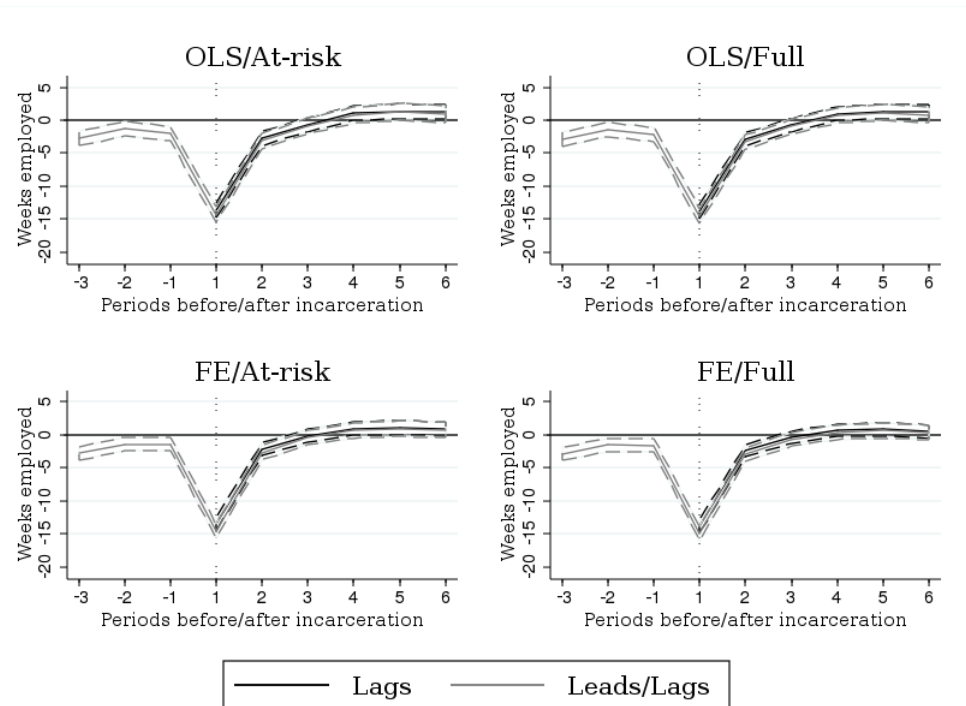
Notes: * Significant at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses. Positive j represent periods since incarceration; k represent periods prior to incarceration.

Table 12. Incarceration-labor force participation trajectories (2), fixed effects estimates.

	$k = 3$	$k = 2$	$k = 1$	$j = 1$	$j = 2$	$j = 3$	$j = 4$	$j = 5$	$j = 6$
	<i>At-risk subsample (N = 31, 102)</i>								
Incarceration			15.1526*** (0.4396)	2.4892*** (0.4256)	0.8601** (0.4234)	-0.6163 (0.4329)	0.1083 (0.4482)	-0.5366 (0.4541)	
Incarceration	1.9207 (0.4458)	1.1144** (0.4461)	0.8656* (0.4518)	16.3850*** (0.4699)	2.9090*** (0.4582)	1.2628*** (0.4673)	-0.5678 (0.4807)	0.3731 (0.5069)	-0.7316 (0.5163)
	<i>Full sample (N = 128, 414)</i>								
Incarceration			15.3739*** (0.4234)	2.6774*** (0.4099)	1.0502** (0.4079)	-0.4225 (0.4170)	0.3404 (0.4316)	-0.2058 (0.4369)	
Incarceration	1.9960*** (0.4350)	1.2412*** (0.4352)	1.0234** (0.4406)	16.7108*** (0.4579)	3.1865*** (0.4467)	1.4974*** (0.4558)	-0.3516 (0.4689)	0.6397 (0.4943)	-0.3560 (0.5029)

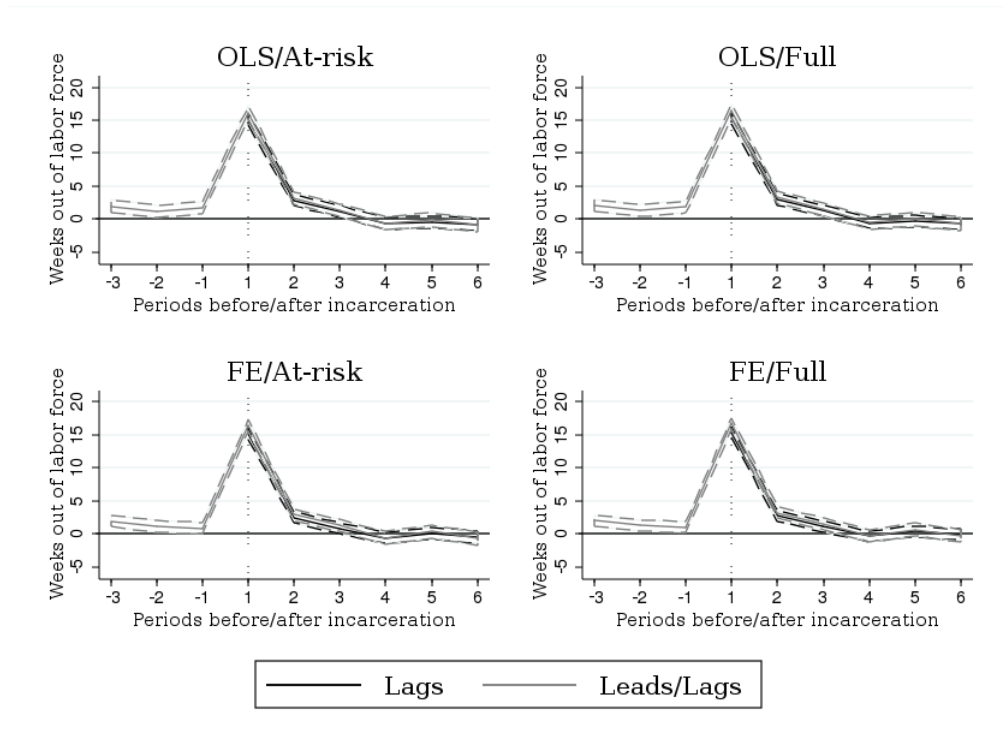
Notes: * Significant at 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors are in parentheses. Positive j represent periods since incarceration; k represent periods prior to incarceration.

Figure 4. Incarceration-employment trajectories (2).



Notes: Dashes represent corresponding 95% confidence intervals. Positive periods represent j periods since incarceration and negative periods represent k periods prior to incarceration in the form of (2).

Figure 5. Incarceration-LFP trajectories (2).



Notes: Dashes represent corresponding 95% confidence intervals. Positive periods represent j periods since incarceration and negative periods represent k periods prior to incarceration in the form of (2).

CONCLUSION

Mass incarceration is a complex and controversial phenomenon in the United States, and its effect on society reaches far beyond its influence on those who are sentenced to jails and prison. It has become increasingly important to estimate the intended and unintended effects of incarceration on the individual, the household, and society. In this dissertation, I focus on several of the unintended consequences of incarceration, particularly with respect to the labor market. Most research hypothesizes that incarceration has a generally negative effect on several common economic outcomes, including education, employment, and earnings. Over the course of three essays, I provide a body of evidence that supports this general hypothesis.

In the first chapter, “Estimating the Economic Consequences of Parental Incarceration,” I examine the long-term labor market performance of individuals that experience the incarceration of a resident parent. Drawing on data from the National Longitudinal Survey of Youth, I evaluate whether parental incarceration has a negative effect on a child’s educational attainment and adult earnings. Using a variety of regression techniques that allow the effect of parental incarceration to vary across parent and child gender, I find evidence that parental incarceration, particularly that of resident mothers, has negative effects on the attainment of higher education and earnings. These negative effects vary across parent and child gender, with little consistent evidence for negative paternal incarceration effects. Daughters of incarcerated mothers, however, face higher educational penalties than sons of incarcerated mothers, and sons experience more depressed earnings than daughters. In total, this study suggests that the incarceration of parents has a substantial effect on long-term behavior and outcomes of children.

The second chapter, “Returns to Post-Incarceration Education for Former Prisoners,” examines the effectiveness of education as a form of releasee rehabilitation. In

this study, I construct a sample of incarceration, education, employment, and earnings histories using National Longitudinal Survey of Youth data. Drawing on a variety of regression and propensity score matching models, I find that there is a positive relationship between post-incarceration education, labor supply, and earnings. This benefit, however, is largely confined to individuals who complete college after an incarceration spell. In addition, after differentiating between high school diploma and GED receipt, I find evidence that the former does not appreciably improve release labor market outcomes in its own right. These results suggest that the positive relationship between education and labor market performance is somewhat weakened. As prisoner education is a common rehabilitative tool, the diminished return to schooling should be taken into account for the formation of prisoner reentry policy.

In the final chapter, “Incarceration and Earnings: Clarification from Quantile Regression,” I extend the current body of literature concerned with the incarceration-induced wage penalty. As the average prisoner varies widely from the average American in many wage-depressing ways, I employ quantile regression techniques to estimate the incarceration penalty across the distribution of wages, with particular emphasis on below-median earnings that former prisoners are likely to report. Using data drawn from the National Longitudinal Survey of Youth, I construct a panel of former prisoners and comparable at-risk individuals that have never been incarcerated. My results suggest that incarceration wage penalties are less severe at lower wage quantiles than mean regression techniques have previously estimated in the literature. This study provides a more complete picture of incarceration’s effect on the earnings of former prisoners.

In total, this dissertation attempts to shed light on several individual aspects of the greater issue of mass incarceration. Over the course of three essays, I attempt to estimate the effects of incarceration on a variety of individual outcomes. Using the theoretical background and empirical methodology of contemporary labor economics,

I present a thorough analysis of some of the major questions surrounding incarceration. The results of each study represent a contribution to the existing body of incarceration literature that is relevant to both researchers and policy makers. Modern American incarceration has a significant effect on the lives of a growing number of individuals, and my findings suggest that these effects often manifest themselves on the market for labor.