THE NEW GLASS CEILING: INCARCERATION'S EFFECTS ON LIFETIME WAGE GROWTH

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Theodore S Corwin III

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THE NEW GLASS CEILING: INCARCERATION'S EFFECTS ON LIFETIME WAGE GROWTH

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Abstract

The United States incarcerates its citizens at rates higher than those of any other developed nation in the world, straining both its budgets and communities. The long-run effects of incarceration have been receiving more attention in the past two decades, but little research addresses incarceration's effects on earnings trajectory. Using the National Longitudinal Survey of Youth for 1997, I implement propensity score matching to model the treatment effects of incarceration on wage growth rates, controlling for individual characteristics that influence labor market outcomes.

KEYWORDS: (Crime, Corrections, Earnings, Incarceration, Labor Income, Prisons, Punishment, Trust)

JEL CODES: (J310, J710, K140, K420, Z130)

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ON MY HONOR, I HAVE NEITHER GIVEN NOR RECEIVED
UNAUTHORIZED AID ON THIS THESIS

Theodore S [illegible] III

Signature

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Introduction

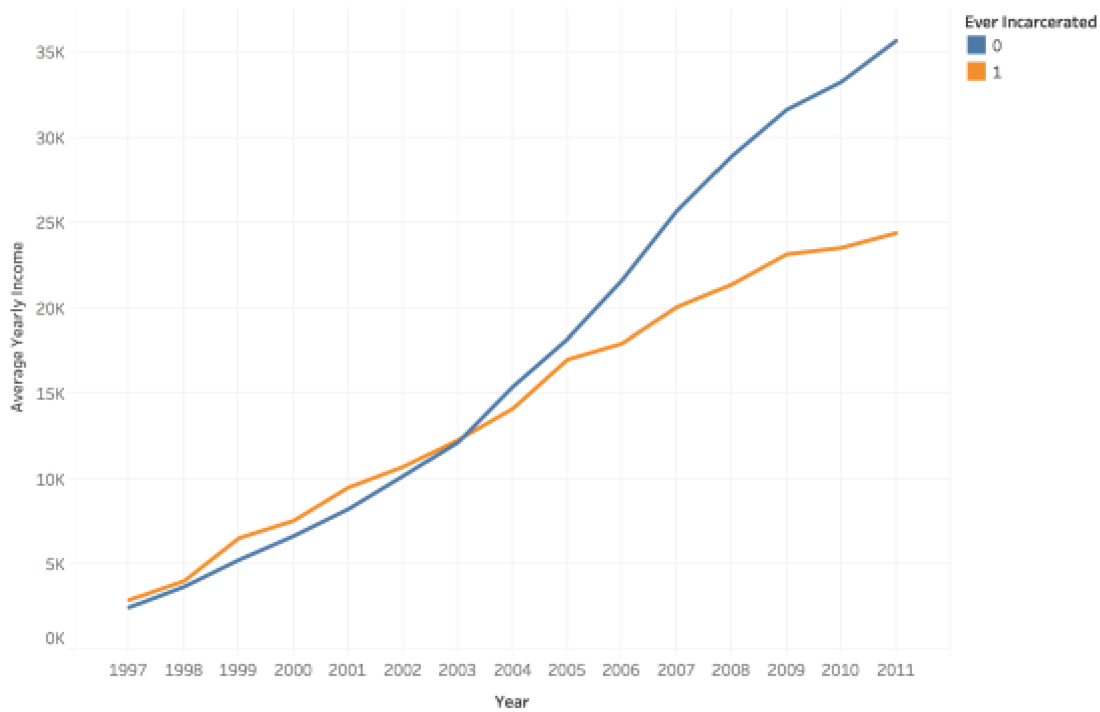
In recent years, public demand for criminal justice reform has been mounting, due in large part to the growing financial burden of incarceration on both state and federal governments. Despite a 39% fall in violent crime and a 52% fall in property crime between 1980 and 2014, the US incarceration rate in that period grew 220% controlling for population growth (CEA 2016). These rates are particularly telling when understood in the context of global norms. The US incarcerates its citizens at four times the global average, maintaining both the largest imprisoned population and the highest rate of incarceration in the world (Hartney 2006). These incarceration rates are not correlated with higher crime rates in the US, but are the result of policy structured to be "tough on crime," a strategy in opposition to the rehabilitation and prevention-based strategies employed in many other developed countries (Blumstein & Beck 1999).

Economic burden notwithstanding, there is little evidence that the US tendency to incarcerate at very high rates is effective at combatting current and future crime (Dilulio & Piehl 1991). Long term social welfare improvement hinges on criminals reintegrating with society, ideally contributing to the economy and supporting their families and friends (Rose & Clear 1998). If the purpose of incarceration is to improve social welfare, we should prioritize inmate reintegration after incarceration. Unfortunately, the reality of the situation is far from this ideal, and reintegration has not been a priority of the US correctional system at large (Lynch 2002). To illustrate our reintegration shortcomings, an examination of recidivism is necessary. Recidivism is loosely defined as the rate at which released prisoners face rearrests, reconviction, or reincarceration within three years of release, and is a common metric in evaluating social justice programs (Sedgley et al.

2010). According to the National Institute of Justice, two thirds of released prisoners are rearrested within three years of release, and three quarters within five years of release (2014). By measure of recidivism, current policies are not likely the most effective way to promote social welfare. With such high rates of prison reentry, it is easy to see how our prison population and spending on corrections have consistently grown together over the past four decades.

Given that the prison population (federal and state combined) is hovering around a staggering 1.5 million inmates (23% of the world's convicted incarcerated population), the direct costs of inmate and administrative services alone sap an estimated $80 billion of taxpayer money every year (VERA Institute; Hartney 2006; BJS.gov). Yet, indirect costs of this policy far exceed direct costs, estimated between $500 billion and $1 trillion yearly, up to 6% of US GDP (Pettus-Davis et al. 2016). These indirect costs include but are not limited to "foregone wages of incarcerated persons, increased infant mortality, and increased criminality of children with incarcerated parents" as well as higher welfare costs associated with families of incarcerated fathers. While these indirect costs may not appear on yearly budgets, their impact has significant consequences on individuals and communities (Pettus-Davis et al. 2016). As the victims of current incarceration policy are by no means "randomly selected" from the general population, minority communities carry a disproportionate amount of these indirect costs (Peck & Theodore 2008; Rose & Clear 1998; Western 2009). At any given time, 10% of all black American men are in prison or jail, and 60% of black Americans without a high school diploma serve time in prison in their lifetime (Peck & Theodore 2008; Pettit & Lyons 2009; Pettit & Western 2004; Warren 2008). Given the well documented negative effect of prison time on labor market prospects, current policy takes a disproportionate toll on black communities in the short and long-term (Kling 2006; Hutcherson 2012; Pettit & Lyons 2009; Western 2002).

Figure 1: NLSY Income ComparisonSheet 1



Among the more enduring consequences of current incarceration policy are the dampened labor market outcomes of released inmates, which often plague them for their entire lives. These consequences include depressed wages and wage growth, inability to secure career-path jobs, weak labor force attachment, and higher likelihood of participating in illegitimate economic opportunities (Western 2002; Hutcherson 2012). As current incarceration policy does not optimize societal health and seems to have no cost ceiling in sight, I aim to isolate one long-run cost of current policy to contribute to the growing pool of research advocating against mass incarceration. In this paper, I use the National Longitudinal Survey of Youth for 1997 to examine the effects of incarceration on future wage growth rates. By framing the prison system as a labor market institution, I explore the economic effects of "tough on crime" policy. To control for individual differences across the population, I implement a form of propensity score matching to pair individuals who have been incarcerated with those who share similar education levels and economic backgrounds, but have never been in prison. I use income growth rates as my dependent variable as opposed to static wages of given years, examining future trajectory rather than current standing.

Previous Literature on Incarceration and Wages

Overview

Broadly speaking, analyses of incarceration's wage effects tend to isolate one of two sides of the labor equation: labor supply, the employers; or labor demand, the incarcerated individuals (Geller, Garfinkel, and Western 2006). In terms of labor supply, incarceration can act as a red flag for employers, signaling unreliability, dishonesty, or greater legal liability in applicants, often termed the stigma effect of incarceration (Pettit & Lyons 2009). On the demand side, incarceration may contribute to deterioration of human or social capital while an individual is in prison, or may strengthen social ties to criminal activity, making illegitimate work relatively more appealing and attainable than legitimate opportunities (Lochner 2004; Rose & Clear 1998).

Theoretical Framework

Until the late 90s, most crime theory followed an individualistic approach, in which criminals were perceived as active participants in crime, and little attention was paid to their background characteristics. With Rose and Clear's *Implications for Social Disorganization Theory* came a new appreciation for crime and social deviancy as a social phenomenon (1998). They assert that if crime is ecologically determined, "an overreliance on public controls may diminish the capacity of private and parochial controls as communities learn to rely on outsiders," decaying the community foundations necessary for social order. They go on to explain that this retrogression would be most pertinent among communities already deficient in social capital, as they would most quickly accept external "formal control" (Rose & Clear 1998). Most research of the past two decades gives some weight to both individual and ecological factors in determining incarceration likelihoods. Sociological examinations, like those of Pettit and Lyons, tend to delve further in to theoretical ecological factors, while econometric research tends to restrict itself to the data available. For this reason, research on the subject varies widely in tone and motive.

Empirical Analysis

Of recent literature, few reviews stand out more than those of Bruce Western, a Criminal Justice Policy professor at Harvard. Western takes a sociological approach at the issue, but employs statistically advanced techniques to substantiate his reviews. His lens has proved rare and highly valued, as many of the sociological examinations of incarceration fall far from standard econometric techniques, yet Western's *The Impact of Incarceration on Wage Mobility and Inequality* has been compelling across disciplines. Previous literature often paired state corrections and unemployment insurance documentation to draw conclusions on incarceration effects, but suffered from incomplete data with scarce background information. These studies generally found amount of time served in prison to be the most significant aspect in determining future wages, and often concluded *having ever been* in prison was not a significant predictor (Waldfogel 1994; Western 2002). Those that didn't use state data were limited to federal prison populations, which vary greatly from the nation's state prison demographic breakdown. Nearly all models used nominal wages as the dependent variable, and didn't account for exogenous wage growth over the life-course, attempting to isolate a "constant [wage] decrement" associated with time in prison (Western 2002).

Using NLSY 79 data, Western's research was among the first to account for age-graded effects of incarceration on wages, introducing an age*wage interaction term into the model. Theory at the time suggested that long-term wage growth was attributable to development opportunities available in career-jobs, and Western looked to the age-graded incarceration variable to shed light on whether ex-cons would experience lower growth with age than did the rest of the population (Western 2002; Rosenfeld 1992) His use of nationally representative longitudinal rather than state-level data allowed him access to much more thorough financial information (such as reporting on undeclared wages and wages from inconsistent employment), while being more reflective of the American population at large. This more thorough data allowed Western to create a subset of the population that was at high risk of crime or delinquency, a then novel way of tackling the problem of endogeneity that plagues incarceration research. Employing both simple OLS and individual fixed-effects modelling, Western found that incarceration had a significant effect on future wages, estimating between 7% and 19% lower wages for persons who had been incarcerated. He also found that the age*incarceration interaction was significant. While market factors caused low-skilled wages to drop dramatically during this time, those who had been incarcerated still lagged behind their never-incarcerated cohorts.

While Western's review was among the first to tackle the issue of enduring disadvantage for ex-cons, use of the NLSY 79 data limited its population age to a narrow range, prompting many scholars to search for alternate data sources. Pettit and Lyons looked to cooperative states for data collection, combining information from the Washington State Department of Corrections with Unemployment Insurance records to consider how the timing of incarceration in one's lifetime may influence wages (2009). Though restricted to a single state, this data contained participants of varying ages (45-

year range) unlike the NLSY, which was limited to a five-year age range. Unlike previous state-level research, Pettit and Lyons' data contained ample background and incarceration information about the population, allowing for a more thorough analysis of covariates in their earnings models. To leverage their age-diverse data, they broke the population into admission-age subsets (20-24, 25-30, 31-35, and 35+) and measured the effect of having been incarcerated on log wages for each of the groups. To control for endogeneity, they matched "early admits" (those who served their sentence in the first half of the ten-year data period) with "at risk" participants (those who served their first sentence in the last few years of the data period) across a list of covariates.[1] This matching technique was more sophisticated than many seen before and was only feasible thanks to the detailed state data they acquired. Its diversity of covariates allowed Pettit and Lyons to isolate the effect of incarceration on labor market outcomes, controlling for conditions of confinement, prior work experience, and many other factors (Pettit & Lyons 2009). Not surprisingly, their estimates of incarceration's effect on wages were lower than most, showing a 4% to 7% hit to wages in the short-term, but represent the most conservative of models given the aggressive matching technique employed. While methodologically robust, their study was restricted by the small earnings data period of 1988 – 2002, making inference of future earnings unreliable. They claim that earnings right after release can be used as a proxy for future earnings, but this is a bold assumption and one that compromises their conclusion if violated.

[1] Covariates: chronological age, race, education, offense type (for first observed offense), and the risk of recidivism as measured by a risk-assessment inventory (The Level of Service Inventory - Revised [LSI-R] p. 739

Jeffery Kling splits the difference between these studies, using detailed multi-state data with pre and post-incarceration earnings. Published in the American Economic Review, his 2006 paper employs both OLS and 2SLS estimations to examine the long and short-term effects of incarceration length on labor market outcomes, controlling for both inmate characteristics and individual judges' propensities for sentencing length. While Kling also uses a narrow income range of ten years, he takes advantage of cases filed before the earnings window, allowing him to examine wages up to seven years after incarceration. Relative to other research in the field, Kling's remains largely impartial towards sentencing policy, ultimately concluding that length of sentence in the medium (7-9 years) has neither positive nor negative significant effect on future earning prospects. This conclusion may support theory that incarceration's wage dampening tendency is at least partially in effect immediately upon sentencing, in accordance with Waldfogel's canonic research on income and employer trust issues as related to criminality (1994).

Though extensive research has sought to address the penal system's effect on labor market outcomes, few studies have specifically addressed changes in wage growth rates, a more reliable indicator of trajectory. I draw on Western's age-graded effects, Pettit and Lyons' covariate complexity, and Kling's model diversity and use of growth rates to estimate differences in incarceration's effect on future earnings across race and ethnicities.

Data

Overview

To examine incarceration's labor market effects, I looked to the National Longitudinal Survey of Youth for 1997 (henceforth NLSY). Unlike most survey data, the NLSY includes institutionalized respondents, as it begins surveying in high school, and includes inquiries about monthly incarceration status. As Western notes, use of the NLSY data allows analysis of both state and federal inmates simultaneously, an advantage over the majority of analyses, which utilize either federal or state administrative records (2002).

The NLSY 97 is freely accessible for any scholarly use and can be accessed via nlsyinfo.org. The dataset consists of roughly nine-thousand respondents, aged 12 to 18 at first interview, representative of the US population as of 1997, and surveyed for 16 rounds thus far. It surveys both the respondent and his or her parent(s) at the outset of the survey, and continues surveying the respondent yearly, collecting information on a wide array of subjects including but not limited to: home life, performance in school, higher education, health, crime, substance use, employment, income, as well as attitude and non-cognitive tests. This diversity of information allows for a rich analysis of covariates when examining labor market outcomes. In terms of involvement with the criminal justice system, the sample is roughly similar to the national breakdown as reported by the Bureau of Justice Statistics (Bonczar 1997).

Independent Variable Selection and Creation

Due to the overwhelming availability of covariates, I used previous literature and theory to generate a list of variables that were likely correlated with either future earnings or criminal activity. Table 1 includes the complete list of variables included in my final wage growth rate regression, including those used to determine propensity of incarceration. All variables were selected via the NLSY Investigator and downloaded as a .csv file. Using the NLSY Documentation Center and Codebook, which outlines the ordering of questions and the "universe" (set of assumptions) associated with each, I cleaned, labelled, and recoded the variables to control for periods of non-interview, valid and non-valid skips, and outliers. While many variables were ready to use once recoded, some had to be manipulated for maximum effectiveness. Following common practice in the literature, I reduced the categorical race variable to a white/non-white dummy variable. Similarly, I collapsed information on respondent's relationship to household parent down to an "absent biological father during childhood" binary variable. Using continuous information on amount of government aid taken, I generated dummies for whether the respondents ever accepted government assistance (excluding unemployment insurance and worker's compensation) or unemployment insurance (UI).[2] The formation of yearly incarceration status required the greatest amount of manipulation, and was accomplished by collapsing monthly reports for each unique ID into yearly binaries. Those binaries were maxed by ID to create an "ever incarcerated" dummy, which became the treatment indicator in my model.

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[2] Government programs include AFDC, TANF, Food Stamps, WIC, and SSI

Dependent Variable Creation

While the survey data of the NLSY offers ample background information on respondents, its yearly collection technique suffers in terms of regularity. Respondents commonly missed at least one year of reporting over the 16 years of available data, causing gaps in the yearly income variable. When working with time-series data, these gaps render Stata unable to generate income growth rates, as it understands the data to be discontinuous and therefore not suited for differencing or lagging during estimations. As most of the NLYS's income gaps are due to respondent non-reporting and do not represent years of zero earnings, I had to sidestep Stata's defense against discontinuous data in order to generate growth rates. Under the assumption that income growth rates could be averaged across periods of missing information, I dropped all periods of missing income data and averaged the growth across those gaps. Using these averaged rates of reported periods, I then generated a mean lifetime growth rate for each ID. In addition to averaging growth over missing periods, I excluded all years with income less than $1,000 to control for deceptively high growth that stems from ultra-low starting incomes.

Due to the variation in prison sentence length and exit timing, I divided my population into three subsets by year of prison exit. The never-incarcerated segment of the population is identical across all sets, meaning each subset varied only by the ex-inmates included. To avoid the complexity of multiple incarceration spells, I examined individuals who served one and only one sentence, which introduces a selection bias but is necessary for my style of analysis. The first subset included those respondents who had exited prison by 2003 and spent most of the surveyed period out of prison, roughly 20% (μ - σ) of the incarcerated population. The second and third included those who had exited by 2007 (μ) and 2011 ($\mu + \sigma$), capturing 50% and 80% of the incarcerated population, respectively. While each subset varies in its inclusion of ex-inmates, they each contain income data on the entire lifespan of the respondent, and are not limited to post-incarceration earnings. An analysis of post-incarceration rates would certainly be interesting, but data limitations prevented me from isolating that aspect in this research. To control for the fact that these growth rates were averaged over the incarceration spell, I included a count of total months incarcerated in the outcome model.

Breaking the ex-inmate population into subsets served two purposes. First, it allowed comparison of treatment effects dependent on incarceration timing within the lifetime. Second, discriminating by exit year enabled me to distinguish the ex-inmates according to their available post-incarceration information, with the 2003 cohort having the most post-incarceration data and the 2011 cohort having the least. Modeling this set of three dependent variables provided a more comprehensive analysis than would a single model of the aggregate population.

Summary Statistics

The NLSY 97 population was selected to be nationally representative of the United States in 1997, and judging by the metrics used in my models, has remained so. Table 1 outlines the variables that remained in the final models as well as some basic demographic info, and a comprehensive list of all variables used for experimentation is available in Appendix A. While there were a great many variables I had hoped to include, the variables in Table 1 were selected for their consistency in reporting, as well as their predictive and theoretical significance.

Table 1: Descriptive Statistics of Independent Variables Used in the Final Outcome and Treatment Models

Variable	**Incarcerated**	**Never Incarcerated**	**Total**
Background			
Male	0.81	0.48	0.51
	(0.396)	(0.499)	(0.499)
White	0.41	0.53	0.52
	(0.492)	(0.499)	(0.500)
Black	0.36	0.25	0.26
	(0.481)	(0.433)	(0.439)
Hispanic	0.21	0.21	0.21
	(0.409)	(0.408)	(0.408)
Nobiofather	0.60	0.42	0.44
	(0.489)	(0.494)	(0.496)
Education			
Asvab	29370.21	46739.06	45323.49
	(24195.6)	(29151.5)	(29169.2)
Hdr	1.22	2.41	2.31
	(0.998)	(1.444)	(1.450)
Delinquency			
Arstby97	6.92	0.73	1.27
	(6.796)	(2.307)	(3.455)
Fight	1.04	0.34	0.40
	(2.702)	(1.569)	(1.708)
Hrddrgs	0.13	0.06	0.06
	(0.337)	(0.230)	(0.242)
Life Course			
Indcod	5804.80	6579.81	6517.75
	(3141.1)	(2474.7)	(2543.2)
Wkswkxrnd	306.69	395.57	388.33
	(188.0)	(197.9)	(198.6)
N	17226	180400	197626

In Table 1, I divided the population by history of incarceration to highlight the inherent differences in the two subsets. Restraint should be exercised when reading Table 1, understanding that it does not offer a complete picture of the social background of the participants, but merely situates the population within measurable metrics.

The population as a whole is roughly evenly split by sex, while the ever-incarcerated subset is over 80% male. In terms of race, whites are underrepresented in prison (accounting for only 41% of ever-incarcerated and 48% of never incarcerated groups) while blacks are overrepresented (at 36% and 25%, respectively). Hispanic prison representation, on the other hand, is similar across groups. Another point of difference occurs with respect to the presence of a biological father during childhood. The ever-incarcerated group suffers from missing fathers at rates nearly 50% higher than those of the general population.

Predictably, these differences are not limited to demographics, but extend through education, social deviancy, and lifestyle. As of 2013, the ever-incarcerated group was married at half the rate of the general population, which may be partially attributed to the life course interruption of prison. The ever-incarcerated group also had lower mean ASVAB scores and less frequently attained higher education than the general population. I offer these statistics not to demean those who have been incarcerated, but to illustrate the trends associated with the victims of current incarceration policy. Interestingly, the incarcerated group's number of weeks worked during the entire survey period (Wkswkxrnd) is within half a standard deviation of the general population, despite time forfeited by prison spells.

While not immediately obvious in Table 1, there are variables across which the groups are similar. As evident in Table 3 (Appendix A) parental education, months in primary school, income at age 21, and time on unemployment insurance are all fairly consistent across groups, though they were not included in the final models.

Methodology and Model Specification

Overview

Using a treatment effects model specification, I examine variation in wage growth rates, the outcome, with respect to having been incarcerated, the treatment. As treatment effects models assume random participation, yet incarceration selection is known to be non-random, I implement inverse probability weighted regression adjustment estimates (IPWRA) to pair individuals of no criminal background with like individuals who spent time in prison. IPWRA is a novel matching technique that controls for missing information better than its predecessors, while boasting doubly robust estimations (Wooldridge 2007; Cattaneo 2010). This matching technique is not a perfect control for unobserved heterogeneity among ex-inmates, as it relies on observable information, but still serves to strengthen the model.

Before moving ahead, I would like to situate a few terms in the context of this study. My model examines income growth rates in relation to the non-random treatment of incarceration. I control for this unobserved heterogeneity via an inverse probability weighted propensity score (IPW) and regression adjustment (RA). These techniques are often used independently so I will outline my process for each separately, though the final model uses them in unison. Due to the multistep nature of my model, I will first outline the theory of propensity scoring and my personal score composition, then the process of inverse-probability weighting that propensity score, then regression adjustment theory and composition, and finally the product, the IPWRA estimator.

Propensity Score Matching and IPWRA

PSM Theory

To appreciate the merit of IPWRA estimation, one must understand the common problem that plagues examinations of incarceration effects. When a researcher asks, "how does incarceration affect ______?" they must also ask, "are the ex-inmate outcomes caused by incarceration, or by individual characteristics that contributed to the individual becoming incarcerated?" This issue may be described as selection bias among the treatment group (in our case those who spend time in prison) or may be characterized as unobserved heterogeneity in the population. Unobservable variation in our population stems from a fundamental limitation of treatment effects models: the counterfactual problem.

Let's imagine a perfect research world in which an exact treatment effects can be isolated in one individual. To truly know how the treatment influenced the outcome, we would need to observe a situation with no treatment $(Y \mid T = 0)$, and simultaneously observe the exact same situation with treatment administered $(Y \mid T = 1)$. The difference in outcomes must then be caused solely by the treatment, as nothing else varied.

$$\Delta = (Y \mid T = 1) - (Y \mid T = 0) \tag{1}$$

Unfortunately, such a fantasy never occurs in reality, and this limitation is known as the counterfactual problem (Gertler et al. 2016). Instead we must attempt to mimic this ideal using the information and techniques available, and propensity score matching (PSM) has been among those techniques, at least theoretically, since the 1950s (Horvitz & Thompson 1952). PSM addresses the issue by "grading" each participant according to a selection of variables, and then matching treatment and control individuals by their scores. Matching allows us to observe both a treatment and non-treatment situation, and as the matching system improves, we will approach the ideal situation we imagined earlier (Gertler et al. 2016).

Propensity score matching has been implemented in incarceration examinations before, and though the statistical tools available are constantly changing, the theory behind these techniques is widely adopted. For instance, Sedgley, Charles, Nancy, and Frederick use a combination of PSM and individual effects "mixture models" to control for unobserved heterogeneity in their examination of program participation in prison and how it affects recidivism (2010). They conclude that funding prison education and training programs has a dramatic cost saving effect in the long run, and this conclusion would not be possible if they were not able to isolate treatment effects via PSM. Michael Massoglia uses PSM in his research on the "penal system as an explanatory factor of health disparities," finding that even after controlling for various health metrics, incarceration has a significant effect on later health outcomes (2008). I lean on these, and many other studies of incarceration effects to formulate my propensity score using the NLSY dataset.

PSM Composition and Specification

To begin building the propensity score, P (x_i), I specify a logistic regression "conditional on a vector of k observable cofounders" (Apel & Sweeten 2010). This logit is of the form

$$P(x_i) = Pr(T_i = 1 \mid x_i) \tag{2}$$

$$= \frac{\exp(X_i^T \beta)}{1 + \exp(X_i^T \beta)}$$

where β represents the column vector of theoretically predictive covariates and the ever-incarcerated dummy variable on the left-hand side. The goal of the model is to find a group of covariates that can estimate likelihood of incarceration, our treatment. When using a logit to build a propensity score, Pseudo R^2 (estimated index of predictive power) is often given more weight than statistical significance, as goodness of fit is the end-goal. However, when choosing variables, it is critical to understand the tradeoffs associated with over-specification. Especially in the context of the variable-rich NLSY, it is tempting to include every variable that might moderately influence the outcome, as each addition will likely contribute to a higher Pseudo R^2. However, if too many variables are included, the model may suffer from missing information on those variables (unbalanced covariates), or may predict participation too well, causing insufficient overlap between the treatment and non-treatment groups (Caliendo & Kopeinig 2008). While higher predictability may sound like a non-issue, no basis for comparison could exist without individuals whose outcomes contradict their scores.

With this in mind, I begin with a handful of theoretically sound variables on the right-hand side of my equation. These include whether the respondent (R) had been arrested or had any criminal convictions by 1997 (first year of the survey), whether R had tried hard drugs in high school, R's sex, Armed Services Vocational Aptitude Battery (ASVAB) scores, and months completed of high school. Employing one of Caliendo and Kopeinig's iterative techniques, I adjust this model, introducing new variables and dropping those that don't contribute to higher Pseudo R^2. As a point of reference, I build a separate "kitchen sink" logit in which I include nearly every possible variable and isolate the most predictive combination, yielding a sixteen-covariate series with no interactions.[3] A complete list of these variables can be found in Table 3 (Appendix A). I then plot kernel densities of the scores for both models, to ensure sufficient overlap. As an additional measure, I utilize the "bfit" user written command in Stata, which creates every possible combination and interaction of selected variables out to the sixth order and reports the model that minimizes the Bayesian information criterion (BIC).[4] This command is discussed by Cattaneo et al. as a good option for building a propensity score from scratch (2013). The bfit models did provide reasonably strong Pseudo R^2 (between .15 and .18) and when tested in my final model, comparable coefficients and P-scores. However, the bfit covariates were not ultimately used in my final models, as they were not particularly well catered to my data constraints. With several reasonable propensity score estimations, I proceed to IPWRA estimations.

IPWRA

As mentioned above, IPWRA is a combination of two separate techniques that are used independently to control for non-random treatment. First, I will outline inverse probability weighting as a standalone technique, and then I will explain regression adjustment and the IPWRA combination.

I, and many others in the field, implement propensity score matching as a means to control for unobserved heterogeneity between treatment and non-treatment groups. By

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[3] Covariates: Number of fights in K-12, average number of days/month that R consumed cigarettes, alcohol, and marijuana, whether R tried hard drugs in high school, whether R had been arrested or convicted of any offense by 1997, R's sex, R's family household net worth (parent reported), R's relationship to household parent (parent reported), R's, NLSY's delinquency index (see Appendix B for details), R's father's highest grade completed, whether R's father had been in prison, R's highest grade completed and highest degree received

[4] BIC = $-2LL + 2\ln(N)q$, where N is the sample size and q is the number of parameters (Drukker 2016)

matching individuals on their likelihood of treatment, it is possible to reduce the unobserved variation that plagues observational data, while addressing the problem of the counterfactual. However, propensity score matching opens the door to new limitations and risks. Basic PSM techniques, such as nearest neighbor matching, weight each treatment predictor equally when determining scores, which is often not representative of the true model. For instance, consider the situation at hand, in which we want to estimate the likelihood of being incarcerated. Nearest-neighbor matching would generate a propensity score that considers a trivial covariate, let's say the number of cigarettes smoked per month, to be equally important as race. This assumption is in flagrant contrast to current research that suggests race is among the most important if not the most important predictor of future incarceration. Inverse probability weighting has been developed to control for this downfall, first generating a score for each participant, and then reweighting each score. The weighting is based on the inverse of how likely the individual is to be observed in his or her actual group. As such, weights can be defined as

$$\omega = \frac{\mathbf{T_i}}{\mathbf{e_i}} + \frac{(\mathbf{1-T_i})}{(\mathbf{1-Z_i})} \tag{3}$$

where T is the treatment status of individual *i,* and e_i is the propensity score of individual *i.* Individuals who were incarcerated but seemed very unlikely to be incarcerated are given greater weight, and so are those who weren't incarcerated but seemed very likely to be incarcerated ($\omega > 1$). Those whose outcomes are in line with their scores receive an uninflated weighting ($\omega \sim 1$). By modelling the treatment, IPW controls for non-random treatment assignment, but this is only half of the IPWRA process.

Regression adjustment provides the second half of the equation, modeling the outcome, instead of the treatment, to control for non-random treatment. In this setting, where IPW would specify a model that predicts probability of incarceration (the treatment), RA would specify a model that predicts wage growth rates (the outcome). An average treatment effect would then be estimated by taking the difference of the two populations' outcome means (treated and control). RA is less prone to instability in the case of small populations or unlikely treatment situations, both of which can adversely affect the IPW estimator.

In specifying my outcome model, I find the propensity score to be a restrictive factor. Though earnings models are prevalent in literature and theory of their specification is abundant, I am highly sensitive to excluding respondents due to missing information. Starting broadly, I initially include a wide range of predictors in my outcome model, including parent's net worth, race/ethnicity, standardized test scores, education metrics, industry, marriage status, and hours worked during the survey. However, inclusion of this wealth of covariates dramatically reduces my sample population, at times to fewer than 10 participants. Through iterative trials balancing sample size and statistical significance, I choose a minimalist model of the following covariates: total months incarcerated, highest degree received, total weeks worked from 1997 – 2013, and ASVAB scores. While I would have liked to include controls for industry, socioeconomic background, and household characteristics, I prioritize sample size over covariate diversity, cautious of the limitations of the IPWRA estimator. With my outcome model specified, I am able to proceed to the combined IPWRA estimator.

IPWRA combines IPW and RA techniques, specifying both a treatment and an outcome model, and informing regression adjustment with weighted probabilities of treatment and non-treatment. The difference between the average outcomes of treatment and control groups is the estimated treatment effect. IPWRA benefits in that only one of the two models must be correctly specified to produce robust results (Wooldridge 2007). Employing the IPWRA estimation is simple, once reliable treatment and outcome models have been found. However, in the context of the NLSY data, though the population is fairly large (P ~ 9000 individuals over 21 years), the incarcerated population is much smaller (P ~ 800) and smaller still when looking only at individuals who served strictly one sentence (P = 426). This population is reduced further by gaps in income reporting, causing matching techniques to be stressed. For this reason, I employ three subsets of dependent growth rate variables, and a variety of treatment and outcome models. Even so, the magnitudes of these results should be taken with a grain of salt, understanding the limitations of dealing with a small population in conjunction with unlikely treatment assignment.

Results and Discussion

The average treatment effects of incarceration are roughly consistent across the three cohorts, though they do vary slightly in magnitude. As noted earlier, the 2011 cohort contains the largest collection of ever-incarcerated respondents, and is therefore the least susceptible to the dangers of small samples, so I expect it to be the most reliable of the three models. In the case of both the 2011 and the 2007 cohorts, a variety of outcome model specifications produced comparable results to those of the final, with average treatment effects ranging from -10% to -30%. In these outcome models, inclusion of industry information and income at age 21 both tended to lower the magnitude of the effect, producing the most conservative estimates, but caused a dramatic reduction in sample size and therefore had to be excluded from the final models. Due to the variety of variable forms in the models (binary, categorical, and string), interpretation must be handled carefully. Table 2 displays information for each step of the IPWRA process, the treatment and two outcomes, and is easiest to digest when broken into pieces.

The treatment model is the foundation for the IPWRA estimator, and can be found at the bottom of Table 2. As mentioned previously, the coefficient values and significance levels of the treatment model are of little value, as goodness-of-fit is the goal. Though it is not displayed in Table 2, the final treatment model achieved a McFadden R^2 of 0.309. As a point of reference, Apel and Sweeten's used propensity scores with an R^2 of 0.187 (2010). As higher R^2s indicate better fit, my R^2 of 0.309 achieves reasonably strong predictability. The significance and values of the coefficients vary somewhat by cohort, but not dramatically.

Table 2: Final IPWRA Model

	2003 Cohort	*2007 Cohort*	*2011 Cohort*
	Avg. Income Growth %	Avg. Income Growth %	Avg. Income Growth %
Average Treatment Effect			
1 vs 0: Ever Incarcerated	-19.38***	-15.71***	-21.84***
	(-14.26)	(-16.53)	(-6.72)
Population Mean			
Never Incarcerated	54.16***	54.98***	62.92***
	(281.66)	(165.57)	(19.42)
Outcome Model | T = 0			
Total Months Incarcerated	10.82***	10.34***	4.748
	(4.84)	(4.52)	(1.01)
Highest Degree Received	5.652***	6.147***	21.77*
	(37.05)	(17.98)	(2.30)
Total Weeks Worked	-0.0477***	-0.0441***	-0.0201**
	(-30.35)	(-16.82)	(-2.63)
ASVAB Score	0.0000160	-0.0000418	-0.00119*
	(1.78)	(-1.18)	(-2.47)
Constant	59.02***	59.35***	71.24***
	(79.74)	(62.34)	(9.13)
Outcome Model | T = 1			
Total Months Incarcerated	0.0686	0.728**	0.529***
	(0.28)	(3.08)	(5.74)
Highest Degree Received	2.257**	-2.289***	2.697***
	(3.28)	(-4.51)	(6.20)
Total Weeks Worked	0.0546***	0.0252***	0.0211***
	(7.65)	(5.51)	(5.86)
ASVAB Score	-0.000515***	-0.00000477	-0.000155***
	(-6.32)	(-0.14)	(-6.05)
Constant	29.92***	34.46***	32.10***
	(6.72)	(12.37)	(14.74)
Treatment Model			
# Fights by Grade 12	-0.131***	-0.0959***	-0.0114
	(-8.44)	(-5.94)	(-0.94)
White	0.888***	0.572***	0.196***
	(11.75)	(11.64)	(5.14)
# Arrests by 1997	0.100***	0.119***	0.169***
	(16.86)	(11.06)	(12.90)
Tried Hard Drugs by 97	0.551***	0.607***	0.488***
	(5.85)	(8.97)	(7.69)
Highest Degree Received	-0.860***	-0.703***	-0.566***
	(-29.06)	(-31.77)	(-28.68)

No Biological Father Present during Childhood	0.224**	0.161***	0.254***
	(3.25)	(3.49)	(6.71)
Constant	-3.821***	-3.089***	-2.772***
	(-47.55)	(-53.89)	(-49.36)
N	5,232	5,258	5,352

t statistics in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Moving up, the outcome models are split for the ever-incarcerated (T = 1) and never incarcerated (T = 0) populations. The coefficients display the average effect on growth rates given a one unit change in the variable, all else constant. The "Highest Degree Received" coefficients in the ever-incarcerated outcome model show some variation across cohorts, likely due to the small population size. Those in the never incarcerated model are consistent across measures. The coefficient for "Weeks Worked" and "ASVAB Score" appear small in both models, but must be understood as growth rate change per unit change. Since the ASVAB is reported in tens of thousands of points, the per unit influence is expected to be small, but is significant when comparing scores that may differ by twenty thousand points. The same concept applies to the "Weeks Worked" variable, meaning its influence can be considerable depending on the individual. That said, the ASVAB variable was not a significant predictor in the general population. Interestingly, and likely a result of the small sample size, higher ASVAB scores had a negative effect on income growth rates in the ever-incarcerated group. "Weeks Worked," on the other hand, shows a positive effect on growth rates for the ever-incarcerated group, but negative effects for the never incarcerated group.

"Total Months Incarcerated" should, in theory, have no effect on the never incarcerated population, but the coefficient was inflated by a coding loophole in the data, which counted a handful of ex-inmates as having been incarcerated but for no amount of time due to very short sentencing lengths. They were kept in the model so as not to reduce the matched population size further, but the resulting coefficients should not be given much weight. Those coefficients for the ever-incarcerated population indicate a positive effect of length of stay on income growth, with each additional month incarcerated associated with a six-percentage point boost in average income growth. Kling found a similar effect in his 2006 short-term IV model of incarceration length, noting that longer sentences may be associated with more serious offenses, and serious offenders tend to have better labor marker prospects.

At the top of the table, the population mean shows the mean income growth over the survey period, with all cohorts averaging around the high 50s or low 60s. Given that the dependent variable in all models represents *rates of rates* of growth, it is most helpful to interpret the values relative to one another and not in absolute terms. Each mean has high variance which, while not included in the table, reflects the wide range of growth rates present in the data. The average treatment effects vary somewhat across cohorts, but all indicate a negative effect of incarceration on average growth rates. The 2011 cohort has the highest effect, possibly influenced by the presence of ex-inmates who are just reentering the workforce and haven't secured stable employment in the two years since release. The 2011 cohort also has the lowest t statistic. This could indicate that, while still significant as a treatment, the effects of incarceration have not been fully realized in terms of wage growth given the short period of post-incarceration income reporting.

Limitations

The results of my IPWRA model, while statistically significant, do have considerable limitations. The NLSY data contains a narrow age range of five years, preventing analysis of incarceration's effects late in the lifespan. It also restricts the study to respondents of a particular generation, which may produce results inconsistent with those of other studies. Use of average income growth over the survey period gives a sense of trajectory rather than current standing, but this linearization may not capture the nuances of career path growth over the life course. More thorough data would allow for stratification of growth periods by age, which could indicate long-run labor market effects more precisely. Similarly, pre and post-incarceration growth could be compared, enabling a differences-in-differences approach to the problem. A larger ever-incarcerated population set would also strengthen the PSM estimators used in the model, allowing for matching on a wider array of covariates, such as parent's socioeconomic background, substance use and abuse, geographic location, and detailed info on behavior in school. In the event of ample data, it would be worth using exact matching on covariates like race and location, both of which contribute heavily to incarceration likelihoods and outcomes.

Given the high frequency of recidivism, it would be helpful to examine wages and multi-spell incarcerations, but such a study would require a very long reporting period to consider post-spell income effects.

Conclusion

In accordance with the findings of Western 2002, my models indicate a dampening effect of incarceration on wage growth in the lifetime. Using inverse probability weighted regression adjusted treatment models, I isolate the wage growth effects of incarceration, controlling for individual characteristics. While the degree of impact varies depending on the cohort, all three models estimate average growth rate reductions of 25% or more, after factoring in education, time spent working, etc. Additionally, the results provide insight into the differences between wage growth models for those who have been to prison, and those who have not. Having been to prison influences the predictor coefficients of the outcome model, illustrating that the effects of human capital investments, such as education, are not consistent across the two populations. This serves to debunk the mentality that poor wage outcomes of ex-inmates result from lack of intelligence or education, and stem more from disproportionate returns on education investment for those ex-inmate populations.

Considering the control for months spent in prison, the models' treatment effects are activated immediately upon being sentenced to prison, regardless of sentence length, and emphasize the role that the US penal system plays as a labor market institution. Due to data limitations, my models do not specifically address how these effects vary by race, but higher incarceration rates of minorities indicate that these wage effects disproportionately impact communities of color. With this understanding, we should approach future corrections policy specifically considering indirect labor costs. If our penal system has such profound impacts on the labor market outcomes of ex-inmates, particularly those of minority background, we may want to use it more discerningly as a tool of justice. Should we continue to incarcerate liberally, we will deteriorate our social capital further, perpetuating the burden of incarceration and inhibiting future cultural and economic growth. Coming research should address the comparative cost of rehabilitation programs for non-violent offenders, to better understand the most welfare-optimizing means of social control. An examination of treatment effects by race would help clarify the proportionality of prison labor influence, but will require larger datasets on ex-inmates.

If the US still champions the virtues of equality and opportunity, we need to depart from punitive corrections strategy and adopt a "justice reinvestment" mindset (Austin et al. 2013). Justice reinvestment strategy prioritizes effective crime reduction, dependent on offense type, and directs spending away from prison construction and toward community development. In this way, it lays the groundwork for incarceration rate reduction and facilitates social capital growth. Justice reinvestment may also serve to mitigate the current social climate, which situates law enforcement dichotomous to minority welfare. Strategically restructuring our penal system with its labor functions in mind would ultimately strengthen American cohesion and future economic prospects, while better representing the values on which our nation was founded.

Appendix A

Table 3: Descriptive Statistics for All Independent Variables used in the Experimental Regression Analyses

Variable	**Incarcerated**	**Never Incarcerated**	**Total**
Background			
Male	0.81	0.48	0.51
	(0.396)	(0.499)	(0.499)
Black	0.36	0.25	0.26
	(0.481)	(0.433)	(0.439)
White	0.41	0.53	0.52
	(0.492)	(0.499)	(0.500)
Hispanic	0.21	0.21	0.21
	(0.409)	(0.408)	(0.408)
Hhnw	51231.24	94060.66	90166.49
	(110347.8)	(139084.6)	(137274.7)
Urbrural	0.83	0.81	0.82
	(0.485)	(0.484)	(0.484)
Nobiofather	0.60	0.42	0.44
	(0.489)	(0.494)	(0.496)
Bdatey	1981.89	1982.02	1982.01
	(1.416)	(1.394)	(1.397)
Hgcdad	11.50	12.75	12.66
	(4.567)	(4.191)	(4.234)
Hgcmom	11.89	12.55	12.50
	(5.163)	(3.490)	(3.662)
Mprs	0.03	0.01	0.01
	(0.171)	(0.107)	(0.115)
Dprs	0.16	0.06	0.07
	(0.365)	(0.233)	(0.248)
Education			
Asvab	29370.21	46739.06	45323.49
	(24195.6)	(29151.5)	(29169.2)
Hdr	1.22	2.41	2.31
	(0.998)	(1.444)	(1.450)
Mnthschool	245.03	249.51	249.12
	(22.13)	(19.67)	(19.94)
Hgc	10.43	11.60	11.50
	(1.540)	(2.527)	(2.479)
Delinquency			
Arstby97	6.92	0.73	1.27
	(6.796)	(2.307)	(3.455)
Fight	1.04	0.34	0.40
	(2.702)	(1.569)	(1.708)
Cigdays	6.09	2.39	2.71
	(11.03)	(7.354)	(7.812)
Alcdays	1.40	0.72	0.78

	(3.768)	(2.533)	(2.669)
Mjdays	2.54	0.64	0.81
	(7.131)	(3.394)	(3.902)
Hrddrgs	0.13	0.06	0.06
	(0.337)	(0.230)	(0.242)
Life Course			
Indcod	5804.80	6579.81	6517.75
	(3141.1)	(2474.7)	(2543.2)
Wkswkxrnd	306.69	395.57	388.33
	(188.0)	(197.9)	(198.6)
Basein1	13846.25	12208.44	12322.89
	(10338.9)	(8943.1)	(9057.3)
Jobsatis	2.26	2.26	2.26
	(1.216)	(1.106)	(1.117)
Uitotmnth	2.32	2.44	2.43
	(5.310)	(5.891)	(5.842)
Uiamt	104.09	125.98	124.08
	(912.0)	(1077.7)	(1064.3)
Govprgmnths	17.92	12.53	12.97
	(31.50)	(27.38)	(27.78)
Gprgamt	681.55	409.93	433.15
	(1987.8)	(1487.0)	(1538.0)
Marstat13	0.14	0.32	0.31
	(0.354)	(0.467)	(0.461)
N	17226	180400	197626

Appendix B

Variable Glossary

Variable Name	**Description, Form**
Black	Black/Non-black, binary
White	White/Non-white, binary
Hispanic	Hispanic/Non-hispanic, binary
Hhnw	Household net worth of respondent's parents, discrete string
Urbrural	Urban/Rural, categorical
Nobiofather	Missing biological father during childhood, binary
Bdatey	Year of birth, discrete string
Hgcdad	Highest grade completed by father, discrete string
Hgcmom	Highest grade completed by mother, discrete string
Mprs	Mother has been in prison, binary
Dprs	Father has been in prison, binary
Asvab	Armed Services Vocational Aptitude Battery Score, discrete string
Hdr	Highest degree received, categorical
Mnthschool	Months spent in school through 12th grade, discrete string
Hgc	Highest grade completed, discrete string
Arstby97	Arrested by 1997, binary
Fight	Number of fights in K-12, discrete string
Cigdays	Average number of days per month in which respondent consumed cigarettes in 1997, discrete string
Alcdays	Average number of days per month in which respondent consumed alcohol in 1997, discrete string
Mjdays	Average number of days per month in which respondent consumed marijuana in 1997, discrete string
Hrddrgs	Tried hard drugs (crack, cocaine, heroin, or any unprescribed medications), binary
Indcod	Yearly industry code, discrete string
Wkswkxrnd	Number of weeks worked across survey period, discrete string
Basein1	Income at age 21, discrete string
Jobsatis	Yearly job satisfaction, categorical
Uitotmnth	Number of months using unemployment insurance, discrete string
Uiamt	Amount collected from unemployment insurance, discrete string
Govprgmnths	Number of months using government programs (excluding UI and worker's compensation), discrete string
Gprgamt	Amount collected from government programs, discrete string
Marstat13	Married and cohabitating as of 2013, binary

Appendix C

The NLSY Delinquency Index was formulated using responses from the following questions:

1. Have you ever run away, that is, left home and stayed away at least overnight without your parent's prior knowledge or permission?
2. Have you ever carried a hand gun? When we say hand gun, we mean any firearm other than a rifle or shotgun.
3. Have you ever belonged to a gang?
4. Have you ever purposely damaged or destroyed property that did not belong to you?
5. Have you ever stolen something from a store or something that did not belong to you worth less than 50 dollars?
6. Have you ever stolen something from a store, person or house, or something that did not belong to you worth 50 dollars or more including stealing a car?
7. Have you ever committed other property crimes such as fencing, receiving, possessing or selling stolen property, or cheated someone by selling them something that was worthless or worth much less than what you said it was?
8. Have you ever attacked someone with the idea of seriously hurting them or have a situation end up in a serious fight or assault of some kind?
9. Have you ever sold or helped sell marijuana (pot, grass), hashish (hash) or other hard drugs such as heroin, cocaine or LSD?
10. Have you ever been arrested by the police or taken into custody for an illegal or delinquent offense (do not include arrests for minor traffic violations)?

For further details on the index composition and population statistics, see pages 149-153 of the NLSY Codebook Supplement, available at:
https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/12125/app9pdf.pdf

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