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Plus a Life Sentence? Incarceration's Effects on Expected Lifetime Wage Growth

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Abstract

The United States incarcerates citizens at rates higher than those of any other developed nation, with impacts on not only government budgets but economic growth rates. Using the National Longitudinal Survey of Youth for 1997, we model the effects of incarceration on wage growth rates using inverse probability weighted regression adjusted (IPWRA) propensity score matching to recognize the selection bias among the members of the sample who serve prison terms. Results show that incarceration reduces average lifetime income growth by one-third even for a relatively short earning period, with that depth depending on length of sentence, employment history, and education level in some surprising ways.

Keywords: crime, corrections, earnings, incarcerations, labor income, prisons, punishment, trust

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1. Introduction

1.1 Overview of the Issue

In recent years, public demand for criminal justice reform has focused on the growing financial burden of incarceration on both state and federal governments (CEA 2016, Hartney 2006, Blumstein and Beck 1999). However, there is little evidence that higher incarceration rates combat current and future crime (Dilulio and Piehl 1991) but rather that 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). Since reintegration has not been a priority of the US correctional system (Lynch 2002), recidivism rates are astonishingly high (Sedgley et al. 2010): two thirds of released prisoners are rearrested within three years of release (National Institute of Justice 2014). Coile and Duggan (2019) mark this as a significant reason for macro-level labor force participation rates.

Given that the American prison population (federal and state combined) hovers around 1.5 million inmates, the direct costs of inmate and administrative services draw an estimated $80 billion of taxpayer money annually (VERA Institute; Hartney 2006; BJS.gov). Yet, indirect costs far exceed direct costs, estimated between $500 billion and $1 trillion annually, 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, and higher welfare costs associated with families of incarcerated parents. Furthermore, 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, incarceration policy takes a disproportionate toll on black communities in the short and long-term (Kling 2006; Hutcherson 2012; Pettit & Lyons 2009; Western 2002). Among the more enduring consequences of current incarceration policy are the dampened labor market outcomes of released inmates: depressed wages and wage growth, inability to secure career-path jobs, weak labor force attachment, and higher likelihood of participating in illegal economic opportunities (Western 2002; Hutcherson 2012). In short, former prisoners face poor employment prospects in part because they disproportionately come from populations that themselves face poor employment prospects (Looney and Turner, 2018).

In this paper, we use the National Longitudinal Survey of Youth for 1997 (as does Maroto and Sykes, 2020) to quantify the effects of incarceration on future wage growth rates. By framing the prison system as a labor market institution, we measure the economic effects of incarceration while controlling for individual differences across the population. We 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 who have never been in prison. We use income growth rates as my dependent variable as opposed to static wages of given years, examining future trajectory rather than current standing. The goal is to make explicit the lifetime effect of incarceration on lifetime earnings, an effect which is implicit and largely ignored in the jurisprudence of sentencing.

1.2 Literature Review

Analyses of incarceration's effects on wages 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 subsequent illegal work relatively more appealing and attainable than legitimate opportunities (Lochner 2004; Rose & Clear 1998).

Until the late 1990s, 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. However, there is a recent appreciation for crime and social deviancy as a social phenomenon (Rose and Clear 1998), where crime may be ecologically determined via social structures including community foundations. As a result, retrogression would be most common among communities already deficient in social capital, as they would most quickly accept external "formal control" (Rose & Clear 1998).

Recent empirical research gives some weight to both individual and ecological factors in determining incarceration likelihoods (Pettit and Lyons 2009, Western 2002, Waldfogel 1994), finding that time served in prison, rather than a binary indicator of any prison term at all, is the most significant aspect in determining future wages (Western 2002, Waldfogel 1994). Increasingly nuanced approaches moved from exogenous wage growth rates to wage-age interactions, but the primary challenge has always been the potential endogeneity that underlies incarceration research. Individual fixed-effects modeling using a subset of the population at high risk of crime or delinquency, Western (2002) found that incarceration had a significant 7-19 percent depressing effect on future wages for persons who had been incarcerated. Pettit and Lyons (2009) confirmed a similar but smaller effect by comparing "early admits" (those who served their sentence in the first half of a 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. Kling (2006) added robustness to this literature by controlling for both inmate characteristics and individual judges' propensities for sentencing length, and found that once pre-incarceration income growth paths were considered, the length of sentence has no significant effect on post-incarceration earning within ten years of prison release. This circles nicely back to Waldfogel's canonic research on income and employer trust issues as related to criminality (1994).

2. Method

2.1 Data

To examine incarceration's labor market effects, we used the first sixteen rounds of the National Longitudinal Survey of Youth for 1997 (henceforth NLSY). Unlike most survey data, the NLSY includes both non-institutionalized and institutionalized respondents, as it begins surveying in high school, and includes inquiries about monthly incarceration status. As Western (2002) notes, use of the NLSY data allows analysis of both state and federal inmates simultaneously.

Consisting of roughly nine-thousand respondents aged 12 to 18 at first interview, and representative of the US population as of 1997, the NLSY 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 and provides a sample roughly similar to the national breakdown as reported by the Bureau of Justice Statistics (Bonczar 1997).

Previous literature suggests quite clearly that some variables are likely correlated with either future earnings or criminal activity. Table 1 includes the complete list of variables included in our final wage growth rate regression, including those used to determine propensity of incarceration. Following common practice in the literature, we reduced the categorical race variable to a white/non-white indicator variable. Similarly, we 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 received, we generated binary indicators for whether respondents ever accepted government assistance (excluding unemployment insurance and worker's compensation) or unemployment insurance (UI).

Respondents commonly missed at least one year of reporting over the 16 years of available data, causing gaps in the yearly income variable. To avoid losing potentially valuable continuity, we averaged income growth across those gaps but excluded all years with income less than $1,000 to the eliminate deceptively high growth rates that stem from ultra-low starting incomes.

Due to variation in prison sentence length and exit timing, the population is divided into three subsets by year of prison exit. The never-incarcerated sub-sample is identical across all sets, meaning each subset varied only by the ex-inmates included. To removed from consideration respondents who experienced multiple incarceration spells, a selection bias necessary for the chosen 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% of the incarcerated population. The second and third included those who had exited by 2007 and 2011, 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.

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Table 1. Means and Standard Deviations of Variables

	Ever-incarcerated		Never incarcerated		Total	
			Background			
Male	0.81		0.48		0.51	
White	0.41		0.53		0.52	
Black	0.36		0.25		0.26	
Hispanic	0.21		0.21		0.21	
No biological father present during childhood	0.60		0.42		0.44	
			Education			
ASVAB score	29370	(24196)	46739	(29151)	45323	(29169)
Highest degree received	1.22	(1.00)	2.41	(1.44)	2.31	(1.45)
			Delinquency			
Arrests by 1997	6.92	(6.80)	0.73	(2.31)	1.27	(3.46)
Fights by grade 12	1.04	(2.70)	0.34	(1.57)	0.40	(1.71)
Hard drugs by 1997	0.13	(0.34)	0.06	(0.23)	0.06	(0.24)
			Life Course (during sample)			
Total weeks worked	306.69	(188.0)	395.57	(197.9)	388.33	(198.6)
Total income growth	40.52	(100.0)	51.40	(154.6)	51.29	(154.5)
Observations		17226		180400		197626

In Table 1, notice that while the population as a whole is roughly evenly split by sex, 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. While marital status was not included in our model, 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. Since the NLSY ranks the highest educational degree attained as 1 for a GED, 2 for a secondary school diploma, and 3 for a two-year associate's degree, the average educational attainment of the control and treatment samples are on either sides of a high school degree but not wildly different in terms of lifetime earning potential.

Interestingly, the incarcerated group's number of weeks worked during the entire survey period is within half a standard deviation of the general population, despite time forfeited by prison spells. Presumably, this is in part because of a shorter average educational period and therefore longer work life by the same age as those never incarcerated. Notice that the average in our entire sample is just under 400 weeks (or roughly 8 years) of work, as the NLSY sample is still relatively early in their lifetime of work.

While not immediately obvious in Table 1, there are variables across which the groups are similar; parental education, months in primary school, income at age 21, and time on unemployment insurance are all fairly consistent across groups, though these variables were not included in the final models.

2.2 Methodology and Model Specification

Using a treatment effects model specification, we examine variation in wage growth rates (the outcome) with respect to having been incarcerated (the treatment). As treatment effects models assume random participation while incarceration selection is known to be non-random, we 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 matching technique that controls for missing information better than its predecessors, while boasting doubly robust coefficient estimates (Wooldridge 2007; Cattaneo 2010). Naturally, 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. The technique is best explained piecemeal.

Propensity score matching is essential in order to discern the effect of selection bias on the effects of a prison sentence. To truly know how the treatment (prison term) influenced the outcome, we would need to observe a situation with no treatment ($Y \mid T=0$), and simultaneously observe the same situation with treatment administered ($Y \mid T=1$). In reality we must attempt to mimic this ideal using the information and techniques available, and propensity score matching (PSM) was developed to address this challenge (Horvitz & Thompson 1952). The PSM technique "grades" each participant according to a selection of variables, then matches treatment and control individuals by their scores. Matching allows us to observe both a treatment and relatively similar non-treatment situation; naturally, as the matching system improves we will approach the ideal situation (Gertler et al. 2016).

Propensity score matching has been implemented in previous incarceration research to conclude that funding prison education and training programs has a dramatic cost saving effect in the long run (Sedgley, Charles, Nancy, and Frederick 2010) and that the penal system helps to explain subsequent health outcomes (Massoglia 2008). So PSM is an established technique in this strand of the literature.

To building propensity scores, P(xi), we specify a logistic regression conditional on a vector of k observable covariates (Apel & Sweeten 2010) to explain incarceration:

$$P(x_i) = Prob(T_i=1 \mid x_i) = - [exp(X^T{}_i\beta)] / [1 + exp((X^T{}_i\beta)] \quad (1)$$

Where Ti is the ever-incarcerated indicator variable (or "treatment"), xi is the vector of theoretically predictive covariates, and β are the estimated coefficients to indicate relatedness of each potential covariate. The challenge is to permit sufficient overlap between the treatment and non-treatment groups (Caliendo & Kopeinig 2008) while predicting prison terms effectively. Kernel densities and Bayesian Information Criterion scores were used to select and limit the covariates included at this stage of the model.

Next, we used inverse probability weighting to reduce the chances of spurious or un-useful covariates obtaining equal weight in the analysis. Reweighting is based on the inverse of how likely the individual is to be observed in his or her actual group. 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. Those whose outcomes are in line with their scores receive an uninflated weighting. Regression adjustment (RA) provides the second stage of analysis technique, now modeling (potentially post-incarceration) incomes using predicted IPW-adjusted probabilities for each individual as an explanatory variable, to control for non-random treatment. An average treatment effect is then 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 (Wooldridge 2007). To balance sample size and statistical significance, we chose a minimalist model of the following covariates: total months incarcerated, highest degree received, total weeks worked from 1997 – 2013, and ASVAB scores. Controls for industry, socioeconomic background, and household characteristics unfortunately curtailed the sample too tightly via covariate correlation.

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 placed under stress. For this reason, we 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 considered with caution, understanding the limitations of dealing with a small population in conjunction with unlikely treatment assignment.

3. Results

Table 2 displays information for the estimated total impacts at the top: while baseline predicted lifetime income growth is 54 to 63 percent for each cohort, incarceration reduces that by roughly one-third (i.e., by 16 to 22 percentage points). This is a staggeringly large penalty over an average of only eight years of work; it penalizes an annual growth rate of 6.4 to 6.9 percent for those never incarcerated to an annual growth rate of 4.3 to 5.0 percent for the ever-incarcerated. Extrapolating that result across a 40-year working lifespan for an individual starting with an annual income of $25,000 results in a lifetime earnings loss by as much as $2.3 million (or 48 percent of lifetime potential earnings), all other things held equal. Results are all statistically significant at the 99.9 percent level.

Robustness tests with additional variables (such as industry information and individual's income at age 21) both showed lower incarceration penalties, but also relied on a dramatically smaller sample size and therefore have not been reported here.

Notice that the apparent penalty to "weeks worked" in the control group is presumably the result of education; degree attained has a large positive effect on income growth but is associated with fewer weeks worked by those who spent years in college. That same tradeoff is presumably less at play in the treatment group, whose time in the workforce was reduced by a prison term rather than educational reasons.

We find it intriguing that ASVAB scores are small and largely insignificant for the control group but actually impact income growth negatively for the treatment group. One conjecture consistent with this result is that conditional income expectations are higher for individuals with higher ASVAB scores, so those incarcerated have been matched with peers who reveal the ever-incarcerated to be more punished if their educational potential was higher.

Coefficients for the ever-incarcerated population indicate a potentially paradoxical positive effect of length of stay on income growth, with each additional month incarcerated associated with a five- to seven-percentage point boost in average income growth. Kling (2006) found a similar effect in his 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.

The treatment model is the foundation for the IPWRA estimator, and can be found at the bottom of Table 2. Coefficient values and significance levels of the treatment model are not or primary interest, but they appear to accord with intuition. 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 (2010) used propensity scores with an R^2 of 0.187.

The results of the IPWRA model, while statistically significant, have considerable limitations. The NLSY data contains a narrow age range, 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. Long-term data would allow for stratification of growth periods by age, which could model (potentially non-linear) 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. Finally, given the high frequency of recidivism, it would be helpful to examine wages over multi-spell incarcerations, but such a study would require a very long reporting period to consider post-spell income effects.

Table 2. Final IPWRA Model

	2003 Cohort		2007 Cohort		2011 Cohort	
Treatment effect (average lifetime income growth in percent)						
Baseline	54.16	(281.66)***	54.98	(165.57)***	62.92	(19.42)***
Ever incarcerated	-19.38	(14.26)***	-15.71	(16.53)***	-21.84	(6.72)***
Control group: income for those never incarcerated (average lifetime growth in percent)						
Total months incarcerated	n/a		n/a		n/a	
Highest degree received	5.652	(37.05)***	6.147	(17.98)***	21.77	(2.30)*
Total weeks worked	-0.0477	(30.35)***	-0.0441	(16.82)***	-0.0201	(2.63)**
ASVAB score	1.60×10^{-5}	(1.78)	-4.18×10^{-5}	(1.18)	-1.19×10^{-3}	(2.47)*
Constant	29.92	(6.72)***	34.46	(12.37)***	32.10	(14.74)***
Treatment group: income for ever-incarcerated (average lifetime growth in percent)						
Total months incarcerated	0.0686	(0.28)	0.728	(3.08)**	0.529	(5.74)***
Highest degree received	2.257	(3.28)**	-2.289	(4.51)***	2.697	(5.86)***
Total weeks worked	0.0546	(7.65)***	0.0252	(5.51)***	0.0211	(6.05)***
ASVAB score	-5.15×10^{-4}	(6.32)***	-4.77×10^{-6}	(0.14)	-1.55×10^{-4}	(6.05)***
Constant	29.92	(6.72)***	34.46	(12.37)***	32.10	(14.74)***
Treatment model (Probability of incarceration)						
Fights by grade 12	-0.131	(8.44)***	-0.0959	(5.94)***	-0.0114	(0.94)
White	0.888	(11.75)***	0.572	(11.64)***	0.196	(5.14)***
Arrests by 1997	0.100	(16.86)***	0.119	(11.06)***	0.169	(12.90)***
Hard drugs by 1997	0.551	(5.85)***	0.607	(8.97)***	0.488	(7.69)***
Highest degree received	-0.860	(29.06)***	-0.703	(31.77)***	-0.566	(28.68)***
No biological father present during childhood	0.224	(3.25)**	0.161	(3.49)***	0.254	(6.71)***
Constant	-3.821	(47.55)***	-3.089	(53.89)***	-2.772	(49.36)***
Observations		5232		5258		5352

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

4. Discussion

In accordance with the findings of Western (2002) our work indicates a dampening effect of incarceration on wage growth in the lifetime. Using inverse probability weighted regression adjusted treatment models (IPWRA), we 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 substantial average growth rate reductions of 25% or more which could reduce lifetime earnings by half, after factoring in education, time spent working, and other salient factors.

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 belief 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 U.S. penal system plays as a labor market institution. Due to data limitations, our 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. Future research should address the comparative cost of rehabilitation programs for non-violent offenders, to better understand the most welfare-optimizing means of social control and might consider "justice reinvestment" models (Austin et al. 2013). An examination of treatment effects by race would help clarify the proportionality of prison labor influence, but will require larger datasets on ex-inmates.

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