Criminal conviction and incarceration: Impact on employment outcomes for former offenders

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1 Introduction

A recent upsurge in popular interest in issues within the criminal justice system have spurred a broad effort for reform. Rather than focusing solely on punitive measures of justice, a growing sense of responsibility for the outcomes of former offenders has emerged. Nearly 3% of the United States population is either incarcerated or subject to other correctional supervision such as probation, as noted in Glaze and Kaeble (2014). This suggests how we structure our treatment of criminal offenders have a substantial impact on society as a whole.

In this same thread, here we are attempting to identify the mechanism by which economic outcomes of offenders are depressed, providing some potential insight in directing future policy. Considering which of several possible reasons for this negative effect on employment is therefore important. These include skill loss due to incarceration, employer bias against former offenders, and other disruptions in social and work attachment. There are however many factors that increase the likelihood of incarceration while simultaneously reducing employment outcomes, including a history of poverty, exposure to violence, and biases against minority groups. This paper attempts to identify the effect on employment and income from incarceration and criminal conviction separately.

2 Literature review

In recent years an increasing number of well constructed articles have sought to estimate the effect of incarceration employment outcomes. In Kling (2006) the author discusses how the length of a prison stay affects employment and earnings for postincarceration individuals. They found small to negligible effects on post incarceration earnings for those with sentences exceeding seven years, but found a positive effect from longer sentences amongst released prisoners who served up to six year.

This paper presents an interesting look at released prisoner outcomes, but raises a number of concerns generally and specifically in applicability to policy. One area of concern is their interesting use of judge assignment as an instrumental variable. The author suggests that the random assignment of judges effects sentencing but is otherwise unrelated to employment outcomes.

Unfortunately, the author demonstrates this makes a weak instrument[1]. After controlling for some observed factors affecting sentencing and employment prospects, they use a 2SLS approach to estimate the impact of sentence length on employment outcomes. Despite this, they only find a statistically significant effect along a subset of sentence ranges.

There are a few more concerns presented by the paper. The analysis uses only officially reported income, which may be a concern given the likelihood of former prisoners earning income in the informal economy. Also, the paper looks only at released prisoners from the federal prison population in California, and the state prison population in Florida. These prisoners may differ substantially from the larger number of low level offenders likely to be used in my analysis.

Sweeten and Apel (2007) estimate the crime reduction resulting from incarceration of individual offenders. They match incarcerated individuals with similar unincarcerated individuals using propensity score matching to provide a suitable counterfactual population. By using NLSY97[2] data with self reported criminal activity they provide a reasonable estimate of crimes that would be committed by those who are incarcerated.

The authors used time-invariant and initial period covariates to account for behavioral, economic, and demographic determinants of criminal behavior and incarceration. Notably, they excluded a small number of individuals with pre-survey (very young) history of incarceration. Using the preincarceration covariates they estimate individual propensity scores, the likelihood of incarceration, using a logit function. When checking covariate balance, they use a very large set of variables in addition to those used for determining the propensity score. They find these are well balanced, lending some support to the Conditional Independence Assumption despite the possibility of unobserved confounding variables.

The authors tried a few variations on matching, but chose a form of kernel matching that yielded the best covariate balance. However, there was poor overlap in some propensity score regions resulting in the authors dropping a small number of incarcerated individuals. This may result in downward bias in the estimation of crime reduction. The authors note that incarcerated individuals demonstrate a crime 'spurt' leading into their arrest and incarceration.

[1] The author uses a technique to correct for bias from this weak instrument, but I'm unfamiliar with the method used.

[2] National Longitudinal Survey of Youth 97

This presents a potential issue when comparing jailed offenders with a similar unincarcerated population, as they may differ in marked ways, some of which are unobserved.

In all, Sweeten and Apel (2007) concludes that 1 year of youth incarceration prevents 6.2 to 14.1 offenses and adult incarceration prevents 4.9 to 8.4 offenses.

Apel and Sweeten (2010) provides an excellent starting point for our own analysis. Here, the authors look at postincarceration employment. They motivate their paper by explaining why being a released prisoner may reduce employment prospects. It may provide a signal to employers that the released offender may potentially be a risk to employ. It may also be associated in the mind of potential employers as associating formerly imprisoned individuals as being lower class, or part of an undesirable cultural element. Also, prisoners may adapt to the offender subculture and become detached from the legitimate work culture. In addition, incarceration reduces human capital accumulation and creates experience gaps. Alternatively, incarcerated individuals gain some degree of training in networking amongst other offenders, potentially increasing opportunities in criminal activity when released.

The author contrast their experimental ideal, random assignment of incarceration, with reality. Imprisoned individuals generally face worse legal labor outcomes than the unimprisoned regardless of incarceration. They note any study looking at employment and incarceration must deal with selection bias. Apel and Sweeten (2010) observe that incarcerated men earn substantially less than their counterparts prior to incarceration. Also, a disproportionate number of the incarcerated are from disadvantaged groups that face difficulty and discrimination in employment.

In their discussion of selection for 'treatment', the authors suggests those working in the criminal justice system are using factors unobserved in the data to make decisions about likelihood of future criminality when making sentencing decisions. The current method is to use measure of pretreatment, self reported, criminal activity to select a comparison sample. Unfortunately, there may be systematic differences between these treated and control populations that are observed at the time but not in the data that introduces bias into estimates. For this reason, they chose controls from those who are convicted but not incarcerated, and in another model, those arrested but never convicted.

The authors attempt to address issues they identify in previous work, including concerns over treating all nonemployed former inmates as unemployed rather than allowing them to be out of the labor force. They also note their NLSY97 data allows them to include illegal employment and income, which is usually not recorded and may lead to understated income.

To estimate the effect on employment, the authors use two strategies using convicted individuals. They use both a fixed effect model and, alternatively, matching using propensity score. In the twoway fixed effect estimation the authors find incarceration reduces the probability of future formal employment by about 11%. Using matching the authors find relatively similar results. Interestingly, they find an uptick in illegal activity following incarceration. Though somewhat limited by the sample period range, the data also suggests that the incarcerated/unincarcerated wage gap increases over time.

In all, Apel and Sweeten (2010) provides an excellent starting point for further work on offender employment outcomes. They use panel data that has continued to be added to for several additional years. Notably, they exclude pre-observation convicted individuals, and they normalize the time periods used to zero for interview wave in which the individual is convicted for the first time. Also, their use of time-invariant and pretreatment covariates to estimate propensity scores for incarceration provide an excellent starting point. They do however, by normalizing time to zero for the year first convicted of a crime, assume that the propensity to be incarcerated is constant over time.

Another interesting, and quite recent, paper by Ramakers et al. (2012) provides a look into criminal justice outcomes in the Netherlands. The authors consider the effect of incarceration beyond the negative effect expected from a similar period of unemployment, hoping to identify specific mechanisms.They compare labor outcomes for released prisoners and a similar group of unemployed future prisoners over a two year period.

A few notable features of their study include distinguishing between crimes leading to conviction, and control for criminal history and background characteristics through propensity score matching. Unfortunately, and likely due to the heavily right skewed age distribution of offenders, the later aged future offenders in their sample have different criminal activity characteristics. Most notably, unemployed future prisoners are dramatically more likely to have committed a violent crime (39.5% vs 28.2%). Aside from being a function of the later age of criminal conviction in this group, their likelihood of criminal activity would seem to be increased given their unemployment.

Also troubling, the future prisoners includes previously incarcerated individuals. This muddles the interpretation as they would also face the same employment reducing stigma as recently released prisoners. For these and a few other reasons mention in the paper, this study may have significant issues with bias.

3 Data

This paper uses the National Longitudinal Survey of Youth 97 data provided by the Bureau of Labor Statistics. This contains panel data for 8984 individuals with collection beginning in the year 1997. All data are collected from residents of the United States who are age 12 through 16 during the initial survey period in 1997. Survey periods continue through 2013, though there's a gap in several key variables in 2012. For most of the analysis the data have been separated into person-year observations, for a total of 143744 observations.[3]

This data contains a wealth of information about criminal activity, arrests, incarceration, and some information about behavior associated with crime. The early age of first collection provides some information about the survey respondents home environment prior to contact with the criminal justice system. Also included are variables related to mental health and tendency towards risky behavior which provides an interesting set of covariates to utilize when matching.

To measure the effect on employment outcome we'll be using yearly income from wages and salary, as well as weeks worked in the previous year for comparison. The primary effect of interest is how criminal conviction, separate from incarceration, affects employment outcomes. As such, both variables have been included. To attempt to separate the affect of current incarceration and conviction from current income we also have cumulative prior year counts of months of incarceration and a dummy variable for criminal conviction.

In addition to the primary variables of interest, additional demographic covariates are included in some specifications. Additional factors that may influence criminal justice outcomes and employment outcomes are also included in some models.[4] These include self reported criminal activity, such as theft,

[3]This excludes year 2012 observations, for which there are data on some criminal behavior but not income or other covariates of interest.

[4]See appendix for the comprehensive covariate summary tables

Table 1: Summary Statistics by Age

	12 to 16	17 to 20	21 to 24	25 to 28	29 to 32
Income	$400	$3722	$10736	$18445	$22128
	(1248)	(6613)	(14561)	(22871)	(29509)
Weeks worked	11	26	29	29	28
	(18)	(21)	(22)	(23)	(24)
Arrests	0.082	0.093	0.066	0.038	0.023
	(0.447)	(0.581)	(0.572)	(0.303)	(0.305)
Convictions	0.03	0.036	0.027	0.019	0.028
	(0.219)	(0.224)	(0.192)	(0.159)	(0.195)
Months Incarcerated	0.012	0.078	0.127	0.137	0.112
	(0.315)	(0.801)	(1.084)	(1.152)	(1.031)

Mean values; Std. dev. in parenthesis

Table 2: Summary Statistics by Demographic Factors

	Male	Female	Urban	Suburban	Rural	Black	Hispanic	Mixed	Other
Sample	51.2%	48.8%	33.7%	41.6%	4%	26%	21.2%	0.9%	51.9%
	(50)	(50)	(47.3)	(49.3)	(19.6)	(43.9)	(40.8)	(9.6)	(50)
Income (29 to 32)	$25951	$18171	$26948	$28966	$21677	$16165	$20762	$19688	$25797
	(33296)	(24368)	(30926)	(30560)	(26793)	(23959)	(25270)	(28214)	(32964)
Weeks Worked (29 to 32)	28	28	34	35	33	27	29	22	28
	(24)	(24)	(21)	(21)	(21)	(23)	(24)	(23)	(24)
Ever Arrested	68.3%	31.7%	36.2%	40.6%	5.2%	30.6%	21.3%	1.1%	47%
	(46.5)	(46.5)	(48.1)	(49.1)	(22.3)	(46.1)	(40.9)	(10.4)	(49.9)
Ever Convicted	72%	28%	43%	48%	9%	32%	18%	1.5%	48.5%
	(45)	(45)	(49.6)	(50.1)	(28.7)	(46.8)	(38.5)	(12.2)	(50.1)
Ever Incarcerated	80.6%	19.4%	34.6%	38.9%	8.2%	36.4%	21.2%	1.3%	41.2%
	(39.6)	(39.6)	(47.6)	(48.8)	(27.4)	(48.1)	(40.9)	(11.2)	(49.3)

Income and Weeks Worked values are annual means; Std. dev. in parenthesis

robbery, fighting/assault, and gang participation. A few risk factors, such as interactions with firearms and running away from home are included as well. Initial survey responses to a few potentially interesting questions regarding the participant's optimism, religiosity and their parent's parenting style have been included. The survey participant's highest grade completed and high school grade point average were also included, as this would be expected to have a significant impact on income and propensity to committing crime.

4 Theoretical framework

There are a number of ways in which criminal activity could impact earnings. Some pecuniary criminal activity would increase income, though it's unclear if respondents would or would not choose to include these in their survey response regarding income.[5] Alternatively, engaging in criminal activity may increase as legal income decreases, either because of reduced opportunity cost (if ap-

[5] Survey respondents are asked about income from selling drugs and property crimes

prehended) or general disaffection leading to antisocial behavior. A history of criminal behavior may affect one's propensity to commit further crimes, either by reducing the perceived likelihood of apprehension if they aren't arrested, or as a result of legal consequences.

Of more concern is how individuals respond to and are affected by legal consequences to criminal behavior. According to Human Capital theory incarceration removes an offender from possible work during the course of their incarceration. This leads to a degradation of work skills, or even a failure to accumulate work skills and functional behavior during formative years. While well run vocational programs for prisoners may reduce some of the negative effect, we would still expect to see a significant negative impact on employment outcomes for released prisoners that increases as the length of their incarceration increases.

Criminal conviction, both with subsequent incarceration and without, may decrease employment outcomes due to strong social stigma against criminals, particularly in hiring. In Signaling theory, as described by Holzer et al. (2007), a history of conviction and incarceration is often interpreted as a signal of poor work ethic and poor skills. If incarceration is a strong driver of decreased earnings and employment, we would expect to see negative effects from increasing length of incarceration. If a history of conviction, when controlling for incarceration, still reduces future income, this would suggest that stigma affects the earnings potential of offenders.

5 Econometric model and methods

To begin, a pooled OLS estimation was used to estimate the effect of prior convictions on log values of individual annual income. Alternatively, a similar estimation was conducted with weeks an individual worked in the last year period as the dependent variable.

$$ln(income_{it}) = \beta_0 + \beta_1 conviction_{it} + \beta_2 incarcerated_{it} + u_{it}$$

After this, a model incorporating several likely relevant covariates were included, each of which affects income and/or affects criminality.

$$ln(income_{it}) = \beta_0 + \beta_1 conviction_{it} + \beta_2 incarcerated_{it} + X_{it}\beta + u_{it}$$

Next, as there are likely many individual, time invariant characteristics not accounted for in the previous specifications a fixed effects model was estimated. Again, this was done with both log annual income and with weeks worked in the preceding year period.

$$ln(income_{it}) = \alpha_i + \beta_0 + \beta_1 conviction_{it} + \beta_2 incarcerated_{it} + X_{it}\beta + u_{it}$$

However, when attempting to estimate the effect of criminal justice mechanisms on offenders and potential offenders it's important to recognize that the vast majority of individuals will not have contact with the criminal justice system.[6] Modeling of employment outcomes of convicted and incarcerated individuals may be muddled by comparison with a general population that has fundamentally different characteristics. To address these concerns matching was used to construct a comparison population of nonconvicted individuals who otherwise resemble those who have been convicted in our larger sample. This 'control' group have been selected based on a similarity, amongst a large number of covariates, with the 'treated' group of convicted individuals.

6 Analysis and Results

The initial pooled OLS model, with only conviction and incarceration history, appears to provide interesting results. While months of incarceration appear to have a negative, economically significant impact on wages and weeks worked, previous criminal convictions appear to increase income by 54% and weeks worked by 1.07. There may be several possible reasons for this counterintuitive result, including increasing dependence on employment and likelihood of criminal conviction as people age.

Once more demographic characteristics are incorporated a more intuitive picture emerges. Incarcerations continue to have a negative effect on income, with a stronger effect for months of incarceration in the most recent year. Each month of incarceration in the current year is associated with about a 13.8% decrease in income and a 1.1 week employment reduction. For prior years there's about a 3.7% decrease in income and an economically insignificant loss of about half a day of work per month incarcerated. All of these impacts

[6] Even in the United States.

Table 3: PRE match

	Dependent variable:					
	ln(income)			weeks.worked		
	OLS	OLS	Fixed	OLS	OLS	Fixed
convictions(prior)	0.429^{***}	-0.070^{*}	-0.227^{***}	1.068^{***}	-1.318^{***}	-2.860^{***}
	(0.042)	(0.039)	(0.053)	(0.187)	(0.177)	(0.237)
convictions(current)		-0.294^{***}	-0.332^{***}		-0.382	-1.112^{***}
		(0.075)	(0.072)		(0.345)	(0.321)
incarceration(prior)	-0.064^{***}	-0.038^{***}	-0.040^{***}	-0.296^{***}	-0.087^{***}	-0.080^{***}
	(0.003)	(0.003)	(0.003)	(0.013)	(0.012)	(0.014)
incarceration(current)		-0.148^{***}	-0.116^{***}		-1.116^{***}	-0.895^{***}
		(0.013)	(0.012)		(0.058)	(0.055)
arrests(prior)		-0.088^{***}	-0.089^{***}		-0.452^{***}	-0.421^{***}
		(0.006)	(0.009)		(0.026)	(0.039)
arrests(current)		-0.041^{*}	-0.030		-0.599^{***}	-0.552^{***}
		(0.022)	(0.022)		(0.103)	(0.098)
covariates	no	yes	yes	no	yes	yes
constant	4.585^{***}	-17.802^{***}		24.900^{***}	-111.758^{***}	
	(0.013)	(0.238)		(0.061)	(1.091)	
Observations	143,744	143,744	143,744	152,728	143,744	143,744
R^2	0.004	0.324	0.257	0.004	0.369	0.327
Adjusted R^2	0.004	0.323	0.241	0.004	0.368	0.307
Residual Std. Error	4.652	3.834		22.253	17.607	
F Statistic	254.038^{***}	$1,041.753^{***}$	$1,014.153^{***}$	274.578^{***}	$1,271.425^{***}$	$1,425.877^{***}$

Note: *p<0.1; **p<0.05; ***p<0.01; Robust standard errors in parenthesis appear to be statistically significant. Arrests have been incorporated into this model, and show a statistically significant negative affect from recent and earlier arrests.

Previous criminal convictions, under this more detailed pooled OLS model, appears to be only statistically significant at a 10% confidence, with a negative effect on income. Interestingly, there does seem to be a statistically significant impact on weeks worked from prior convictions, but none from current year convictions. A history of convictions in previous years appears to reduce weeks worked by 1.3 weeks, and current year convictions reduces income by 25.4%.

Given the strong likelihood of important, omitted individual characteristics that may affect these results fixed effects panel estimation was also used. Here, there does appear to be a statistically significant downward affect on income and weeks worked from previous and current year criminal convictions, and previous and current year incarceration. Prior convictions reduce income by 20% and current year convictions by 28%, with a reduction in weeks worked of 2.9 and 1.1. Incarcerations remain significant and of a similar scale as in the pooled OLS.

Matching was done using propensity scores estimated on a set of time invariant and initial 1997 survey period variables, as well as crime linked current period covariates likely to affect an individual's likelihood of contact with the criminal justice system[7]. First, fitted propensity score values, indicating the likelihood each person year observation would have ever (up to current observation year) been convicted of a crime, were generated using a logit model. A graphical check for overlap between treated and control propensity scores shows sufficient overlap for matching on the generated propensity scores.[8]

Using inverse propensity score weighting the pooled OLS specifications were reestimated.[9] [10] Oddly, this yields statistically insignificant effects from the variables of interest in the pooled OLS model using relevant covariates. In the simple OLS model, there is a statistically significant negative impact on income from prior convictions of -95%. This suggests the average treatment effect of incarceration is immense. Alternatively, the simple OLS regression

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[7]See appendix for complete list of variables, as well as pre and post matching summary statistics.

[8]see appendix for pscore overlap graph.

[9]$invpw = \frac{1}{pscore}$ if $ever.convicted = 1$, and $\frac{1}{1-pscore}$ if $ever.convicted = 0$

[10]I was unable to figure out how to use fixed effects estimation with pweights.

Table 4: Inverse Propensity Score weighting

	Dependent variable:			
	ln(income)		weeks.worked	
	OLS	OLS	OLS	OLS
convictions(prior)	-2.968^{***}	-1.942	0.663	-243.374
	(0.905)	(4,683.984)	(73,155.630)	(24,964.470)
convictions(current)		-6.215		-172.318
		(10,725.700)		(57,165.330)
incarcerations(prior)	0.200	-0.388	1.718^{***}	-3.682
	(0.279)	(10.369)	(0.021)	(55.262)
incarcerations(current)		3.174		32.647
		(66.549)		(354.690)
arrests(prior)		-0.101^{***}		-8.631^{***}
		(0.004)		(0.023)
arrests(current)		-0.050^{***}		-8.257^{***}
		(0.004)		(0.022)
covariates	no	yes	no	yes
Constant	7.324^{***}	31.534	20.942^{***}	-738.198
	(0.778)	(825.506)	(0.056)	(4,399.739)
Observations	143,744	143,744	143,744	143,744
R^2	0.020	0.877	0.045	0.890
Adjusted R^2	0.020	0.877	0.045	0.890
Residual Std. Error	3,441,047.000	1,218,758.000	19,150,496.000	6,495,678.000
F Statistic	$1,438.503^{***}$	$15,531.900^{***}$	$3,422.044^{***}$	$17,654.280^{***}$
Note:	*p<0.1; **p<0.05; ***p<0.01; Robust standard errors in parenthesis			

on weeks worked in the previous year suggests no effect from convictions but a loss of 1.7 weeks of work in the last year per month of incarceration.

In another specification, calculating the average treatment affect on the treated (ATT) suggests a criminal conviction reduces income for a convicted individual by 34%.[11] An estimation of the ATT of criminal conviction on weeks worked in the previous year suggests a reduction of 1.6 weeks.[12]

Next, to control for other covariates, the original pooled and fixed effects regressions were run again on the postmatch data. The relatively simple model, regressing income on prior convictions and incarcerations only, shows a more intuitive effect than previously with the initial unmatched data. Here, prior convictions now decreases income by 18%. A similar value, decreasing income by 16%, is found when controlling for relevant covariates. However, postmatch estimates for the effect of convictions on income are approximately the same as they were using the prematch data. Using postmatch data with the fixed

[11] ai SE of 0.050907

[12] ai SE of 0.23109

Table 5: POST match

	Dependent variable:					
	ln(income)			weeks.worked		
	OLS	OLS	Fixed	OLS	OLS	Fixed
convictions(prior)	0.402***	−0.053	−0.193***	1.762***	−0.754***	−2.004***
	(0.045)	(0.041)	(0.056)	(0.206)	(0.189)	(0.251)
convictions(current)		−0.254***	−0.269***		−0.023	−0.497
		(0.077)	(0.073)		(0.354)	(0.330)
incarceration(prior)	−0.064***	−0.036***	−0.038***	−0.301***	−0.083***	−0.069***
	(0.003)	(0.003)	(0.003)	(0.014)	(0.013)	(0.015)
incarceration(current)		−0.148***	−0.120***		−1.124***	−0.920***
		(0.013)	(0.012)		(0.059)	(0.056)
arrests(prior)		−0.081***	−0.080***		−0.386***	−0.294***
		(0.006)	(0.009)		(0.027)	(0.041)
arrests(current)		−0.037	−0.027		−0.598***	−0.489***
		(0.023)	(0.022)		(0.106)	(0.101)
covariates	no	yes	yes	no	yes	yes
constant	4.612***	−17.491***		24.060***	−100.491***	
	(0.021)	(0.369)		(0.097)	(1.700)	
Observations	66,288	66,288	66,288	66,288	66,288	66,288
R^2	0.008	0.303	0.228	0.008	0.325	0.277
Adjusted R^2	0.007	0.303	0.214	0.008	0.324	0.260
Residual Std. Error	4.660	3.906		21.818	18.007	
F Statistic	251.320***	436.978***	399.764***	252.403***	482.254***	518.236***
Note:	*p<0.1; **p<0.05; ***p<0.01; Robust standard errors in parenthesis					

effects model again returns a negative effect from prior convictions of 17%.

Finally, using post matching weights for individual person years was applied to each person observation (for all years) to generate an approximate ATT. In the following specifications, individuals who have been convicted are compared with a similar group (based on propensity score matching) of individuals who have never been convicted of a crime. The initial estimates of conviction and incarceration on the population might be assumed to be downward biased as we expect there to be unobserved individual characteristics that decrease income and employment while also increasing the propensity of individuals to commit crimes. Fittingly, when using matching we see an attenuation of the negative coefficient estimates for our variables of interest. However, they are largely of a similar magnitude to prematch estimates.

7 Conclusions

Overall, estimation using post matching data and log income as the dependent variable provides coefficient estimates that are relatively consistent with prematch estimates. Simple estimates of ATT, as well as ATE estimates using inverse propensity score weighting, differ in value but not direction of effect. This strongly suggests that criminal convictions significantly reduce wage and salary income beyond the negative effect from incarceration alone.

Offenders who are convicted of a crime, even if they are given probation, treatment, or otherwise serve no time, can see a decrease in annual income of about 24% in the first year following conviction, and about a 18% decrease in subsequent years. Initial decreases may be caused by work life disruptions associated with court and any post conviction court requirements. However, it is plausible that much of the effect persists due to stigma attached with criminal behavior. Notably, formerly convicted individuals see, on average, a reduction of 2 weeks worked per year, but no reduction directly from conviction in the current year weeks worked.

Beyond the effect from conviction, incarcerated individuals can expect to see additional negative effects on their earnings. For an offender serving the median sentence of six months we would expect them to earn about 51% less, relative to those serving no sentence, within the following year. We would expect them to earn about 19% less in subsequent years. The large initial loss of income is undoubtedly directly due to incarceration limiting income. The continued loss of income in later years though is likely a result of skill loss and disruptions in work networks that aren't fully recovered from, as they appear to affect hourly earnings rather than employment directly. The same six month sentence would, unsurprisingly, reduce weeks worked by about 5.5 weeks. However, a history of incarceration of six months would only reduce annual weeks worked by 3 days.

It appears a history of conviction, and less so incarceration, is a persistent driver of lower wages for offenders. In the absence of consideration regarding crime reduction, and concerning ourselves only with improving outcomes for former offenders, this suggests some policy options. Reducing convictions, perhaps through drug courts or other diversion programs, can potentially improve long term employment outcomes. Alternatively, it may be possible to mitigate some of the negative effects from conviction through 'ban the box' initiatives. 'Ban the box' laws prevent employers from asking, at least initially in the job application process, if the applicant has a criminal conviction.

Further investigation into specific mechanisms may be warranted. Further, there may be a significant issue with endogeneity in these and other models of exoffender employment outcomes given the likelihood of low legal employment prospects increasing one's tendency towards criminality.

8 Appendix

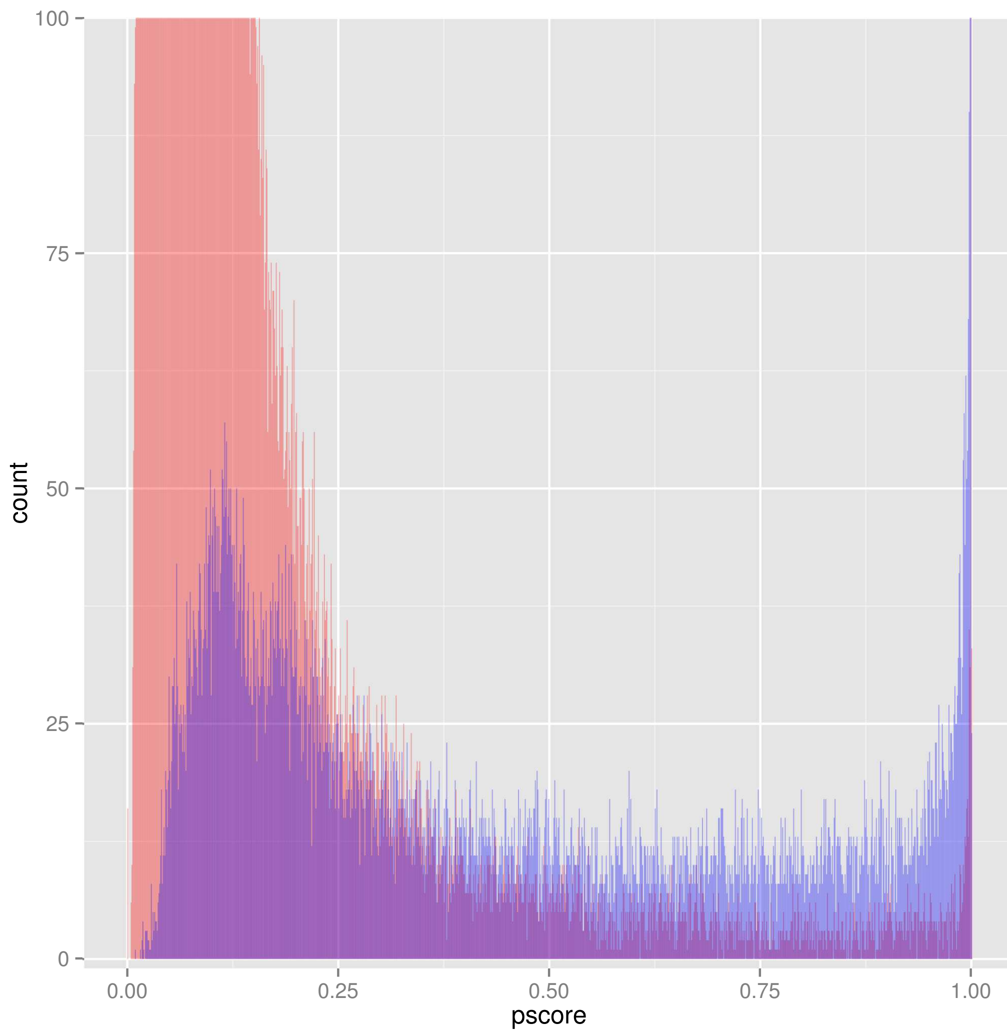

Table 6: Pre and Post match covariates: table 1 of 2

	Pre Match			Post Match		
	Means Treated	Means Control	SD Control	Means Treated	Means Control	SD Control
distance	0.4594	0.0756	0.1155	0.4594	0.2662	0.2236
arrests.prior	3.4218	0.2774	1.3503	3.4218	1.6615	3.2379
arrests.current	0.3156	0.0314	0.3473	0.3156	0.179	0.8888
age	23.4461	21.8045	5.019	23.4461	23.5929	4.9253
black	0.2774	0.2575	0.4372	0.2774	0.2624	0.4399
hispanic	0.2027	0.2128	0.4093	0.2027	0.1972	0.3979
mixed	0.0122	0.0088	0.0935	0.0122	0.0164	0.1272
male	0.7379	0.4803	0.4996	0.7379	0.7842	0.4114
expect.best	0.3841	0.4549	0.498	0.3841	0.365	0.4814
rarely.expect.good	0.2573	0.2677	0.4428	0.2573	0.243	0.4289
optomistic	0.4217	0.4932	0.5	0.4217	0.3978	0.4895
hardly.expect.go.your.way	0.2214	0.213	0.4094	0.2214	0.2027	0.402
region0	0.1246	0.1438	0.3509	0.1246	0.1181	0.3227
region1	0.1282	0.1422	0.3493	0.1282	0.1196	0.3245
region2	0.2245	0.1819	0.3858	0.2245	0.225	0.4176
region3	0.3358	0.3383	0.4731	0.3358	0.3353	0.4721
region4	0.187	0.1938	0.3953	0.187	0.202	0.4015
citizen	1.0351	1.0998	0.7118	1.0351	1.016	0.66
MSA.city	0.3499	0.2993	0.4579	0.3499	0.3531	0.4779
MSA.suburb	0.4195	0.4097	0.4918	0.4195	0.4336	0.4956
MSA.rural	0.102	0.1274	0.3334	0.102	0.0938	0.2915
hh.income	29072.4209	34685.659	42163.4723	29072.4209	28844.8153	35594.5013
highest.grade.dad	6.8837	8.3941	6.8894	6.8837	6.8483	6.645
highest.grade.mom	10.5246	11.3144	5.0536	10.5246	10.6288	5.2392
highest.grade	7.5913	8.4876	5.4777	7.5913	7.9024	5.5909
gpa.weighted	1.4913	1.9384	1.4244	1.4913	1.4906	1.3361
gen.health	2.0248	1.8098	1.1389	2.0248	2.0809	1.2001
carried.gun.12months	0.0679	0.0354	0.1848	0.0679	0.0712	0.2571
carried.gun.30days.count	0.4938	0.2584	2.4163	0.4938	0.5163	3.3775
gangs.in.neighborhood	0.1534	0.1127	0.3162	0.1534	0.1499	0.3569
ever.in.gang	0.0096	0.007	0.0833	0.0096	0.013	0.1132
belong.gang.12months	0.0221	0.0063	0.0788	0.0221	0.0202	0.1407
ever.destroy.prop	0.0073	0.0182	0.1338	0.0073	0.0094	0.0966
destroy.prop.12months	0.0539	0.0401	0.1961	0.0539	0.0531	0.2242
ever.steal.less.50	0.0087	0.0214	0.1449	0.0087	0.0111	0.1046
steal.less.50.12months	0.064	0.0476	0.2129	0.064	0.0653	0.2471
ever.steal.more.50	0.0053	0.0048	0.0689	0.0053	0.0092	0.0957
steal.more.50.12months	0.0052	0.0047	0.0688	0.0052	0.0091	0.0951
ever.other.prop.crime	0.0051	0.0053	0.0727	0.0051	0.0079	0.0888
other.prop.crime.12months	0.0336	0.0134	0.1148	0.0336	0.0305	0.1719
attacked	0.0069	0.012	0.1091	0.0069	0.0096	0.0974
attacked.12months	0.0904	0.0387	0.1929	0.0904	0.093	0.2904
ever.sell.drugs	0.0044	0.0041	0.0641	0.0044	0.0073	0.0849
sell.drugs.12months	0.0876	0.0238	0.1524	0.0876	0.0843	0.2778
steal.less.50.store	0.0336	0.0348	0.1832	0.0336	0.0396	0.1951
steal.less.50.purse.wallet	0.0044	0.0017	0.0415	0.0044	0.0043	0.0651
steal.less.50.burglary	0.0095	0.0027	0.0515	0.0095	0.0102	0.1003
steal.less.50.armed	0.0044	0.0008	0.028	0.0044	0.0026	0.0505
steal.more.50.store	0.0157	0.0068	0.0824	0.0157	0.0188	0.1359
steal.more.50.purse.wallet	0.0036	0.0011	0.0337	0.0036	0.0038	0.0615
steal.more.50.burglary	0.0091	0.0021	0.0453	0.0091	0.0096	0.0974
steal.more.50.armed	0.0053	0.0007	0.0264	0.0053	0.0029	0.0537
steal.auto	0.0096	0.0015	0.0393	0.0096	0.0084	0.0912
prop.crime.income	196.1935	45.6847	5867.7579	196.1935	49.2662	1949.2111
sell.marijuana	0.0665	0.0178	0.1322	0.0665	0.0645	0.2456
sell.hard.drugs	0.0423	0.0079	0.0885	0.0423	0.0309	0.1729
ever.hard.times	0.0714	0.0452	0.2078	0.0714	0.0708	0.2566
autonomy.youth	1.0792	1.3329	1.8959	1.0792	0.9389	1.6562
ever.run.away	0.2522	0.0834	0.2764	0.2522	0.2529	0.4347
autonomy.parent	1.3757	1.5984	2.2234	1.3757	1.2638	2.1068

Table 7: Pre and Post match covariates: table 2 of 2

	Pre Match			Post Match		
	Means Treated	Means Control	SD Control	Means Treated	Means Control	SD Control
parent.style.mom.1997	2.5822	2.7584	1.199	2.5822	2.5688	1.2719
parent.style.dad.1997	1.7525	2.0777	1.5724	1.7525	1.7073	1.5642
hh.religiousity	234.4016	252.4184	221.3761	234.4016	217.0547	216.8331
perc.peers.gang.1997	1.7614	1.5367	0.9764	1.7614	1.7457	1.1196
gen.health.1997	2.0429	1.9273	0.9053	2.0429	2.0912	0.9699
days.hear.gunshot.1997	0.4019	0.3097	1.0498	0.4019	0.3569	1.1367
ever.carried.handgun.1997	0.2212	0.0783	0.2687	0.2212	0.224	0.417
carried.gun.12months.1997	0.1362	0.0419	0.2002	0.1362	0.1295	0.3358
carried.gun.30days.count.1997	0.6213	0.1251	1.4314	0.6213	0.479	2.8448
gangs.in.neighborhood.1997	0.559	0.4404	0.4964	0.559	0.5885	0.4921
ever.in.gang.1997	0.1391	0.0404	0.197	0.1391	0.1179	0.3225
belong.gang.12months.1997	0.0785	0.0185	0.1346	0.0785	0.0592	0.2359
ever.destroy.prop.1997	0.4733	0.2414	0.4279	0.4733	0.5327	0.4989
ever.steal.less.50.1997	0.5206	0.2892	0.4534	0.5206	0.5939	0.4911
ever.steal.more.50.1997	0.2069	0.0587	0.2351	0.2069	0.1898	0.3922
ever.other.prop.crime.1997	0.2163	0.0658	0.2479	0.2163	0.2175	0.4125
attacked.1997	0.3814	0.1539	0.3608	0.3814	0.4175	0.4932
ever.sell.drugs.1997	0.174	0.0513	0.2206	0.174	0.1728	0.3781
steal.less.50.store.1997	0.4505	0.2344	0.4236	0.4505	0.5141	0.4998
steal.less.50.purse.wallet.1997	0.0455	0.0113	0.1056	0.0455	0.0435	0.204
steal.less.50.burglary.1997	0.0847	0.0143	0.1187	0.0847	0.0672	0.2504
steal.less.50.armed.1997	0.0157	0.0036	0.0603	0.0157	0.0117	0.1077
steal.more.50.store.1997	0.1389	0.0378	0.1907	0.1389	0.119	0.3238
steal.more.50.purse.wallet.1997	0.0305	0.0067	0.0813	0.0305	0.0254	0.1574
steal.more.50.burglary.1997	0.07	0.0095	0.097	0.07	0.0446	0.2065
steal.more.50.armed.1997	0.0142	0.0031	0.0555	0.0142	0.0099	0.0991
steal.auto.1997	0.058	0.007	0.0833	0.058	0.0393	0.1942
prop.crime.income.1997	57.8146	23.3293	1408.6081	57.8146	28.246	337.8283
sell.marijuana.1997	0.1417	0.0377	0.1904	0.1417	0.1387	0.3457
sell.hard.drugs.1997	0.0833	0.0186	0.1349	0.0833	0.0773	0.267

Table 8: POST match - FULL

	Dependent variable:					
	ln(income)			weeks.worked		
	OLS	OLS	Fixed	OLS	OLS	Fixed
convictions(prior)	0.402***	−0.053	−0.193***	1.762***	−0.754***	−2.004***
	(0.045)	(0.041)	(0.056)	(0.206)	(0.189)	(0.251)
convictions(current)		−0.254***	−0.269***		−0.023	−0.497
		(0.077)	(0.073)		(0.354)	(0.330)
incarceration(prior)	−0.064***	−0.036***	−0.038***	−0.301***	−0.083***	−0.069***
	(0.003)	(0.003)	(0.003)	(0.014)	(0.013)	(0.015)
incarceration(current)		−0.148***	−0.120***		−1.124***	−0.920***
		(0.013)	(0.012)		(0.059)	(0.056)
arrests(prior)		−0.081***	−0.080***		−0.386***	−0.294***
		(0.006)	(0.009)		(0.027)	(0.041)
arrests(current)		−0.037	−0.027		−0.598***	−0.489***
		(0.023)	(0.022)		(0.106)	(0.101)
age		1.229***	1.375***		7.966***	8.267***
		(0.033)	(0.031)		(0.152)	(0.142)
I(ageˆ2)		−0.020***	−0.024***		−0.152***	−0.160***
		(0.001)	(0.001)		(0.003)	(0.003)
black		−1.341***			−4.958***	
		(0.042)			(0.191)	
hispanic		−0.397***			−0.292	
		(0.046)			(0.211)	
mixed		−0.419***			−3.767***	
		(0.128)			(0.588)	
male		0.841***			2.244***	
		(0.036)			(0.165)	
expect.best		0.024			0.148	
		(0.042)			(0.194)	
rarely.expect.good		−0.130***			0.009	
		(0.043)			(0.198)	
optomistic		0.013			−0.863***	
		(0.044)			(0.201)	
hardly.expect.go.your.way		−0.091**			−0.354*	
		(0.044)			(0.203)	
region1		4.233***	4.254***		20.259***	21.147***
		(0.125)	(0.146)		(0.578)	(0.658)
region2		4.787***	4.746***		21.563***	22.135***
		(0.122)	(0.138)		(0.563)	(0.620)
region3		4.299***	4.249***		19.236***	19.099***
		(0.120)	(0.128)		(0.555)	(0.576)
region4		4.587***	4.528***		19.250***	18.880***
		(0.124)	(0.137)		(0.570)	(0.615)
citizen		0.012			0.057	
		(0.026)			(0.119)	
MSA.city		1.335***	0.799***		8.388***	6.980***
		(0.112)	(0.107)		(0.515)	(0.481)
MSA.suburb		1.415***	0.793***		9.047***	6.989***
		(0.111)	(0.110)		(0.513)	(0.494)
MSA.rural		1.062***	0.685***		7.899***	5.877***
		(0.105)	(0.103)		(0.484)	(0.462)
hh.income		0.00000***			0.00001***	
		(0.00000)			(0.00000)	
highest.grade.dad		0.006**			0.058***	
		(0.003)			(0.014)	
highest.grade.mom		0.022***			0.009	
		(0.003)			(0.014)	
highest.grade		0.021***	−0.024***		0.244***	0.168***
		(0.004)	(0.004)		(0.018)	(0.018)
gpa.weighted		0.204***			0.822***	
		(0.012)			(0.055)	
gen.health		−0.172***	0.021		−0.475***	0.162*
		(0.017)	(0.019)		(0.077)	(0.086)
carried.gun.12months		0.330***	0.145*		−0.309	0.172
		(0.080)	(0.077)		(0.367)	(0.348)
carried.gun.30days.count		0.009	0.006		−0.024	−0.051**
		(0.006)	(0.006)		(0.027)	(0.026)
gangs.in.neighborhood		0.048	0.036		0.118	0.166
		(0.047)	(0.048)		(0.218)	(0.215)
ever.in.gang		0.150	0.183		−0.370	0.254
		(0.156)	(0.143)		(0.720)	(0.645)
belong.gang.12months		−0.664***	−0.048		−1.407**	−0.111
		(0.141)	(0.136)		(0.648)	(0.613)
ever.destroy.prop		0.249	0.108		−2.192***	−2.618***
		(0.161)	(0.150)		(0.741)	(0.672)

Table 9: POST match - FULL, part 2

	Dependent variable:					
		ln(income)			weeks.worked	
	OLS	OLS	Fixed	OLS	OLS	Fixed
destroy.prop.12months		−0.127	−0.145*		−0.884**	−0.750**
		(0.088)	(0.083)		(0.406)	(0.375)
ever.steal.less.50		0.492***	0.398***		−1.981***	−1.785***
		(0.155)	(0.145)		(0.717)	(0.652)
steal.less.50.12months		−0.226**	−0.230**		−1.358***	−1.365***
		(0.108)	(0.103)		(0.498)	(0.462)
ever.steal.more.50		0.164	0.303		−4.574	−2.312
		(1.964)	(1.822)		(9.054)	(8.191)
steal.more.50.12months		−0.061	−0.359		6.142	2.639
		(1.968)	(1.826)		(9.072)	(8.209)
ever.other.prop.crime		0.313	0.046		1.426	1.498
		(0.221)	(0.205)		(1.018)	(0.920)
other.prop.crime.12months		−0.161	−0.054		−0.680	−0.530
		(0.124)	(0.116)		(0.571)	(0.524)
attacked		0.491***	0.336**		0.833	−0.401
		(0.155)	(0.144)		(0.714)	(0.646)
attacked.12months		−0.079	0.019		−0.672**	−0.074
		(0.071)	(0.068)		(0.327)	(0.306)
ever.sell.drugs		−0.073	−0.129		−0.082	−0.087
		(0.224)	(0.208)		(1.033)	(0.935)
sell.drugs.12months		−0.157	−0.220		1.069	0.918
		(0.145)	(0.137)		(0.670)	(0.617)
steal.less.50.store		0.301**	0.062		2.682***	1.786***
		(0.128)	(0.120)		(0.590)	(0.539)
steal.less.50.purse.wallet		−0.084	−0.017		1.326	0.980
		(0.302)	(0.281)		(1.390)	(1.261)
steal.less.50.burglary		−0.152	−0.130		−1.233	−1.161
		(0.247)	(0.230)		(1.137)	(1.032)
steal.less.50.armed		0.261	0.578		3.966**	5.686***
		(0.389)	(0.361)		(1.793)	(1.621)
steal.more.50.store		−0.035	0.093		−0.102	0.115
		(0.168)	(0.157)		(0.776)	(0.705)
steal.more.50.purse.wallet		0.581	0.350		−0.724	−0.620
		(0.366)	(0.340)		(1.686)	(1.527)
steal.more.50.burglary		−0.172	−0.051		0.968	0.880
		(0.280)	(0.260)		(1.289)	(1.168)
steal.more.50.armed		−0.362	−0.302		−3.099*	−4.287***
		(0.384)	(0.356)		(1.769)	(1.601)
steal.auto		−0.112	−0.027		−1.793	−0.055
		(0.239)	(0.223)		(1.101)	(1.005)
prop.crime.income		0.00000	0.00000		0.00001	0.00001
		(0.00000)	(0.00000)		(0.00001)	(0.00001)
sell.marijuana		0.333**	0.150		2.218***	1.483**
		(0.147)	(0.139)		(0.679)	(0.624)
sell.hard.drugs		0.100	0.329***		−1.105*	−0.258
		(0.130)	(0.123)		(0.598)	(0.555)
ever.hard.times		−0.334***			−1.060***	
		(0.063)			(0.289)	
autonomy.youth		−0.033**			−0.190***	
		(0.013)			(0.061)	
ever.run.away		−0.323***			−1.632***	
		(0.041)			(0.189)	
autonomy.parent		0.004			−0.156***	
		(0.011)			(0.051)	
parent.style.mom.1997		−0.031**			0.152**	
		(0.013)			(0.061)	
parent.style.dad.1997		0.002			−0.156***	
		(0.012)			(0.056)	
hh.religiousity		0.0001*			0.001**	
		(0.0001)			(0.0004)	
constant	4.612***	−17.491***		24.060***	−100.491***	
	(0.021)	(0.369)		(0.097)	(1.700)	
Observations	66,288	66,288	66,288	66,288	66,288	66,288
R^2	0.008	0.303	0.228	0.008	0.325	0.277
Adjusted R^2	0.007	0.303	0.214	0.008	0.324	0.260
Residual Std. Error	4.660	3.906		21.818	18.007	
F Statistic	251.320***	436.978***	399.764***	252.403***	482.254***	518.236***

Note: *p<0.1; **p<0.05; ***p<0.01; Robust standard errors in parenthesis

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