The Causal Effects of Criminal Convictions on Labor Market Outcomes in Young Men: A Nonparametric Bounds Analysis

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Abstract

This paper examines the causal effects of criminal convictions on labor market outcomes in young men using data from the National Longitudinal Survey of Youth 1997 cohort. Unlike previous research in this area which relies on assumptions strong enough to obtain point identification, this paper imposes relatively weak nonparametric assumptions that provide tight bounds on treatment effects. Even in the absence of a parametric model, under certain specifications, a zero effect can be ruled out, though after a bias correction this result is lost. In general the results for the effect on yearly earnings align well with previous findings, though the estimated effect on weeks worked are smaller than in previous findings. Results of a novel sensitivity analysis test how the estimated bounds respond to a weakening/strengthening of two key assumptions. Even under a significant strengthening of a key assumption a negative treatment effect cannot be ruled out.

JEL Classification: C14, J30, K40

Key Words: Endogeneity, Nonparametric estimation, Criminal convictions.

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1 Introduction

In April of 2011, the city of Philadelphia enacted a “ban the box” ordinance making it illegal for employers to inquire into applicants’ criminal histories on initial job applications. Four states have similar state-wide measures: New Mexico, Connecticut, Hawaii, and Minnesota. In the same year, the U.S. Department of Labor released nearly $12 million to 10 organizations to provide adult offenders with job market assistance. Motivating these measures is the conventional wisdom that individuals with criminal records face unique difficulties in the labor market. One statistic that might stand as evidence of the existence of these difficulties is the observed negative relationship between criminal convictions and average earnings. But to some extent convictions may simply be a mark of individuals with poor labor market skills, hence the evidentiary value of this statistic is questionable.

The sheer number of people affected marks the link between convictions and employment outcomes as an area that warrants attention. In 2009, nearly 7.2 million adults, or 3.1% of the adult population, were incarcerated, on parole, or on probation (U.S. Department of Justice 2010). These figures are significantly higher than they were several decades ago - the correctional population has quadrupled in the last 30 years - and this trend has been overwhelmingly concentrated among young, less educated men (Western, Kling, Weiman 2001). Given this concentration, any stigmatizing effect of convictions would work to further hinder a group already disadvantaged in the labor market.

The labor market effects of interactions with the criminal justice system - be it arrests, convictions, or incarcerations - are a well studied area in which several authors have used various empirical strategies to point identify causal effects of interest. Freeman (1991), using the 1979 National Longitudinal Survey of Youth (NLSY), finds individuals who had been in jail worked substantially fewer weeks several years after incarceration (between a 8 and 16 week reduction). He employs both a simple cross sectional regression and one that exploits the longitudinal nature of the 1979 NLSY controlling for before incarceration labor market experience. Grogger (1995), also addressing possible endogeneity concerns over convictions with a fixed effect panel model, focuses on California data from individuals arrested between 1973 and 1987 to estimate the effect of arrests on earnings and employment levels over the years 1980-1984 (using the 'as yet to be arrested' as a control group). He finds arrests to have a negative effect on young mens' earnings in the range of about 4% but that this effect disipates after 6 quarters and the effect of convictions above arrest is insignificant. He also finds simple arrests to have no negative effects on employment (even significant positive effects) though multiple arrests have significant negative effects on employment lasting up to five quarters. Allgood, Mustard and Warren (2003), also using the 1979 NLSY cohort, relying on a selection-on-observables assumption, focus on youth (aged 14-21) criminal arrests and convictions on 1983 and 1989 earnings, and find a criminal conviction causes a reduction in earnings of 12% which lasts up to ten years. They also find being charged but not convicted as a youth has no effect. Finlay (2008) using the 1997-2004 waves of the 1997 NLSY and a fixed effect panel strategy investigates the effect of incarceration on several labor market outcomes. He fails to find a significant effect on wages or employment but finds a very large effect on yearly earnings in the range of a 20% reduction.

Another strand of the literature uses some form of experiment or instrument to identify other specific causal effects of interest. Pager (2003) uses and experimental audit to assess employers' responses to job applicants with criminal histories. She finds white men with self reported criminal records are only 50% as likely to receive a 'callback' from an employer. Black men were found to be even more penalized for a criminal record and were only 33% as likely to receive a callback (and this is beyond the already 50% reduction in callbacks non-criminal black men received compared to non-criminal white men). Finlay (2008) investigates how the expanded availability of criminal history data through the internet affects labor outcomes of those with and without criminal histories. He finds the effects of incarceration on employment and earnings to be larger in states with open record policies. Kling (2006) uses multiple estimation strategies, including using randomly assigned judges and their history of leniency as an instrument for incarceration length, and fails to find strong evidence of substantial effects of incarceration length on employment or earnings.

This paper investigates the effects criminal convictions have on several labor market outcomes of interest and adds to the literature in two ways. First, it uses a newer data set than used in most previous studies, the 1997 National Longitudinal Survey of Youth, and focuses on 2006 labor market outcomes. Given the dramatic rise in the correctional population in the last few decades this seems warranted. Second, this paper differs from previous studies in the choice of identification strategy. In a similar strand to Kling (2006), given the latitude given to prosecutors over charges and deferred prosecutions, one might consider the variation in local district attorneys' prosecution record (or a similar measure) as an instrument for criminal convictions much as Kling used judges' record as an instrument for prison length. However, the exogeneity of this variable is likely to be more contentious as the prosecutors' record is likely to be much more reflective of local conditions. Furthermore, using a fixed effect panel approach to capture individual heterogeneity as a means to control for the endogeneity of convictions also seems less than appealing in the current setting as many convictions appear very early in adulthood prior to much being revealed regarding individual labor market potential. Thus, as an alternative, this paper applies a partial identification strategy that derives its power from relatively weaker assumptions than those typically imposed. Though point identification of the causal parameters is not obtained, informative identification regions emerge. In particular, I estimate identification regions for three causal effects: the causal effect of criminal convictions on yearly earnings, hourly wages and weeks worked. In Section 2, I articulate the identification problem within the potential outcomes framework and discusses in detail assumptions used in this analysis. Section 3 introduces the data, estimation methods and inference. Section 4 discusses results and a sensitivity analysis. Section 5 concludes.

2 Framework and Assumptions

2.1 *Potential Outcomes Framework*

Causal effects are common subjects of interest in a wide range of fields. When the impact variable is dichotomous, as in the present setting, it is convention to refer to the causal effect as a treatment effect. The potential outcomes framework presented below provides an intuitive setting in which to analyze questions of this sort.

Each individual j in the population J is characterized by covariates $x_j \in X$ and response function $y_j(\cdot) : T \to Y$ mapping mutually exclusive and exhaustive treatments $t \in T$ into outcomes $y_j(t) \in Y$. The treatment is dichotomous: being convicted of a crime or not. Each individual j has a realized treatment $z_j \in T$ and a realized outcome $y_j \equiv y_j(z_j)$ which are both observed. Latent outcomes $y_j(t)$, $t \neq z_j$ are not observed. Treatment effect is defined as $y(1) - y(0)$.

A distributional characteristic of usual interest is the average treatment effect (ATE):

$$\text{ATE} = E[y(1) - y(0)|x] = E[y(1)|x] - E[y(0)|x]. \tag{1}$$

The ATE is defined as the expected treatment effect if treatment were randomly assigned to the population. If interest is in the ATE, what is problematic is that neither $E[y(1)|x]$ nor $E[y(0)|x]$ is observed, but rather $E[y(1)|x, z = 1]$ and $E[y(0)|x, z = 0]$. Given that individuals self-select into criminal activities, and that these individuals are likely to exhibit other unobserved characteristics which also affect their labor market outcomes, one is likely to be reluctant to assume $E[y(t)|x, z = t] = E[y(t)|x]$. This is simply the endogeneity problem stated in a potential outcomes framework.

To see where further assumptions are necessary for identification, we can rewrite $E[y(t)|x]$ using the law of iterated expectations:

$$E[y(t)|x] \quad = \quad E[y(t)|x, z = t]P(z = t|x) + E[y(t)|x, z = t']P(z = t'|x). \tag{2}$$

The data identify sample analogues of all of the right hand side quantities except the counterfactual $E[y(t)|x, z = t']$. This might represent expected income under a conviction treatment for those who actually received the non-conviction treatment. The data bring us part of the way towards identifying the ATE, but the remaining distance must be covered by credible assumptions.

Rather than resting on assumptions strong enough to point identify the ATE, this paper uses several assumptions to partially identify the ATE. The main results of this paper emerge from the imposition of three assumptions: mean monotone treatment response (MMTR), monotone treatment selection (MTS), and monotone instrumental variable (MIV). These assumptions are explained in full in the following sections[2].

2.2 *Worst Case Bounds*

Even if a researcher is not willing to impose any assumptions on the response function or selection mechanism, it is still possible to bound the treatment effect if the support of the outcome variable is bounded (Manski, 1989). Though the counterfactuals in Equation (2) are not observed, they can be bounded if Y has a bounded outcome space. Let $E[y(t)|x, z =$

[2]What follows is by no means a comprehensive review of the bounding literature which is a vast and growing field. Rather what follows is a brief explanation of the assumptions used in this analysis.

$(t')] \in [K_l, K_u]$. Note that when Y is binary, these expectations can be viewed as probabilities which necessarily lie between 0 and 1 implying the natural values $K_l = 0$ and $K_u = 1$. When Y is continuous, the researcher may choose finite values for these parameters based, for example, on the range of the data or relevant prior knowledge. Imposing a bounded outcome space on the counterfactuals leads to *worst case bounds* on the unknowns $E[y(t)|x]$:

$$LB_t \leq E[y(t)|x] \leq UB_t \tag{3}$$

where

$$\begin{aligned} LB_t &= E[y(t)|x, z = t]P(z = t|x) + K_l P(z = t'|x) \\ UB_t &= E[y(t)|x, z = t]P(z = t|x) + K_u P(z = t'|x). \end{aligned}$$

Applying these results to Equations (1) and (2) lead to worst case bounds on the average treatment effect:

$$LB_1 - UB_0 \leq ATE \leq UB_1 - LB_0. \tag{4}$$

Worst case bounds tend to be limited in the information they convey because they necessarily include zero. More informative bounds on the ATE require further assumptions.

2.3 *Monotone Treatment Selection*

As previously noted, exogenous selection is unlikely to be a credible assumption in the current setting. However, a weaker Monotone Treatment Selection assumption (Manski and Pepper 2000) seems much more realistic:

MTS Assumption: *Let T be ordered. For each $t \in T$, each $x \in X$ and all $(u_0, u_1) \in T \times T$*

such that $u_1 \geq u_0$,

$$E[y(t)|x, z = u_1] \geq E[y(t)|x, z = u_0]. \tag{5}$$

MTS assumes a characteristic concerning the relationship between the selection process and the outcome process (in this context one should view the self-selected treatment as its own realized covariate). Specifically, MTS presumes, for example, that those with a 'lower' realized treatment (z = criminal conviction) exhibit other unobserved characteristics that would lead them to have no greater expected incomes under either potential treatment than those with a 'higher' realized treatment (z = not convicted). This is precisely why one might find standard regression methods unappealing in the current setting. One would likely be skeptical of inferring a causal interpretation of the coefficient on criminal convictions in a standard regression due to concerns over correlation between the regressor and the error term. The MTS assumption turns this concern into an assumption to be used for identification.

2.4 *Monotone Treatment Response*

The Monotone Treatment Response (Manski 1997) assumption specifies a relationship between $y(1)$ and $y(0)$. It maintains that if treatments have some natural ordering then outcomes vary monotonically with them.

MTR Assumption: *Let* T *be ordered. For each* $j \in J$

$$t_1 \geq t_0 \Rightarrow y_j(t_1) \geq y_j(t_0). \tag{6}$$

In the present study, this assumption implies, for example, that yearly income for each individual will be no greater if convicted of a crime than if not convicted. MTR also implies a weaker variant:

Mean MTR (MMTR):

$$E[y(1)|x, z] \geq E[y(0)|x, z]. \tag{7}$$

This follows from MTR by definition of the expectation function. Under this assumption, expected incomes would be no greater for the population under a conviction treatment than under a non-conviction treatment. This, though admittedly a strong assumption, seems reasonable in the current setting and in accordance with previous literature. Furthermore, as the goal is to identify a region of consensus as to the magnitude of the treatment effects, results stemming from a relaxation of this assumption are given in the sensitivity analysis section.

Though the worst case bounds depend on the imposed bounded support on expected outcomes (K_l, K_u), bounds stemming from the imposition of MMTR and MTS do not. The imposition of the joint MMTR and MTS assumptions can have significant identification power and directly relate to the response and selection process. In what follows, monotone instrumental variables (MIV) brings to bear a different type of assumption that, when invoked along with MMTR and MTS, can further tighten the identification region.

2.5 *Monotone Instrumental Variables*

The method of instrumental variables is widely used in the evaluation of treatment effects when endogeneity is a concern. Though standard IV assumptions can aid greatly in identification, the credibility of the instrument is often a matter of disagreement, specifically whether the exclusion restriction is a valid assumption. This provides motivation for considering weaker, and thus more credible, assumptions to aid identification. First, consider a *mean independence* form of the standard IV condition:

IV Assumption *Covariate v is an instrumental variable if, for each $t \in T$, each value of*

x, and all $(u, u') \in (V \times V)$,

$$E[y(t)|x, v = u'] = E[y(t)|x, v = u].$$

A Monotone Instrumental Variable (Manski and Pepper, 2000) assumption weakens this IV condition by replacing the equality with an inequality:

MIV Assumption *Let V be an ordered set. Covariate v is a monotone instrumental variable if, for each $t \in T$, each value of x, and all $(u, u') \in (V \times V)$ such that $u_2 \geq u_1$,*

$$E[y(t)|x, v = u_2] \geq E[y(t)|x, v = u_1].$$

The instrument used in this analysis is the respondents' test scores from the Armed Services Vocational Aptitude Battery (ASVAB) administered between the summer of 1997 and spring of 1998. In treating this variable as an MIV, it is assumed that under either treatment, those with lower instrument levels (low test score) have expected outcomes no better than those with higher instrument levels (high test scores). This assumption stems from the belief that standardized test scores are likely correlated with some level of innate ability or intelligence which is valued in the labor market and thus labor market functions should be (weakly) increasing functions in these measures.

3 Data and Estimation

3.1 *Data*

The data used in this study come from the 1997 cohort of the NLSY. The 1997 NSLY is a nationally representative sample of nearly 9,000 youths born between 1980 and 1984 with an over-sample of minorities. This paper's population of interest is black and white men with at most a high school diploma who are not enrolled in school (thus, in the above notation, x is defined over gender, race, education and enrollment status). The income variable is reported income earned from an outside employer for 2006. If self reported income is not available but an income range is, then the mean of that range is used for yearly income. The hourly wage variable is a weighted average of all wages earned from an outside employer, excluding military, in 2006, where the weights used are hours employed. Weeks worked is the number of weeks for which an individual reported having gainful employment. For yearly income I restrict the sample to those reporting at least \$5,000. For hourly wages responses less than \$5 an hour or above \$50 an hour are not included. For weeks worked I restrict the sample used to those working at least 1 week in the year. The conviction variable is based on self reported criminal convictions not settled in juvenile court prior to 2006. Incarceration is measured as if an individual spent time in jail during any week prior to 2006. There is a potential concern regarding measurement error given that conviction status is self reported. Though one is partially reassured noting that the frequency of 'refusal to answer' sensitive questions in the NLSY questionnaire considerably declines in the latter years of the survey compared to initial years, and that many refused questions are subsequently asked again and answered in later years, the issue cannot be fully dismissed. While this issue is noted as a potential concern, I do not directly address a solution. For a recent paper addressing such concerns in a similar bounding framework see Kreider et al. (2012).

Table 1 gives sample sizes for subpopulations used for the analysis of each outcome of interest covering the non-convicted individuals, convicted individuals, as well as convicted individuals who where never incarcerated. Due to different restrictions regarding inclusion in the analysis, these sample sizes differ. When looking at the full NLSY sample of white men there is a conviction rate of 17.1%, of which 36% spent time in jail, for an ever-incarcerated rate of 6.2%. For black males there is a conviction rate of 21.7%, of which 59% spent time in jail, for an ever-incarcerated rate of 12.8%. When looking at these percentages for the samples used in this study they vary due to individuals going to college or not being present for the study or not being in the labor market. For example, of white men present in the 2007 wave and valid for the study 24% were convicted of crimes verses 21.5% for black men. While attrition rates are similar for the two groups (16.1% of black men are absent from 2007 wave and 17.6% of white men), college rates differ markedly for the two groups within the reporting population (24% for whites and 9.5% for blacks). Furthermore, valid 'participation' in the labor markets vary greatly between the groups as well with participation generally being higher for white men. One should note that some who were not present in the 2007 wave nonetheless have 2006 data available due to data being collected in subsequent years. This is why, for example, there are more white men used in the weeks worked analysis then were present in the 2007 wave.

Table 2 gives mean outcomes and test statistics based on simple bivariate regressions for mean differences between convicted and non-convicted respondents. When looking at differences in white men between convicted and never convicted individuals there are clear differences with non-convicted individuals having higher incomes, wages, and weeks worked. When comparing non-convicted individuals to those convicted but who never spent time in jail, the differences still persist, though not always at the 5% confidence level (though well within the 10% confidence level). For black men, though the difference is present and significant when comparing yearly income between non-convicted and convicted individuals, it disappears completely when the treatment group is those convicted but who never spent time in jail. No differences appear at all in hourly wages for either comparison group, though differences persist regardless of comparison group for weeks worked.

3.2 *Estimation and Inference*

Estimated bounds are functions of expected wages, probabilities of being convicted, and probabilities of realized instrument values, all of which can easily be computed nonparametrically. For worst case bounds and bounds under MMTR/MTS, these values are calculated by sample analogs. For bounds under the test score MIV, expectations and probabilities are estimated via smoothing splines (penalized least squares) using generalized cross validation for the degrees of freedom selection. Although nonparametric estimators allow researchers to estimate free of functional form, they are limited by the number of conditioning variables. The estimates in this paper condition on gender, race, education, enrollment status and relevant instrument where an MIV is utilized. But this limited number of conditioning variables should not affect the consistency of the results as long as the assumptions defined above hold. Due to data limitations an increase in the number of control variables is not feasible.

An important concern when estimating bounds with MIVs is that analog estimates of such bounds exhibit finite-sample bias which lead the bounds to be narrower (more optimistic) than the true bounds. By Jensen's Inequality, the estimated lower bound on $E[y(t)|x]$ is biased upwards because of the maxima operator and the estimated upper bound is biased downward because of the minima operator. To counter this bias, I implement a correction proposed by Kreider and Pepper (2007). The approach is to estimate the bias by using the bootstrap distribution and then adjust the analogue estimate in accordance with the estimated bias. For a random sample of size N, let LB_N be the analogue estimate of the lower bound in question, and let $E^b(LB_N)$ be the mean of the estimate from the bootstrap distribution (a parallel procedure is used for an upper bound). The bias is then estimated as $E^b(LB_N) - LB_N$. The bias-corrected estimate is then $LB_N - \left[E^b(LB_N) - LB_N\right] = 2LB_N - E^b(LB_N)$. While heuristic and not derived from theory, this correction seems reasonable and performs well in Monte Carlo simulations (Manski and Pepper, 2009).

Statistical inference for partially identified parameters is somewhat more challenging than estimation itself and is the focus of a currently active literature. A consensus on the 'correct' type of confidence interval that should be reported is still evolving. The results of partial identification analysis are regions of identification defined by upper and lower bounds which contain the parameter of interest. When considering confidence intervals in these settings, the question arises of whether to construct intervals over the region of identification or over the actual parameter of interest. Intervals presented here cover the parameter of interest with fixed probability and were derived by Imbens and Manski (2004).

4 Findings

4.1 *Main Results*

Results for white men are given in Table 3 and those for black men are given in Table 4. Note that since criminal convictions have negative impacts on outcomes, the largest effect in magnitude is the lower bound. Initial worst case bounds on the ATE of criminal convictions on yearly income are quite large and are not very informative.[3] They confine the identification region to a range of \$20,000 and necessarily contain zero. Once the MMTR and MTS assumptions are imposed the bounds shrink dramatically. For white men, when

[3]To obtain these bounds K_l is set to \$15,000 and K_u is set to \$35,000.

including the full convicted group as the treatment group, they span the general range of \$0 to -\$5,000; they then shrink to a lower bound of -\$3,500 when the comparison group is convicted individuals who never spent time in prison. This is a similar range for black men when using the entire convicted group as treatment group. Adding the MIV assumption further tightens the bounds on the ATE and signs the treatment effects away from zero. For white mean the effect of convictions lowers yearly wages by at least \$390 to \$277 depending on the treatment group and at most between about \$2,500 and \$3,500. However, once the bias correction is implemented the bounds can no longer exclude a zero effect. For black men, when using the entire convicted population as the treatment group, the joint MIV/MMTR/MTS bounds on the effect on yearly income range from -\$2,064 to -\$433, this larger effect being about an 8% reduction. However, again once the bias correction is included these bounds cannot exclude a zero effect.

These finding are well in line with the results found by Grogger (1995). His results, after adjusting for inflation, amount to about a \$320 to \$1,000 decline in yearly earnings due to arrests. The results found by Allgood, Mustard and Warren (2003) and Finlay (2009) are also well within the ranges found here.

Initial worst case bounds on the ATE on hourly wages span a range of \$20 and necessarily contain zero.[4] Bounds under the joint MMTR-MTS assumptions significantly reduce this range. The lower bound for white men, when all those convicted are the treatment group, is -\$1.59, though it is slightly higher at -\$1.28 when the treatment group is focused only on those who never spent time in jail. In this case the MIV, though cannot bound the ATE away from zero, does tighten the bounds when the treatment group is the never incarcerated group to a lower bound of -\$0.58, though again this is gain is lost once the bias correction is added. The analysis is not done for black men as convicted and non-convicted individuals do not show significantly different outcomes.

[4]To obtain these bounds K_l is set to \$5 and K_u is set to \$25.

Worst case bounds on the ATE on weeks worked span a range of 38 weeks and by definition contain zero.[5] The joint MMTR-MTS assumptions significantly tighten these bounds to a range of less than 2 weeks for white men under either treatment group and about 4 weeks for black men under either treatment group. The addition of the MIV then bounds the treatment effect away for black men away from zero and cuts the maximum magnitude of the effect on black men to almost 3 weeks when using men who were never incarcerated as the treatment group. However, once the bias correction is accounted for none of these upper bounds are significantly different than zero and the gain from the MIV is lost. Nonetheless the upper bounds on the magnitudes of these effects are much smaller than those found by Freeman (1991), though his focus is on having spent time in jail rather than convictions.

It is worth noting the differences in the effects for white and black men on wages, income and weeks worked when the treatment group is changed from all those convicted to only those convicted but who never spent time in jail. For white men, the bounds of the effect of convictions tend to be similar regardless of the definition of the treatment group (though slightly smaller when the treatment group is those who were never incarcerated, as one might expect). This implies the pure stigma from having a conviction likely causes losses(since the one treatment group was convicted but spent no time in jail and thus there is no direct loss of experience). Yet for black men, though the effect of convictions on income can only be bound when using all those convicted as the treatment group, when those who spent time in jail are removed from the treatment group, those convicted and those not convicted do not even have significantly different incomes. Also, under neither treatment group do convicted vs. non-convicted black men exhibit significantly different hourly wages. Only in weeks worked do black and white men exhibit similar causal effects of criminal convictions, in that it reduces weeks worked under either treatment group definition. One might speculate that this is due to black men already facing dimmer job prospects, so wages have less downward mobility then white men, while it may still be harder to actually find work, thus the effect

[5] These bounds are obtained by setting K_l to 12 and K_u to 50.

on weeks worked still comes through regardless of treatment group.

4.2 *Sensitivity Analysis*

One interesting question is how sensitive the estimated bounds are to a relaxation/strengthening of the MMTR and MTS assumptions. Assuming MMTR imposes ex ante a non-positive ATE while MTS imposes the direction of the selection mechanism, both of which can be strengthened or weakened.

Take the following modification of the MTS assumption:

$$(1+\alpha)\cdot E[y(t)|x, z=u_1] \geq E[y(t)|x, z=u_0] \tag{8}$$

and allow α to vary. Setting $\alpha = 0.05$ implies, for example, expected earnings under either potential treatment for those convicted of a crime are at most 5% *greater* than expected earnings under the same potential treatment for those not convicted of a crime (a weakening of the MTS assumption), while setting $\alpha = -0.05$ implies expected earnings under either potential treatment for those convicted of a crime are at most 5% *less* than expected earnings under the same potential treatment for those not convicted of a crime (a strengthening of the MTS assumption). A parallel modification to the MMTR assumption is straightforward. A graphical depiction of this analysis for bounds when using the full sample is given in Figures 1-5[6].

When looking at the lower bounds on the treatment effects (stemming from MTS and MIV) one can see that even strengthening the MTS assumption by setting $\alpha = -0.1$ still leaves

[6] Because the current application's treatment of interest is binary only the MMTR assumption applies to the upper bound and only the MTS assumption applies to the lower. If one were to apply this analysis to the case of a multi-valued treatment one would need to take care and note that in general both the MMTR and MTS assumptions apply to both upper and lower bounds.

the possibility of a fairly large causal effect on all outcomes, and in some cases, such as the effect of convictions on income, even setting $\alpha = -0.15$ still leaves room for very large causal effects. This implies that if one is willing go beyond simply assuming the direction of the selection mechanism and even enforce a large impact on the outcome of interest from the selection mechanism, a causal effect from criminal convictions still cannot be ruled out. The effect of a weakening of the MMTR assumption results in trivial results due to the fact that in the current application the MIV does not lend any identifying power beyond the MMTR assumption on the upper bound (all figures are graphed post bias correction), though in other settings where identifying power is gained by the MIV beyond that of MMTR this analysis is likely to reveal interesting results.

5 Conclusion

This paper investigates the causal effects of criminal convictions on yearly income, hourly wages, and weeks worked. Unlike previous research in this area which relies on assumptions strong enough to yield point identification, this paper focuses on weaker assumptions that yield tight bounds on the ATE. Imposing two relatively innocuous restrictions (MMTR and MTS) stemming from economic theory regarding the response and selection mechanism are sufficient to provide informative identification regions of the average treatment effects of criminal convictions on labor market outcomes. Furthermore, using a plausible monotone instrumental variable, standardized test scores, in many cases further narrows the bounds on the average treatment effect and in some cases signs the ATE away from zero, though after a correction for finite sample bias this gain is lost.

The bounds on the effect of criminal convictions on early incomes found here align well with results found in previous studies. However the bounds on the effect on weeks worked resulting from this analysis rule out the estimated effect found by Freeman (1991), though estimated confidence intervals cannot rule out his estimates. Interestingly, when those who have spent time in jail are eliminated from the conviction treatment group for black men, there is no longer a significant difference in yearly earnings when compared to the control group, though the bounds on the effect on weeks worked are very similar under either specification. Additionally a novel sensitivity analysis is conducted to test how the estimated bounds respond to a weakening/strengthening of the MMTR and MTS assumptions. I find that even significantly strengthening the MTS assumption still results in bounds that are unable to rule out a negative treatment effect for the effect of convictions on any of the outcomes of interest.

When estimating the treatment effects of criminal convictions on labor market outcomes, endogenous selection requires the researcher to make explicit assumptions regarding data generation. This paper has studied the identifying power of various assumptions. Assumptions directly related to the selection and response functions have substantial identifying power. The inclusion of a variant of the traditional instrumental variable assumption yields informative bounds on the ATEs but still fall short of being able to point identify the average treatment effects. Stronger conclusions about treatment effects require stronger statistical or structural assumptions.

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Table 1: Sample size of full NLSY sample and restricted sample for analysis.

			exclude if ever incarcerated	
	convicted	not convicted	convicted	not convicted
White men	*full NLSY sample*			
	414	1,999	263	1,999
	present in 2007 wave and valid for study †			
	240	750	141	750
	sample used to analyze yearly income			
	194	676	117	676
	sample used to analyze hourly wage			
	202	656	115	656
	sample used to analyze weeks worked			
	236	776	144	776
Black men	*full NLSY sample*			
	254	915	104	915
	present in 2007 wave and valid for study †			
	130	476	54	476
	sample used to analyze yearly income			
	59	327	29	327
	sample used to analyze hourly wage			
	81	382	39	382
	sample used to analyze weeks worked			
	104	435	46	435

† Valid for study implies not being enrolled in school, having at most a high school diploma, and having a valid test score on record.

Table 2: Mean values of outcome variables of interest and tests of mean differences.

	convicted	not convicted	exclude if ever incarcerated: convicted	exclude if ever incarcerated: not convicted
White men	*dependent variable: yearly income*			
	26,261	31,218	27,710	31,218
	(3.18)		(1.83)	
	dependent variable: hourly wage			
	13.02	14.61	13.33	14.61
	(3.15)		(1.98)	
	dependent variable: weeks worked			
	43.04	45.73	43.59	45.73
	( )		( )	
Black men	*dependent variable: yearly income*			
	19,564	23,100	22,459	23,100
	(1.92)		(0.25)	
	dependent variable: hourly wage			
	11.96	11.87	12.13	11.87
	(0.10)		(0.24)	
	dependent variable: weeks worked			
	38.24	42.15	38.45	42.15
	( )		( )	

T-statistics for mean differences are in parenthesis.

Table 3: Bounds on the effect of a criminal convictions on outcomes of interest under various assumptions.

	Worst Case	MTR/MTS	MIV + MTR/MTS
White men	*dependent variable: yearly income*		
	[-14,550, 5,450]	[-4,957, 0]	[-3,641 , -277]
			{-7,261 , 0 }
	(-15,613, 6,512)	(-7,492, 0)	(-14,198 , 0)
	dependent variable: hourly wages		
	[-10.17, 9.83]	[-1.59, 0]	[-1.57 , 0]
			{-3.07 , 0}
	(-10.51, 10.17)	(-2.39, 0)	(-5.31 , 0)
	dependent variable: weeks worked		
	[-28.17, 9.82]	[-1.76, 0]	[-2.57 , 0]
			{-5.45 , 0 }
	(-28.98, 10.64)	(-3.10, 0)	(-9.01 , 0)
White men	*dependent variable: yearly income*		
excluding	[-14,901, 5,099]	[-3,508, 0]	[-2,446 , -390]
those ever			{-9,092 , 0 }
incarcerated	(-16,005, 6,203)	(-6,782, 0)	(-21,558 , 0)
	dependent variable: hourly wages		
	[-9.92, 10.08]	[-1.28, 0]	[-0.58 , 0]
			{-2.58 , 0 }
	(-10.30, 10.46)	(-2.40, 0)	(-6.17 , 0)
	dependent variable: weeks worked		
	[-30.39, 7.60]	[-1.55, 0]	[-2.15 , 0]
			{-5.22 , 0 }
	(-31.22, 8.42)	(-3.03, 0)	(-10.17 , 0)

95% confidence intervals are in parenthesis. For bounds under an MIV Assumption square brackets contain uncorrected bounds and brackets contain corrected.

Table 4: Bounds on the effect of a criminal convictions on outcomes of interest under various assumptions.

	Worst Case	MTR/MTS	MIV + MTR/MTS
Black men	*dependent variable: yearly income*		
	[-9,221, 10,779]	[-3,536, 0]	[-2,064 , -433]
			{-4,837 , 0 }
	(-10,363, 11,921)	(-6,316, 0)	(-9,948 , 0)
	dependent variable: weeks worked		
	[-28.15, 9.85]	[-3.43, 0]	[-3.49 , 0.03]
			{-7.13 , 0}
	(-29.21, 10.90)	(-5.86, 0)	(-12.37 , 0)
Black men	*dependent variable: weeks worked*		
excluding	[-30.45, -7.55]	[-3.97, 0]	[-3.19 , -0.03]
those ever			{-6.25 , 0]
incarcerated	(-31.53, 8.63)	(-7.69, 0)	(-12.46 , 0)

95% confidence intervals are in parenthesis. For bounds under an MIV Assumption square brackets contain uncorrected bounds and brackets contain corrected.

Figure 1: Sensitivity of bounds of ATE on income for white men to alpha.

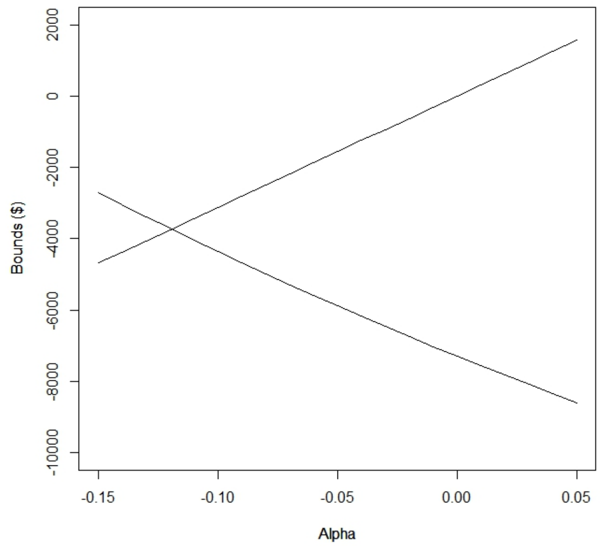

Figure 2: Sensitivity of bounds of ATE on wages for white men to alpha.

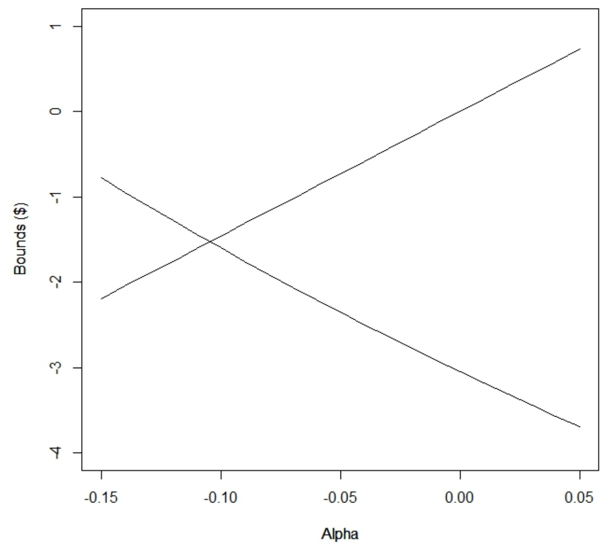

Figure 3: Sensitivity of bounds of ATE on weeks worked for white men to alpha.

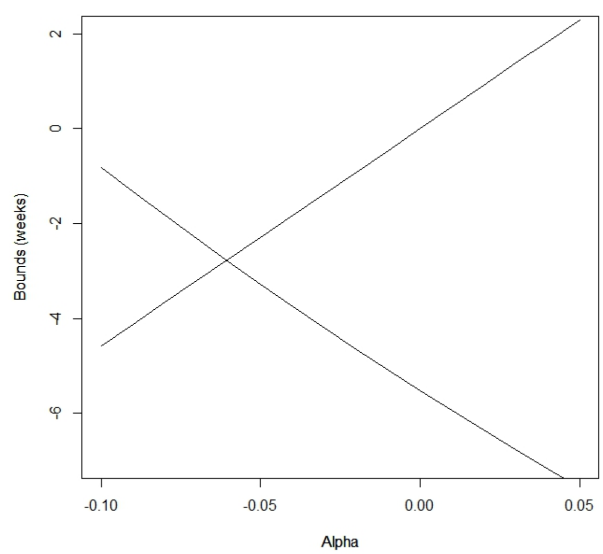

Figure 4: Sensitivity of bounds of ATE on income for black men to alpha.

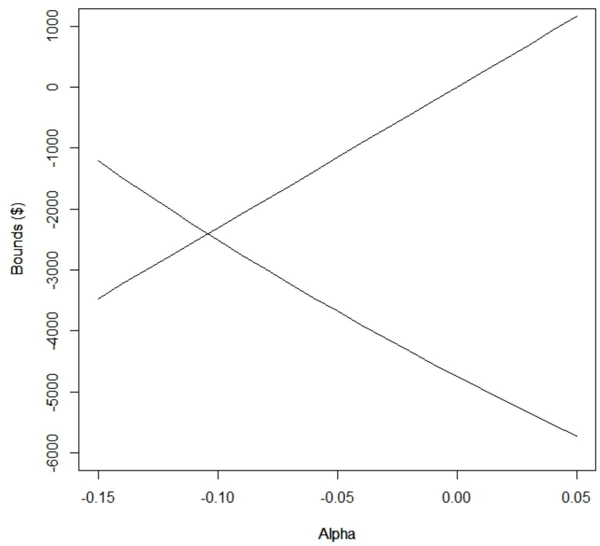

Figure 5: Sensitivity of bounds of ATE on weeks worked for black men to alpha.