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Shackled labor markets: Bounding the causal effects of criminal convictions in the U.S.☆

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ABSTRACT

This paper examines the causal effects of criminal convictions on labor market outcomes in young men using U.S. 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 which focused on the effects of incarceration. The bounds here indicate the penalty from convictions, but not incarceration, lowers weeks worked by at most 1.55 weeks for white men and at most 4 weeks for black men. Interestingly, when those ever incarcerated are removed from the treatment group for black men, there does not appear to be any effect of convictions on earnings or wages but only on weeks worked.

1. Introduction

In April of 2011, the city of Philadelphia, Pennsylvania enacted a "ban the box" ordinance making it illegal for employers to inquire into applicants' criminal histories on initial job applications. Four U.S. 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, thus 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 U.S. 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 et al., 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

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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 dissipates 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 et al. (2006), 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 (2009) 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 (2009) 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 their relation to past findings. 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. Define y to be an outcome of interest, x a set of covariates, and t and z potential and actual received treatment each of which equals either 0 or 1. In this setting there are two 'potential' outcomes: $y(t=0)$ and $y(t=1)$. However there is only one observed outcome $y(z)$ while $y(t \neq z)$ is an unobserved counterfactual.

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

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

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:

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

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.[1]

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 Eq. (2) are not observed, they can be bounded if Y has a bounded outcome space. Let $E[y(t)|x, z=(t')] \in [K_l, K_u]$. 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

[1] 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.