The Effect of Degree Attainment on Crime: Evidence from a Randomized Social Experiment*

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Abstract

We examine the effect of educational attainment on criminal behavior using the random assignment into Job Corps (JC)—the country's largest education and vocational training program for disadvantaged youth—as a source of exogenous variability in educational attainment. We allow such random assignment to violate the exclusion restriction by employing nonparametric bounds. Our results indicate that the attainment of a degree is estimated to reduce arrest rates by at most 11.8 percentage points. We also find suggestive evidence that the effects may be larger for males relative to females, and larger for black males relative to white males. Remarkably, our 95 percent confidence intervals on the causal effect of education on crime are very similar to the corresponding confidence intervals on the same effect from studies exploiting changes in compulsory schooling laws as an instrumental variable in the estimation of the effect of education on arrest rates (e.g., Lochner and Moretti (2004) and Anderson (2014)).

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1 Introduction

Crime is an important public policy issue in the U.S. Despite falling crime rates since the 1980s, the U.S. incarceration rate is still more than 6 times that of the typical OECD nation (Kearney et al, 2014). Crime has significant negative effects on the mental wellbeing of both victims and non-victims. Juvenile incarceration can also have lasting impacts on a young person's future. Moreover, youth from low-income families engage in riskier criminal behavior relative to higher-income counterparts (Kearney et al, 2014). Spending on police and law enforcement is the traditional policy mechanism to reduce crime. Policies to promote education attainment could present an alternative. Recent studies using variation in education arising from changes in compulsory schooling laws (CSLs) find that there are large crime reducing effects of education.

We contribute to the evidence of education on crime by estimating the effect of obtaining a high school, General Educational Development (GED), or vocational degree on crime using an alternative source of exogenous variation in education under relatively weak assumptions. Specifically, we exploit exogenous variation from a randomly assigned U.S. training program for disadvantaged youth (Job Corps or JC) that generated significant variation in educational attainment. This population is of interest since its educational attainment is low and is at risk of committing crimes.

Importantly, our work complements the important literature using CSLs in several ways. First, that literature usually focuses on teenagers, who are likely to be affected by CSLs, while the intervention we consider affected the educational attainment of 16 to 24 year olds. Second, we also consider other types of education such as GED and vocational degrees, which in some cases may be better suited for disadvantaged youths. Third, since CSLs are not randomly assigned a potential threat to using them as a source of exogenous variation is that they could be endogenous. Indeed, some researchers have cast doubt on their exogeneity (e.g., Lang and Kropp, 1986; Stephens and Yang, 2014).[1] Instead, we exploit exogenous variation coming from a randomized experiment. Fourth, in contrast to studies using CSLs as instruments, we let our instrument (random assignment to JC) violate the exclusion restriction by allowing it to have a direct effect on crime. Instead, we replace the exclusion restriction assumption with weak monotonicity assumptions described below. Finally, the instrument we consider has a large effect on the education attainment of the individuals in our sample (about a 20% effect). In contrast, CSLs typically have modest effects on

[1] We do not take a stand as to whether CLSs are exogenous in this context or not. Indeed, as noted before, our purpose is to provide complementing evidence by focusing on a different source of exogenous variability in education.

educational attainment (e.g., in Lochner and Moretti (2004; hereafter LM) the effect of CSLs on the probability of completing high school was 3% to 5%). In exchange for those potential benefits, we forgo point identification and instead use nonparametric bounds to estimate, under relatively weak assumptions, a range of values where the true causal effect of education on crime lies.

Remarkably, in spite of using a different source of exogenous variability, methodology, and sample, our 95 percent confidence interval on the causal effect of education on crime is very similar to the corresponding confidence interval on the same effect from studies exploiting changes in compulsory schooling laws as an instrumental variable in the estimation of the effect of education on arrest rates. We interpret this finding as providing more confidence that both approaches point to plausible values where the true effect of education on crime lies.[2]

A final contribution of the present work is to provide separate analyses of the relationship between education and crime for different demographic groups by gender and race/ethnicity. This is important as few studies report effects for the different groups (especially by race/ethnicity), while the lifetime likelihood of imprisonment varies widely over these groups. For instance, as of 2009, the lifetime likelihood of imprisonment was 5.9, 17, and 32 percent for white, black, and Hispanic males, respectively (Cox, 2010).

2 Some Recent CSL Studies

In their seminal paper, LM (2004) instrument educational attainment with changes in state-specific CSLs in the the U.S. The instrumental variable (IV) estimates show that a one-year increase in the average education level reduces state arrest rates by 15%. Anderson (2014) estimates the effect of CSLs on crime in the U.S. He finds that exposure to a minimum dropout age of 18 reduces arrest rates of 16–18 year old males by 17%, but there is no effect for females.

Large crime reducing effects have also been recently found in Europe. Hjalmarsson, Holmlund, and Lindquist (2014) find that an additional year of education reduces the conviction rate for men by 6.7% in Sweden. Machin, Marie, and Vujić (2011) find that a one-year increase in the average education of men significantly reduces the conviction rate by 20% in the U.K. Both studies find no evidence of any causal effects for women.

[2] The general idea that using different sources of variation and methods to arrive at similar results (i.e., "consistency") increases the confidence in finding a causal relation is common in fields such as epidemiology (see, e.g., Stein and Kline, 1983).

3 The Job Corps and the National Job Corps Study Data

We analyze the effect of earning a degree on the probability of being arrested employing data from the National Job Corps Study (NJCS). JC is the largest U.S. training program for disadvantaged youth aged 16–24. It provides academic and vocational training leading to the attainment of degrees (high school or GED) or certificates in a myriad of trades. JC offers other services such as residential accommodation, social skills training, counseling, health services, and job placement. The average program duration for participants is eight months. JC was evaluated in the NJCS through a social experiment (Schochet, Burghardt, and Glazerman, 2001).

Our data is representative of all eligible applicants to JC in 1994–1996. Among the main eligibility criteria for JC is being a legal U.S. resident, economically disadvantaged, aged 16–24, and in need of education and training.[3] Youth also have to be deemed free of serious behavioral issues. The latter is somewhat broadly defined, allowing individuals who have been previously arrested to still be eligible for JC participation (Schochet et al., 2003). This is an important population in what pertains to societal crime concerns (Kearney et al., 2014), and it is likely a similar population to that affected by CSLs (i.e., youth that would remain in school only due to the law). Our sample consists of 7,953 youth (3,161 assigned to control and 4,792 to treatment), of which 3,979 are black, 1,404 Hispanic, 2,036 white, 4,724 male, and 3,229 female.[4]

Our instrumental variable is an indicator for the random assignment to JC in the NJCS.[5] The treatment variable of interest is an indicator for degree attainment (whether or not it was earned in JC) between random assignment and the 8 subsequent quarters.[6] Our outcome variable is an indicator for having been arrested between quarters 1 and 16 after randomization (the latter is the last quarter of the follow-up period int he NJCS). In the NJCS, random assignment into JC was significantly related to the attainment of a degree (12.9 percentage points for high school or GED and 22.3 percentage points for vocational degrees) and to the incidence of arrests (a reduction of

[3] Being economically disadvantaged, as defined by JC, means that the applicant's family receives public assistance (e.g., welfare or food stamps) and/or has an income that falls well below the poverty line.

[4] Throughout the analysis below, we utilize NJCS-supplied probability weights to account for interview non-response items and the different sampling into control and treatment groups for some of our subsamples.

[5] Random assignment in the NJCS is subject to noncompliance: about 27 percent of individuals did not comply with their treatment status. However, the presence of noncompliance has no consequence for our estimated bounds.

[6] We carefully construct the degree attainment indicator from the NJCS data so that, for each individual in the sample, it is defined 8 quarters after the week in which randomization took place. Also, note that youth in the control group were not prevented from earning a degree outside of JC. Indeed, 27% of them earned degrees during the NJCS.

3.7 percentage points). Thus, our instrument has strong reduced-form effects on both education and arrests.

While random assignment is independent of our outcome (an assumption of the IV estimator), it may not satisfy the exclusion restriction: there may be channels other than the attainment of a degree through which random assignment impacts crime. For instance, random assignment can directly impact the incidence of arrests through the variety of other services offered by JC (e.g., counseling, social skills training). For this reason, we employ the nonparametric bounds in Flores and Flores-Lagunes (2013; hereafter FF-L) that allow the instrument to have a direct effect on crime, in addition to its strong effect on degree attainment.

4 Econometric Methods

Under heterogeneous effects, IV methods point identify the local average treatment effect, or *LATE*, for compliers (those who change treatment status because of the the instrument), under the following standard assumptions (see, e.g., Imbens and Angrist, 1994): (A1) Random assignment of the instrument; (A2) Non-zero average effect of the instrument on the treatment; (A3) Individual-level monotonicity of the instrument on the treatment (also known as the "no defiers" assumption; see below); and (ER) The exclusion restriction: the instrument has an effect on the outcome exclusively through the treatment. In our context, (A1) is satisfied by design since we employ the random assignment into JC as our instrumental variable; similarly, (A2) is satisfied given the documented strong and statistically significant average effect of random assignment into JC on degree attainment. (A3) implies that there are no individuals that would not attain a degree if assigned to JC but would attain a degree if not assigned to JC. Given that JC's aim is to facilitate earning a degree through subsidized training in an environment conducive to that end, (A3) is plausibly satisfied—although we recognize that this IV assumption is often criticized for requiring that all individuals in the sample satisfy such monotonicity.

Given that the exclusion restriction (ER) is likely not satisfied in our context, as previously explained, we discard this assumption and estimate nonparametric bounds on a *LATE*. FF-L (2013) show that, in the IV setting above, the exclusion restriction can be discarded by allowing the instrument to have a direct (net) effect on the outcome in addition to the indirect (mechanism) effect that works through the treatment. They relate the mechanism effect to a *LATE* parameter for compliers under exposure to the instrument. Using this relation and the bounds on mechanism effects in FF-L (2010), bounds can be constructed on that $LATE$. The formal expressions of the bounds can be found in FF-L (2013), which are not reproduced here to save space. Those bounds rest on two additional assumptions (besides (A1)–(A3)) involving weak monotonicity of mean potential outcomes at the level of specific subpopulations.[7]

To formally present the assumptions, denote the potential outcomes by $Y(z, D(z))$, where z denotes values of the instrument ($z \in \{0, 1\}$) and $D(z)$ the potential values of the treatment. $D(1)$ is degree attainment when randomized into JC, and $D(0)$ when randomized out. We can partition the population into three (latent) principal strata based on values of $\{D(0), D(1)\}$ once (A1)–(A2) have been imposed (ruling out "defiers"): "compliers" ($\{D(0) = 0, D(1) = 1\}$; only earn a degree if $z = 1$), "always-takers" ($\{D(0) = 1, D(1) = 1\}$; earn a degree irrespective of z), and "never-takers" ($\{D(0) = 0, D(1) = 0\}$; do not earn a degree irrespective of z). We refer to them as c, at, and nt, respectively. The $LATE$ for which we construct bounds is defined as:

$$LATE \equiv E[Y(1, 1) - Y(1, 0)|D(1) - D(0) = 1]. \tag{1}$$

$LATE$ gives the effect of attaining a degree on the probability of being arrested for compliers, under assignment to JC (i.e., under $z = 1$). The two additional assumptions employed to construct bounds on $LATE$ are:

Assumption 4 *(Weak Monotonicity of Mean Potential Outcomes Within Strata)*

(a) $E[Y(1, D(1))|c] \leq E[Y(1, D(0))|c]$ *and*

(b) $E[Y(1, D(0))|k] \leq E[Y(0)|k]$, *for* $k = nt, at, c,$

and,

Assumption 5 *(Weak Monotonicity of Mean Potential Outcomes Across Strata)*

(a) $E[Y(1, D(0))|c] \leq E[Y(1, D(1))|nt]$; *(b)* $E[Y(1, D(1))|at] \leq E[Y(1, D(0))|c]$,

(c) $E[Y(0, D(0))|c)] \leq E[Y(0, D(0))|nt]$; *(d)* $E[Y(0, D(0))|at] \leq E[Y(0, D(0))|c]$

(e) $E[Y(1, D(1))|c] \leq E[Y(1, D(1))|nt]$; *(f)* $E[Y(1, D(1))|at] \leq E[Y(1, D(1))|c]$.

[7] In fact, bounds can be obtained by employing each of these weak monotonicity assumptions separately, and by combining them. In this paper, we present estimates of the bounds using both assumptions since we do not find evidence of violations of their testable implications.

(A4) posits that the attainment of a degree has an average non-positive effect on the incidence of arrests for c (part (a)); and that the average *net* effect (i.e., through channels other than D) of random assignment on the incidence of arrests is non-positive for each stratum (part (b)). The overall characteristics of the JC program argue in favor of (A4), as they are intended to improve participants' labor market outcomes and well-being (e.g., through its health services and counseling), both of which are predicted to have a non-positive average effect on arrests.

In turn, (A5) formalizes the notion that at likely have no worse average potential outcomes (i.e., no higher arrest incidence) than c, which in turn have no worse average potential outcomes than nt. Intuitively, the "no worse" strata have more favorable characteristics that lead to (weakly) better mean potential outcomes. We can gather some indirect evidence that this notion is plausible by comparing the means of the pre-treatment arrest rates across the three strata. Table 1 shows the mean pre-treatment arrest rate for all of our subgroups. We find that the three strata are weakly ranked (statistically) by their average pre-treatment arrest rate in the way posited by (A5), for all subgroups analyzed, except for Hispanics. For Hispanics, c have an unusually low pre-treatment arrest rate relative to that of at, which makes the difference statistically significant. This is the only instance of a statistically significant violation of the posited strata weak ranking among the six samples considered.

Table 1: Average Pre-Treatment Arrest Rates by Strata

	nt	c	at	$nt-c$	$c-at$	$nt-at$
Full Sample	0.291***	0.284***	0.243***	0.007	0.042*	0.049**
	(0.009)	(0.014)	(0.019)	(0.016)	(0.023)	(0.021)
Males	0.353***	0.322***	0.304***	0.031	0.017	0.049*
	(0.013)	(0.018)	(0.026)	(0.022)	(0.032)	(0.029)
Females	0.202***	0.209***	0.150***	-0.007	0.060*	0.053*
	(0.012)	(0.016)	(0.020)	(0.020)	(0.029)	(0.024)
Black Males	0.326***	0.353***	0.256***	-0.027	0.097**	0.070*
	(0.016)	(0.025)	(0.035)	(0.030)	(0.043)	(0.038)
Hispanic Males	0.365***	0.155***	0.344***	0.210***	-0.189***	0.021
	(0.028)	(0.033)	(0.059)	(0.043)	(0.068)	(0.066)
White Males	0.412***	0.291***	0.343***	0.121***	-0.052	0.069
	(0.023)	(0.029)	(0.041)	(0.037)	(0.050)	(0.049)

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively.

In addition, the combination of assumptions (A1) to (A5) provide three testable implications that can be used to falsify them (FF-L, 2013). Table 2 provides the results of the testable implications (their expressions listed in the first column) for all samples analyzed. The testable implications are not statistically rejected in our data for any sample. Thus, we proceed to estimate the nonparametric bounds on $LATE$ in (1) under assumptions (A1) to (A5).

Table 2: Testable Implications of Assumptions (A1) to (A5)

	Full	Males	Females	Black Males	Hispanic Males	White Males
$\mathrm{E}[Y\|Z{=}0,D{=}1]-\mathrm{E}[Y\|Z{=}0,D{=}0] \leq 0$	-0.118***	-0.183***	-0.002	-0.281***	-0.102	-0.055
	(0.022)	(0.029)	(0.033)	(0.038)	(0.064)	(0.049)
$\mathrm{E}[Y\|Z{=}1,D{=}1]-\mathrm{E}[Y\|Z{=}1,D{=}0] \leq 0$	-0.110***	-0.146***	-0.037*	-0.204***	-0.154***	-0.106***
	(0.015)	(0.021)	(0.019)	(0.029)	(0.048)	(0.035)
$\mathrm{E}[Y\|Z{=}1,D{=}1]-\mathrm{E}[Y\|Z{=}0,D{=}0] \leq 0$	-0.129***	-0.182***	-0.036*	-0.236***	-0.089*	-0.116***
	(0.016)	(0.022)	(0.021)	(0.029)	(0.048)	(0.035)

Notes: Standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively.

5 Results

Table 3 presents nonparametric bounds for the $LATE$ in (1) of degree attainment on the incidence of arrest between quarters 1–16 after random assignment, under assumptions (A1)-(A5). The estimated bounds for all groups are non-positive, as implied by our assumptions. For the full sample, the estimated lower bound is -0.118 percentage points, which represents an effect of -32.6% relative to the mean of the outcome in the no-degree-attainment "control" group (presented in the last column). Thus, negative effects larger than that (or -38.3% based on the 95% confidence interval) can be ruled out. This information is relevant for policy purposes. For example, if JC administrators were to take the necessary steps to increase the effect of JC on degree attainment to 0.50, then, *ceteris paribus*, JC can be expected to reduce the probability of being arrested by *at most* $(0.50)(0.118) = 0.059$, or about 17%.[8]

All of the point estimates of the effect of education on crime in Section 2 fall within our bounds for the full sample. Remarkably, even though we use a very different source of exogenous variation and methods, our results are very similar to those found using CSLs when comparing 95% confidence intervals (CI) covering the true effect of education on crime. We interpret this as evidence that our results, as well as those from using CSLs, may indeed be capturing a true causal effect of education on crime. The 95% CI for the percentage effect of one additional year of schooling on arrest rates in LM (2004) is $[-32.5\%, 2.6\%]$ for males, which is very close to our 95% CI for males of $[-39.7\%, 0.0\%]$.[9] Moreover, the average number of hours of academic and

[8] Clearly, these calculations rely on linear extrapolations.

[9] We converted the effects in LM's (2004) Table 10 from log-points to percentages, and adjusted the standard errors using the delta method.

Table 3: Bounds on the Local Average Treatment Effect of Attaining a Degree on the Incidence of Arrest under Assumptions (A1) to (A5)

	Lower Bound	Upper Bound	Percentage Arrested in "Control"[a]	Largest Percentage Effect
Full Sample	-0.118	0.000	36.23	-32.60
	[-0.139,	0.000]		
Males	-0.165	0.000	48.64	-33.95
	[-0.193,	0.000]		
Females	-0.043	0.000	18.16	-23.63
	[-0.069,	0.000]		
Black Males	-0.245	0.000	52.45	-46.67
	[-0.280,	0.000]		
Hispanic Males	-0.021	0.000	42.82	-4.91
	[-0.213,	0.000]		
White Males	-0.147	0.000	46.73	-31.48
	[-0.196,	0.000]		

[a] "Control" refers to group of individuals not attaining a degree.
Note: The table shows half-median unbiased estimates of the bounds and 95 percent confidence intervals (in brackets) for the true parameter value based on the method proposed by Chernozhukov, Lee, and Rosen (2011). See FF-L (2013) for details.

vocational instruction received while enrolled in JC for those individuals who participated and obtained a degree is 1,448. Considering that a typical high school student receives the equivalent of 1,080 hours of instruction during the school year (Schochet et al., 2001), obtaining a degree in JC is comparable to 1.34 years of schooling. Thus, our results for males suggest a 95% CI on the effect of a one-year increase in schooling on the probability of being arrested of about $[-29.6\%, 0.0\%]$ (dividing by 1.34), which is slightly tighter than the one in LM (2004). The 95% CI corresponding to the other estimates discussed in Section 2 are: $[-34.3\%, 0.3\%]$ in Anderson (2014); $[-14\%, 0.5\%]$ in Hjalmarsson, Holmlund, and Lindquist (2014); and $[-35.5\%, -0.07\%]$ in Machin, Marie, and Vujić (2011).

Comparing the results by gender, the estimated lower bound on the percentage-point effect for males is about four times that of females. Since females have considerably lower arrest rates than males, the estimated lower bound on their percentage effect is somewhat closer to that of males (23.63% vs. 33.95%). This is in line with U.S. evidence from Anderson (2014), where percentage effects of CSLs on arrest rates for men are 17%, and a statistically insignificant 10.5% for females. In turning to the results by race/ethnicity, we focus on males since they observe considerably higher arrest rates than females. While there is a large degree of overlap among their bounds, there are marked differences in the estimated lower bounds and largest percentage effects for black, Hispanic, and white males. The lower bound on the $LATE$ for Hispanic males is -0.021, implying that, at most, degree attainment reduces the arrests of Hispanic males by only 4.91%. For black (white) males, the lower bound shows that there is, at most, a 24.5 (14.7) percentage-point reduction in the arrest rate due to the attainment of a degree, which is related to a percentage effect of −46.67% (−31.48%). While it is not possible to conclude from these results that the effect is larger for blacks than whites (or even Hispanics), the fact that the estimated lower bound is much larger (in absolute value) for blacks than for whites is consistent with the finding in LM (2004) that the effect of schooling on crime reduction is larger for blacks. While none of the studies mentioned in Section 2 explicitly examine the effect of education on crime for Hispanics, one ought to be cautious when looking at our results. There seems to be much heterogeneity within the Hispanic sample, leading to very imprecisely estimated bounds for this group.

6 Conclusion

We estimate the effect of attaining a high school, GED, or vocational degree on arrests, focusing on a nationally representative group of disadvantaged youth in the U.S. A number of recent studies analyzing the causal effect of education on crime have employed changes in compulsory schooling laws—a natural experiment—as an instrumental variable for educational attainment. We employ the randomization into the Job Corps program within the National Job Corps Study as an source of exogenous variability in educational attainment. Instead of employing traditional instrumental variables estimators, we construct bounds on the causal effect of interest under relatively weak assumptions that do not require the exclusion restriction (the instrument having an impact on crime only through educational attainment) to hold.

Our main finding is that, despite the different assumptions, methods, and source of exogenous variation we exploit, our results are fairly consistent with the previous estimates of the causal effect of education on crime based on compulsory schooling laws. We interpret this as suggestive evidence that our estimated bounds and the results of those studies plausibly capture a true causal effect of education on crime. However, further research is needed since, unfortunately, our estimated bounds and the 95% confidence intervals in studies using compulsory schooling laws remain relatively wide, and in many cases are not able to exclude a zero effect. This is particularly the case for females because of their arrest rates—and very likely also their effects—are substantially lower than those of males. Finally, the heterogeneity in our estimated lower bounds for Hipanics, blacks, and whites suggests that there may be considerable differences in the effect of education on arrest rates among these groups.

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