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# Bounds on Average and Quantile Treatment Effects of Job Corps Training on Wages

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## ABSTRACT

*We review and extend nonparametric partial identification results for average and quantile treatment effects in the presence of sample selection. These methods are applied to assessing the wage effects of Job Corps, United States' largest job-training program targeting disadvantaged youth. Excluding Hispanics, our estimates suggest positive program effects on wages both at the mean and throughout the wage distribution. Across the demographic groups analyzed, the statistically significant estimated average and quantile treatment effects are bounded between 4.6 and 12 percent, and 2.7 and 14 percent, respectively. We also document that the program's wage effects vary across quantiles and demographic groups.*

## I. Introduction

Sample selection is a well-known and commonly found problem in applied econometrics that arises when there are factors simultaneously affecting both the outcome and whether or not the outcome is observed. Sample selection arises, for

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example, when analyzing the effects of a given policy on the performance of firms, as there are common factors affecting both the performance of the firm and the firm's decision to exit or remain in the market or when evaluating the effects of an intervention on students' test scores if students can self-select into taking the test. Even in a controlled or natural experiment in which the intervention is randomized, outcome comparisons between treatment and control groups yield biased estimates of causal effects if the probability of observing the outcome is affected by the intervention. For instance, Sexton and Hebel (1984) employ data from a controlled experiment to analyze the effect of an antismoking assistance program for pregnant women on birth weight. Sample selection arises in this context if the program has an effect on fetal death rates. An example of a natural experiment where sample selection bias may arise is on the study of the effects of the Vietnam-era draft status on future health, as draft-eligible men may experience higher mortality rates (Hearst, Newman, and Hulley 1986; Angrist, Chen, and Frandsen 2010; Dobkin and Shabani 2009; Eisenberg and Rowe 2009). In this paper, we review and extend recent nonparametric partial identification results for average and quantile treatment effects in the presence of sample selection. We do this in the context of assessing the wage effects of Job Corps, which is the largest job training program targeting disadvantaged youth in the United States.

The vast majority of both empirical and methodological econometric literature on the evaluation of labor market programs focuses on estimating their causal effects on total earnings (for example, Heckman, LaLonde, and Smith 1999; Imbens and Wooldridge 2009). Evaluating the impact on total earnings, however, leaves open a relevant question about whether these programs have a positive effect on the wages of participants through the accumulation of human capital, which is an important goal of active labor market programs. Earnings have two components: price and quantity supplied of labor. By focusing on estimating the impact of program participation on earnings, one cannot distinguish how much of the effect is due to human capital improvements. Assessing the labor market effect of program participation on human capital requires focusing on the price component of earnings—that is, wages—because wage increases are directly related to the improvement of participants' human capital through the program. Unfortunately, estimation of the program's effect on wages is not straightforward due to sample selection: Wages are observed only for those individuals who are employed (Heckman 1979). As in the previous examples, randomization of program participation does not solve this problem because the individual's decision to become employed is endogenous and occurs after randomization.

Recently, new partial identification results have been introduced that allow the construction of nonparametric bounds for average and quantile treatment effects that account for sample selection. These bounds typically require weaker assumptions than those conventionally employed for point identification of these effects.<sup>1</sup> We review

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1. Many of the methods employed for point identification of treatment effects under sample selection require strong distributional assumptions that may not be satisfied in practice, such as bivariate normality (Heckman 1979). One may relax this distributional assumption by relying on exclusion restrictions (Heckman 1990; Imbens and Angrist 1994; Abadie, Angrist, and Imbens 2002), which require variables that determine selection into the sample (employment) but do not affect the outcome (wages). It is well known, however, that in the case of employment and wages it is difficult to find plausible exclusion restrictions (Angrist and Krueger 1999; Angrist and Krueger 2001).

these techniques and extend them by presenting a method to use covariates to narrow the bounds for quantile treatment effects. Subsequently, we use data from the National Job Corps Study (NJCS), a randomized evaluation of the Job Corps (JC) program, to empirically assess the effect of JC training on wages. We analyze effects both at the mean and at different quantiles of the wage distribution of participants, as well as for different demographic groups. We focus on estimating bounds for the subpopulation of individuals who would be employed regardless of participation in JC, as previously done in Lee (2009) and Zhang, Rubin, and Mealli (2008), among others. Wages are nonmissing under both treatment arms for this group of individuals, thus requiring fewer assumptions to construct bounds on their effect. This is also an important group of participants: It is estimated to be the largest group among eligible JC participants, accounting for close to 60 percent of them.

We start by considering the Horowitz and Manski (2000) bounds, which exploit the randomization in the NJCS and use the empirical support of the outcome. However, they are wide in our application. Subsequently, we proceed to tighten these bounds through the use of two monotonicity assumptions within a principal stratification framework (Frangakis and Rubin 2002). The first states individual-level weak monotonicity of the effect of the program on employment. This assumption was also employed by Lee (2009) to partially identify average wage effects of JC. The second assumption (not considered by Lee 2009) is on mean potential outcomes across strata, which are subpopulations defined by the potential values of the employment status variable under both treatment arms. These assumptions result in informative bounds for our parameters.

We contribute to the literature in two ways. First, we review, extend, and apply recent partial identification results to deal with sample selection. In particular, we illustrate a way to analyze treatment effects on different unconditional quantiles of the outcome distribution in the presence of sample selection by employing the set of monotonicity assumptions described above.<sup>2</sup> Thus, our focus is on treatment effects on quantiles of the unconditional or marginal distribution of the outcome (for example, Firpo, Fortin, and Lemieux 2009) rather than on conditional quantiles (for example, Koenker and Bassett 1978). In addition, we propose a method to employ a covariate to narrow trimming bounds for unconditional quantile treatment effects. Second, we add to the literature analyzing the JC training program by evaluating its effect on wages with these methods. With a yearly cost of about \$1.5 billion, JC is America's largest job training program. As such, this federally funded program is under constant examination and, given legislation seeking to cut federal spending, the program's operational budget is currently under scrutiny (see, for example, Korte 2011). Our results suggest that the program is effective in increasing wages. Moreover, they contribute to a policy-relevant question regarding the potential heterogeneity of the wage impacts of JC at different points of the wage distribution, and across different demographic groups. In this way, we add to a growing literature analyzing the effectiveness of active labor market programs across different demographic groups (Heckman and Smith 1999; Abadie, Angrist, and Imbens 2002; Flores-Lagunes, Gonzalez, and Neumann 2010; Flores et al. 2012).

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2. Other recent work (to be discussed below) that employs bounds on quantile treatment effects under different monotonicity assumptions are Blundell et al. (2007) and Lechner and Melly (2010).

Our empirical results characterize the heterogeneous impact of JC training at different points of the wage distribution. The estimated bounds for a sample that excludes the group of Hispanics suggest positive effects of JC on wages, both at the mean and throughout the wage distribution. For the various non-Hispanic demographic groups analyzed, the statistically significant estimated average effects are bounded between 4.6 and 12 percent, while the statistically significant quantile treatment effects are bounded between 2.7 and 14 percent. Our analysis by race and gender reveals that the positive effects for blacks appear larger in the lower half of their wage distribution, while for whites the effects appear larger in the upper half of their wage distribution. Non-Hispanic females in the lower part of their wage distribution do not show statistically significant positive effects of JC on their wages, while those in the upper part do. Lastly, our set of estimated bounds for Hispanics are wide and include zero.<sup>3</sup>

The paper is organized as follows. Section II presents the sample selection problem and the Horowitz and Manski (2000) bounds. Sections III and IV discuss, respectively, bounds on average and quantile treatment effects, as well as the additional assumptions we consider. Section V describes the JC program and the NJCS, and Section VI presents the empirical results from our application. Section VII concludes.

## II. Sample Selection and the Horowitz-Manski Bounds

We describe the sample selection problem in the context of estimating the causal effect of a training program (for example, JC) on wages, where the problem arises because—even in the presence of random assignment—only the wages of those employed are observed. Formally, consider having access to data on  $N$  individuals and define a binary treatment  $T_i$ , which indicates whether individual  $i$  has participated in the program ( $T_i = 1$ ) or not ( $T_i = 0$ ). We start with the following assumption:

*Assumption A.  $T_i$  is randomly assigned.*

To illustrate the sample selection problem, assume for the moment that the individual's wage is a linear function of a constant term, the treatment indicator  $T_i$  and a set of pretreatment characteristics  $X_{1i}$ .<sup>4</sup>

$$(1) \quad Y_i^* = \beta_0 + T_i\beta_1 + X_{1i}\beta_2 + U_{1i},$$

where  $Y_i^*$  is the latent wage for individual  $i$ , which is observed conditional on the self-selection process into employment. This process is also assumed (for the moment) to be linearly related to a constant, the treatment indicator  $T_i$  and a set of pretreatment characteristics  $X_{2i}$ ,

$$(2) \quad S_i^* = \delta_0 + T_i\delta_1 + X_{2i}\delta_2 + U_{2i},$$

3. Our set of estimated bounds for Hispanics does not employ one of our assumptions (individual-level monotonicity of the treatment on employment) for reasons that will be discussed in subsequent sections.

4. Linearity is assumed here to simplify the exposition of the sample selection problem. The nonparametric approach to address sample selection employed in this paper does not impose linearity or functional form assumptions to partially identify the treatment effects of interest.

Similarly,  $S_i^*$  is a latent variable representing the individual's propensity to be employed. Let  $S_i$  denote the observed employment indicator that takes values  $S_i = 1$  if individual  $i$  is employed and 0 otherwise. Then,  $S_i = \mathbb{1}[S_i^* \geq 0] = 1$ , where  $\mathbb{1}[\cdot]$  is an indicator function. Therefore, we observe individual  $i$ 's wage,  $Y_i$ , when  $i$  is employed ( $S_i = 1$ ) and it remains latent when unemployed ( $S_i = 0$ ). In this setting, which assumes treatment effects are constant over the population, the parameter of interest is  $\beta_1$ .

Conventionally, point identification of  $\beta_1$  requires strong assumptions such as joint independence of the errors ( $U_{1i}, U_{2i}$ ) in the wage and employment equations and the regressors  $T_i, X_{1i}$ , and  $X_{2i}$ , plus bivariate normality of ( $U_{1i}, U_{2i}$ ) (Heckman 1979). The bivariate normality assumption about the errors can be relaxed by relying on exclusion restrictions (Heckman 1990; Imbens and Angrist 1994), which requires variables that determine employment but do not affect wages, or equivalently, variables in  $X_{2i}$  that do not belong in  $X_{1i}$ . However, it is well known that finding such variables that go along with economic reasoning in this situation is extremely difficult (Angrist and Krueger 1999, 2001). More generally, in many economic applications it is difficult to find valid exclusion restrictions.

An alternative approach suggests that the parameters can be bounded without relying on distributional assumptions or on the availability and validity of exclusion restrictions. Horowitz and Manski (2000; HM hereafter) proposed a general framework to construct bounds on treatment effects when data are missing due to a nonrandom process, such as self-selection into nonemployment ( $S_i^* < 0$ ), provided that the outcome variable has a bounded support.

To illustrate HM's bounds, let  $Y_i(0)$  and  $Y_i(1)$  be the potential (counterfactual) wages for unit  $i$  under control ( $T_i = 0$ ) and treatment ( $T_i = 1$ ), respectively. The relationship between these potential wages and the observed  $Y_i$  is that  $Y_i = Y_i(1)T_i + Y_i(0)(1 - T_i)$ . Define the average treatment effect (ATE) as:

$$(3) \quad ATE = E[Y_i(1) - Y_i(0)] = E[Y_i(1)] - E[Y_i(0)].$$

Conditional on  $T_i$  and the observed employment indicator  $S_i$ , the ATE in Equation 3 can be written as:

$$(4) \quad ATE = E[Y_i | T_i = 1, S_i = 1] \Pr(S_i = 1 | T_i = 1) \\ + E[Y_i(1) | T_i = 1, S_i = 0] \Pr(S_i = 0 | T_i = 1) \\ - E[Y_i | T_i = 0, S_i = 1] \Pr(S_i = 1 | T_i = 0) \\ - E[Y_i(0) | T_i = 0, S_i = 0] \Pr(S_i = 0 | T_i = 0).$$

Examination of Equation 4 reveals that, under random assignment, we can identify from the data all the conditional probabilities ( $\Pr(S_i = s | T_i = t)$ , for  $(t, s) = (0, 1)$ ) and also the expectations of the wage when conditioning on  $S_i=1$  ( $E[Y_i | T_i = 1, S_i = 1]$  and  $E[Y_i | T_i = 0, S_i = 1]$ ). Sample selection into nonemployment makes it impossible to point identify  $E[Y_i(1) | T_i = 1, S_i = 0]$  and  $E[Y_i(0) | T_i = 0, S_i = 0]$ . We can, however, construct HM bounds on these unobserved objects provided that the support of the outcome lies in a bounded interval  $[Y^{LB}, Y^{UB}]$ , because this implies that the values for these unobserved objects are restricted to such interval. Thus, HM's upper and lower bounds ( $UB^{HM}$  and  $LB^{HM}$ , respectively) are identified as follows:

$$\begin{aligned}
 (5) \quad UB^{HM} &= E[Y_i | T_i = 1, S_i = 1] \Pr(S_i = 1 | T_i = 1) + Y^{UB} \Pr(S_i = 0 | T_i = 1) \\
 &\quad - E[Y_i | T_i = 0, S_i = 1] \Pr(S_i = 1 | T_i = 0) - Y^{LB} \Pr(S_i = 0 | T_i = 0) \\
 LB^{HM} &= E[Y_i | T_i = 1, S_i = 1] \Pr(S_i = 1 | T_i = 1) + Y^{LB} \Pr(S_i = 0 | T_i = 1) \\
 &\quad - E[Y_i | T_i = 0, S_i = 1] \Pr(S_i = 1 | T_i = 0) - Y^{UB} \Pr(S_i = 0 | T_i = 0).
 \end{aligned}$$

Note that these bounds do not employ distributional or exclusion restrictions assumptions. They are nonparametric and allow for heterogeneous treatment effects—that is, nonconstant effects over the population. On the other hand, a cost of imposing only Assumption A and boundedness of the outcome is that the HM bounds are often wide. Indeed, this is the case in our application, as will be shown below. For this reason, we take this approach as a building block and proceed by imposing more structure through the use of assumptions that are typically weaker than the distributional and exclusion restriction assumptions needed for point identification.

### III. Bounds on Average Treatment Effects

Lee (2009) and Zhang, Rubin, and Mealli (2008) employ monotonicity assumptions that lead to a trimming procedure that tightens the HM bounds. They implicitly or explicitly employ the principal stratification framework of Frangakis and Rubin (2002) to motivate and derive their results. Principal stratification provides a framework for analyzing causal effects when controlling for a posttreatment variable that has been affected by treatment assignment. In the context of analyzing the effect of JC on wages, the affected posttreatment variable is employment. In this framework, individuals are classified into “principal strata” based on the potential values of employment under each treatment arm. Comparisons of outcomes by treatment assignment within strata can be interpreted as causal effects because which strata an individual belongs to is not affected by treatment assignment.

More formally, let the potential values of employment be denoted by  $S_i(0)$  and  $S_i(1)$  when  $i$  is assigned to control and treatment, respectively. We can partition the population into strata based on the values of the vector  $\{S_i(0), S_i(1)\}$ . Because both  $S_i$  and  $T_i$  are binary, there are four principal strata defined as  $NN : \{S_i(0) = 0, S_i(1) = 0\}$ ,  $EE : \{S_i(0) = 1, S_i(1) = 1\}$ ,  $EN : \{S_i(0) = 1, S_i(1) = 0\}$ , and  $NE : \{S_i(0) = 0, S_i(1) = 1\}$ . In the context of the application to JC,  $NN$  is the stratum of those individuals who would not be employed regardless of treatment assignment, while  $EE$  is the stratum of those who would be employed regardless of treatment assignment. The stratum  $EN$  represents those who would be employed if assigned to control but unemployed if assigned to treatment, and  $NE$  is the stratum of those who would be unemployed if assigned to control but employed if assigned to treatment. Given that strata are defined based on the potential values of  $S_i$ , the stratum an individual belongs to is unobserved. A mapping of the observed groups based on  $(T_i, S_i)$  to the unobserved strata above is depicted in the first two columns of Table 1.

Lee (2009) and Zhang, Rubin, and Mealli (2008) focus on the average effect of a program on wages for individuals who would be employed regardless of treatment status—that is, the  $EE$  stratum. This stratum is the only one for which wages are

**Table 1**  
*Observed Groups Based on Treatment and Employment Indicators ( $T_i, S_i$ ) and Principal Strata (PS) Mixture Within Groups.*

Observed groups by ( $T_i, S_i$ )	Principal Strata (PS)	PS after imposing Individual-level monotonicity
(0,0)	<i>NN and NE</i>	<i>NN and NE</i>
(1,1)	<i>EE and NE</i>	<i>EE and NE</i>
(1,0)	<i>NN and EN</i>	<i>NN</i>
(0,1)	<i>EE and EN</i>	<i>EE</i>

observed under both treatment arms, and thus fewer assumptions are required to construct bounds for its effects. The average treatment effect for this stratum, which we also consider, is:

$$(6) \quad ATE_{EE} = E[Y_i(1) | EE] - E[Y_i(0) | EE].$$

**A. Bounds Adding Individual-Level Monotonicity**

To tighten the HM bounds, we employ the following individual-level monotonicity assumption about the relationship between the treatment (JC) and the selection indicator (employment):

*Assumption B. Individual-Level Positive Weak Monotonicity of S in T:  $S_i(1) \geq S_i(0)$  for all  $i$ .*

This assumption states that treatment assignment can affect selection in only one direction, effectively ruling out the *EN* stratum. Both Lee (2009) and Zhang, Rubin, and Mealli (2008) employed this assumption, and similar assumptions are widely used in the instrumental variable (Imbens and Angrist 1994) and partial identification literatures (Manski and Pepper 2000; Bhattacharya, Shaikh, and Vytlačil 2008; Flores and Flores-Lagunes 2010). Although Assumption B is directly untestable, Assumptions A and B imply (but are not implied by)  $E[S_i | T_i = 1] - E[S_i | T_i = 0] \geq 0$ , which provides a testable implication for Assumption B (Imai 2008) in settings where Assumption A holds by design, as in our application. In other words, if the testable implication is not satisfied, then Assumption B is not consistent with the data. If it is satisfied, it only means that the data is consistent with this particular testable implication. However, it does not imply that Assumption B is valid. Thus, this testable implication is not to be interpreted as a statistical test of Assumption B.

In the context of JC, Assumption B seems plausible because one of the program’s stated goals is to increase the employability of participants. It does so by providing academic, vocational, and social skills training to participants, as well as job-search assistance. Indeed, the NJCS reported a positive and highly statistically significant average effect of JC on employment of four percentage points (Schochet, Burghardt, and Glazerman 2001). Nevertheless, because this assumption is imposed at the individual

level, it may be hard to satisfy as it requires that no individual has a negative effect of the program on employment (or, more generally, on selection).

Two factors that may cast doubt on this assumption in our application are that individuals are “locked-in” away from employment while undergoing training (van Ours 2004), and the possibility that trained individuals may have a higher reservation wage after training and thus may choose to remain unemployed (see, for example, Blundell et al. 2007). Note, however, that these two factors become less relevant the longer the time horizon after randomization at which the outcome is measured. For this reason, in Section VI we focus on wages at the 208th week after random assignment, which is the latest wage measure available in the NJCS.<sup>5</sup> In addition, there is one demographic group in our sample for which Assumption B is more likely to be violated. Hispanics in the NJCS were the only group found to have negative but statistically insignificant mean effects of JC on earnings and employment of  $-\$15.1$  and  $-3.1$  percent, respectively (Schochet, Burghardt, and Glazerman 2001; Flores-Lagunes, Gonzalez, and Neumann 2010). Although this does not show that the testable implication of Assumption B is statistically rejected for Hispanics, it casts doubt on the validity of this assumption for this group. Thus, we conduct a separate analysis for Hispanics that does not employ Assumption B in Section VI G.

Assumption B, by virtue of eliminating the *EN* stratum, allows the identification of some individuals in the *EE* and *NN* strata, as can be seen after deleting the *EN* stratum in the last column of Table 1. Furthermore, the combination of Assumptions A and B point identifies the proportions of each principal stratum in the population. Let  $\pi_k$  be the population proportions of each principal stratum,  $k = NN, EE, EN, NE$ , and let  $p_{s|t} \equiv \Pr(S_i = s \mid T_i = t)$  for  $t, s = 0, 1$ . Then,  $\pi_{EE} = p_{1|0}$ ,  $\pi_{NN} = p_{0|1}$ ,  $\pi_{NE} = p_{1|1} - p_{1|0} = p_{0|0} - p_{0|1}$ , and  $\pi_{EN} = 0$ . Looking at the last column of Table 1, we know that individuals in the observed group with  $(T_i, S_i) = (0, 1)$  belong to the stratum of interest *EE*. Therefore, we can point identify  $E[Y_i(0) \mid EE]$  in Equation 6 as  $E[Y_i \mid T_i = 0, S_i = 1]$ . However, it is not possible to point identify  $E[Y_i(1) \mid EE]$ , because the observed group with  $(T_i, S_i) = (1, 1)$  is a mixture of individuals from two strata, *EE* and *NE*. Nevertheless, it can be bounded. Write  $E[Y_i \mid T_i = 1, S_i = 1]$  as a weighted average of individuals belonging to the *EE* and *NE* strata:

$$(7) \quad E[Y_i \mid T_i = 1, S_i = 1] = \frac{\pi_{EE}}{(\pi_{EE} + \pi_{NE})} E[Y_i(1) \mid EE] + \frac{\pi_{NE}}{(\pi_{EE} + \pi_{NE})} E[Y_i(1) \mid NE].$$

Because the proportion of *EE* individuals in the group  $(T_i, S_i) = (1, 1)$  can be point-identified as  $\pi_{EE} / (\pi_{EE} + \pi_{NE}) = p_{1|0} / p_{1|1}$ ,  $E[Y_i(1) \mid EE]$  can be bounded from above by the expected value of  $Y_i$  for the  $(p_{1|0} / p_{1|1})$  fraction of the largest values of  $Y_i$  in the observed group  $(T_i, S_i) = (1, 1)$ . In other words, the upper bound is obtained under the scenario that the largest  $(p_{1|0} / p_{1|1})$  values of  $Y_i$  belong to the *EE* individuals. Thus, computing the expected value of  $Y_i$  after trimming the lower tail of the distribution of  $Y_i$  in  $(T_i, S_i) = (1, 1)$  by  $1 - (p_{1|0} / p_{1|1})$  yields an upper bound for the *EE* group. Similarly,  $E[Y_i(1) \mid EE]$  can be bounded from below by the expected value of  $Y_i$  for the  $(p_{1|0} / p_{1|1})$  fraction of the smallest values of  $Y_i$  for those in the same observed group.

5. Zhang, Rubin, and Mealli (2009) provide some evidence that the estimated proportion of individuals who do not satisfy the individual-level assumption (the *EN* stratum) falls with the time horizon at which the outcome is measured after randomization.

The resulting upper ( $UB_{EE}$ ) and lower ( $LB_{EE}$ ) bounds for  $ATE_{EE}$  are (Lee 2009; Zhang, Rubin, and Mealli 2008):

$$(8) \quad \begin{aligned} UB_{EE} &= E[Y_i | T_i = 1, S_i = 1, Y_i \geq y_{1-(p_{10}/p_{11})}^{11}] - E[Y_i | T_i = 0, S_i = 1], \\ LB_{EE} &= E[Y_i | T_i = 1, S_i = 1, Y_i \leq y_{(p_{10}/p_{11})}^{11}] - E[Y_i | T_i = 0, S_i = 1], \end{aligned}$$

where  $y_{1-(p_{10}/p_{11})}^{11}$  and  $y_{(p_{10}/p_{11})}^{11}$  denote the  $1 - (p_{10} / p_{11})$  and the  $(p_{10} / p_{11})$  quantiles of  $Y_i$  conditional on  $T_i = 1$  and  $S_i = 1$ , respectively. Lee (2009) shows that these bounds are sharp (that is, there are no shorter bounds possible under the current assumptions).

The bounds in Equation 8 can be estimated with sample analogs:

$$(9) \quad \begin{aligned} \widehat{UB}_{EE} &= \frac{\sum_{i=1}^n Y_i \cdot T_i \cdot S_i \cdot \mathbb{I}[Y_i \geq \widehat{y}_{1-\hat{p}}]}{\sum_{i=1}^n T_i \cdot S_i \cdot \mathbb{I}[Y_i \geq \widehat{y}_{1-\hat{p}}]} - \frac{\sum_{i=1}^n Y_i \cdot (1 - T_i) \cdot S_i}{\sum_{i=1}^n (1 - T_i) \cdot S_i} \\ \widehat{LB}_{EE} &= \frac{\sum_{i=1}^n Y_i \cdot T_i \cdot S_i \cdot \mathbb{I}[Y_i \leq \widehat{y}_{\hat{p}}]}{\sum_{i=1}^n T_i \cdot S_i \cdot \mathbb{I}[Y_i \leq \widehat{y}_{\hat{p}}]} - \frac{\sum_{i=1}^n Y_i \cdot (1 - T_i) \cdot S_i}{\sum_{i=1}^n (1 - T_i) \cdot S_i}, \end{aligned}$$

where  $\widehat{y}_{1-\hat{p}}$  and  $\widehat{y}_{\hat{p}}$  are the sample analogs of the quantities  $y_{1-(p_{10}/p_{11})}^{11}$  and  $y_{(p_{10}/p_{11})}^{11}$  in (8), respectively, and  $\hat{p}$ , the sample analog of  $(p_{10} / p_{11})$ , is calculated as follows:

$$(10) \quad \hat{p} = \frac{\sum_{i=1}^n (1 - T_i) \cdot S_i}{\sum_{i=1}^n (1 - T_i)} / \frac{\sum_{i=1}^n T_i \cdot S_i}{\sum_{i=1}^n T_i}.$$

Lee (2009) shows that these estimators are asymptotically normal.

**B. Tightening the Bounds by Adding Weak Monotonicity of Mean Potential Outcomes Across Strata**

We present a weak monotonicity assumption of mean potential outcomes across the *EE* and *NE* strata that tightens the bounds in Equation 8. This assumption was originally proposed by Zhang and Rubin (2003) and employed in Zhang, Rubin, and Mealli (2008):

*Assumption C. Weak Monotonicity of Mean Potential Outcomes Across the EE and NE Strata:*  
 $E[Y(1) | EE] \geq E[Y(1) | NE].$

Intuitively, in the context of JC, this assumption formalizes the notion that the *EE* stratum is likely to be comprised of more “able” individuals than those belonging to the *NE* stratum. Because “ability” is positively correlated with labor market outcomes, one would expect wages for the individuals who are employed regardless of treatment status (the *EE* stratum) to weakly dominate on average the wages of those individuals who are employed only if they receive training (the *NE* stratum). Hence, Assumption C requires a positive correlation between employment and wages. Although Assumption C is not directly testable, one can indirectly gauge its plausibility by comparing

the average of pretreatment covariates that are highly correlated with wages between the *EE* and *NE* strata, as we illustrate in Section VIB below.

Employing Assumptions A, B, and C results in tighter bounds. To see this, recall that the average outcome in the observed group with  $(T_i, S_i) = (1, 1)$  contains units from two strata, *EE* and *NE*, and can be written as the weighted average shown in Equation 7. By replacing  $E[Y(1) | NE]$  with  $E[Y(1) | EE]$  in Equation 7 and using the inequality in Assumption C, we have that  $E[Y_i | T_i = 1, S_i = 1] \leq E[Y_i(1) | EE]$ , and thus that  $E[Y(1) | EE]$  is bounded from below by  $E[Y_i | T_i = 1, S_i = 1]$ . Therefore, the lower bound for  $ATE_{EE}$  becomes:  $E[Y_i | T_i = 1, S_i = 1] - E[Y_i | T_i = 0, S_i = 1]$ . Imai (2008) shows that these bounds are sharp.

To estimate the bounds under Assumptions A, B, and C, note that the upper bound estimator of Equation 8 remains  $UB_{EE}$  from Equation 9, although the estimator of the lower bound is the corresponding sample analog of  $E[Y_i | T_i = 1, S_i = 1] - E[Y_i | T_i = 0, S_i = 1]$ :

$$(11) \quad \widehat{LB}_{EE}^C = \frac{\sum_{i=1}^n Y_i \cdot T_i \cdot S_i}{\sum_{i=1}^n T_i \cdot S_i} - \frac{\sum_{i=1}^n Y_i \cdot (1 - T_i) \cdot S_i}{\sum_{i=1}^n (1 - T_i) \cdot S_i}$$

**C. Narrowing Bounds on  $ATE_{EE}$  Using a Covariate**

Under Assumptions A and B, Lee (2009) shows that (i) grouping the sample based on the values of a pretreatment covariate *X*, (ii) applying the trimming procedure to construct bounds for each group, and (iii) averaging the bounds across these groups, results in narrower bounds for  $ATE_{EE}$  as compared to those in Equation 8. This result follows from the properties of trimmed means, and thus it is applicable only to bounds that involve trimming.<sup>6</sup>

Let *X* take values on  $\{x_1, \dots, x_j\}$ . By the law of iterated expectations, we can write the nonpoint identified term in Equation 6 as:

$$(12) \quad E[Y_i(1) | EE] = E_X\{E[Y_i(1) | EE, X_i = x_j] | EE\}.$$

Recall from Equation 8 that the bounds on  $E[Y_i(1) | EE]$  without employing *X* are given by  $E[Y_i | T_i = 1, S_i = 1, Y_i \geq y_{1-(p_{10}/p_{11})}^{11}] \geq E[Y_i(1) | EE] \geq E[Y_i | T_i = 1, S_i = 1, Y_i \leq y_{(p_{10}/p_{11})}^{11}]$ . Thus, it is straightforward to construct bounds on the terms  $E[Y_i(1) | EE, X_i = x_j]$  for the different values of *X* by implementing the trimming bounds on  $E[Y_i(1) | EE]$  discussed in Sections IIIA within cells with  $X_i = x_j$ . Let these bounds be denoted by  $LB_{EE}^{Y(1)}(x_j)$  and  $UB_{EE}^{Y(1)}(x_j)$ , so that  $UB_{EE}^{Y(1)}(x_j) \geq E[Y_i(1) | EE, X_i = x_j] \geq LB_{EE}^{Y(1)}(x_j)$ . It is important to note that the trimming proportions will differ across groups with different values of *X*, as the conditional probabilities  $p_{s|t}$  are now computed within cells with  $X_i = x_j$ . After substituting the trimming bounds on  $E[Y_i(1) | EE, X_i = x_j]$  into Equation 12 we obtain the bounds on  $ATE_{EE}$ , which are given by

6. The key property is that the mean of a lower (upper) tail truncated distribution is greater (less) than or equal to the average of the means of lower (upper) tail truncated distributions conditional on  $X = x$ , given that the proportion of the overall population that is eventually trimmed is the same.

$$(13) \quad \begin{aligned} UB_{EE}^* &= E_X\{UB_{EE}^{Y(0)}(x_j) \mid EE\} - E[Y_i \mid T_i = 0, S_i = 1] \\ LB_{EE}^* &= E_X\{LB_{EE}^{Y(0)}(x_j) \mid EE\} - E[Y_i \mid T_i = 0, S_i = 1] \end{aligned}$$

Lee (2009) shows that, under Assumptions A and B, these bounds are sharp and that, as compared to those in Equation 8,  $UB_{EE}^* \leq UB_{EE}$  and  $LB_{EE}^* \geq LB_{EE}$ .

An important step in the computation of the bounds in Equation 13 is the estimation of the term  $\Pr(X = x_j \mid EE)$  used in computing the outer expectation in the first term. By Bayes' rule, we can write  $\Pr(X = x_j \mid EE) = \pi_{EE}(x_j) \cdot \Pr(X = x_j) / [\sum_{j=1}^J \pi_{EE}(x_j) \cdot \Pr(X = x_j)]$ , where  $\pi_{EE}(x_j) = \Pr(EE \mid X = x_j)$  is the *EE* stratum proportion in the cell  $X = x_j$ . Thus, the sample analog estimators of the bounds in Equation 13 are:

$$(14) \quad \begin{aligned} \widehat{UB}_{EE}^* &= \sum_{j=1}^J \widehat{UB}_{EE}^{Y(0)}(x_j) \widehat{\Pr}(X = x_j \mid EE) - \frac{\sum_{i=1}^n Y_i \cdot (1 - T_i) \cdot S_i}{\sum_{i=1}^n (1 - T_i) \cdot S_i} \\ \widehat{LB}_{EE}^* &= \sum_{j=1}^J \widehat{LB}_{EE}^{Y(0)}(x_j) \widehat{\Pr}(X = x_j \mid EE) - \frac{\sum_{i=1}^n Y_i \cdot (1 - T_i) \cdot S_i}{\sum_{i=1}^n (1 - T_i) \cdot S_i} \end{aligned}$$

where  $\widehat{UB}_{EE}^{Y(0)}(x_j)$  and  $\widehat{LB}_{EE}^{Y(0)}(x_j)$  are the estimators of  $UB_{EE}^{Y(0)}(x_j)$  and  $LB_{EE}^{Y(0)}(x_j)$ , respectively, which are computed using the estimators of the bounds on  $E[Y_i(1) \mid EE]$  in the first term of Equation 9 for individuals with  $X_i = x_j$ , and

$$(15) \quad \begin{aligned} \widehat{\Pr}(X = x_j \mid EE) &= \frac{[\sum_{i=1}^n (1 - T_i) \cdot S_i \cdot \mathbb{1}\{X_i = x_j\}] / (\sum_{i=1}^n (1 - T_i) \cdot \mathbb{1}\{X_i = x_j\})}{[\sum_{i=1}^n \mathbb{1}\{X_i = x_j\}]} \\ &= \frac{[\sum_{i=1}^n (1 - T_i) \cdot S_i \cdot \mathbb{1}\{X_i = x_j\}] / (\sum_{i=1}^n (1 - T_i) \cdot \mathbb{1}\{X_i = x_j\})}{\sum_{j=1}^J \{[\sum_{i=1}^n (1 - T_i) \cdot S_i \cdot \mathbb{1}\{X_i = x_j\}] / (\sum_{i=1}^n (1 - T_i) \cdot \mathbb{1}\{X_i = x_j\})\}} \end{aligned}$$

Finally, under Assumptions A, B and C the procedure above is only applied to the upper bound on  $ATE_{EE}$ , as the lower bound does not involve trimming.

### IV. Bounds on Quantile Treatment Effects

We now extend the results presented in the previous section to construct bounds on quantile treatment effects (*QTE*) based on results by Imai (2008). The parameters of interest are differences in the quantiles of the marginal distributions of the potential outcomes  $Y(1)$  and  $Y(0)$ ; more specifically, we define the  $\alpha$ -quantile effect for the *EE* stratum:

$$(16) \quad QTE_{EE}^\alpha = F_{Y_i(1)|EE}^{-1}(\alpha) - F_{Y_i(0)|EE}^{-1}(\alpha),$$

where  $F_{Y_i(t)|EE}^{-1}(\alpha)$  denotes the  $\alpha$ -quantile of the distribution of  $Y_i(t)$  for the *EE* stratum.

Two recent papers have focused on partial identification of *QTE*. Blundell et al. (2007) derived sharp bounds on the distribution of wages and the interquantile range to study income inequality in the United Kingdom. Their work builds on the bounds on the conditional quantiles in Manski (1994), which are tightened by imposing stochastic dominance assumptions. Their stochastic dominance assumption is applied to

the distribution of wages of individuals observed employed and unemployed, whereby the wages of employed individuals are assumed to weakly dominate those of unemployed individuals (that is, positive selection into employment). In addition, they explore the use of exclusion restrictions to further tighten their bounds. Lechner and Melly (2010) analyze *QTE* of a German training program on wages. They impose an individual-level monotonicity assumption similar to our Assumption B, and employ the stochastic dominance assumption of Blundell et al. (2007) to tighten their bounds. In contrast to those papers, we take advantage of the randomization in the NJCS to estimate *QTE* by employing individual-level monotonicity (Assumption B) and by strengthening Assumption C to stochastic dominance applied to the *EE* and *NE* strata. Another difference is the parameters of interest: Blundell et al. (2007) focus on the population *QTE*, Lechner and Melly (2010) focus on the *QTE* for those individuals who are employed under treatment, and our focus is on the *QTE* for individuals who are employed regardless of treatment assignment.<sup>7</sup>

Let  $F_{Y_i|T_i=t, S_i=s}(\cdot)$  be the cumulative distribution of individuals' wages conditional on  $T_i = t$  and  $S_i = s$ , and let  $y_\alpha^{ts}$  denote its corresponding  $\alpha$ -quantile, for  $\alpha \in (0,1)$ , or  $y_\alpha^{ts} = F_{Y_i|T_i=t, S_i=s}^{-1}(\alpha)$ . Under Assumptions A and B, we can partially identify  $QTE_{EE}^\alpha$  as  $LB_{EE}^\alpha \leq QTE_{EE}^\alpha \leq UB_{EE}^\alpha$ , where (Imai 2008):

$$(17) \quad UB_{EE}^\alpha = F_{Y_i|T_i=1, S_i=1, Y_i \geq y_{1-(p_{10}/p_{11})}^{11}}^{-1}(\alpha) - F_{Y_i|T_i=0, S_i=1}^{-1}(\alpha)$$

$$LB_{EE}^\alpha = F_{Y_i|T_i=1, S_i=1, Y_i \leq y_{(p_{10}/p_{11})}^{11}}^{-1}(\alpha) - F_{Y_i|T_i=0, S_i=1}^{-1}(\alpha).$$

The intuition behind this result is the same as that for the bounds on  $ATE_{EE}$  in Equation 8.

$$F_{Y_i|T_i=1, S_i=1, Y_i \geq y_{1-(p_{10}/p_{11})}^{11}}^{-1}(\alpha)$$

and

$$F_{Y_i|T_i=1, S_i=1, Y_i \leq y_{(p_{10}/p_{11})}^{11}}^{-1}(\alpha)$$

correspond to the  $\alpha$ -quantile of  $Y_i$  after trimming, respectively, the lower and upper tail of the distribution of  $Y_i$  in  $(T_i, S_i) = (1, 1)$  by  $1 - (p_{10} / p_{11})$ , and thus they provide an upper and lower bound for  $F_{Y_{(1)EE}}^{-1}(\alpha)$  in Equation 16. Similar to Equation 8, the quantile  $F_{Y_{(0)EE}}^{-1}(\alpha)$  is point identified from the group with  $(T_i, S_i) = (0, 1)$ . Imai (2008) shows that the bounds in Equation 17 are sharp.

Using the same notation as in Equation 9, we estimate the bounds in Equation 17 using their sample analogs:

7. The treated-and-employed subpopulation of Lechner and Melly (2010) is a mixture of two strata: *EE* and *NE*. In our application, the *EE* stratum and the treated-and-employed subpopulation account for about the same proportion of the population (57 and 61 percent, respectively).

$$\begin{aligned}
(18) \quad \widehat{UB}_{EE}^{\alpha} &= \min \left\{ y : \frac{\sum_{i=1}^n T_i \cdot S_i \cdot \mathbb{I}[Y_i \geq \widehat{y}_{1-p}]}{\sum_{i=1}^n T_i \cdot S_i \cdot \mathbb{I}[Y_i \geq \widehat{y}_{1-p}]} \geq \alpha \right\} \\
&\quad - \min \left\{ y : \frac{\sum_{i=1}^n (1 - T_i) \cdot S_i \cdot \mathbb{I}[Y_i \leq y]}{\sum_{i=1}^n (1 - T_i) \cdot S_i} \geq \alpha \right\} \\
\widehat{LB}_{EE}^{\alpha} &= \min \left\{ y : \frac{\sum_{i=1}^n T_i \cdot S_i \cdot \mathbb{I}[Y_i \leq \widehat{y}_p]}{\sum_{i=1}^n T_i \cdot S_i \cdot \mathbb{I}[Y_i \leq \widehat{y}_p]} \geq \alpha \right\} \\
&\quad - \min \left\{ y : \frac{\sum_{i=1}^n (1 - T_i) \cdot S_i \cdot \mathbb{I}[Y_i \leq y]}{\sum_{i=1}^n (1 - T_i) \cdot S_i} \geq \alpha \right\}.
\end{aligned}$$

### A. Tightening Bounds on $QTE_{EE}^{\alpha}$ Using Stochastic Dominance

We tighten the bounds in Equation 17 by strengthening Assumption C to stochastic dominance. Let  $F_{Y_{(1)|EE}(\cdot)}$  and  $F_{Y_{(1)|NE}(\cdot)}$  denote the cumulative distributions of  $Y_i(1)$  for individuals who belong to the *EE* and *NE* strata, respectively:

*Assumption D. Stochastic Dominance Between the EE and NE Strata:*  $F_{Y_{(1)|EE}(y)} \leq F_{Y_{(1)|NE}(y)}$ , for all  $y$ .

This assumption directly imposes restrictions on the distribution of potential outcomes under treatment for individuals in the *EE* stratum, which results in a tighter lower bound relative to that in Equation 17. Under Assumptions A, B, and D, the resulting sharp bounds are (Imai 2008):  $LB_{EE}^{\alpha} \leq QTE_{EE}^{\alpha} \leq UB_{EE}^{\alpha}$ , where  $UB_{EE}^{\alpha}$  is as in Equation 17 and

$$(19) \quad LB_{EE}^{\alpha} = F_{Y_{(1)|T=1, S_1=1}^{-1}(\alpha) - F_{Y_{(1)|T=0, S_1=1}^{-1}(\alpha).$$

The estimator of the upper bound is still given  $\widehat{UB}_{EE}^{\alpha}$  by in Equation 18, while the estimator for  $LB_{EE}^{\alpha}$  is now given by:

$$\begin{aligned}
(20) \quad \widehat{LB}_{EE}^{\alpha} &= \min \left\{ y : \frac{\sum_{i=1}^n T_i \cdot S_i \cdot \mathbb{I}[Y_i \leq y]}{\sum_{i=1}^n T_i \cdot S_i} \geq \alpha \right\} \\
&\quad - \min \left\{ y : \frac{\sum_{i=1}^n (1 - T_i) \cdot S_i \cdot \mathbb{I}[Y_i \leq y]}{\sum_{i=1}^n (1 - T_i) \cdot S_i} \geq \alpha \right\}
\end{aligned}$$

### B. Narrowing Bounds on $QTE_{EE}^{\alpha}$ Using a Covariate

In this section we propose a way to use a pretreatment covariate  $X$  taking values on  $\{x_1, \dots, x_J\}$  to narrow the trimming bounds on  $F_{Y_{(1)|EE}^{-1}(\alpha)$  and, thus, the bounds on  $QTE_{EE}^{\alpha}$  in Equation 17. The idea is similar to that in Lee (2009) described in Section III.C, however, the nonlinear form of the quantile function  $F_{Y_{(1)|EE}^{-1}(\alpha)$  prevents us from directly using the law of iterated expectations as in Equation 12. To circumvent this

difficulty, we first focus on the cumulative distribution function (CDF) of  $Y_i(1)$  for the stratum  $EE$  at a given point  $\tilde{y}$ ,  $F_{Y_i(1)|EE}(\tilde{y})$ , and write it as the mean of an indicator function, which allows us to use iterated expectations. A similar approach was also used in Lechner and Melly (2010) to control for selection into treatment based on covariates. Using this insight we can write:

$$(21) \quad F_{Y_i(1)|EE}(\tilde{y}) = E[\mathbb{I}[Y_i(1) \leq \tilde{y}] | EE] = E_X\{E[\mathbb{I}[Y_i(1) \leq \tilde{y}] | EE, X_i = x_j] | EE\}.$$

Note that Equation 21 is similar to Equation 12, except that we now employ  $\mathbb{I}[Y_i(1) \leq \tilde{y}]$  as the outcome instead of  $Y_i(1)$ . Thus, the methods discussed in Section IIIC (and more generally, the trimming bounds in Section IIIA) can be used to bound  $F_{Y_i(1)|EE}(\tilde{y})$ . As in Section IIIC, let  $UB_{EE}^{\tilde{y}}(x_j)$  and  $LB_{EE}^{\tilde{y}}(x_j)$  denote the upper and lower bounds on  $E[\mathbb{I}[Y_i(1) \leq \tilde{y}] | EE, X_i = x_j]$  under Assumptions A and B, which are just the trimming bounds on  $E[Y_i(1) | EE]$  in the first part of Equation 8 within cells with  $X_i = x_j$  and employing as outcome the indicator  $\mathbb{I}[Y_i(1) \leq \tilde{y}]$  instead of  $Y_i$ . After substituting  $UB_{EE}^{\tilde{y}}(x_j)$  and  $LB_{EE}^{\tilde{y}}(x_j)$  into (21) we obtain the following upper and lower bounds on  $F_{Y_i(1)|EE}(\tilde{y})$  under Assumptions A and B:

$$(22) \quad F_{UB}(\tilde{y}) = E_X\{UB_{EE}^{\tilde{y}}(x_j) | EE\}$$

$$F_{LB}(\tilde{y}) = E_X\{LB_{EE}^{\tilde{y}}(x_j) | EE\}.$$

Importantly, by the results in Lee (2009) discussed in Section IIIC, these trimming bounds on  $F_{Y_i(1)|EE}(\tilde{y})$  are sharp and tighter than those not employing the covariate  $X$ .

Given bounds on  $F_{Y_i(1)|EE}(\tilde{y})$  for all  $\tilde{y} \in \mathfrak{R}$ , the lower (upper) bound on the  $\alpha$ -quantile of  $Y_i(1)$  for the  $EE$  stratum,  $F_{Y_i(1)|EE}^{-1}(\alpha)$ , is obtained by inverting the upper (lower) bound on  $F_{Y_i(1)|EE}(\tilde{y})$ . Using the bounds on  $F_{Y_i(1)|EE}(\tilde{y})$  in (22), the lower and upper bounds on  $F_{Y_i(1)|EE}^{-1}(\alpha)$  are obtained by finding the value  $y_\alpha$  such that  $F_{UB}(y_\alpha) = \alpha$  and  $F_{LB}(y_\alpha) = \alpha$ , respectively.<sup>8</sup> Therefore, the bounds on  $QTE_{EE}^\alpha$  under Assumptions A and B are given by:

$$(23) \quad UB_{EE}^{*\alpha} = F_{LB}^{-1}(\alpha) - F_{Y_i|T_i=0, S_i=1}^{-1}(\alpha)$$

$$LB_{EE}^{*\alpha} = F_{UB}^{-1}(\alpha) - F_{Y_i|T_i=0, S_i=1}^{-1}(\alpha).$$

We implement this procedure by estimating the bounds on  $F_{Y_i(1)|EE}(\tilde{y})$  in (22) at  $M$  different values of  $\tilde{y}$  spanning the support of the outcome, and then inverting the resulting estimated bounds to obtain the estimate of the bounds on the  $\alpha$ -quantile  $F_{Y_i(1)|EE}^{-1}(\alpha)$ . This last set of estimated bounds are then combined with the estimate of  $F_{Y_i|T_i=0, S_i=1}^{-1}(\alpha)$  to compute estimates of the bounds on  $QTE_{EE}^\alpha$  in (23). The bounds on  $F_{Y_i(1)|EE}(\tilde{y}_m)$  at each point  $\tilde{y}_m$  ( $m = 1, \dots, M$ ) in (22) are estimated employing the estimators of the bounds on  $E[Y_i(1) | EE]$  in the first term of (9) for individuals with  $X_i = x_j$  and using as outcome the indicator function  $\mathbb{I}[Y_i(1) \leq \tilde{y}_m]$  instead of  $Y_i$ . Finally, just as in the case of the  $ATE_{EE}$  in Section III.C, under Assumptions A, B and D, the procedure above is only applied to the upper bound because the lower bound does not involve trimming.

8. Note that, because we are inverting the CDF, the lower (upper) bound on the quantile is computed employing the upper (lower) bound of the CDF.

## V. Job Corps and the National Job Corps Study

We employ the methods described in the previous sections to assess the effect of Job Corps (JC) on the wages of participants. JC is America's largest and most comprehensive education and job training program. It is federally funded and currently administered by the U.S. Department of Labor. With a yearly cost of about \$1.5 billion, JC annual enrollment ascends to 100,000 students (U.S. Department of Labor 2010). The program's goal is to help disadvantaged young people, ages 16 to 24, improve the quality of their lives by enhancing their labor market opportunities and educational skills set. Eligible participants receive academic, vocational, and social skills training at over 123 centers nationwide (U.S. Department of Labor 2010), where they typically reside. Participants are selected based on several criteria, including age, legal U.S. residency, economically disadvantage status, living in a disruptive environment, in need of additional education or training, and be judged to have the capability and aspirations to participate in JC (Schochet, Burghardt, and Glazerman 2001).

Being the nation's largest job training program, the effectiveness of JC has been debated at times. During the mid-1990s, the U.S. Department of Labor funded the National Job Corps Study (NJCS) to determine the program's effectiveness. The main feature of the study was its random assignment: Individuals were taken from nearly all JC's outreach and admissions agencies located in the 48 continuous states and the District of Columbia and randomly assigned to treatment and control groups. From a randomly selected research sample of 15,386 first time eligible applicants, approximately 61 percent were assigned to the treatment group (9,409) and 39 percent to the control group (5,977), during the sample intake period from November 1994 to February 1996. After recording their data through a baseline interview for both treatment and control experimental groups, a series of followup interviews were conducted at weeks 52, 130, and 208 after randomization (Schochet, Burghardt, and Glazerman 2001).

Randomization took place before participants' assignment to a JC center. As a result, only 73 percent of the individuals randomly assigned to the treatment group actually enrolled in JC. Also, about 1.4 percent of the individuals assigned to the control group enrolled in the program despite the three-year embargo imposed on them (Schochet, Burghardt, and Glazerman 2001). Therefore, in the presence of this noncompliance, the comparison of outcomes by random assignment to the treatment has the interpretation of the "intention-to-treat" (*ITT*) effect—that is, the causal effect of being offered participation in JC. Focusing on this parameter in the presence of noncompliance is common practice in the literature (see, for example, Lee 2009; Flores-Lagunes, Gonzalez, and Neumann 2010; Zhang, Rubin, and Mealli 2009). Correspondingly, our empirical analysis estimates nonparametric bounds for *ITT* effects, although for simplicity we describe our results in the context of treatment effects.

Our sample is restricted to individuals who have nonmissing values for weekly earnings and weekly hours worked for every week after random assignment, resulting in a sample size of 9,145.<sup>9</sup> This is the same sample employed by Lee (2009), which facilitates comparing the informational content of our additional assumption (Assump-

9. As a consequence, we implicitly assume—as do the studies cited in the previous paragraph—that the missing values are "missing completely at random."

tion C) to tighten the estimated bounds. We also analyze the wage effects of JC for the following demographic groups: non-Hispanics, blacks, whites, non-Hispanic males, non-Hispanic females, and Hispanics. As we further discuss in Section VIA, we separate Hispanics in order to increase the likelihood that Assumption B holds. Finally, we employ the NJCS design weights (Schochet 2001) throughout the analysis, because different subgroups in the population had different probabilities of being included in the research sample.

A potential concern with the NJCS data is measurement error (ME) in the variables of interest (wages, employment, and random assignment) and the extent to which it may affect our estimated bounds. Although random assignment ( $T$ ) is likely to be measured without error, both employment ( $S$ ) and wages ( $Y$ ) are self-reported and thus more likely to suffer from this problem. In principle, it is hard to know a priori the effect of ME on the estimated bounds, although accounting for plausible forms of ME will likely lead to wider bounds.<sup>10</sup> Note that ME in  $S$  may lead to misclassification of individuals into strata, affecting the trimming proportions of the bounds involving trimming, while ME in  $Y$  will likely affect the trimmed distributions and their moments.

Summary statistics of selected variables for the full sample of 9,145 individuals, by treatment assignment, are presented in Table 2.<sup>11</sup> This sample can be characterized as follows: Females comprise around 45 percent of the sample; blacks account for 50 percent of the sample, followed by whites with 27 percent, and by the ethnic group of Hispanics with 17 percent. As expected, given the randomization, the distribution of pretreatment characteristics in the sample is similar between treatment and control groups, with almost all of the differences in the means of both groups being not statistically significant at a 5 percent level (last two columns of Table 2). Importantly, the mean difference is not statistically significant for earnings in the year prior to randomization, which is the covariate ( $X$ ) employed in Sections VIC and VIF to narrow bounds. The corresponding differences in labor market outcomes at Week 208 after randomization for this sample is consistent with the previously found positive effect of JC on participants' weekly earnings of 12 percent and the positive effect on employment of four percentage points (Schochet, Burghardt, and Glazerman 2001).

The unadjusted difference in log wages 208 weeks after random assignment (our outcome of interest  $Y$ ) equals 0.037 and is statistically significant. This naïve estimate of the average effect of JC on wages is affected by sample selection bias, since the treatment, JC training, simultaneously affects both the outcome (log wages) and whether or not the outcome is observed (employment). In what follows, we present estimated bounds on the effect of JC on participants' wages that account for selection into employment.

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10. We are unaware of work assessing the effect of ME on estimated bounds that do not account for this feature of the data. A growing literature that employs bounding techniques to deal with ME includes Horowitz and Manski (1995), Bollinger (1996), Molinari (2008), and Gundersen, Kreider, and Pepper (2012). Extending the bounds in this paper to account for ME is beyond its scope.

11. A complete set of summary statistics for the full sample of 9,145 individuals and for each of the demographic groups to be analyzed is presented in the Internet Appendix.

**Table 2**  
*Summary Statistics of Selected Variables for the Full Sample, by Treatment Status*

Variable	Treatment status		Difference	Standard Error
	Control ( $T_i = 0$ )	Treatment ( $T_i = 1$ )		
Selected variables at baseline				
Female	0.458	0.452	-0.006	0.010
Age	18.800	18.891	0.091**	0.045
White	0.263	0.266	0.002	0.009
Black	0.491	0.493	0.003	0.010
Hispanic	0.172	0.169	-0.003	0.008
Other race	0.074	0.072	-0.002	0.005
Never married	0.916	0.917	0.002	0.006
Married	0.023	0.020	-0.003	0.003
Living together	0.040	0.039	-0.002	0.004
Separated	0.021	0.024	0.003	0.003
Has a child	0.192	0.189	-0.003	0.008
Number of child	0.272	0.274	0.002	0.013
Education	10.105	10.114	0.008	0.032
Selected labor market variables at baseline				
Earnings (X)	2814.362	2904.113	89.751	111.140
Have a job	0.192	0.197	0.006	0.008
Months employed	6.032	6.014	-0.018	0.064
Usual hours/week	34.904	35.436	0.532**	0.250
Usual weekly earnings	107.057	114.771	7.714	5.406
Selected variables after random assignment				
Employment at Week 208 (S)	0.566	0.607	0.041***	0.010
Log wages at Week 208 (Y)	1.991	2.028	0.037***	0.011
Observations	3,599	5,546		

Note: \*, \*\*, and \*\*\* denote statistical significance at a 90, 95, and 99 percent confidence level, respectively. Earnings (X) is the covariate used to narrow the bounds in Sections VIC and VIF.

## VI. Bounds on the Effect of Job Corps on Wages

We start by presenting the HM bounds, which are the basis for the other bounds discussed in Sections III and IV. To construct bounds on the average treatment effect of JC on wages, the HM bounds combine the random assignment in the NJCS (Assumption A) with the empirical bounds of the outcome (log wages at Week 208 after randomization). The empirical upper bound on the support of log wages at Week 208, denoted by  $Y^{UB}$  in Equation 5, is 5.99; while the lower bound,  $Y^{LB}$ , is  $-1.55$ . Using the expressions in Equation 5, the HM bounds are  $UB^{HM} = 3.135$  and  $LB^{HM} = -3.109$ , with a width of 6.244. Clearly, these bounds are wide and largely uninformative. In what follows, we add assumptions to tighten them.

### A. Bounds on $ATE_{EE}$ : Adding Individual-Level Monotonicity

Under individual-level monotonicity of JC on employment (Assumption B) we partially identify the average effect of JC on wages for those individuals who are employed regardless of treatment assignment (the *EE* stratum). Therefore, it is of interest to estimate the size of that stratum relative to the full population, which can be done under Assumptions A and B. Table 3 reports the estimated strata proportions for the full sample and for the demographic groups we consider. The *EE* stratum accounts for close to 57 percent of the population, making it the largest stratum. The second largest stratum is the “never employed” or *NN*, accounting for 39 percent of the population. Lastly, the *NE* stratum accounts for 4 percent (the stratum *EN* is ruled out by Assumption B). The relative magnitudes of the strata largely hold for all demographic groups (except *NE* for Hispanics). Interestingly, whites have the highest proportion of *EE* individuals at 66 percent, while blacks have the lowest at 51 percent.

The first column in Panel A of Table 4 reports estimated bounds for  $ATE_{EE}$  for the full sample using Equation 9 under Assumptions A and B.<sup>12</sup> Relative to the HM bounds, these bounds are much tighter: their width goes from 6.244 in the HM bounds to 0.121. Unlike the HM bounds, the present bounding procedure does not depend on the empirical support of the outcome. However, the bounds still include zero, as do the Imbens and Manski (2004; IM hereafter) confidence intervals reported in the last row of the panel. These confidence intervals include the true parameter of interest with a 95 percent probability. Thus, although Assumption B greatly tightens the HM bounds, it is not enough to rule out zero or a small negative effect of JC on log wages at Week 208.

As discussed in Section IIIA, the untestable individual-level weak monotonicity assumption of the effect of JC on employment may be dubious in certain circumstances. In the context of JC, it has been documented that Hispanics in the NJCS exhibited negative (albeit not statistically significant) average effects of JC on both their employment and weekly earnings, although for the other groups these effects were positive and highly statistically significant (Schochet, Burghardt, and Glazerman 2001; Flores-Lagunes, Gonzalez, and Neumann 2010). Although this evidence does not show that the testable implication of Assumption B discussed in Section IIIA is statistically rejected for Hispanics, it casts doubt on the validity of Assumption B for

12. Note that the results for the full sample reported in Panel A of Table 4 do not replicate those in Lee (2009) because he employs a transformation of the reported wages (see footnote 14 in that paper).

**Table 3**  
*Estimated Principal Strata Proportions by Demographic Groups under Analysis*

PS	Full Sample	Non-Hispanics	Whites	Blacks	Non-Hispanic Males	Non-Hispanic Females	Hispanics
<i>EE</i>	0.566	0.559	0.657	0.512	0.583	0.530	0.598
<i>NN</i>	0.393	0.392	0.303	0.436	0.377	0.410	0.400
<i>NE</i>	0.041	0.049	0.040	0.052	0.040	0.060	0.002
Observations	9,145	7,573	2,358	4,566	4,280	3,293	1,572

Note: All estimated proportions are statistically significant at the 1 percent level, except the proportion of NE individuals for the group of Hispanics.

**Table 4**  
*Bounds on the Average Treatment Effect of the EE Stratum for Log Wages at Week 208, by Demographic Groups*

	Full Sample	Non-Hispanics	Whites	Blacks	Non-Hispanic Females	Non-Hispanic Males
<b>Panel A—Under Assumptions A and B</b>						
Upper bound	0.099 (0.014)	0.118 (0.015)	0.120 (0.028)	0.116 (0.020)	0.120 (0.024)	0.114 (0.020)
Lower bound	-0.022 (0.016)	-0.018 (0.017)	0.000 (0.031)	-0.012 (0.021)	-0.023 (0.026)	-0.009 (0.023)
Width	0.121	0.136	0.120	0.129	0.143	0.123
95 percent IM confidence interval	[-0.049, 0.122]	[-0.046, 0.143]	[-0.050, 0.166]	[-0.047, 0.149]	[-0.066, 0.159]	[-0.047, 0.147]
<b>Panel B—Under Assumptions A, B, and C</b>						
Upper bound	0.099 (0.014)	0.118 (0.015)	0.120 (0.028)	0.116 (0.020)	0.120 (0.024)	0.114 (0.020)
Lower bound	0.037 (0.012)	0.050 (0.013)	0.056 (0.022)	0.053 (0.016)	0.046 (0.020)	0.052 (0.016)
Width	0.062	0.068	0.064	0.063	0.074	0.061
95 percent IM confidence interval	[0.018, 0.122]	[0.029, 0.143]	[0.019, 0.166]	[0.027, 0.149]	[0.014, 0.159]	[0.026, 0.147]

Note: Bootstrap standard errors in parentheses (based on 5,000 replications). IM refers to the Imbens and Manski (2004) confidence interval, which contains the true value of the parameter with a given probability.

this group. Therefore, we consider a sample that excludes Hispanics, as well as other Non-Hispanic demographic groups (whites, blacks, non-Hispanic males, and non-Hispanic females). We defer the analysis of Hispanics that does not employ Assumption B to Section VIG, where we also discuss other features of this group in the NJCS.

The remaining columns in Panel A of Table 4 present estimated bounds under Assumptions A and B for various demographic groups, along with their width and 95 percent IM confidence intervals. The second column presents the corresponding estimated bounds for the non-Hispanics sample. The upper bound for this group is larger than the one for the full sample, while the lower bound is less negative, which is consistent with the discussion above regarding Hispanics. The IM confidence intervals are wider for the non-Hispanics sample relative to the full sample, but they are more concentrated on the positive side of the real line. For the other groups (whites, blacks, and non-Hispanic females and males), none of the estimated bounds exclude zero, although whites and non-Hispanic males have a lower bound almost right at zero. In general, the IM confidence intervals for the last four demographic groups are wider than those of the full sample and non-Hispanics groups, which is a consequence of their smaller sample sizes.

We now check the testable implication of Assumption B mentioned in Section IIIA:  $E(S_i | T_i = 1) - E(S_i | T_i = 0) \geq 0$ . The left-hand-side of this expression is the proportion of individuals in the *NE* stratum ( $\pi_{NE}$ ), which is reported in Table 3 for all groups. From that table, it can be seen that for all non-Hispanic groups the estimated *NE* stratum proportions are between 0.04 and 0.06, and they are statistically significant at a 1 percent level (not shown in the table). For Hispanics, however, the corresponding proportion is a statistically insignificant 0.002. Thus, while the testable implication of Assumption B is strongly satisfied for all non-Hispanic groups, the data does not provide evidence in favor of it for Hispanics, making Assumption B dubious for this demographic group.

We close this section by noting, as does Lee (2009), that small and negative estimated lower bounds on the effect of JC on wages under the current assumptions can be interpreted as pointing toward positive effects. The reason is that the lower bound is obtained by placing individuals in the *EE* stratum at the bottom of the outcome distribution of the observed group with  $(T_i, S_i) = (1, 1)$ . Although this mathematically identifies a valid lower bound, it implies a perfect negative correlation between employment and wages that is implausible from the standpoint of standard models of labor supply, in which individuals with higher predicted wages are more likely to be employed. Indeed, one interpretation that can be given to Assumption C (employed in the next section) is that of formalizing this theoretical notion to tighten the lower bound.

### ***B. Bounds on $ATE_{EE}$ Adding Weak Monotonicity of Mean Potential Outcomes Across Strata***

Panel B of Table 4 presents the estimated bounds adding Assumption C for the full sample (first column) and the non-Hispanic demographic groups. This assumption has considerable identifying power as it results in tighter bounds for the  $ATE_{EE}$  compared to the previously estimated bounds, with the width being cut in about half for the full sample. Importantly, employing Assumption C yields estimated bounds that rule out

negative effects of JC training on log wages at Week 208. Looking at the IM confidence intervals on the bounds adding Assumption C, we see that with 95 percent confidence the estimated effect is positive. Thus, the effect of JC on log wages at Week 208 for *EE* individuals is statistically positive and between 3.7 and 9.9 percent. Note that the naïve estimate from Table 2 is numerically equivalent to the lower bound under Assumptions A, B and C, which indicates that failing to account for sample selection likely yields an underestimate of the true effect, under the current assumptions.

Comparing the first and second columns in Panel B of Table 4, it can be seen that the full sample and non-Hispanics have estimated bounds of similar width, although the bounds for non-Hispanics are shifted higher to an effect of JC on wages between 5 to 11.8 percent. The IM confidence intervals show that, despite the smaller sample size of non-Hispanics, this average effect is statistically significant with 95 percent confidence. The estimated bounds for the other demographic groups show some interesting results. All of the bounds and IM confidence intervals exclude zero, with the smallest lower bound being that of the full sample at 3.7 percent (all others are 4.6 percent and higher). Remarkably, the estimated bounds for all the demographic groups that exclude Hispanics are relatively similar, suggesting that their average effect of JC on wages for the *EE* stratum is between 4.6 and 12 percent. The differences in the confidence intervals across groups are likely driven by the differences in sample sizes. Overall, these results suggest positive average effects of JC on wages across the non-Hispanic demographic groups, and they reinforce the notion of a strong identifying power of Assumption C.

Given the strong identifying power of Assumption C, it is important to gauge its plausibility. Although a direct statistical test is not feasible, we can indirectly gauge its plausibility by looking at one of its implications. Assumption C formalizes the idea that the *EE* stratum possesses traits that result in better labor market outcomes relative to individuals in the *NE* stratum. Thus, we look at pretreatment covariates that are highly correlated with log wages at Week 208 and test whether, on average, individuals in the *EE* stratum indeed exhibit better characteristics at baseline relative to individuals in the *NE* stratum. We focus mainly on the following pretreatment variables: earnings, whether the individual held a job, months employed (all three in the year prior to randomization), and education at randomization.

To implement this idea, we compute average pretreatment characteristics ( $W$ ) for the *EE* and *NE* strata. Computing average characteristics for the *EE* stratum is straightforward because, under Assumptions A and B, the individuals in the observed group  $(T_i, S_i) = (0, 1)$  belong to and are representative of this stratum. Similarly, the mean  $E[W \mid NN]$  can be estimated from the individuals with  $(T_i, S_i) = (1, 0)$ , who belong to and are representative of the *NN* stratum. To estimate average characteristics for the *NE* stratum, note that their average can be written as a function of the averages of the whole population and the other strata, all of which can be estimated under Assumptions A and B. Let  $W$  be a pretreatment characteristic of interest, then,  $E[W \mid NE] = \{E[W] - \pi_{EE}E[W \mid EE] - \pi_{NN}E[W \mid NN]\} / \pi_{NE}$ .

The estimated differences between the average pretreatment variables employed for this exercise for the *EE* and *NE* strata were all positive, indicating “better” pretreatment labor market characteristics for the *EE* stratum. Formal tests of statistical significance for these differences, however, did not reject their equality (mainly because of the high variance in the estimation of  $E[W \mid NE]$ ). We conclude that this exercise

does not provide evidence against Assumption C, although the estimated differences suggest that it is a plausible assumption.<sup>13</sup>

### C. Narrowing Bounds on $ATE_{EE}$ Using a Covariate

We employ earnings in the year prior to randomization as a covariate  $X$  to narrow the bounds on  $ATE_{EE}$  based on the results in Section IIIC. This pretreatment covariate is highly correlated with log wages at Week 208. For each demographic group, we proceeded by splitting the sample into three groups based on values of  $X$ , each containing roughly the same number of observations. Then, bounds were computed for each group and averaged across groups using the weights  $\Pr(X = x_j | EE)$  from Section IIIC.<sup>14</sup>

Table 5 presents the estimated bounds when using earnings in the year prior to randomization to narrow the bounds in Table 4. Panel A shows that the estimated bounds are indeed narrower. The width reduction relative to Panel A of Table 4 is reported in the next to last row. The smallest width reduction is for blacks at 3.9 percent, while the largest is for whites at 28 percent. In this application, despite the width reduction, the qualitative results in Panel A of Table 4 are upheld, with the exception of the estimated bounds for whites, which now exclude zero (although the 95 percent IM confidence interval includes it). Panel B of Table 5 presents narrower estimated bounds under Assumptions A, B, and C. Although in this case the procedure only narrows the estimated upper bound that involves trimming, the width reduction achieved (relative to Panel B of Table 4) is comparable to that of the bounds under Assumptions A and B (Panel A of Table 5). As in the top panel, the conclusions that can be gathered from the narrower bounds are qualitatively similar to those in Table 4 (Panel B).

### D. Bounds on $QTE_{EE}^{\alpha}$ Under Individual-Level Monotonicity

We proceed to analyze the effects of JC on participant's wages beyond the average impact by providing estimated bounds for quantile treatment effects ( $QTE$ ) for the  $EE$  stratum,  $QTE_{EE}^{\alpha}$ . Before presenting these bounds, we first briefly discuss HM bounds on the population  $QTE$  that are analogous to the HM bounds on the  $ATE$  and that employ Assumption A and the empirical bounds of the outcome. To compute HM lower (upper) bounds on the population  $QTE$ , we assign to all unemployed individuals in the control group the empirical upper (lower) bound of the outcome and to all unemployed individuals in the treatment group the lower (upper) bound of the outcome. Subsequently, we compare the empirical outcome distributions of both groups by performing quantile regressions of the outcome on the treatment status.<sup>15</sup> For quantiles below 0.45 and above 0.55, the width and value of the estimated bounds on the population  $QTE$  (not shown in tables for brevity) are comparable to those of the estimated HM bounds on the  $ATE$  presented above. Although we obtain narrower bounds for the

13. The tables corresponding to this exercise can be found in the Appendix. Employing other pretreatment variables provided similar results (in essence, no evidence against Assumption C).

14. We decided to employ three groups to avoid having too few observations per group in the demographic groups with smaller sample sizes. The results are qualitatively similar when more groups are employed.

15. Note that the HM bounds on  $ATE$  provided at the beginning of Section VI can be calculated from a linear regression of the outcome on the treatment status after imputing the wages for the unemployed individuals as described above.

**Table 5**  
*Bounds on Average Treatment Effect of the EE Stratum for Log Wages at Week 208, by Demographic Groups, Employing Earnings in the Year Prior to Randomization (X) to Narrow the Bounds.*

	Full Sample	Non-Hispanics	Whites	Blacks	Non-Hispanic Females	Non-Hispanic Males
<b>Panel A: Under Assumptions A and B</b>						
Upper bound	0.096 (0.014)	0.113 (0.015)	0.102 (0.025)	0.113 (0.019)	0.117 (0.023)	0.107 (0.019)
Lower bound	-0.018 (0.016)	-0.012 (0.017)	0.015 (0.027)	-0.010 (0.020)	-0.018 (0.025)	-0.002 (0.021)
Width	0.114	0.125	0.087	0.123	0.134	0.109
Width reduction (vs. Table 3.B)	5.7%	8.0%	27.8%	3.9%	5.9%	11.4%
95 percent level IM confidence interval	[-0.043, 0.119]	[-0.040, 0.138]	[-0.030, 0.144]	[-0.043, 0.144]	[-0.059, 0.155]	[-0.038, 0.138]
<b>Panel B: Under Assumptions A, B, and C</b>						
Upper bound	0.096 (0.014)	0.113 (0.015)	0.102 (0.025)	0.113 (0.019)	0.117 (0.023)	0.107 (0.019)
Lower bound	0.037 (0.012)	0.050 (0.013)	0.056 (0.022)	0.053 (0.016)	0.046 (0.020)	0.052 (0.016)
Width	0.059	0.063	0.046	0.060	0.071	0.054
Width reduction (vs. Table 3.B)	4.9%	7.3%	28.2%	4.8%	4.3%	11.4%
95 percent level IM confidence interval	[0.018, 0.119]	[0.029, 0.138]	[0.019, 0.144]	[0.027, 0.144]	[0.014, 0.155]	[0.026, 0.138]

Note: Bootstrap standard errors in parentheses (based on 5,000 replications). IM refers to the Imbens and Manski (2004) confidence interval, which contains the true value of the parameter with a given probability.

quantiles 0.45, 0.50, and 0.55, they remain wide and practically uninformative. For example, the shortest bounds are found for the median and equal  $[-0.64, 0.64]$ . The assumptions we consider below substantially tighten these bounds.

To summarize our estimated bounds at several quantiles, we provide a series of figures for the different groups under analysis. To illustrate how the figures are constructed, Table 6 provides detailed numerical results for the sample of non-Hispanics.<sup>16</sup> The estimated bounds on  $QTE_{EE}^{\alpha}$  under Assumptions A and B, along with their corresponding IM confidence intervals, are shown in Figure 1. Recall that the estimated bounds for the  $ATE_{EE}$  under the same assumptions presented in Section VIA did not rule out zero for any of the groups under analysis. Looking at the estimated bounds on  $QTE_{EE}^{\alpha}$  for the full sample in Figure 1a, they rule out zero for all lower quantiles up to 0.7. Once IM confidence intervals are computed, though, only the bounds for the 0.2 quantile imply statistically significant positive effects of JC on log wages with 95 percent confidence. Consistent with the results from bounds on average effects, the estimated bounds on  $QTE_{EE}^{\alpha}$  for non-Hispanics in Figure 1b, which corresponds to the first and second columns in Table 6, are generally shifted towards the positive space relative to those of the full sample. For non-Hispanics, the estimated bounds also exclude zero for all lower quantiles up to 0.7, and the 95 percent IM confidence intervals rule out zero for the 0.5 quantile. The estimated bounds for these two samples suggest that JC is more likely to have positive effects on log wages for the lower quantiles of the wage distribution.

Looking at the results by race, Figures 1c and 1d show that the estimated bounds on  $QTE_{EE}^{\alpha}$  exclude zero for a number of lower quantiles up to 0.75 (with the exception of the 0.05 quantile for whites and the 0.75 quantile for blacks). However, probably due to the smaller sample sizes, when looking at the 95 percent IM confidence intervals for these groups only quantiles 0.55 and 0.65 for whites and the 0.05 quantile for blacks are statistically significant. It is worth noting that these two figures suggest that blacks may experience more positive effects of JC on wages in the lower quantiles of the wage distribution, while whites may experience more positive effects at the upper quantiles.

Figures 1e and 1f show the corresponding estimated bounds and 95 percent IM confidence intervals for non-Hispanic males and females, respectively. The bounds reflect a trend of excluding zero at the lower quantiles that is similar to that of the previous groups, albeit less clear for non-Hispanic females. Interestingly, non-Hispanic males show a greater number of estimated bounds excluding zero, which is probably due to a lower degree of heterogeneity in this group relative to non-Hispanic females.<sup>17</sup> Looking at the IM confidence intervals, none of them exclude zero for non-Hispanic females, while they do for quantiles 0.05, 0.1, and 0.45 for non-Hispanic males. These results suggest that inference for non-Hispanic females is more difficult due to their greater heterogeneity and smaller sample size.

To end this subsection, we remark that, while the bounds and IM confidence intervals for the average treatment effect of JC on wages under Assumptions A and B were

16. The complete numerical results for all the demographic groups under analysis are shown in the Internet Appendix.

17. By greater heterogeneity of non-Hispanic females relative to non-Hispanic males we mean that the former group shows higher standard deviation in key variables such as age, marital and cohabitation status, separated, presence of a child, number of children, and education. This is also true for the average characteristics of the corresponding subset of individuals in the *EE* stratum.

**Table 6**

*Bounds on Quantile Treatment Effects of the EE Stratum for Non-Hispanics, under Different Assumptions*

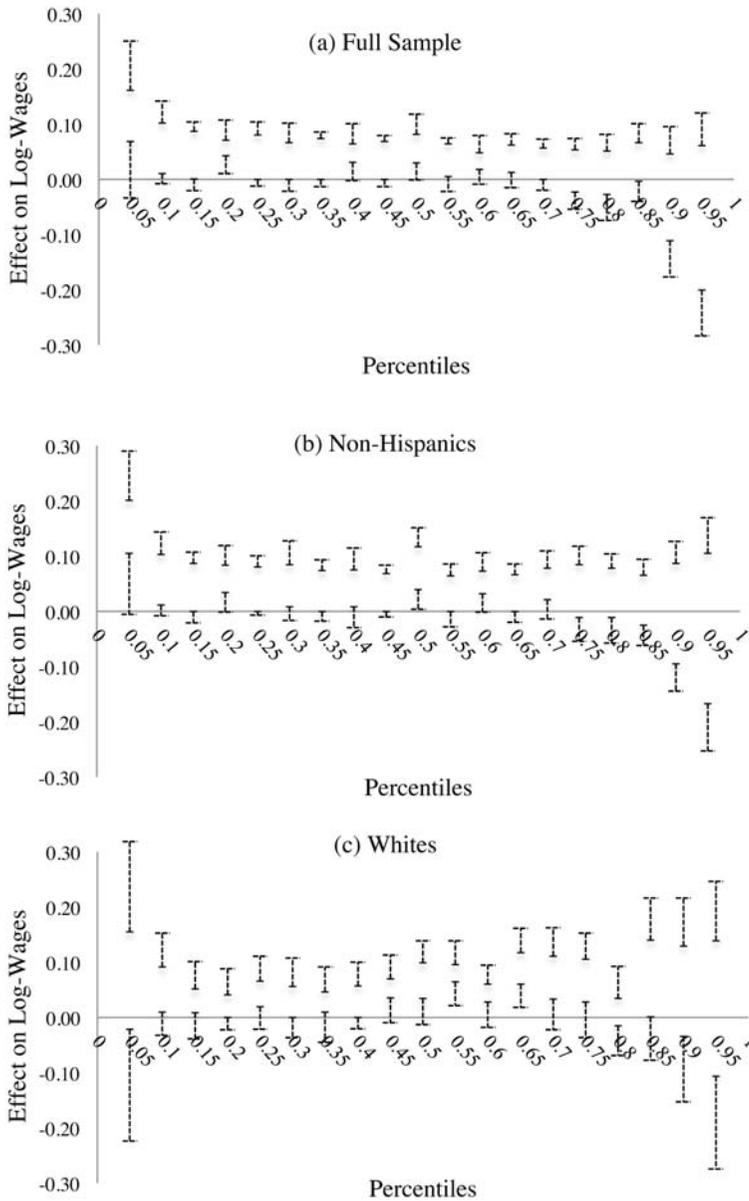
$\alpha$ -percentile	Bounds under Assumptions A and B		Bounds under Assumptions A, B, and D		Bounds under Assumptions A, B, D, and covariate $X$	
	Lower	Upper	Lower	Upper	Lower	Upper
0.05	0.105	0.201	0.105**	0.201**	0.105**	0.192**
0.10	0.011	0.102	0.011	0.102	0.011	0.102
0.15	0.000	0.087	0.027**	0.087**	0.027**	0.087**
0.20	0.034	0.083	0.043**	0.083**	0.043**	0.071**
0.25	0.000	0.080	0.020	0.080	0.020	0.080
0.30	0.008	0.085	0.044**	0.085**	0.044**	0.072**
0.35	0.000	0.074	0.023	0.074	0.023	0.074
0.40	0.009	0.074	0.039**	0.074**	0.039**	0.068**
0.45	0.000	0.069	0.035**	0.069**	0.035**	0.069**
0.50	0.039**	0.117**	0.065**	0.117**	0.065**	0.110**
0.55	0.000	0.065	0.065**	0.065**	0.065**	0.065**
0.60	0.032	0.073	0.038**	0.073**	0.038**	0.063**
0.65	0.000	0.066	0.061**	0.066**	0.061**	0.061**
0.70	0.021	0.078	0.054**	0.078**	0.054**	0.078**
0.75	-0.011	0.084	0.066**	0.084**	0.066**	0.077**
0.80	-0.011	0.078	0.078**	0.078**	0.078**	0.078**
0.85	-0.025	0.065	0.049**	0.065**	0.049**	0.065**
0.90	-0.095	0.087	0.071**	0.087**	0.071**	0.087**
0.95	-0.167	0.105	0.074**	0.105**	0.074**	0.105**

Notes: Using earnings in the year prior to randomization as a covariate ( $X$ ) to narrow the bounds. \*\* denotes that the corresponding 95 percent Imbens and Manski (2004) confidence interval for the true parameter value excludes zero.

inconclusive about its sign, the analysis of  $QTE_{EE}^{\alpha}$  suggests that positive effects of JC on wages tend to occur for lower and middle quantiles of the distribution. This is the case even when looking at groups with smaller sample sizes. Furthermore, the demographic groups analyzed seem to experience different  $QTE_{EE}^{\alpha}$ , both across quantiles and groups. Blacks appear to have larger positive effects at lower quantiles, while whites appear to have larger effects in the upper quantiles. Also, non-Hispanic males show more informative results than non-Hispanic females. Next, we add Assumption D (stochastic dominance) to tighten these bounds.

### *E. Bounds on $QTE_{EE}^{\alpha}$ Adding Stochastic Dominance*

Estimated bounds on  $QTE_{EE}^{\alpha}$  under Assumptions A, B, and D are summarized in Figure 2. The first noteworthy feature of these estimated bounds is that all of them exclude



**Figure 1**  
*Bounds and 95 Percent Imbens and Manski (2004) Confidence Intervals for QTE of the EE Stratum by Demographic Groups, under Assumptions A and B.*

Note: Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical lines.

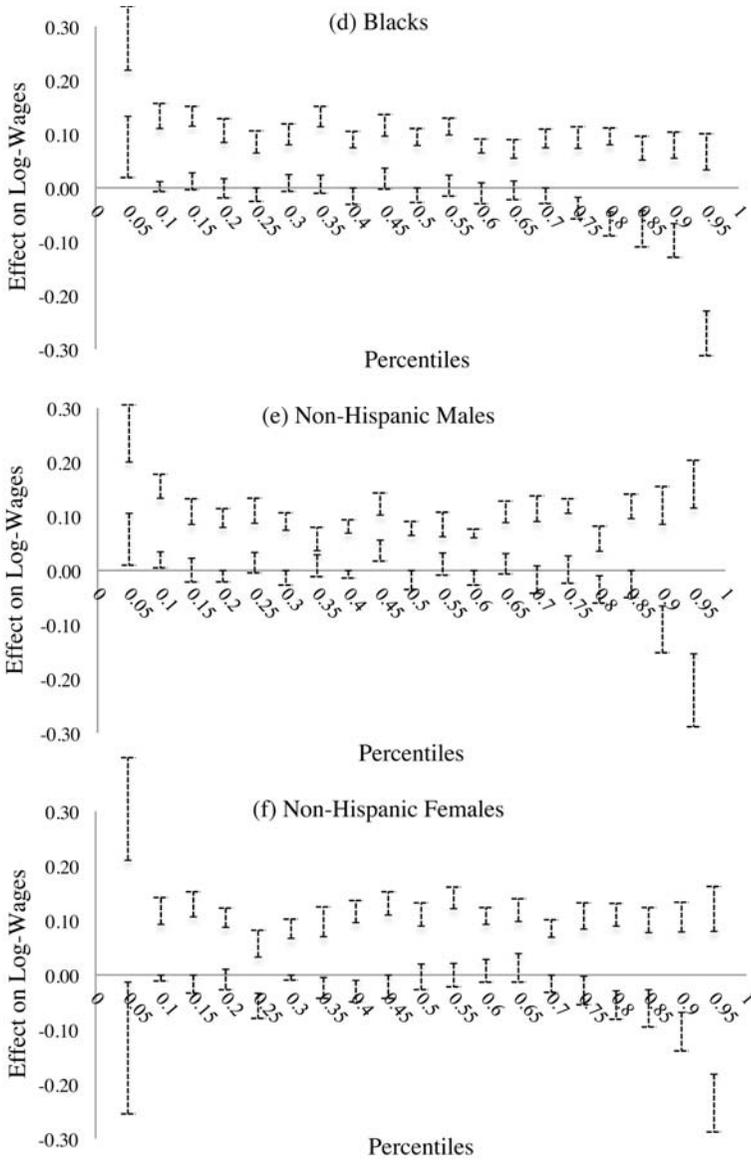
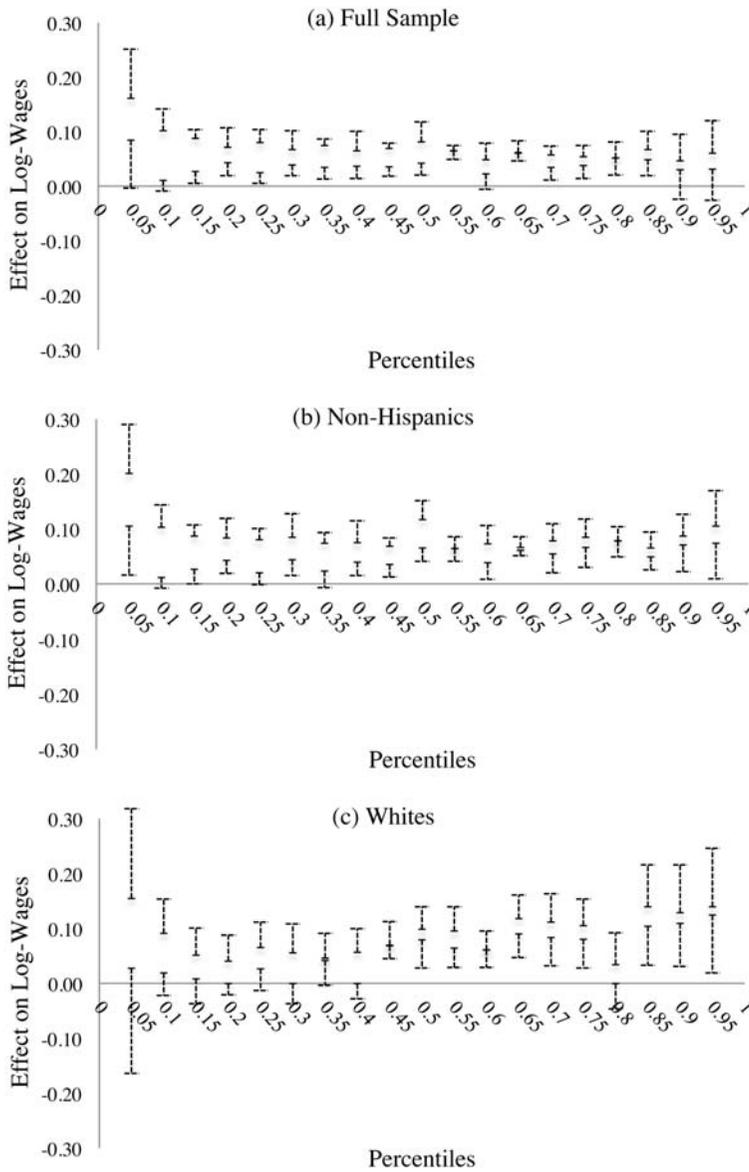


Figure 1 (continued)



**Figure 2**  
*Bounds and 95 Percent Imbens and Manski (2004) Confidence Intervals for QTE of the EE Stratum by Demographic Groups, under Assumptions A, B, and D.*

Note: Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical lines.



zero at all quantiles, which suggests that the effect of JC on wages is positive along the wage distribution for these groups. These bounds speak to the identifying power of the stochastic dominance assumption (Assumption D). Also noteworthy is that the general conclusions drawn from the estimated bounds in the previous subsection are maintained and reinforced in several instances.

Looking at the results for the full sample and non-Hispanics (Figures 2a and 2b, where the latter corresponds to the third and fourth columns in Table 6), we see again a shift toward more positive effects when Hispanics are excluded. Interestingly, in both samples, the lower and upper bounds for the quantiles 0.55 and 0.8 coincide, resulting in a point-identified effect of JC on wages for these two quantiles. Also, adding the stochastic dominance assumption results in 95 percent IM confidence intervals that exclude zero for most of the quantiles except for 0.05, 0.1, 0.6, 0.9, and 0.95 for the full sample and 0.1, 0.25, and 0.35 for the non-Hispanic sample. Concentrating on the latter sample, for which Assumption B is more likely to be satisfied, and excluding the bounds for the quantile 0.05 that differ from the rest, the bounds that exclude zero are between (roughly) 2.7 and 14 percent. In addition, the IM confidence intervals that exclude zero largely overlap, suggesting that the effects of JC on wages do not differ substantially across quantiles. The only clear outliers are the estimated bounds on the 0.05 quantile, which are between 10.5 and 20 percent.

The results by race are shown in Figures 2c and 2d. Adding Assumption D reinforces the notion that blacks likely exhibit larger positive impacts of JC on log wages in the lower portion of the wage distribution, while whites likely exhibit larger impacts on the upper quantiles. Indeed, the 95 percent IM confidence intervals for blacks in the lowest quantiles exclude zero but not those at the highest quantiles. The opposite is true for whites. However, despite this evidence being stronger than before, it appears inconclusive when looking at the IM confidence intervals, because there is a considerable amount of overlap on the intervals for both groups within quantiles. The IM confidence intervals also show that blacks have statistically significant positive effects of JC on wages throughout their wage distribution (except at quantiles 0.1, 0.25, 0.9, and 0.95), with estimated bounds that are between 3.1 and 11.5 percent (excluding the 0.05 quantile). Whites show statistically significant positive effects only for quantiles larger than 0.4 (except 0.8), with estimated bounds that are between 6.1 and 14 percent.

Figures 2e and 2f present the results by non-Hispanic gender groups. All the estimated bounds for these groups exclude zero at all quantiles, suggesting positive effects of JC on wages and illustrating the identifying power of adding the stochastic dominance assumption. When taking into consideration the 95 percent IM confidence intervals, we find statistically significant positive effects of JC on log wages for more than half of the quantiles considered. Interestingly, non-Hispanic females do not have any statistically significant effects throughout the lower half of their wage distribution up to quantile 0.4 (except at the 0.2 quantile), suggesting that non-Hispanic females in the upper half of the distribution are more likely to benefit from higher wages due to JC training. Aside from this distinction, there does not seem to be other substantial differences between gender groups, as judged by the large overlap in their IM confidence intervals. Considering confidence intervals that exclude zero, non-Hispanic females have estimated bounds that are between 4.4 to 12.1 percent, while those estimated

bounds for non-Hispanic males are between 3.6 to 13.4 percent (excluding the 0.05 quantile).<sup>18</sup>

### ***F. Narrowing Bounds on $QTE_{EE}^{\alpha}$ Using a Covariate***

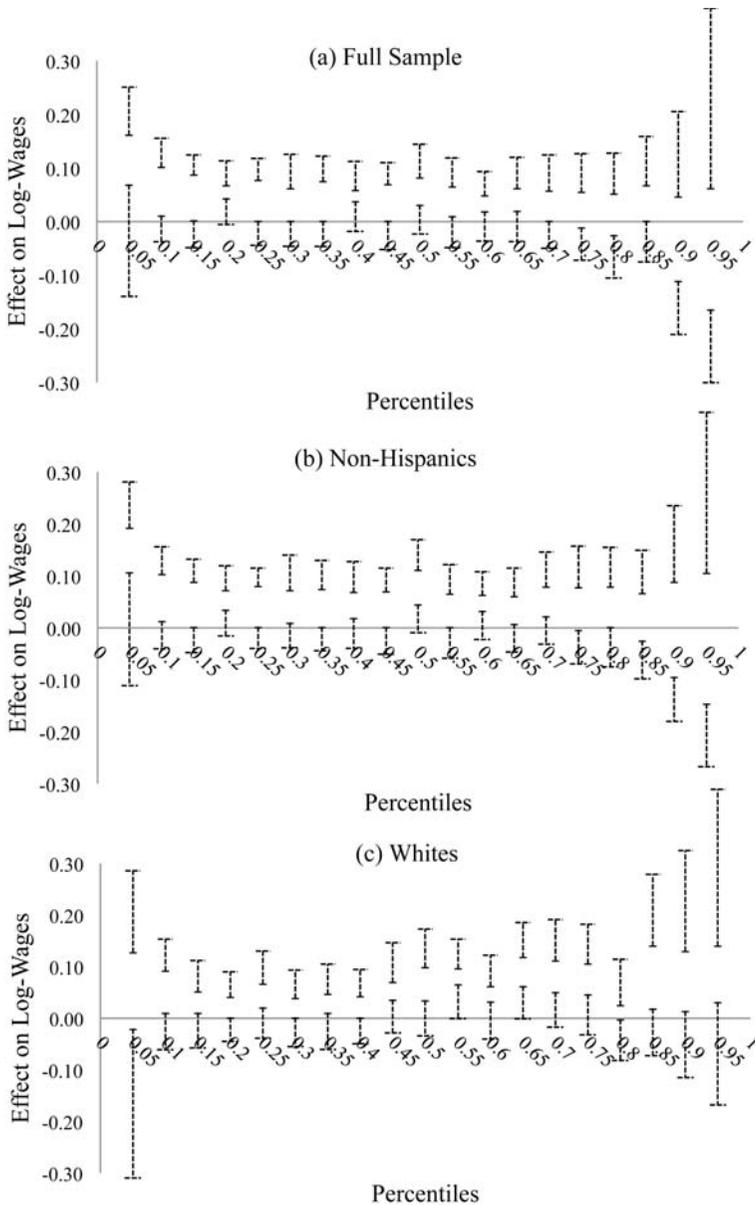
To narrow the trimming bounds on  $QTE_{EE}^{\alpha}$  we follow the procedure outlined in Section IVB employing earnings in the year prior to randomization as a covariate and breaking up the sample into three groups, as in Section VIC. To estimate the bounds on  $F_{Y(1)EE}(\tilde{y}_m)$  in Equation 22, we use 300 values of  $\tilde{y}$  that span the support of the outcome. The results are presented in Figures 3 and 4 for bounds under Assumptions A and B, and Assumptions A, B, and D, respectively.

The main insights can be summarized as follows. First, reductions in the width of the estimated bounds on  $QTE_{EE}^{\alpha}$  are observed in Figures 3 and 4 relative to those in Figures 1 and 2. Although most of the reductions are less than 20 percent, they range from 0 (no reduction) to 100 percent (point identification). Comparing the results in Figures 1 and 3, the reductions in estimated bounds' width across quantiles for the analyzed groups were, on average, 6 percent for the full sample, 9 percent for non-Hispanics, 14 percent for whites, 2 percent for blacks and non-Hispanic females, and 16 percent for non-Hispanic males. Comparing the results in Figures 2 and 4 that employ stochastic dominance, the reductions in width are more modest as only about a quarter of the estimated bounds' width were reduced (recall that only the lower bounds are subject to trimming and thus to reductions). On average, the reductions in width across quantiles were 9 percent for the full sample, 14 percent for non-Hispanics, 10 percent for whites, 2 percent for blacks, 4 percent for non-Hispanic females, and 11 percent for non-Hispanic males. Second, for this empirical application, the reductions in the width of the estimated bounds do not change the qualitative results that were discussed in previous sections. Third, looking at the IM confidence intervals of the estimated bounds that employ  $X$  to narrow them, it is evident that in our application the procedure results in wider IM confidence intervals. This is likely due to the required nonparametric estimation of the trimming bounds in Equation 22, which has to be performed for each of the three groups based on  $X$ .

### ***G. Estimated Bounds for Hispanics***

As previously discussed, the original reports in the NJCS found that the program effects on Hispanics' employment and earnings were negative and statistically insignificant (Schochet, Burghardt, and Glazerman 2001). This casts doubt on the individual-level monotonicity assumption of the program on employment that was used in analyzing the other demographic groups. The NJCS findings for Hispanics could not be explained by differences (relative to other groups) in baseline characteristics, program participation and degree attainment, duration of enrollment, characteris-

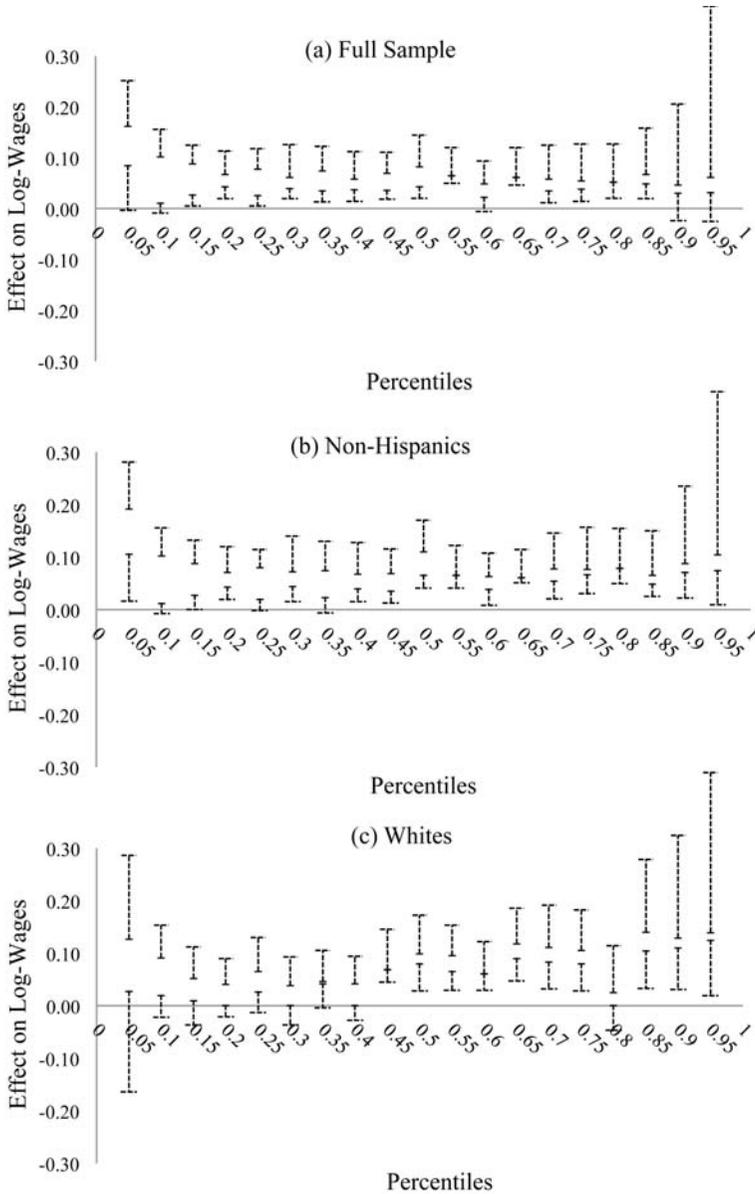
18. To indirectly gauge the plausibility of Assumption D in a similar fashion as Assumption C (see Section VIB), we proceeded to divide each corresponding sample into quintiles based on a given pretreatment covariate (we employ the same covariates as in Section VIB). Then, for each quintile we compute and test the difference in the average pretreatment covariate between the *EE* and *NE* strata. As it was the case with Assumption C, we do not find evidence against the stochastic dominance assumption for any of the samples analyzed. The results of this exercise can be found in the Internet Appendix.



**Figure 3**  
*Bounds and 95 Percent Imbens and Manski (2004) Confidence Intervals for QTE of the EE Stratum by Demographic Groups, Under Assumptions A and B, Using Earnings in the Year Prior to Randomization as a Covariate to Narrow the Bounds.*

Note: Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical lines.





**Figure 4**  
*Bounds and 95 Percent Imbens and Manski (2004) Confidence Intervals for QTE of the EE Stratum by Demographic Groups, Under Assumptions A, B and D, Using Earnings in the Year Prior to Randomization as a Covariate to Narrow the Bounds.*

Note: Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical line.

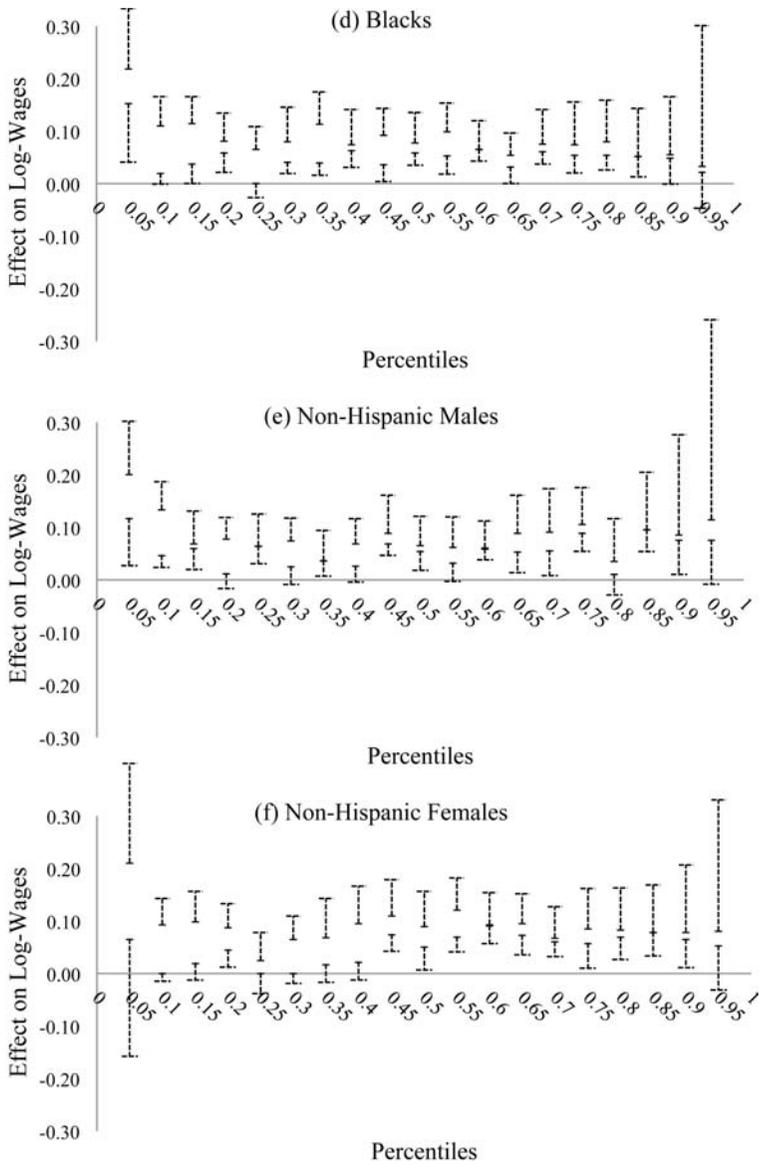


Figure 4 (continued)

tics of the centers attended, among others (Schochet, Burghardt, and Glazerman 2001). Subsequently, Flores-Lagunes, Gonzalez, and Neumann (2010) documented that the lack of effect on Hispanics can be partly attributed to the higher local unemployment rates that they face, and to the greater negative impact that Hispanics experience from the local unemployment rates that they face (both factors are especially contrasting relative to whites).<sup>19</sup> In this section, we do not attempt to provide new explanations for the lack of effect of JC on Hispanics' employment and earnings. Instead, we analyze the wage effects of JC for this group by presenting estimated bounds that do not employ the assumption of a nonnegative effect of JC on employment (Assumption B).

Zhang and Rubin (2003) and Zhang, Rubin, and Mealli (2008) show that, under Assumption A (randomly assigned  $T_i$ ), the four strata proportions in the second column of Table 1 are partially identified as follows (noting that they should sum up to one and are bounded between zero and one):  $\pi_{EE} = p_{1|0} - \pi_{EN}$ ,  $\pi_{NE} = p_{1|1} - p_{1|0} + \pi_{EN}$ , and  $\pi_{NN} = p_{0|1} - \pi_{EN}$ , with  $\pi_{EN}$  satisfying  $\max(0, p_{1|0} - p_{1|1}) \leq \pi_{EN} \leq \min(p_{1|0}, p_{0|1})$ . These bounds on the strata proportions can be used to construct bounds on  $ATE_{EE}$  and  $QTE_{EE}^{\alpha}$  that do not require Assumption B. However, the resulting bounds are expected to be wide given the considerable identifying power of Assumption B.<sup>20</sup> To conserve space, the expressions of the bounds that do not use Assumption B are presented in the Internet Appendix.

Table 7 reports, for the Hispanic sample, two sets of estimated bounds on the  $ATE_{EE}$  of JC on log wages that do not employ Assumption B. The first column shows estimated bounds under Assumption A only, while the last column adds Assumption C. As expected, the estimated bounds are wide:  $-0.451$  to  $0.359$  (under Assumption A) and  $-0.448$  to  $0.359$  (under Assumptions A and C). Interestingly, adding Assumption C now results in a fairly small tightening of the bounds. This is in contrast to the results presented in the previous sections that made use of Assumption B, where Assumption C had considerable identifying power. Figure 5 reports the estimated bounds on  $QTE_{EE}^{\alpha}$  for Hispanics under Assumption A (top panel) and under Assumptions A and D (bottom panel). Three points are noteworthy. First, as it was the case with the bounds on  $ATE_{EE}$ , the bounds without Assumption B are wide across the quantiles analyzed. Second, adding Assumption D now results in a very small tightening of the bounds, as it was the case above with Assumption C. Third, despite the wideness of both sets of estimated bounds for Hispanics, a pattern emerges in which they are considerably narrower for the upper part of the distribution of log wages. Thus, while positive or negative effects of JC on log wages for Hispanics cannot be ruled out, both large (but potentially plausible) negative and positive effects can be ruled out for the upper part of their wage distribution.

Finally, for comparison purposes, we computed bounds that do not employ Assumption B for all other demographic groups (which are available in the Internet Appendix). These estimated bounds (for both  $ATE_{EE}$  and  $QTE_{EE}^{\alpha}$ ) are also wide and include zero, although they are less wide than those for Hispanics. Hence, the inconclusiveness of

19. Additionally, Flores-Lagunes, Gonzalez, and Neumann (2010) document that JC appears to "shield" whites from the effects of adverse local unemployment rates, but not Hispanics or blacks.

20. Recall that Assumption B allows point identification of not only the strata proportions to trim the data, but also of  $E[Y_i(0) | EE]$  in Equation 6 and  $F_{Y_i(0)|EE}^{-1}(\alpha)$  in Equation 16.

**Table 7**

*Bounds on the Average Treatment Effect of the EE Stratum for Log Wages at Week 208 for Hispanics*

	Assumption A	Assumption A & C
Upper bound	0.359 (0.044)	0.359 (0.044)
Lower bound	-0.451 (0.053)	-0.448 (0.049)
Width	0.810	0.807
95 percent level IM confidence interval	[-0.538, 0.431]	[-0.528, 0.431]

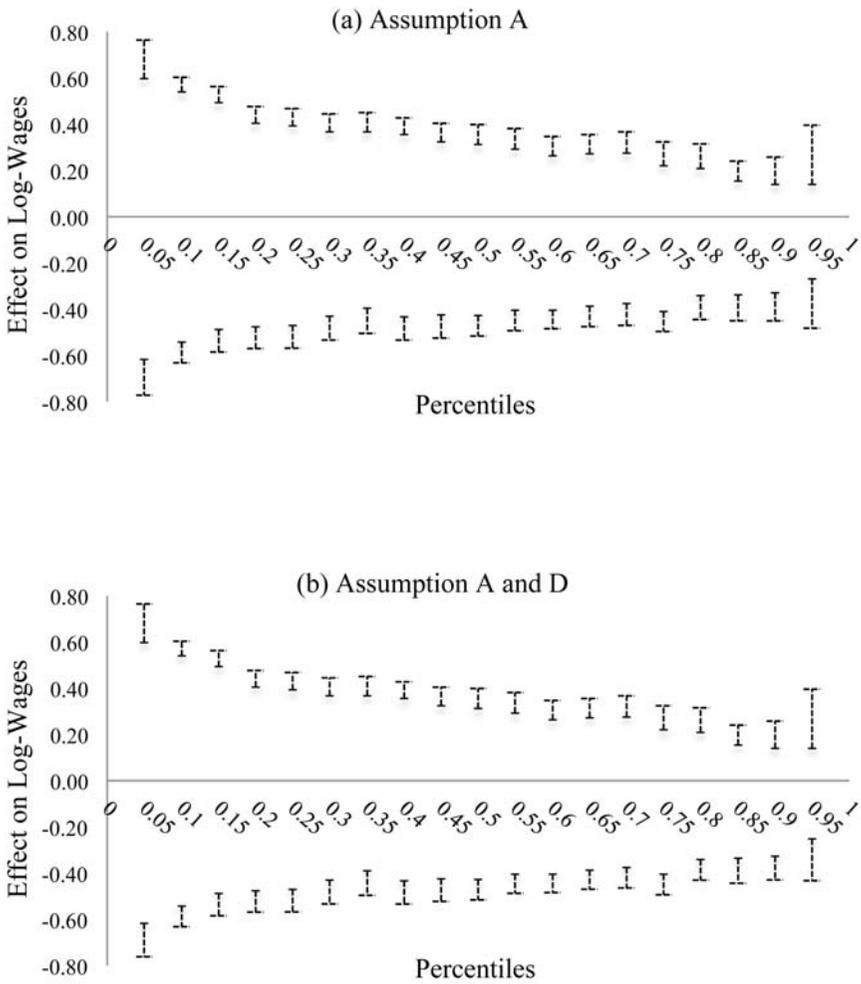
Note: Bootstrap standard errors in parentheses (with 5,000 replications). IM refers to the Imbens and Manski (2004) confidence interval, which contains the true value of the parameter with a given probability.

the estimated effects of JC on log wages for Hispanics can be due to a true lack of effect for them or to the relative uninformative nature of the assumptions being employed to estimate their bounds.

## VII. Conclusion

We review and extend recent nonparametric bounds for average and quantile treatment effects that account for sample selection, and that require weaker assumptions than those conventionally employed for point identification of these effects. These techniques are applied to the problem of assessing the effect of Job Corps (JC) training on wages accounting for nonrandom selection into employment. Because JC's stated goal is to enhance participants' human capital and labor market outcomes, research shedding light on the effects of JC on wages is important because positive wage effects can be related to human capital improvements. Under the assumptions we consider, our results suggest that JC has positive and statistically significant effects on wages for the individuals who would be employed regardless of participation in JC, not only at the mean but also at different points of the wage distribution, and for different demographic groups of interest (with the exception of Hispanics).

We start by exploiting the random assignment into the program in our data to construct Horowitz and Manski (2000) bounds, and then add an individual-level monotonicity assumption on the effect of JC on employment to tighten them. While the latter bounds cannot rule out negative average effects of JC on wages for those employed irrespective of treatment assignment, by constructing bounds on unconditional quantile treatment effects we find that for certain quantiles and demographic groups we are able to statistically rule out zero or negative effects. These results are noteworthy given that the lower bound under these assumptions is likely too pessimistic because it implies a theoretically implausible perfect negative correlation between wages and employment.



**Figure 5**  
*Bounds and 95 Percent Imbens and Manski (2004) Confidence Intervals for QTE of the EE Stratum for Hispanics, Under (a) Assumption A, and (b) Assumptions A and D.*

Note: Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical lines.

To further tighten the above bounds, we add assumptions formalizing the notion that individuals in some strata are likely to have better labor market outcomes than others, hence avoiding the perfect negative correlation between wages and employment implied by the previous bounds. The estimated bounds for the average effect of JC on wages for the individuals employed irrespective of treatment assignment suggest statistically significant positive effects. The estimated bounds for groups that exclude Hispanics are remarkably similar, with an estimated lower bound of about 4.6 percent and an upper bound of about 12 percent. We obtain interesting insights when analyzing bounds on quantile treatment effects for individuals employed irrespective of treatment assignment. In particular, our results suggest that the positive effects of JC on wages largely hold across quantiles, but that there are differences across quantiles and demographic groups. The effects for blacks appear larger in the lower half of their wage distribution, while the effects appear larger for whites in the upper half of their wage distribution. In addition, non-Hispanic females show statistically significant positive effects of JC on wages in the upper part of their wage distribution, but not in the lower part. Our preferred estimated bounds on quantile effects—those imposing individual-level monotonicity and stochastic dominance—for the non-Hispanic groups suggest that the statistically significant effects of JC on wages across quantiles range from about 2.7 to 14 percent. We also discuss how to employ a pretreatment covariate to narrow these bounds, which in our application results in average width reductions of about 10 percent for  $ATE_{EE}$  and 8 percent for  $QTE_{EE}^{\alpha}$ . Importantly, we achieved the latter reductions while maintaining the focus on the quantiles of the unconditional (marginal) outcome distribution.

For Hispanics we conduct a separate analysis that does not employ the assumption of individual-level monotonicity of the effect of JC on employment, because prior evidence suggests that this assumption is less likely to hold for them. Estimated bounds without this assumption are wide and include zero, implying that we are unable to rule out zero, negative, or positive effects of JC on wages for Hispanics. However, estimated bounds across quantiles of the wage distribution indicate that large (but potentially plausible) positive and negative effects can be ruled out for the upper quantiles of this group.

In general, our application illustrates the usefulness of the nonparametric bounds discussed in this paper in settings where sample selection is present, as well as the insights that can be gained from employing these techniques to analyze quantile treatment effects. For instance, consider the example given in the introduction about the antismoking assistance program for pregnant women studied in Sexton and Hebel (1984). In this case, the present methods could be used to bound the average and quantile effects of the program on the birth weight of those babies who would not die during gestation regardless of their mothers' participation in the program. In addition, the JC application points toward some caveats of the approach and important directions in which these methods could be extended. Although similar assumptions are commonly used in the literature, the (untestable) individual-level monotonicity assumption of the treatment on the selection indicator is nontrivial and can be hard to justify in practice. Similarly, the bounds can be affected by measurement error. Therefore, the derivation of tighter bounds that do

not rely on that monotonicity assumption and an analysis of the effects of measurement error on the bounds we consider would be valuable econometric contributions to the literature.

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