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

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Abstract

This paper assesses the effect of the U.S. Job Corps (JC), the nation’s largest and most comprehensive job training program targeting disadvantaged youth, on wages. We employ partial identification techniques to construct nonparametric bounds for the average causal effect and the quantile treatment effects of the JC program on participants’ wages. Our preferred estimates point toward convincing evidence of positive impacts of JC on participants’ wages throughout the conditional wage distribution, falling between 1.5 and 15.5 percent. Furthermore, when breaking up the sample into demographic subgroups, we find that the program’s effect on wages varies, with Black participants in the lower part of the wage distribution likely realizing larger impacts relative to Whites, whose larger impacts occur in the upper part of their distribution. Non-Hispanic Females in the lower part of the wage distribution do not observe statistically significant positive effects of JC on their wages.
1 Introduction

Assessment of the effect of federally funded labor market programs on participants’ outcomes (e.g., earnings, education, employment, etc.) is of great importance to policy makers. To answer the question about these programs’ effectiveness vis-a-vis their public cost, one relies on the ability to estimate causal effects of program participation, which is usually a difficult task. The vast majority of both substantive and methodological econometric literature on program evaluation (see Angrist and Krueger, 1999, Blundell and Dias, 2009, and Heckman, LaLonde and Smith, 1999) focuses on estimating causal effects of participation on total earnings, which is a basic step for a cost-benefit analysis. Evaluating the impact on total earnings, however, leaves open a relevant question about whether or not these programs have a positive effect on the human capital of participants, which is an important goal of active labor market programs.

Total earnings are the product of the individual’s wage times hours worked. In other words, earnings have two components: price of labor and quantity supplied of labor. Therefore, 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. The assessment of the effect of program participation on human capital requires to focus on the price component of earnings, i.e., wages. The reason is that wages are directly related to the improvement of participants’ human capital through the program. In addition, the estimation of the effect of the program on wages allows policy makers to better understand the channels through which it leads to more favorable labor market outcomes. Unfortunately, estimation of the program’s effect on wages is not straightforward due to the well-known sample selection problem (Heckman, 1979). Essentially, wages are observed only for those individuals who are employed. Even in an experimental setting, randomization does not solve this problem as the comparison of wages from individuals in treatment and control groups do not result in causal effects since the individual decision to become employed is endogenous and occurs after randomization and training has been completed.

In this paper, we use the data from the National Job Corps Study, a randomized evaluation of the Job Corps (JC) program to empirically assess the effect of JC training on participants’ wages. Our analysis assess effects both at the mean and at different quantiles of the wage distribution of participants, as well as for different demographic groups. To accomplish this objective we construct nonparametric bounds that require weaker assumptions than those conventionally used for point identification of average treatment effects in the presence of sample selection. Given that wages are not defined for individuals who are not employed, we focus on estimating bounds on the population.

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1Point identification of average treatment effects typically requires strong distributional assumptions
of individuals that would be employed regardless of participation in JC, as previously
done in Lee (2009) and Zhang et al. (2008), among others. This group of individuals is
estimated to be the largest group among those eligible JC participants.

Our analysis starts by computing the Horowitz and Manski (2000) "worst-case"
bounds, which do not require the use of monotonicity assumptions or exclusion restric-
tions. However, these bounds are too wide (i.e., uninformative) in our application. Sub-
sequently, we proceed to tighten the bounds through the use of monotonicity assumptions
within a principal stratification (PS) framework (Frangakis and Rubin, 2002). We em-
ploy three types of monotonicity assumptions. The first type states individual level weak
monotonicity on the effect of the program on employment. This assumption is commonly
used throughout the program evaluation literature (see, e.g., Imbens and Angrist, 1994),
and was employed by Lee (2009) to partially identify wage effects. The other two types
of weak monotonicity assumptions are at the level of mean potential outcomes and are
applied within or across subpopulations defined by the potential value of the employment
status variable (i.e, strata). These assumptions, particularly the across-strata assump-
tion, result in especially informative bounds for the parameters of interest. They are
not new to the growing body of literature on partial identification. The assumption of
monotonicity within strata was employed by Flores and Flores-Lagunes (2010) to identify
bounds on mechanism and net average treatment effects. The across-strata monotonicity
assumption was previously considered by Zhang and Rubin (2003) and by Zhang et al.
(2008). Related versions of these two assumptions that are applied to observed employ-
ment groups instead of (latent) strata were considered in Blundell et al. (2007) and in
Lechner and Melly (2010).

We contribute to the literature in two ways. First, we provide a substantive em-
pirical analysis of the effect of the Job Corps training program on participants’ wages.
The analysis is considered substantive for two reasons. The first is due to the current im-
portance of Job Corps. With a yearly cost of about $1.5 billion, Job Corps 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 opera-
tional budget is currently under scrutiny (e.g., USA Today, 2011). The second reason is

such as bivariate normality (Heckman, 1979). One may relax this distributional assumption by relying
on exclusion restrictions (Heckman, 1990; Imbens and Angrist, 1994), which are variables that determine
selection into the sample (i.e., employment) but do not affect the outcome (i.e., wages). It is well known,
however, that in the case of employment and wages both types of assumptions are hard to satisfy in

2Lechner and Melly (2010) relax this individual-level monotonicity by making it hold conditional on
observed covariates.
that our results provide evidence to answer a policy relevant question about the impact of Job Corps on the wage distribution of participants. In this way, we complement Lee (2009) who analyzed the average effect of JC on wages.\(^3\) Importantly, data to derive our results come from the first nationally representative experimental evaluation of an active labor market program for disadvantaged youth (Schochet et al., 2001), and thus implications can be generalized to the program at a national level. The second contribution is methodological: we show how to analyze treatment effects on different quantiles of the distribution of an outcome in the presence of sample-selection by employing the set of weak monotonicity assumptions described above. To do so we construct bounds on the “Local Quantile Treatment Effect” (\(LQTE\)).\(^4\) Intuitively, after identifying the upper and lower bounding distributions of individuals that are always employed independently of their treatment assignment, bounds on the \(LQTE\) are constructed by looking at the difference between quantiles of these trimmed marginal distributions and the distribution of control individuals who are employed. This contribution builds upon and complements the work by Blundell et al. (2007), Lechner and Melly (2010), Lee(2009), and Zhang et al. (2008).

Our results characterize the heterogeneous impact of JC training on different points of the participants’ wage distribution. Our bounds for a sample that excludes Hispanics strongly suggest positive effects of JC on wages that are at least 1.5 and at most 15 percent with 95 percent confidence level.\(^5\) Importantly, the bounds show large overlap across quantiles, suggesting that the positive effects are not significantly different over the distribution of wages.\(^6\) Our separate analysis by race and gender reveals that the positive effects for Blacks appear larger in the lower half of the wage distribution while the opposite is true for Whites. Lastly, Non-Hispanic Females in the lower part of the wage distribution appear not to obtain positive effects of JC on their wages.

The rest of the paper is organized as follows. Section 2 briefly describes the Job Corps program, and the data and its source, the National Job Corps Study. Section 3 formally defines the sample selection problem and introduces the general identification strategy we employ to bound treatment effects. In section 4 we introduce the principal

\(^3\)We also employ the weak monotonicity assumptions within and across strata to tighten the bounds on the average treatment effect by Lee (2009).
\(^4\)Other models of quantile treatment effects rely on instrumental variables (Abadie, Angrist and Imbens (2002) and Chernozhukov and Hansen (2005)), while the partial identification strategy we propose does not.
\(^5\)The reason why Hispanics are excluded from the analysis is discussed in the next section.
\(^6\)As noted by Lechner and Melly (2009), this similarity of effects actually implies that such effects are larger in relative terms for individuals in the lower portion of the wage distribution. Under this view, the program has an effect of wage compression within eligible JC applicants (excluding Hispanics).
stratification framework and the assumptions necessary to construct and tighten bounds on the treatment effects of interest. Section 5 proposes bounds on quantile treatment effects. Section 6 contains the results of our analysis of the Job Corps program. We conclude in section 7.

2 Job Corps and the National Job Corps Study

Job Corps (JC) is America’s largest and most comprehensive residential education and job training program. This federally funded program was established in 1964 as part of the War on Poverty under the Economic Opportunity Act, and is currently administered by the US Department of Labor (USDOL). With a yearly cost of about $1.5 billion, JC annual enrollment ascends to 100,000 students (USDOL, 2010). The program’s goal is to help disadvantaged young people, ages ranging from 16 to 24, improve the quality of their lives by enhancing their labor and educational skills set. Eligible participants are provided with the opportunity to benefit from the program’s goal through academic, vocational, and social skills training provided at over 123 centers nationwide (USDOL, 2010). Participants are selected based on several criteria, including age (16-24 years), legal US 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 et al., 2001).

Being the nation’s largest job training program, the effectiveness of JC has been debated at times. During the mid 1990’s, the US Department of Labor commissioned Mathematica Policy Research, Inc. to design and implement a randomized evaluation, the National Job Corps Study (NJCS), in order to determine the program’s effectiveness. The main feature of the study was its random assignment, namely, 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. During the sample intake period between November 1994 and February 1996, a total of 80,883 first time eligible applicants were included in the study. From this total, approximately 12% were assigned to the treatment group (9,409) while 7% of the eligible applicants were assigned to the control group (5,977). The remaining 65,497 were assigned to a program non-research group (Schochet et al., 2001). After recording their data through a baseline interview for both treatment and control experimental groups, a series of follow up interviews were conducted at weeks 52, 130, and 208 after randomization.

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 individuals assigned to the control group
enrolled in the program despite the three-year embargo imposed on them (Schochet et al., 2001). Therefore, in the presence of this non-compliance, the comparison of average outcomes between groups of individuals 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 to JC. Focusing on this parameter in the presence of non-compliance is common practice in the literature (e.g., Lee, 2009; Flores-Lagunes et al., 2009; Zhang et al., 2009). Correspondingly, our empirical analysis focuses on estimating informative non-parametric bounds for the ITT effect. For a recent study accounting for the issues of non-compliance and missing observations along with the sample selection problem see Frumento et al. (2010).

We start our analysis with the same sample employed by Lee (2009), who developed an intuitive trimming procedure for bounding the average treatment effect of JC on participants’ wages. This sample is restricted to individuals who have non-missing values for weekly earnings and weekly hours for every week after random assignment, resulting in a sample size of 9,145 that is smaller than the original NJCS sample size. We start with this sample to compare our results to Lee (2009) and thus see the informational content of our assumptions to tighten the estimated bounds. Subsequently, we further restrict the sample by excluding Hispanics, which results in a sample size of 7,573. The reason to drop Hispanics is that it is has been documented that this group in the NJCS showed negative (albeit not statistically significant) impacts from JC on both employment and earnings (see, e.g., Schochet et al., 2001 and Flores-Lagunes et al., 2009). Since one of our main assumptions is individual-level monotonicity of the effect of JC on employment, we prefer to leave this group out of the remaining analysis. Finally, due to both programmatic and research reasons in the NJCS, different subgroups in the study population had different probabilities of being included in the research sample. Thus, throughout our analysis, we employ the NJCS design weights (Schochet, 2001).

Summary statistics for the sample of 9,145 are presented in Table 1, which essentially replicates that of Lee (2009). Pretreatment variables in the data include: demographic

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7 We only include individuals who had no missing values for the post-treatment variables weekly earnings and weekly hours worked, thus assuming that they are "missing at random". The studies cited above also take this approach.

8 Nevertheless, we obtained a full set of results for the subsample of Hispanics. Accordingly, most of the estimated bounds were uninformative, and in some instances they could not be computed, likely due to a strong failure of the individual monotonicity assumption.

9 For example, outreach and admissions agencies had struggle recruiting females for residential slots. Therefore, sampling rates to the control group were intentionally set lower for females in some areas to overcome difficulties with unfilled slots. See Schochet (2001) for more details on reasons and calculation of design weights.
variables (rows 1 to 12), education and background variables (rows 13 to 16), income variables (rows 17 to 25) and employment information (rows 26 to 31). As expected, given the randomization, the distribution of these pretreatment characteristics is similar across treatment and control groups, i.e., the difference in the next to last column is not statistically significant at a 5 percent level of confidence. The resulting difference for post-treatment earnings across groups, reported in penultimate column, is quantitatively equivalent and consistent with the previously found 12 percent positive effect of JC on participants’ weekly earnings (Burghardt et al., 1999; Schochet et al., 2001; Flores-Lagunes et al., 2009). Results are also consistent with those obtained in previous studies when looking at the effect of JC on participants’ weekly hours worked (Schochet et al., 2001). Summary statistics for the subsamples to be analyzed (Non-Hispanics, Blacks, Whites, Non-Hispanic Males, and Non-Hispanic Females) are relegated to the Appendix.

3 The Sample Selection Problem and the Framework to Bounding Treatment Effects

Assessing the impact of job training programs on participants’ wages is fundamentally distinct than assessing the program’s impact on earnings. Earnings are the product of the individual’s wage times hours worked, therefore, the latter impact encompasses the effect on the likelihood of being employed (labor supply effect) and the effect on wages. The impact on participants’ wages can be interpreted as a pure price effect since significant increases in wages can be directly related to the improvement of the participants’ human capital due to the program, which is essential for individuals to boost their labor market opportunities. Indeed, one of JC’s main goals is the enhancement of participants’ human capital through academic and vocational training. Thus, it is of considerable importance to evaluate the program’s impact on wages.

It is well known, however, that estimation of a program’s causal effect on participants’ wages is complicated due to the fact that only the wages of those employed are observed. This is referred to in the literature as the sample selection problem (Heckman, 1979). Formally, we have access to data on $N$ individuals and define a binary treatment $\tau_i$, where $\tau_i=1$ indicates that individual $i$ has participated in the program and $\tau_i=0$ if not. We start with an assumption that accords with our data:

**Assumption A:** Randomly Assigned Treatment.

$Y_i$, individual $i$’s wage, is assumed for the moment to be a linear function of the treatment indicator $\tau_i$ and a set of pretreatment characteristics $x_{it}$.

\[^{10}\text{Linearity is assumed here to simplify the exposition of the sample selection problem. The nonpar-}\]
\[ Y_i = \beta_1 \tau_i + \beta_2 x_{1i} + u_{1i}. \] (1)

The self-selection process into employment is also assumed to be linearly related to the treatment indicator \( \tau_i \) and a set of pretreatment characteristics \( x_{2i} \),

\[ S^*_i = \delta_1 \tau_i + \delta_2 x_{2i} + u_{2i}, \] (2)

where \( S^*_i \) is a latent variable representing the 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. In notation,

\[ S_i = 1[S^*_i \geq 0], \]

where \( 1[\cdot] \) is an indicator function. Therefore, \( Y_i \) is only observed when the individual self-selects into employment, i.e., \( S_i = 1 \).

Conventionally, point identification of the parameter of interest \( \beta_1 \), which is assumed to be constant over the population in this setting, requires strong assumptions such as joint independence of the errors \( (u_{1i}, u_{2i}) \) in the wage and employment equations (1) and (2), respectively, and the regressors \( \tau_i, x_{1i} \) and \( x_{2i} \), and bivariate normality of the errors \( (u_{1i}, u_{2i}) \). One may relax the bivariate normality assumption about the errors by relying on exclusion restrictions (Heckman, 1990; Heckman and Smith, 1995; Imbens and Angrist, 1994), which are variables that determine employment but do not affect wages, or equivalently, variables in \( x_{2i} \) that do not belong in \( x_{1i} \); but it is well known that in general finding such variables that go along with economic reasoning in this situation is difficult (Angrist and Krueger, 1999; Angrist and Krueger, 2001).

An alternative approach to the estimation of treatment effects in the presence of sample selection suggests that the parameters can be bounded without making distributional assumptions or relying on the availability and validity of exclusion restrictions. Horowitz and Manski (2000; HM hereafter) propose a conservative general framework to construct bounds on treatment effects when data is missing due to a nonrandom process, such as the self-selection into not working \( (S^*_i < 0) \), provided that the outcome variable has a bounded support. These bounds are known in the literature as “worst-case” scenario bounds.

metric 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.

\[^{11}\] Horowitz and Manski (2000) derived conservative bounds on population parameters of interest using nonparametric analysis applied to experimental settings with problems of missing binary outcomes and covariates.
Define the average treatment effect (ATE) as

$$ATE = E[Y_i(1) - Y_i(0)] = E[Y_i(1)] - E[Y_i(0)],$$

(3)

where $Y_i(0)$ and $Y_i(1)$ are the potential (counterfactual) wages for unit $i$ under control ($\tau_i=0$) and treatment ($\tau_i=1$), respectively. The relationship between this potential outcomes and the observed $Y_i$ is that $Y_i = Y_i(1)\tau_i + Y_i(0)(1 - \tau_i)$. Conditional on $\tau_i$ and the observed employment indicator $S_i$, the $ATE$ in (3) can be written as:

$$ATE = E[Y_i|\tau_i = 1, S_i = 1]Pr(S_i = 1|\tau_i = 1) +
E[Y_i|\tau_i = 1, S_i = 0]Pr(S_i = 0|\tau_i = 1)$$

$$-E[Y_i|\tau_i = 0, S_i = 1]Pr(S_i = 1|\tau_i = 0) -
E[Y_i|\tau_i = 0, S_i = 0]Pr(S_i = 0|\tau_i = 0)$$

(4)

Examination of Equation (4) reveals that we can identify from the data all the conditional probabilities ($Pr(S_i|\tau_i)$) and also the expectations of the wage when conditioning on $S_i=1$ ($E[Y_i|\tau_i = 1, S_i = 1]$ and $E[Y_i|\tau_i = 0, S_i = 1]$). Unfortunately, sample selection makes it impossible to identify $E[Y_i(1)|\tau_i = 1, S_i = 0]$ and $E[Y_i(0)|\tau_i = 0, S_i = 0]$. We can, however, impute “worst-case” scenario bounds on these unobserved objects provided that the support of the outcome lies in a bounded interval ($Y_i^{LB}, Y_i^{UB}$), since it implies that the values for the unobserved objects are restricted to such interval. In this case, HM’s worst case lower and upper bounds ($LB^{HM}$ and $UB^{HM}$, respectively) are identified as follows:

$$LB^{HM} = E[Y_i|\tau_i = 1, S_i = 1]Pr(S_i = 1|\tau_i = 1) + Y_i^{LB}Pr(S_i = 0|\tau_i = 1)$$

$$-E[Y_i|\tau_i = 0, S_i = 1]Pr(S_i = 1|\tau_i = 0) - Y_i^{UB}Pr(S_i = 0|\tau_i = 0)$$

$$UB^{HM} = E[Y_i|\tau_i = 1, S_i = 1]Pr(S_i = 1|\tau_i = 1) + Y_i^{UB}Pr(S_i = 0|\tau_i = 1)$$

$$-E[Y_i|\tau_i = 0, S_i = 1]Pr(S_i = 1|\tau_i = 0) - Y_i^{LB}Pr(S_i = 0|\tau_i = 0)$$

(5)

Note that the worst case bounds do not employ any distributional or exclusion restrictions assumptions. They are nonparametric and allow for heterogeneous treatment effects (i.e., non-constant effects over the population). On the other hand, a cost of disposing of those assumptions is that the worst case bounds are often uninformative. Indeed, this is the case in our application as will be shown below. For this reason, while we follow this general bounding approach in spirit, we proceed by imposing more structure through the use of assumptions that are typically weaker than the distributional and exclusion restriction assumptions.
4 Identification and Estimation of Bounds on Average Treatment Effects

We follow the approach by Lee (2009) and Zhang et al. (2008) to tightening the HM worst case bounds. Both papers employ monotonicity assumptions that lead to a trimming procedure that tightens the worst case bounds. Zhang et al. (2008) explicitly employ the principal stratification framework of Frangakis and Rubin (2002) to motivate their results, while Lee’s (2009) results can be seen as implicitly employing the same framework. We employ this framework in what follows. Both papers focus on the average treatment effect of a program on wages for individuals that would be employed regardless of treatment status, which is a particular strata of individuals. We will focus on this same strata and will introduce another monotonicity assumption that also tightens the worst case bounds. This other monotonicity assumption was introduced by Flores and Flores-Lagunes (2010) in a different but related context.

The principal stratification framework (Frangakis and Rubin, 2002) allows analyzing average causal effects when controlling for a post-treatment variable that has been affected by treatment assignment. In the context of analyzing the effect of JC on wages, the affected post-treatment variable is employment. In this setting, individuals can be classified into principal strata based on the potential values of employment under each treatment arm. It follows that comparisons of average outcomes by treatment assignment within strata can be interpreted as causal effects since individuals within strata are affected by the treatment in the same way (Frangakis and Rubin, 2002).

More formally, the employment variable $S_i$ is affected by the treatment $\tau_i$ and thus the potential values of employment can be denoted as $S_i(0)$ and $S_i(1)$ when $i$ is assigned to control and treatment, respectively. We can partition individuals into strata based on the values of the vector $\{S_i(0), S_i(1)\}$. Since both $S_i$ and $\tau_i$ are binary, there are four principal strata:

\[
\begin{align*}
NN & : \{S_i(0) = 0, S_i(1) = 0\} \\
EE & : \{S_i(0) = 1, S_i(1) = 1\} \\
EN & : \{S_i(0) = 1, S_i(1) = 0\} \\
NE & : \{S_i(0) = 0, S_i(1) = 1\}
\end{align*}
\]

In the context of JC, $NN$ is the strata of those individuals who would be unemployed independently of treatment assignment, $EE$ is the strata of those who would be employed independently of treatment assignment. Accordingly, $EN$ represent those who would be employed if assigned to control but unemployed if assigned to treatment, and $NE$ is the
strata for 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 strata an individual belongs to is unobserved. A mapping of the observed groups based on \((\tau_i, S_i)\) to the unobserved strata above is depicted in Table 2.

Recall that our outcome, wage, is not defined for individuals if they are not employed. Therefore, we focus on the strata of individuals with defined wages independently of treatment assignment, that is, the \( EE \) strata. Since it is necessary to compare wages within strata to obtain causal interpretation, the treatment effects are only well-defined for this strata. Thus, the average treatment effect parameter we concentrate on is:

\[
ATE_{EE} = E[Y_i(1)|EE] - E[Y_i(0)|EE].
\] (7)

Identification and estimation of causal effects within the \( EE \) strata will result in effects of JC on wages that control for selection into employment. Studies that concentrate on this parameter are Lee (2009) and Zhang et al. (2008), among others.

### 4.1 Identification and Estimation of Bounds Using the Individual-Level Monotonicity Assumption

To tighten the worst case bounds introduced in the last section, we make the following individual-level monotonicity assumption about the relationship between the treatment and the employment indicator:

**Assumption B:** Individual-Level Positive Monotonicity of \( \tau \) on \( S(\tau) \).

This assumption states that treatment assignment can only affect employment in one direction, \( S_i(1) \geq S_i(0) \) for all \( i \), ruling out the \( EN \) strata. Both Lee (2009) and Zhang et al. (2008) employed this assumption, which is also commonly invoked throughout the instrumental variable literature about the effect of the instrument on treatment status (Imbens and Angrist, 1994; and Angrist, Imbens and Rubin, 1996).

In the context of JC, Assumption B is plausible since one of the program’s stated goals is to increase the employability of participants, and in line with this it offers participants job search assistance in addition to training. Nevertheless, this untestable assumption has been criticized since it assumes the sign of the individual treatment effect of the program on employment (e.g., Lechner and Melly, 2010). Two factors that may cast doubt on this assumption are that individuals undergoing training are "locked-in" away from employment (van Ours, 2004), and the possibility that, after training, treated individuals have a higher reservation wage and thus may choose to remain unemployed.
individuals from two strata, however, note that we can point identify the proportions of each principal strata in the population. Let \( \pi_k \) be the population proportions of each principal strata \( k = NN, EE, EN, NE \), and let \( p_{s|\tau} \equiv Pr(S_i = s|\tau_i = t) \) 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 2, we know that individuals in the observed group with \( (\tau_i, S_i) = (0, 1) \) belong to the strata of interest \( EE \). Therefore, from \( 7 \) we can point identify \( E[Y_i(0)|EE] \) with \( E[Y_i|\tau = 0, S_i = 1] \). However, is not possible to point identify \( E[Y_i(1)|EE] \), since the observed group with \( (\tau_i, S_i) = (1, 1) \) is a mix of individuals from two strata, \( EE \) and \( NE \). With the known population proportions \( \pi_k \), however, note that \( E[Y_i|\tau = 1, S_i = 1] \) can be written as a weighted average of individuals in \( EE \) and \( NE \):

\[
E[Y_i|\tau = 1, S_i = 1] = \frac{\pi_{EE}}{\pi_{EE} + \pi_{NE}} E[Y_i(1)|EE] + \frac{\pi_{NE}}{\pi_{EE} + \pi_{NE}} E[Y_i(1)|NE] \tag{8}
\]

Since the proportion of \( EE \) individuals in the group \( (\tau_i, S_i) = (1, 1) \) can be point identified as \( \pi_{EE}/(\pi_{EE} + \pi_{NE}) = p_{1|0}/p_{1|1} \), \( E[Y_i(1)|EE] \) 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 \( (\tau_i, S_i) = (1, 1) \). In other words, the upper bound is obtained under the scenario that the largest values \( (p_{1|0}/p_{1|1}) \) 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 \( (\tau_i, S_i) = (1, 1) \), by \( 1 - (p_{1|0}/p_{1|1}) \) yields an upper bound for the \( EE \) group. Similarly, \( E[Y_i(1)|EE] \) 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. The resulting upper (\( UB_{EE} \))

\textsuperscript{12}Zhang et al. (2009) provide some evidence that the estimated proportion of individuals that do not satisfy the individual-level assumption (the \( EN \) strata) falls with the time horizon with which the outcome is measured after randomization.
and lower \((LB_{EE})\) bounds for \(ATE_{EE}\) are (Lee, 2009 and Zhang et al., 2008):

\[
\begin{align*}
UB_{EE} &= E[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y_{11}^{[p_{110}/p_{111}]}) - E[Y_i|\tau_i = 0, S_i = 1] \tag{9}
\end{align*}
\]

\[
\begin{align*}
LB_{EE} &= E[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y_{11}^{[p_{110}/p_{111}]}) - E[Y_i|\tau_i = 0, S_i = 1],
\end{align*}
\]

where \(y_{11}^{[p_{110}/p_{111}]}\) and \(y_{11}^{[p_{110}/p_{111}]}\) denote the \(1 - \) \((p_{110}/p_{111})\) and the \((p_{110}/p_{111})\) quantile of \(Y_i\) conditional on \(\tau_i = 1\) and \(S_i = 1\), respectively. Lee (2009) shows that these bounds are sharp and that they are asymptotically normally distributed.

To estimate the bounds in (9) we can simply substitute sample quantities for population quantities:

\[
\begin{align*}
\hat{UB}_{EE} &= \frac{\sum_{i=1}^n Y_i \cdot \tau_i \cdot S_i \cdot 1[Y_i \geq \hat{y}_{1-\hat{p}}]}{\sum_{i=1}^n \tau_i \cdot S_i \cdot 1[Y_i \geq \hat{y}_{1-\hat{p}}]} - \frac{\sum_{i=1}^n Y_i \cdot (1 - \tau_i) \cdot S_i}{\sum_{i=1}^n (1 - \tau_i) \cdot S_i},
\end{align*}
\]

\[
\begin{align*}
\hat{LB}_{EE} &= \frac{\sum_{i=1}^n Y_i \cdot \tau_i \cdot S_i \cdot 1[Y_i \leq \hat{y}_{\hat{p}}]}{\sum_{i=1}^n \tau_i \cdot S_i \cdot 1[Y_i \leq \hat{y}_{\hat{p}}]} - \frac{\sum_{i=1}^n Y_i \cdot (1 - \tau_i) \cdot S_i}{\sum_{i=1}^n (1 - \tau_i) \cdot S_i},
\end{align*}
\]

where \(\hat{p}\), the sample analog of \((p_{110}/p_{111})\) that is used to pin down the quantiles \((\hat{y}_{1-\hat{p}}\) and \(\hat{y}_{\hat{p}}\)) of the treatment group outcome distribution (analogs to the quantiles \(y_{11}^{[p_{110}/p_{111}]}\) and \(y_{11}^{[p_{110}/p_{111}]}\) in (9), respectively), is calculated as follows:

\[
\hat{p} = \frac{\sum_{i=1}^n (1 - \tau_i) \cdot S_i}{\sum_{i=1}^n (1 - \tau_i)} / \frac{\sum_{i=1}^n \tau_i \cdot S_i}{\sum_{i=1}^n \tau_i}, \tag{11}
\]

Lee (2009) employs these bounds to estimate the average effect of JC on participant’s wages at different time horizons after randomization. Below, we will replicate his results for wages at week 208 after randomization. We will also obtain corresponding estimates for relevant subgroups, and estimate alternative tighter bounds that impose more structure that we argue is plausible in the current setting.

### 4.2 Tightening Bounds: Weak Monotonicity Within and Across Strata

We move on to introduce two separate weak monotonicity assumptions for mean potential outcomes at the strata level that allow tightening the bounds presented thus far. The first assumption imposes weak monotonicity on the mean potential outcomes for those individuals in the \(EE\) strata. Formally,

\[E[Y(1)|EE] \geq E[Y(0)|EE]\]

**Assumption C:** Weak Monotonicity of Mean Potential Outcomes Within the \(EE\) Strata.
An immediate implication of adding this assumption to tighten the bounds in (9) is that the average treatment effect of interest in (7) is non-negative, i.e., \( ATE_{EE} \geq 0 \). Therefore, the lower bound in (9) becomes \( \max\{0, LB_{EE}\} \) while the upper bound remains unchanged. Thus, a potentially unattractive feature of Assumption C is that it restricts the sign of the effect of interest (see Flores and Flores-Lagunes, 2010 for a discussion of this assumption). In the context of JC, however, Assumption C is plausible given that participants are exposed to substantial academic and vocational instruction.\(^{13}\) Thus, consistent with human capital theory, one would expect, on average, a non-negative effect of JC participation on wages.\(^{14}\)

The second assumption we consider to tighten the bounds imposes weak monotonicity on the mean potential outcomes across the \( EE \) and \( NE \) strata. This assumption to tighten the bounds was proposed by Zhang and Rubin (2003) and employed in Zhang et al. (2008). Formally:

**Assumption D: Weak Monotonicity of Mean Potential Outcomes Across the EE and NE Strata.**

\[
E[Y(1)|EE] \geq E[Y(1)|NE]
\]

Intuitively, this assumption formalizes the notion that the \( EE \) strata is likely to be comprised of more "capable" individuals than those belonging to the \( NE \) strata. Since "ability" is positively correlated with labor market outcomes (e.g., wages and employment), one would expect wages for the individuals that are employed regardless of treatment status (the \( EE \) strata) to weakly dominate on average those wages for individuals that are only employed if they receive training (the \( NE \) strata). While Assumption D is not directly testable, one can indirectly gauge its plausibility by looking at average pre-treatment variables for these strata that are correlated with wages.\(^{15}\) We illustrate

\(^{13}\)On average, JC participants receive about 440 hours of academic instruction (Schochet et al., 2001). In a sample of participants similar to the one in the present study, Flores et al. (2011) report the average hours of both academic and vocational instruction to be 1,215 hours.

\(^{14}\)Note that Assumption C is related to, but different from, Manski (2003) and Manski and Pepper (2000) "monotone treatment response" assumption. Their assumption states that the individual potential outcomes are a monotone function of the treatment, i.e., \( Y_i(1) \geq Y_i(0) \) for all \( i \). In contrast to the monotone treatment response, Assumption C is weaker since it allows some individual effects of the treatment on the outcome to be negative, as long as the weak inequality is satisfied on average.

\(^{15}\)In a setting where the outcome is not truncated, Flores and Flores-Lagunes (2010) show that Assumption D provides testable implications that can be employed to falsify it. Unfortunately, in this context, the unobservability of wages for those unemployed prevents the computation of this testable implication.
this in our analysis below.

Assumption D is related to, but different from Manski and Pepper (2000) “monotone instrumental variable” assumption. Their assumption states that mean responses vary weakly monotonically across subpopulations defined by specific values of the instrument. In contrast, the present assumption conditions mean responses across two of the basic principal strata. Recently, Blundell et. al. (2007) and Lechner and Melly (2010) employed a similar assumption of stochastic dominance applied to individuals that are observed employed, that is, a positive selection into employment. Under this assumption, the wages of those employed are assumed to weakly dominate those of individuals unemployed. These studies focus on the treatment effects on the subpopulation of individuals that are employed. In contrast, we focus on principal strata.

Employing assumptions A, B, and D results in tighter bounds. To see this, recall that the average outcome in the observed group with \((\tau_i, S_i) = (1, 1)\) contains units from two strata, \(EE\) and \(NE\), and can be written as a weighted average shown in (8). After solving for \(E[Y_i(1)j|EE]\) and substituting it into the inequality in Assumption D, the result implies that \(E[Y_i(1)|EE] \geq E[Y_i|\tau_i = 1, S_i = 1]\), and thus that \(E[Y_i(1)|EE]\) is bounded from below by \(E[Y_i|\tau_i = 1, S_i = 1]\). Therefore, the lower bound in (9) becomes: \(E[Y_i|\tau_i = 1, S_i = 1] - E[Y_i|\tau_i = 0, S_i = 1]\).

For estimation of the bounds when adding to Assumptions A and B Assumption C or D, note that the upper bound estimate of (9) remains \(\widehat{UB_{EE}}\) from (10). When adding Assumption C, the estimate for the lower bound is now the maximum of zero and the lower bound in (10), formally,

\[
\widehat{LB_{EE}^c} = \max\{0, \widehat{LB_{EE}}\} \tag{12}
\]

To estimate the lower bound when adding Assumption D, the corresponding sample analog to \(E[Y_i|\tau_i = 1, S_i = 1] - E[Y_i|\tau_i = 0, S_i = 1]\) is:

\[
\widehat{LB_{EE}^d} = \frac{\sum_{i=1}^{n}Y_i \cdot \tau_i \cdot S_i}{\sum_{i=1}^{n} \tau_i \cdot S_i} - \frac{\sum_{i=1}^{n}Y_i \cdot (1 - \tau_i) \cdot S_i}{\sum_{i=1}^{n} (1 - \tau_i) \cdot S_i} \tag{13}
\]

5 Identification and Estimation of Bounds on Quantile Treatment Effects

We now extend the results presented in the previous section to the estimation of local quantile treatment effects (LQTE). The parameter of interest, the LQTE, is defined as the difference in quantiles between the treated and control groups’ outcomes at a given
quantile level "$\alpha$" (Abadie, et. al., 2002; and Chernozhukov and Hansen, 2005). This difference is well-defined as long as the marginal distributions of potential outcomes are point or partially identified.

Two recent papers have focused on partial identification of quantile treatment effects (QTEs). In the first, Blundell, et. al., (2007) derived sharp bounds on the distribution of wages and the interquantile range to study income inequality in the U.K. Their work builds on the worst case bounds on the conditional quantiles by Manski (1994), and impose stochastic dominance assumptions to tighten the bounds on the QTEs. Specifically, their stochastic dominance assumption is applied to the distribution of wages of individuals observed employed and unemployed. In addition, they also explore the use of exclusion restrictions to further tighten their bounds. The second paper is Lechner and Melly (2010), who analyze the LQTEs of German training program on wages. They impose an individual-level monotonicity assumption as our Assumption B that is weakened by conditioning on covariates $X$, and they subsequently 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 LQTEs employing individual-level monotonicity and our weak mean-level monotonicity assumptions C and D. Another difference among these studies is the parameters of interest. While Blundell et al. (2007) focus on the population QTEs, Lechner and Melly (2010) focus on those individuals that are employed under treatment. Our focus is individuals who are employed regardless of treatment assignment (the EE strata).

Let $F(Y|EE)$ be the distribution of individuals' wages that belong to the EE principal strata, and $F_\alpha[\cdot]$ be the $\alpha$-quantile of $F[\cdot]$ for $\alpha \in (0, 1)$. Following the same intuition for partial identification of $E[Y_i(1)|EE]$ by way of trimming of the observed quantity $E[Y_i|\tau_i = 1, S_i = 1]$, we can partially identify the local quantile treatment effect, $LQTE_{EE}^\alpha$, as follows:

**Proposition 1** Under assumptions A and B, $LB_{EE}^\alpha \leq LQTE_{EE}^\alpha \leq UB_{EE}^\alpha$, where

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

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

(14)

Analogous to (9), $F[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y_{11}^{11}(p_{10}/p_{11})]$ and $F[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y_{11}^{11}(p_{10}/p_{11})]$ correspond to the upper and lower bounding distributions of infra-marginal...
individuals, i.e., those individuals that belong to $EE$ in the observed group $(\tau_i, S_i) = (1, 1)$. As such, $UB_{EE}^\alpha$ is an upper bound of the difference in quantiles between the treated and control groups’ outcomes at a given $\alpha$-quantile. Similarly, $LB_{EE}^\alpha$ represents a lower bound for this difference.

To estimate the bounds in (14) we proceed as follows:

$$UB_{EE}^\alpha = \hat{y}_u^\alpha - \hat{y}_l^\alpha$$
$$LB_{EE}^\alpha = \hat{y}_u^\alpha - \hat{y}_l^\alpha,$$

where the $\alpha$-quantiles for each marginal distribution are calculated as:

$$\hat{y}_u^{bd} = \min\{y : \frac{\sum_{i=1}^n \tau_i \cdot S_i \cdot 1[Y_i^{bd} \leq y]}{\sum_{i=1}^n \tau_i \cdot S_i} \geq \alpha\},$$

with $bd = \{u, l\}$ for the upper and lower bounding distribution, respectively, and $Y_i^{bd}$ represents the distribution $F[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y_{1-\tau_i/p_i}]$ for $bd = u$ or $F[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y_{1-\tau_i/p_i}]$ for $bd = l$. Similarly, the $\alpha$-quantiles for the observed control group with $(\tau_i, S_i) = (0, 1)$, are calculated as:

$$\hat{y}_l^c = \min\{y : \frac{\sum_{i=1}^n (1 - \tau_i) \cdot S_i \cdot 1[Y_i^{c} \leq y]}{\sum_{i=1}^n (1 - \tau_i) \cdot S_i} \geq \alpha\},$$

with $Y^c$ representing the distribution $F[Y_i|\tau_i = 0, S_i = 1]$ of individuals in the observed control group.

5.1 Tightening Bounds on LQTEs using Stochastic Dominance Assumptions

We seek to tighten the bounds in (14) by employing assumptions similar to Assumptions C and D of the previous section. For the case of LQTEs, the assumptions have to be strengthened to stochastic dominance as follows:

**Assumption E:** Stochastic Dominance Within the EE Strata.

$$F(Y(1) | EE) \geq F(Y(0) | EE)$$

**Assumption F:** Stochastic Dominance Across the EE and NE Strata.

$$F(Y(1) | EE) \geq F(Y(1) | NE)$$

These assumptions impose direct restrictions on the distributions of infra-marginal individuals that result in tighter bounds relative to those in (14). Under each of these assumptions, the resulting bounds are:
Proposition 2 Under assumptions A, B, and E, $LB_{EE}^\alpha \leq LQT_E^\alpha \leq UB_{EE}^\alpha$; where $UB_{EE}^\alpha$ is as in (14) and

$$LB_{EE}^\alpha = \max\{0, LB_{EE}^\alpha\}. \tag{16}$$

Estimation of the bounds in (16) follows from modifying the estimate of the lower bound $LB_{EE}^\alpha\epsilon$, and remains the same for the upper bound estimate $UB_{EE}^\alpha$. Specifically,

$$LB_{EE}^\alpha\epsilon = \max\{0, LB_{EE}^\alpha\}. \tag{17}$$

The corresponding bounds when Assumption F is added instead are:

Proposition 3 Under assumptions A, B, and F, $LB_{EE}^\alpha f \leq LQT_E^\alpha \leq UB_{EE}^\alpha$; where $UB_{EE}^\alpha$ is as in (14) and

$$LB_{EE}^\alpha f = \max\{0, LB_{EE}^\alpha\}. \tag{18}$$

As before, estimation of the upper bound is still given by $UB_{EE}^\alpha$, while the estimate for $LB_{EE}^\alpha f$ is now given by:

$$LB_{EE}^\alpha f = \hat{y}_\alpha - \hat{y}_\alpha^t, \tag{19}$$

where $\hat{y}_\alpha^t = \min\{y : \frac{\sum_{i=1}^{n} Y_{i} \cdot S_{i}}{\sum_{i=1}^{n} \tau_{i} \cdot S_{i}} \leq \alpha\}$, and $Y^t$ represents the untrimmed distribution $F[Y_i|\tau_i = 1, S_i = 1]$.

6 Estimation of Bounds on the Effect of Job Corps on Participants’ Wages

In this section we empirically assess the effect of JC training on wages using data from the NJCS. First, we concentrate on the average treatment effect and compute the HM worst case bounds (Section 6.1) under random assignment (Assumption A). Subsequently, we estimate bounds that add different assumptions in order to tighten these benchmark bounds. Section 6.2 reports bounds derived under Assumptions A and B, while Section 6.3 explores the identifying power of Assumptions C and D. Section 6.4 computes and discusses different bounds on the $LQT_E$s.

6.1 Horowitz and Manski (HM) worst-case bounds

Table 3 reports the HM “worst-case” scenario bounds for the average treatment effect of JC on log wages in week 208 after randomization that only employ Assumption
A. The table shows two sets of bounds. In the first, we follow Lee (2009) and transform log wages to minimize the effect of outliers on the width of these bounds by splitting the variable into 20 percentile groups ($5^{th}$, $10^{th}$, ..., and $95^{th}$ percentile of log wages) and individuals belonging to a particular group are assigned the mean log wage for that group. The last column computes the HM bounds using the original log wages to allow for the original variation in log wages and be able to use these bounds as benchmark when adding other assumptions and when computing bounds on the local quantile treatment effects.

Table 3 shows that Lee's transformed log wages have an upper bound of the support, denoted by $Y_{UB}$ in (5), of 2.77, and the lower bound of the support, $Y_{LB}$, is 0.90. As expected, the “smoothing” of wages has a large impact on the support of the outcome since in the last column showing the original wages the upper and lower bounds of the support are 5.99 and -1.55, respectively. Consequently, the worst case bounds' width for original log wages (6.244) is considerably larger than that for the transformed ones (1.548). Detailed calculations of all quantities needed to construct bounds in (5) are shown in the second column of Table 3. Despite large differences between the two measures of wages, the evidence in Table 3 has the same qualitative implication about the worst case bounds: they are largely uninformative. The estimated worst case bounds using transformed log wages are 0.802 (upper bound) and -0.746 (lower bound), while using original log wages are 3.135 (upper bound) and -3.109 (lower bound). Nevertheless, recall that these bounds are the basis upon which we add assumptions to tighten them.

6.2 Bounds Adding Assumption B

Recall that under individual-level monotonicity (Assumption B), the average effect of JC on wages that is identified is for those 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 4 reports the estimated strata proportions for the full sample (labeled "All") and for subgroups of interest. 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 (recall that the strata $EN$ is ruled out by assumption). Note that these observations hold for all subgroups of interest. Interestingly, Whites have the highest proportion of $EE$ individuals at 66 percent, while Blacks have the lowest at 51 percent.

Table 5 reports estimated bounds for the full sample using (10) under Assumptions A and B. Population quantities needed for the construction of these bounds using the
transformed log wages are in column 3, which exactly replicate the results by Lee (2009). Relative to the worst case bounds, the present bounds are much tighter: their width goes from 1.548 in the worst case bounds to 0.112. However, the present bounds still include zero, as does the Imbens and Manski (2004; IM hereafter) confidence intervals reported in the last row. These confidence intervals include the true parameter of interest with a 95 percent probability. The last column of Table 4 also reports estimated bounds for $ATE_{EE}$ using the original log wages. Unlike with the worst-case bounds, the present bounding procedure does not depend on the empirical support of the outcome, thereby the effect of transforming log wages is negligible. While the width of the bounds is similar (0.121), both the bounds and the IM confidence intervals include zero. Thus, from Table 5 we see that Assumption B greatly tightens the worst case bounds but not enough to rule out zero or a negative effect of JC on participant’s wages.

As mentioned before, the untestable individual-level weak monotonicity assumption of the effect of JC on employment may be inadequate in certain circumstances. In the context of JC, the group of Hispanics has been found to be peculiar in the sense that the NJCS calculated negative but statistically insignificant effects of the program on both their employment and weekly earnings at the week 208, while for the other groups the same effects were positive and highly statistically significant (Schochet et al., 2001). This evidence casts doubt on the validity of Assumption B for the group of Hispanics. Therefore, we consider a sample that excludes this group in the remaining analysis, which includes 7,573 individuals.

Table 6 presents estimated bounds under Assumptions A and B for different demographic subgroups, along with their width and IM confidence intervals. The second column reproduces the bounds in Table 5 for the full sample (All). The third column presents the corresponding estimated bounds for the sample that excludes Hispanics (Non-Hispanics) for the reason described above. As could have been expected, the upper bound for this subgroup is larger than that for All while the lower bound is less negative, but the overall width of the bounds is larger. The IM confidence intervals are also wider but more concentrated on the positive side of the real line. In terms of the other subgroups (Whites, Blacks, and Non-Hispanic Males and Females), none of the estimated bounds exclude zero, although Whites and Non-Hispanic Males have a lower bound almost right at zero. Their IM confidence intervals, though, clearly include zero under these assumptions.

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The computation of the standard errors for all bounds and objects of interest were computed employing the bootstrap with 5,000 replications.
6.3 Bounds Adding Assumptions C or D

Table 7 presents estimated bounds for the full sample employing different assumptions. Columns 3 and 6 in the table (subheading C) report estimated bounds after adding Assumption C, for transformed and original log wages, respectively. Recall that the assumption of weak monotonicity of mean potential outcomes within the EE strata (Assumption C) tightens the bounds by setting the lower bound to the maximum of zero or the lower bound in (10). In both instances of log wages, the resulting lower bound is zero. Thus, while the present bounds are tighter relative to those under Assumption B only (by about 17 percent), the IM confidence intervals do not rule out negative effects of JC on log wages. Therefore, the identifying power of Assumption C is limited in this application to a modest tightening of the previous bounds, although in general the IM confidence intervals will include zero when zero is binding in the construction of the present bounds. Since this is also the case in the construction of bounds on the LQTEs, we do not report these bounds in the rest of the paper.

Columns 4 and 7 in Table 7 (subheading D) report estimated bounds after adding the weak monotonicity assumption of mean potential outcomes across strata (Assumption D), for transformed and original log wages, respectively. This assumption results in much tighter bounds for the $ATE_{EE}$ when compared to bounds under Assumptions A and B only, with the width being cut in about half for both measures of log wages. Note also that these estimated bounds under Assumption D are also significantly tighter than those estimated under Assumption C.

Importantly, employing Assumption D enables us to estimate bounds that are informative about the sign of the effect of JC training on participants’ log wages. Bounds on the transformed log wages are 0.034 to 0.093, while those on the original log wages are 0.037 to 0.099, with both sets ruling out negative effects. This illustrates the identifying power of Assumption D, since the bounds clearly point toward positive effects in contrast to the estimated bounds employing only Assumption B. Lastly, when computing IM confidence intervals on the bounds adding Assumption D, we see in the last row of Table 7 that, with 95 percent confidence level, both measures of log-wages exclude zero, indicating statistically significantly positive effects. Focusing on original wages, the effect of JC on log wages for EE individuals is between 0.017 and 0.122, that is, the effect of JC is to raise wages somewhere in between 1.7 to 12.2 percent.

Given the strong identifying power of Assumption D, it is important to gauge its

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17To calculate standard errors of bounds that involve the maximum of zero and another lower bound, we follow the procedure of Cai et al. (2008) in which the standard error is obtained by employing the formula for a truncated (at zero) normal distribution.
plausibility in this application. As mentioned before, a direct statistical test is not feasible since the assumption is untestable. However, we indirectly gauge its plausibility by looking at one of its implications. Recall that Assumption D formalizes the idea that the EE strata possesses traits that result in better labor market outcomes relative to individuals in the NE strata. Thus, one possibility to gauge the validity of Assumption D is to look at pre-treatment (baseline) covariates that are highly correlated with log wages at week 208 and test whether, on average, individuals in the EE strata indeed exhibit better outcomes relative to those in the NE strata. We focus below on pre-treatment hourly wages.

To implement this idea, we need to compute average pre-treatment characteristics for the EE and NE strata. Computing average characteristics for the EE strata is straightforward since under Assumptions A and B the individuals in the observed group \((\tau_i, S_i) = (0,1)\) belong to this strata. Note that average characteristics for the NN strata can be estimated similarly with the \((\tau_i, S_i) = (1,0)\) group. To estimate average characteristics for the NE strata, 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. Let \(W\) be a pre-treatment characteristic of interest, then,

\[
E[W|NE] = \{E[W] - \pi_{EE}E[W|EE] - \pi_{NN}E[W|NN]\}/\pi_{NE}.
\]

We estimate the average characteristics for the NE strata in this way. The estimated average hourly wage for the EE strata is 3.49 (0.08) (standard errors in parenthesis) while that of the NE strata is 3.94 (1.08), which are not statistically different from each other at conventional levels. Thus, this exercise does not provide statistical evidence against Assumption D.\(^{18}\) Similar conclusions are reached when considering pre-treatment weekly earnings and hours worked, and also for each of the subgroups analyzed.\(^{19}\)

Table 8 presents the estimated bounds, width, and IM confidence intervals for all subgroups under analysis under Assumptions A, B, and D. The second and third columns compare the inference for the full population and the Non-Hispanic group. Although the two sets of bounds are of similar width, the bounds for Non-Hispanics are significantly higher at 0.050 to 0.118. In fact, the IM confidence intervals show that, despite the

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\(^{18}\)Note that Assumption D can be extended to include the NN strata, for which we can also compute average baseline characteristics. For the NN strata the pre-treatment average hourly wage (and standard error) is 2.56 (0.05). This figure is statistically smaller than the average hourly wage for the EE strata and statistically equal to that of the NE strata.

\(^{19}\)Pre-treatment weekly earnings are as follows: for the EE and NN strata 119.46 (2.63) and 89.19 (1.91), respectively, and 198.97 (62.54) for the NE stratum. Weekly hours worked at the baseline are as follows: for the EE and NN strata 34.69 (0.77) and 35.25 (0.76), respectively, and 40.78 (5.34) for the NE stratum.
smaller sample size, this group has an effect of JC on wages that falls between 2.9 and 14.3 percent with 95 percent confidence, while that of the full sample is between 1.8 and 12.2 percent.

The estimated bounds for the other subgroups in Table 8 show a number of interesting results. First, all of the bounds and IM confidence intervals exclude zero, with the smallest lower confidence interval endpoint being that of Non-Hispanic Females at 1.4 percent. These unequivocal positive effect across subgroups reinforce the notion of a strong identifying power of Assumption D. Second, Blacks have the highest lower confidence interval endpoint, which implies that the effect of JC on their wages is at least 2.7 percent. Note that the estimated bounds for Whites and Blacks are fairly close to each other, such that the reason why the IM confidence intervals for Blacks are tighter may be due partly to their having a larger sample in the NJCS data. Finally, the results show that Non-Hispanics Males have estimated bounds and IM confidence intervals that are tighter than those of Non-Hispanic Females, which is likely a combination of the last group’s greater heterogeneity and smaller sample size.

6.4 Estimated Local Quantile Treatment Effects Under Assumptions A and B

We proceed to analyze the effects of JC on participant’s wages beyond the average impact by providing estimated bounds for local quantile treatment effects ($LQTE$s) for the $EE$ strata. We start by estimating bounds under Assumptions A and B in this subsection. To summarize the evidence from the computation of $LQTE$s, we provide a series of figures for the different subgroups under analysis. We concentrate only on the log of the original wages for brevity and to fully exploit the original variation in this variable. The estimated $LQTE$s under Assumptions A and B, along with corresponding IM confidence intervals, are shown in Figure 1.

Recall that the estimated bounds for the $ATE_{EE}$ under the same assumptions did not rule out zero for any of the groups under analysis. Looking at the estimated bounds for the $LQTE$s for the full sample in Figure 1(a), they rule out zero for all lower quantiles up to 0.7. Once IM confidence intervals are computed, though, only the bounds for quantiles 0.2, 0.4, and 0.5 rule out zero, implying positive effects of JC on log wages at those quantiles with 95 percent confidence. Given the argument that Assumption B is likely not satisfied for Hispanics, we look at the subgroup Non-Hispanics in Figure 1(b). Consistent with the results from bounds on average effects, the estimated bounds for
this subgroup are generally shifted towards the positive space for the quantiles. For this subgroup, the estimated bounds also exclude zero for all lower quantiles up to 0.7, while the IM confidence intervals rule out zero for quantiles 0.2, 0.5, and 0.6. One implication of the bounds for these samples is that JC is more likely to have positive effects on log wages for lower quantiles of the distribution.

Figures 1(c) to 1(f) present the results for the remaining subgroups. Looking at the results by race, Figures 1(c) and 1(d) show that, once again, the estimated bounds for the $LQTE$s exclude zero for a number of lower quantiles up to 0.7 (with the exception of the 0.05 quantile for Whites). However, probably due to the smaller sample sizes, when looking at the IM confidence intervals for these groups only a couple of quantiles for Blacks (0.15 and 0.45) are significantly positive. Worthy of note in these two figures is the propensity of Blacks to exhibit more positive effects of JC on wages in lower quantiles of the distribution, while the opposite being true for Whites. Of course, the large width of the IM confidence intervals prevents us from being conclusive about this point thus far.

Figures 1(e) and 1(f) show the corresponding estimated bounds and IM confidence intervals for Non-Hispanic Males and Females. The bounds reflect a similar trend of excluding zero at lower quantiles as the previous subgroups, albeit less clear. Interestingly, Non-Hispanic Males show a greater number of estimated bounds excluding zero, which is probably due to a greater heterogeneity of the Non-Hispanic Female group. 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 partial inference for Non-Hispanic Females is more difficult due to their greater heterogeneity and smaller sample size.

To end this subsection, we remark the new information brought about by the estimated bounds on $LQTE$s relative to the estimated bounds on the $ATE_{EE}$. Specifically, while the bounds and IM confidence intervals for the average treatment effect of JC on wages under Assumptions A and B were uninformative, the analysis of $LQTE$s reveals that positive effects of JC on wages generally occur for lower quantiles of the distribution. Furthermore, the subgroups analyzed seem to experience different $LQTE$s, with Blacks probably having larger positive effects at lower quantiles relative to Whites, and Non-Hispanic females showing more uninformative results relative to Non-Hispanic Males. In the next subsection we add Assumption F (stochastic dominance) to further tighten these bounds.
6.5 Estimated Local Quantile Treatment Effects Under Assumptions A, B, and F

Estimated bounds for the $LQT E_{EE}^0$ under Assumptions A, B, and F are summarized in Figure 2. The first noteworthy feature of these estimated bounds is that all of them exclude zero at all quantiles for all subgroups, which speaks to the identifying power of the stochastic dominance assumption (Assumption F). Also noteworthy is that the general conclusions drawn from the estimated bounds in the previous subsection are maintained or reinforced.

Looking at the results for the full sample and the Non-Hispanics (Figures 2(a) and 2(b)), the shift toward more positive effects for the latter sample is reinforced. Interestingly, in both of these 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 IM confidence intervals excluding 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 this latter sample for which Assumption B is likely satisfied, we see that the IM confidence intervals that exclude zero largely overlap, suggesting that the effects of JC on log wages are likely of similar magnitude between (roughly) 1.5 and 15 percent. In this regard, the only clear outlier are the bounds on the 0.05 quantile, which are between 1.6 and 29 percent. We take these results as clear indication that JC has a significantly positive effect on wages under the maintained assumptions.

The results by race are shown in Figures 2(c) and 2(d). Again, adding Assumption F reinforces the notion that Blacks show larger positive impacts of JC on log wages in the lower portion of the distribution while the reverse is true for Whites. Interestingly, the IM confidence intervals for Blacks in the lowest quantiles exclude zero while White’s do not; and the opposite occurs at the highest quantiles. However, despite this evidence being stronger than before, it appears inconclusive when looking at the IM confidence intervals, since there is a considerable amount of overlap on the intervals for both groups within quantiles. The IM confidence intervals also show that Blacks have estimated positive effects of JC on log wages throughout the distribution (except quantiles 0.25, 0.9, and 0.95) that range (roughly) between 1.3 and 14 percent, while Whites shows significant positive effects only for quantiles larger than 0.4 (except 0.8) that range (roughly) between 1.9 to 21 percent. The more pervasive insignificance of results for Whites may be due to their smaller sample size.

Figures 2(e) and 2(f) present the results by Non-Hispanic gender groups. All the estimated bounds under Assumptions A, B, and F for these subgroups exclude zero at
all quantiles, indicating again the identifying power of the stochastic dominance assumption. When taking into consideration the uncertainty in the sample by constructing IM confidence intervals, it is clear that they are fairly similar for both subgroups, indicating significant positive effects of JC on log wages for more than half of the quantiles considered. Notwithstanding, it is also evident that Non-Hispanic Females do not have any significant positive effects throughout the lower half of the distribution up to quantile 0.4 (except at the single quantile 0.2), 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, both gender groups have similar effects as judged by the large overlap in their IM confidence intervals. Considering intervals that exclude zero, Non-Hispanic Females show significant positive effects that range (roughly) from 1 to 14 percent, while those of Non-Hispanic Males range (roughly) form 1 to 15 percent.

7 Conclusion

We empirically assess the effect of training on wages using data from the National Job Corps Study (NJCS), a randomized evaluation of the U.S. Job Corps (JC), the nation’s largest and most important job training program targeting disadvantaged youth. These data come from the first nationally representative experimental evaluation of an active labor market program (Schochet et al., 2001), and thus implications can be generalized to JC at a national level. Research shedding light on the effects of JC on participants’ wages is important given recent discussions in the public arena seeking to cut federal spending on training programs. In general, our results provide substantial evidence that JC has positive and significant effects on participants’ wages. In addition, we explore the heterogeneity of these effects by looking at different points of the wage distribution, and by looking at different demographic subgroups of interest.

Our empirical approach makes use and extends recent partial identification results for treatment effects in the presence of an endogenous post-treatment variable (in this case employment) due to Zhang et al., (2008), Lee (2009), and Flores and Flores-Lagunes (2010). Using this bounding strategy allows us to construct informative nonparametric bounds on the average and the quantile treatment effect of JC on wages accounting for non-random selection into employment under weaker assumptions than those conventionally invoked for point identification. We exploit the random assignment in the NJCS to construct worst case bounds, and then add an individual-level monotonicity assumption on the effect of JC on employment to tighten them. While these bounds are not very informative for the average effect of JC on wages (for those employed irrespective of treatment assignment), by constructing bounds for the local quantile treatment effect we
are able to rule out zero or negative effects of JC on wages for certain quantiles and subgroups.

Subsequently, we add a mean-level weak monotonicity assumption across strata similar to Zhang et al. (2008) that further tightens the bounds. The estimated bounds for the average effect of JC on wages indicate significant positive effects for all groups analyzed. Furthermore, we obtain interesting insights when computing bounds on the local quantile treatment effects, which require strengthening the previous assumption to stochastic dominance of the outcome across strata. In particular, we find that the positive effects of JC on wages largely hold across quantiles but that there are differences between Blacks and Whites in that the effects for the former group appear larger in the lower half of the wage distribution while the opposite is true for the latter group. Finally, Non-Hispanic Females in the lower part of the wage distribution seem not to obtain positive effects of JC on their wages.

In summary, our preferred estimated bounds—those imposing both of the assumptions described above—for the Non-Hispanic population suggest that the effect of JC on wages across quantiles range from about 1.5 to 15 percent with 95 percent confidence level. This strongly suggests that the JC program has a positive and significant effect on the human capital of participants, and that this investment is rewarded in the labor market in the form of higher wages. These results can be taken as encouraging with regard to the effectiveness of JC and provide new insights about how the program serves different demographic subgroups.
8 References


Table 1. Summary statistics by treatment status for our sample from the NJCS data.

<table>
<thead>
<tr>
<th>Row #</th>
<th>Variable</th>
<th>Control</th>
<th>Program</th>
<th>Difference</th>
<th>S.E.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Proportion nonmissing</td>
<td>Mean</td>
<td>S.D.</td>
<td>Proportion nonmissing</td>
<td>Mean</td>
</tr>
<tr>
<td>1</td>
<td>Female</td>
<td>1.00</td>
<td>0.458</td>
<td>1.00</td>
<td>0.452</td>
</tr>
<tr>
<td>2</td>
<td>Age</td>
<td>1.00</td>
<td>18.351</td>
<td>0.98</td>
<td>18.436</td>
</tr>
<tr>
<td>3</td>
<td>White</td>
<td>1.00</td>
<td>0.263</td>
<td>1.00</td>
<td>0.266</td>
</tr>
<tr>
<td>4</td>
<td>Black</td>
<td>1.00</td>
<td>0.491</td>
<td>1.00</td>
<td>0.493</td>
</tr>
<tr>
<td>5</td>
<td>Hispanic</td>
<td>1.00</td>
<td>0.172</td>
<td>1.00</td>
<td>0.169</td>
</tr>
<tr>
<td>6</td>
<td>Other race</td>
<td>1.00</td>
<td>0.074</td>
<td>1.00</td>
<td>0.072</td>
</tr>
<tr>
<td>7</td>
<td>Never married</td>
<td>0.98</td>
<td>0.916</td>
<td>0.98</td>
<td>0.917</td>
</tr>
<tr>
<td>8</td>
<td>Married</td>
<td>0.98</td>
<td>0.023</td>
<td>0.98</td>
<td>0.020</td>
</tr>
<tr>
<td>9</td>
<td>Living together</td>
<td>0.98</td>
<td>0.040</td>
<td>0.98</td>
<td>0.039</td>
</tr>
<tr>
<td>10</td>
<td>Separated</td>
<td>0.98</td>
<td>0.021</td>
<td>0.98</td>
<td>0.024</td>
</tr>
<tr>
<td>11</td>
<td>Has a child</td>
<td>0.99</td>
<td>0.193</td>
<td>0.98</td>
<td>0.189</td>
</tr>
<tr>
<td>12</td>
<td># of child</td>
<td>0.99</td>
<td>0.268</td>
<td>0.98</td>
<td>0.270</td>
</tr>
<tr>
<td>13</td>
<td>Education</td>
<td>0.98</td>
<td>10.105</td>
<td>0.98</td>
<td>10.114</td>
</tr>
<tr>
<td>14</td>
<td>Mother's ed.</td>
<td>0.81</td>
<td>11.461</td>
<td>0.82</td>
<td>11.483</td>
</tr>
<tr>
<td>15</td>
<td>Father's ed.</td>
<td>0.61</td>
<td>11.540</td>
<td>0.62</td>
<td>11.394</td>
</tr>
<tr>
<td>16</td>
<td>Ever arrested</td>
<td>0.98</td>
<td>0.249</td>
<td>0.98</td>
<td>0.249</td>
</tr>
<tr>
<td>17</td>
<td>household income:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>18</td>
<td>&lt;3,000</td>
<td>0.65</td>
<td>0.251</td>
<td>0.63</td>
<td>0.253</td>
</tr>
<tr>
<td>19</td>
<td>3,000 - 6,000</td>
<td>0.65</td>
<td>0.208</td>
<td>0.65</td>
<td>0.206</td>
</tr>
<tr>
<td>20</td>
<td>6,000 - 9,000</td>
<td>0.65</td>
<td>0.114</td>
<td>0.63</td>
<td>0.117</td>
</tr>
<tr>
<td>21</td>
<td>9,000 - 18,000</td>
<td>0.65</td>
<td>0.245</td>
<td>0.63</td>
<td>0.245</td>
</tr>
<tr>
<td>22</td>
<td>&gt;18,000</td>
<td>0.65</td>
<td>0.182</td>
<td>0.63</td>
<td>0.179</td>
</tr>
<tr>
<td>23</td>
<td>Personal income</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>24</td>
<td>&lt;3,000</td>
<td>0.92</td>
<td>0.789</td>
<td>0.92</td>
<td>0.789</td>
</tr>
<tr>
<td>25</td>
<td>3,000 - 6,000</td>
<td>0.92</td>
<td>0.131</td>
<td>0.92</td>
<td>0.127</td>
</tr>
<tr>
<td>26</td>
<td>6,000 - 9,000</td>
<td>0.92</td>
<td>0.046</td>
<td>0.92</td>
<td>0.053</td>
</tr>
<tr>
<td>27</td>
<td>&gt;9,000</td>
<td>0.92</td>
<td>0.034</td>
<td>0.92</td>
<td>0.031</td>
</tr>
<tr>
<td>28</td>
<td>At baseline</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>29</td>
<td>Have a job</td>
<td>0.98</td>
<td>0.192</td>
<td>0.98</td>
<td>0.198</td>
</tr>
<tr>
<td>30</td>
<td>Months employed</td>
<td>1.00</td>
<td>3.530</td>
<td>0.60</td>
<td>3.596</td>
</tr>
<tr>
<td>31</td>
<td>Had a job</td>
<td>0.98</td>
<td>0.627</td>
<td>0.98</td>
<td>0.635</td>
</tr>
<tr>
<td>32</td>
<td>Earnings</td>
<td>0.93</td>
<td>2810.482</td>
<td>0.94</td>
<td>2906.453</td>
</tr>
<tr>
<td>33</td>
<td>Usual hrs/week</td>
<td>1.00</td>
<td>20.908</td>
<td>0.61</td>
<td>21.816</td>
</tr>
<tr>
<td>34</td>
<td>Usual weekly earnings</td>
<td>1.00</td>
<td>102.894</td>
<td>0.97</td>
<td>110.993</td>
</tr>
<tr>
<td></td>
<td>After random assignment</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>35</td>
<td>Week 52 weekly hrs.</td>
<td>1.00</td>
<td>17.784</td>
<td>1.00</td>
<td>15.297</td>
</tr>
<tr>
<td>36</td>
<td>Week 104 weekly hrs.</td>
<td>1.00</td>
<td>21.977</td>
<td>1.00</td>
<td>22.645</td>
</tr>
<tr>
<td>37</td>
<td>Week 156 weekly hrs.</td>
<td>1.00</td>
<td>23.881</td>
<td>1.00</td>
<td>25.879</td>
</tr>
<tr>
<td>38</td>
<td>Week 208 weekly hrs.</td>
<td>1.00</td>
<td>25.833</td>
<td>1.00</td>
<td>27.786</td>
</tr>
<tr>
<td>39</td>
<td>Week 208 weekly earnings</td>
<td>1.00</td>
<td>200.500</td>
<td>1.00</td>
<td>227.912</td>
</tr>
</tbody>
</table>

N = 3599 5546

Notes:  Missing values for each pretreatment characteristic were imputed with the mean of that variable. Computation used design weights.
* Indicates that the difference is statistically significant at a 5% level.
Table 2. Observed groups based on treatment and employment indicators ($\tau_i$, $S_i$) and PS mix within groups.

<table>
<thead>
<tr>
<th>Groups by observed ($\tau_i$, $S_i$)</th>
<th>PS</th>
<th>PS (individual monotonicity)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(0,0)</td>
<td>$NN$ and $NE$</td>
<td>$NN$ and $NE$</td>
</tr>
<tr>
<td>(1,1)</td>
<td>$EE$ and $NE$</td>
<td>$EE$ and $NE$</td>
</tr>
<tr>
<td>(1,0)</td>
<td>$NN$ and $EN$</td>
<td>$NN$</td>
</tr>
<tr>
<td>(0,1)</td>
<td>$EE$ and $EN$</td>
<td>$EE$</td>
</tr>
</tbody>
</table>

Notes: PS stands for principal strata.

Table 3. Worst Case Bounds (Horowitz and Manski, 2000) on treatment effects for week 208 ln(wage).

<table>
<thead>
<tr>
<th>Bounds on Support of wages</th>
<th>Quantity in eq. (5)</th>
<th>Transformed wages</th>
<th>Original wages</th>
</tr>
</thead>
<tbody>
<tr>
<td>$5^{th}$ percentile mean wage</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$95^{th}$ percentile mean wage</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$Y_{LB}^*$</td>
<td>$Y_{LB}^*$</td>
<td>2.46</td>
<td>4.77</td>
</tr>
<tr>
<td>$Y_{UB}^*$</td>
<td>$Y_{UB}^*$</td>
<td>15.96</td>
<td>14.00</td>
</tr>
</tbody>
</table>

Control group

(i) Employment rate $Pr(S_i=1 \mid \tau_i=0)$ | 0.566 | 0.566 |
(ii) Mean ln(wage) $E[Y_i \mid \tau_i=0, S_i=1]$ | 1.997 | 1.991 |
(a) Upper bound $(i)*(ii)+(1-(ii))*Y_{UB}^*$ | 2.332 | 3.729 |
(b) Lower bound $(i)*(ii)+(1-(ii))*Y_{LB}^*$ | 1.52  | 0.451 |

Treatment group

(iii) Employment rate $Pr(S_i=1 \mid \tau_i=1)$ | 0.607 | 0.607 |
(iv) Mean ln(wage) $E[Y_i \mid \tau_i=1, S_i=1]$ | 2.031 | 2.028 |
(c) Upper bound $(iii)*(iv)+(1-(iii))*Y_{UB}^*$ | 2.321 | 3.587 |
(d) Lower bound $(iii)*(iv)+(1-(iii))*Y_{LB}^*$ | 1.586 | 0.620 |

ITT Effect

Upper bound $UB_{HM}^*$ | 0.802 | 3.135 |
Lower bound $LB_{HM}^*$ | -0.746 | -3.109 |
Width $UB_{HM}^*-LB_{HM}^*$ | 1.548 | 6.244 |

Notes: The population quantities $Pr(S_i=0 \mid \tau_i=0)$ and $Pr(S_i=0 \mid \tau_i=1)$ are calculated as $(1-Pr(S_i=1 \mid \tau_i=0))$ and $(1-Pr(S_i=1 \mid \tau_i=1))$, respectively.

Equivalently to using Equations in (5) to calculate $UB_{HM}^*$ and $LB_{HM}^*$, one may use the upper and lower bounds for the control and treatment group, labeled (a), (b), (c), (d), respectively, and compute:

$UB_{HM}^* = (c)-(b)$ and $LB_{HM}^* = (d)-(a)$.

The variable wage was transformed as described in Section 6.1; these results are reported under the column heading “Transformed wages”.
Table 4. Principal strata population proportions by race group.

<table>
<thead>
<tr>
<th>PS</th>
<th>All</th>
<th>Non-Hispanics</th>
<th>Whites</th>
<th>Blacks</th>
<th>Non-Hispanic Males</th>
<th>Non-Hispanic Females</th>
</tr>
</thead>
<tbody>
<tr>
<td>EE</td>
<td>0.566</td>
<td>0.559</td>
<td>0.657</td>
<td>0.512</td>
<td>0.583</td>
<td>0.530</td>
</tr>
<tr>
<td>NN</td>
<td>0.393</td>
<td>0.392</td>
<td>0.303</td>
<td>0.436</td>
<td>0.377</td>
<td>0.410</td>
</tr>
<tr>
<td>NE</td>
<td>0.041</td>
<td>0.049</td>
<td>0.040</td>
<td>0.052</td>
<td>0.040</td>
<td>0.060</td>
</tr>
</tbody>
</table>

Observations 9145
Trimming proportion 0.068

Table 5. Bounds on treatment effects for ln(wage) in week 208.

<table>
<thead>
<tr>
<th>Control group</th>
<th>PS framework</th>
<th>Transformed wages</th>
<th>Original wages</th>
</tr>
</thead>
<tbody>
<tr>
<td>(ii) Proportion of nonmissing</td>
<td>$p_{1</td>
<td>0} = Pr(S_i = 1</td>
<td>\tau_i = 0)$</td>
</tr>
<tr>
<td>(iii) Mean ln(wage) for employed</td>
<td>$E[Y_i</td>
<td>\tau_i = 0, S_i = 1]$</td>
<td>1.997</td>
</tr>
<tr>
<td>Treatment group</td>
<td>Number of observations</td>
<td>5546</td>
<td>5546</td>
</tr>
<tr>
<td>(v) Proportion of nonmissing</td>
<td>$p_{1</td>
<td>1} = Pr(S_i = 1</td>
<td>\tau_i = 1)$</td>
</tr>
<tr>
<td>Mean ln(wage) for employed</td>
<td>$E[Y_i</td>
<td>\tau_i = 1, S_i = 1]$</td>
<td>2.031</td>
</tr>
<tr>
<td>$p = [(v)-(ii)]/(v)$</td>
<td>$I \cdot p_{1</td>
<td>0}/p_{1</td>
<td>1}$</td>
</tr>
<tr>
<td>$p$th quantile</td>
<td>$y_{11-(p_{1</td>
<td>0}/p_{1</td>
<td>1})}$</td>
</tr>
<tr>
<td>(ix) Trimmed mean: $E[Y_i \mid y &gt; y_{p}]$</td>
<td>$E[Y_i</td>
<td>\tau_i = 1, S_i = 1, Y_i \geq y_{11-(p_{1</td>
<td>0}/p_{1</td>
</tr>
<tr>
<td>$(1-p)$th quantile</td>
<td>$y_{p_{1</td>
<td>0}/p_{1</td>
<td>1}}$</td>
</tr>
<tr>
<td>(xi) Trimmed mean: $E[Y_i \mid y &lt; y_{1-p}]$</td>
<td>$E[Y_i</td>
<td>\tau_i = 1, S_i = 1, Y_i \leq y_{11-(p_{1</td>
<td>0}/p_{1</td>
</tr>
<tr>
<td>Effect</td>
<td>Upper bound</td>
<td>$UB_{EE} = (ix)-(iii)$</td>
<td>0.093</td>
</tr>
<tr>
<td></td>
<td>(xiii)</td>
<td>(0.014)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Lower bound</td>
<td>$LB_{EE} = (xi)-(iii)$</td>
<td>-0.019</td>
<td>-0.022</td>
</tr>
<tr>
<td>(xv)</td>
<td>(0.018)</td>
<td>(0.016)</td>
<td></td>
</tr>
<tr>
<td>Width</td>
<td>$UB_{EE} - LB_{EE}$</td>
<td>0.112</td>
<td>0.121</td>
</tr>
<tr>
<td>Confidence interval (Imbens and Manski)</td>
<td>[$LB_{EE} - 1.645\times(xv), UB_{EE} + 1.645\times(xiii)$]</td>
<td>[-0.049, 0.116]</td>
<td>[-0.048, 0.122]</td>
</tr>
<tr>
<td></td>
<td>All</td>
<td>Non-Hispanics</td>
<td>Whites</td>
</tr>
<tr>
<td>------------------</td>
<td>------------</td>
<td>---------------</td>
<td>------------</td>
</tr>
<tr>
<td><strong>Upper bound</strong></td>
<td>0.099</td>
<td>0.118</td>
<td>0.120</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.015)</td>
<td>(0.028)</td>
</tr>
<tr>
<td><strong>Lower bound</strong></td>
<td>-0.022</td>
<td>-0.018</td>
<td>8.989E-05</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.017)</td>
<td>(0.031)</td>
</tr>
<tr>
<td><strong>Width</strong></td>
<td>0.121</td>
<td>0.136</td>
<td>0.120</td>
</tr>
<tr>
<td><strong>Confidence interval (Imbens and Manski)</strong></td>
<td>[-0.049, 0.122]</td>
<td>[-0.046, 0.143]</td>
<td>[-0.050, 0.166]</td>
</tr>
</tbody>
</table>
Table 7. Bounds on treatment effects for ln(wage) in week 208 using assumptions A and B, C, and D.

<table>
<thead>
<tr>
<th>Assumption:</th>
<th>Transformed wages</th>
<th>Original wages</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>A and B</td>
<td>C</td>
</tr>
<tr>
<td>Control group</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of observations</td>
<td>3599</td>
<td>3599</td>
</tr>
<tr>
<td>(ii) Proportion of nonmissing</td>
<td>0.566</td>
<td>0.566</td>
</tr>
<tr>
<td>Mean ln(wage) for employed</td>
<td>1.997</td>
<td>1.997</td>
</tr>
<tr>
<td>Treatment group</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of observations</td>
<td>5546</td>
<td>5546</td>
</tr>
<tr>
<td>(v) Proportion of nonmissing</td>
<td>0.607</td>
<td>0.607</td>
</tr>
<tr>
<td>Mean ln(wage) for employed</td>
<td>2.031</td>
<td>2.031</td>
</tr>
<tr>
<td>p= [(v)-(ii)]/(v)</td>
<td>0.068</td>
<td>0.068</td>
</tr>
<tr>
<td>pth quantile</td>
<td>1.636</td>
<td>1.636</td>
</tr>
<tr>
<td>Trimmed mean: E[Y</td>
<td>y&gt;y_p]</td>
<td>2.090</td>
</tr>
<tr>
<td>1-p quantile</td>
<td>2.768</td>
<td>2.768</td>
</tr>
<tr>
<td>Trimmed mean: E[Y</td>
<td>y&lt;y_{1-p}]</td>
<td>1.978</td>
</tr>
<tr>
<td>Effect</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Upper bound</td>
<td>0.093</td>
<td>0.093</td>
</tr>
<tr>
<td>Lower bound</td>
<td>-0.019</td>
<td>0.000</td>
</tr>
<tr>
<td>Width</td>
<td>0.112</td>
<td>0.093</td>
</tr>
<tr>
<td>Confidence interval</td>
<td>[-0.049,</td>
<td>[-0.018,</td>
</tr>
<tr>
<td></td>
<td>0.116]</td>
<td>0.116]</td>
</tr>
</tbody>
</table>
Table 8. Bounds and confidence intervals on average treatment effects for ln(wage) in week 208 by subgroups, using assumptions A,B and D.

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Non-Hispanics</th>
<th>Whites</th>
<th>Blacks</th>
<th>Non-Hispanic Females</th>
<th>Non-Hispanic Males</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Upper bound</strong></td>
<td>0.099</td>
<td>0.118</td>
<td>0.120</td>
<td>0.116</td>
<td>0.120</td>
<td>0.114</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.015)</td>
<td>(0.028)</td>
<td>(0.020)</td>
<td>(0.024)</td>
<td>(0.020)</td>
</tr>
<tr>
<td><strong>Lower bound</strong></td>
<td>0.037</td>
<td>0.050</td>
<td>0.056</td>
<td>0.053</td>
<td>0.046</td>
<td>0.052</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.013)</td>
<td>(0.022)</td>
<td>(0.016)</td>
<td>(0.020)</td>
<td>(0.016)</td>
</tr>
<tr>
<td><strong>Width</strong></td>
<td>0.062</td>
<td>0.068</td>
<td>0.064</td>
<td>0.063</td>
<td>0.074</td>
<td>0.061</td>
</tr>
<tr>
<td><strong>Confidence interval</strong></td>
<td>[0.018, 0.122]</td>
<td>[0.029, 0.143]</td>
<td>[0.019, 0.166]</td>
<td>[0.027, 0.149]</td>
<td>[0.014, 0.159]</td>
<td>[0.026, 0.147]</td>
</tr>
</tbody>
</table>
Figure 1. Bounds and IM confidence intervals of LQTE by subgroups, under Assumptions A & B. Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of a vertical line.
Figure 2. Bounds and IM confidence intervals of $LQTE$ by subgroups, under Assumptions A, B & F. Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of a vertical line.