Bounds on Quantile Treatment Effects of Job Corps 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 youths, on wages. We employ partial identification techniques and construct informative nonparametric bounds for the causal effect of interest under weaker assumptions than those conventionally used for point identification of treatment effects in the presence of sample selection. In addition, we propose and estimate bounds on quantile treatment effects of the program on participants' wages. In general, we find convincing evidence of positive impacts of JC on participants' wages. Importantly, we find that estimated impacts on lower quantiles of the distribution are higher, with the highest impact being in the 5^{th} percentile where a positive effect on wages is bounded between 8.4 and 16.1 percent. These bounds suggest that JC results in wage compression within eligible participants.

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 usually is a difficult task.[1] The vast majority of both substantive and methodological econometric literature of program evaluation (see Angrist and Krueger, 1999, Blundell and Dias, 2009, and Imbens and Wooldridge, 2009) focuses on estimating causal effects of participation on total earnings, which is, as pointed out by Lee (2009), 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 the ultimate the goal of active labor market programs.

Total earnings are the product of the individual's wage times hours worked, in other

[1]When conducting evaluation of these kind of programs one has to deal with a missing data problem, i.e., an individual may either be participating in the program or not, but no one individual can be in both states simultaneously. In the econometric literature this inherent fact of both experimental and observational studies is referred to as "the fundamental problem of causal inference" (Holland, 1986).

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 can not distinguish how much of the effect is due to human capital improvements. Clearly, assessment of the effect of program participation on human capital requires to focus on the price component of earnings, i.e., wages. The importance of the effect of labor market programs on participants' wages stems from its direct relationship with the improvement of the participants' human capital due to the program, which is essential for individuals to boost their labor market opportunities. In addition, the estimation of this effect allows policy makers to better understand the components through which these programs lead to more favorable labor market outcomes.

Unfortunately, estimation of the causal effect of program participation on individuals' wages is not straightforward due to the sample selection problem (Heckman, 1979). Basically, we only observe wages for those individuals who are employed, and thus, comparable individuals' wages may or may not be observed. Even in experimental settings, randomization does not guarantee the comparability of individuals' wages in treatment and control groups, since a person's decision to be employed is endogenous and occurs after 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 which is funded by the U.S. Department of Labor, to empirically assess the effect of training on participants' wages. To accomplish this objective we construct informative nonparametric bounds for the causal effect of participation. This strategy requires weaker assumptions than those conventionally used for point identification of the average treatment effect in the presence of sample selection.[2]

Similarly to Lee (2009), our analysis starts by computing the Horowitz and Manski (2000) "worst-case" scenario bounds. Their general approach imputes missing data with either the largest or the smallest possible values, using these extremes to compute

[2]Point identification of average treatment effects typically requires strong distributional assumptions such as bivariate normality (Heckman, 1979). One may relax this distributional assumption by relying on exclusion restrictions (Heckman, 1990; Heckman and Smith, 1995), 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 practice (Angrist and Krueger, 1999; Angrist and Krueger, 2001).

the largest or the smallest possible treatment effect, which constitute bounds that are consistent with the observed data. As such these bounds do not require the use of exclusion restrictions nor making distributional assumptions. While the approach encompasses non-refutable assumptions in settings where the outcome data is missing, it requires the availability of a bounded support of the outcome. As a result, the "worst-case" scenario bounds' width is uninformative in our particular application.

Subsequently, we proceed by imposing more structure through the use of several assumptions and derive results using the Principal Stratification (PS) framework (Frangakis and Rubin, 2002).[3] In addition to assuming random assignment of a binary treatment, which is satisfied in our application, the construction of our bounds requires assuming weak monotonicity of mean potential outcomes at three levels: individual, within subpopulation and across subpopulations.[4] These subpopulations (strata under PS framework) are defined by the values of two variables: the potential treatment status and an employment indicator that determines the observability of the outcome (i.e., wages). Given the binary nature of both variables, this set up gives rise to four principal strata.

These assumptions are not totally new to the growing body of literature on partial identification. For example, in a setting similar to ours, Zhang et al., (2008) and Lee (2009) derive bounds for the effect of a job training program on wages, assuming random assignment of treatment and individual level monotonicity.[5] While the former uses PS to derive results, both studies in essence devised the same identification strategy of a trimming proportion of the outcome distribution that allows tightening the bounds for average treatment effects. Relative to the Horowitz and Manski (2000) "worst-case" bounds, resulting bounds in Zhang et al., (2008) and Lee (2009) are tighter and do not rely on the availability of a bounded support of the outcome.

[3]The PS framework has its roots in the analysis of identification of local average treatment effects using instrumental variables in Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996)

[4]Even though, bounds on the parameter of interest involve only one subpopulation, i.e., always employed individuals independent of treatment assignment, the assumption of weak monotonicity across subpopulations requires interaction with other subpopulations found in a sample.

[5]The assumption of individual level monotonicity has also been used in different settings. For example, Flores and Flores-Lagunes (2010) use PS to derive bounds on the population net and mechanism average treatment effect in a setting where outcome data was always observed. Zhang and Rubin (2003) use PS to estimate causal effects when some outcomes are truncated by death.

An example of the identifying power of assuming monotonicity within subpopulation can be found in Flores and Flores-Lagunes (2010, FF hereafter). They use in spirit the same identification strategy as Zhang et al., (2008) and Lee (2009), and assume weak monotonicity of mean potential outcomes within subpopulations to construct bounds on the net and mechanism average treatment effects. Finally, the assumption of weak monotonicity across subpopulations is also considered in Blundell et al., (2007), Zhang et al., (2008), and more recently in Lechner and Melly (2010) and FF (2010). This last level of monotonicity, which is also known in the literature as stochastic dominance (Manski and Pepper, 2000), imposes further restrictions resulting in tighter bounds. In particular, the identification strategies in Blundell, et al., (2007), who derive sharp bounds on the distribution of wages and the interquantile range to study income inequality in the U.K., and Lechner and Melly (2010), who use partial identification to bound wage effects of a German job training program, are similar since they both require non-parametric estimation of the conditional distributions of the outcome. In contrast, the identification strategy that we follow is similar to Zhang et al., (2008), Lee (2009), and FF (2010), which does not rely on a non-parametric estimate of a conditional distribution of the outcome.

We contribute to the literature in two ways. First, we provide a substantive empirical 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 importance 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 scrutiny, and given that its effectiveness has always been debatable, with legislation seeking to cut federal spending, the program's operational budget is currently under threat. The second reason is that our results provide evidence to answer a policy relevant question about the impact of Job Corps on more disadvantage participants, and hence its effectiveness. 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., 2008), and thus implications can be generalized, with confidence, to Job Corps at a national level.

The second contribution is methodological in nature. Using the PS framework and relying on a set of weak monotonicity assumptions to tighten nonparametric bounds, we provide the basis for analyzing treatment effects in different quantiles of the distribution of an outcome in the presence of sample-selection. In doing so we propose the construction of bounds on the "Local Quantile Treatment Effect" ($LQTE$). Intuitively, after identifying the upper and lower bounding distributions of individuals that are always employed independent of their treatment assignment (our stratum of interest), 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. Our strategy of identification of bounds is similar to Zhang et. al., (2008), Lee(2009), and FF (2010). Our approach is distinguished from these three in that we go a step further into analyzing quantiles.[6]

In summary, by exploiting the ability of the proposed quantile model we characterize the heterogeneous impact of Job Corps training on different points of the participants' wage distribution. Compared to Lee (2009), who uses the same dataset as we do and assumes individual level monotonicity only, our bounds are tighter and more informative about the sign of the effect of training on wages, suggesting a positive effect on wages bounded between 3.4 and 9.3 percent. We go a step further in our analysis and report bounds for treatment effects on the $5^{th}, 10^{th}, ...,$ and 95^{th} percentile of participants' post-treatment wages. Our results suggest that the impact on lower quantiles of the distribution is higher, with the highest impact being in the 5^{th} percentile where a positive effect on wages is bounded between 8.4 and 16.1 percent. In other words, after accounting for the systematic heterogeneity in the impact of Job Corps on participants' wages we conclude that in addition to having a positive impact on wages, across the entire distribution, the program has an effect of wage compression within disadvantage groups. To our knowledge, the latter effect has not been previously identified, and thus it sheds light on the effectiveness of Job Corps at a new, important level.

The rest of the paper is organized as follows. Section 2 briefly describes the Job Corps, the data and its source the National Job Corps Study. Section 3 formally defines sample selection and briefly introduces a general identification strategy of bounding treat-

[6]Conventionally, other models of quantile treatment effect rely on instrumental variables (Abadie, Angrist and Imbens (2002) and Chernozhukov and Hansen (2005)), while the partial identification strategy we propose does not.

ment effects. In section 4 we introduce the Principal Stratification framework and the assumptions necessary to construct bounds on the treatment effect. Section 5 proposes bounds on quantiles treatment effect needed to analyze the heterogeneity of effects of Job Corps training on participants' wages. Section 6 contains our empirical application results. We conclude in section 7.

2 Job Corps and the National Job Corps Study

This subsection briefly describes both the Job Corps program and the randomized experiment, known as the National Job Corps Study (NJCS), which generated the data used in this empirical analysis.

Job Corps 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 (DOL). With a yearly cost of about $1.5 billion, Job Corps annual enrollment ascends to 100,000 students (DOL, 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 (DOL, 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 Job Corps (Schochet et al., 2008).

Being the nation's largest job training program, the Job Corps' history is full of controversy and its effectiveness has always been debatable. During the mid 1990's, the US Department of Labor commissioned Mathematica Policy Research, Inc. (MPR) to design and implement a randomized evaluation, the NJCS, in order to determine the program's effectiveness. The main feature of the study was its random assignment, namely, individuals were sampled from nearly all outreach and admissions agencies (OA)[7] located in 48

[7]Outreach and admissions (OA) agencies conduct recruitment and screening for Job Corps. OA

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 (N_t = 9,409) while 7% of the eligible applicants were assigned to the control group (N_c = 5,977). The remaining 65,497 were assigned to a program non-research group (Schochet et al., 2001). After recording the data in a baseline interview, for both treatment and control experimental groups, a series of follow up interviews were conducted at weeks 52, 104, 156, and 208 after randomization.

Randomization took place at the OA level, that is, before participants' assignment to a JC center. As a result, only 73% of the individuals randomly assigned to the treatment group actually enrolled in JC. Even though individuals assigned to the control group were embargoed from participating in JC for a period of 3 years after random assignment, 1.4% of them enrolled in the program within the prohibited period (Schochet et al., 2008). Therefore, in the presence of this non-compliance, the difference-in-means estimator, which compares average outcomes between individuals by random assignment to a treatment or a control group, represents the "Intention-to-Treat" (ITT) effect (Flores-Lagunes et al., 2009). Similarly to Lee (2009) and Flores and Flores-Lagunes (2010), the present empirical analysis focuses on estimating informative non-parametric bounds for the ITT parameter. However, we go a step further and use bounds to analyze JC's effect on different quantiles of the distribution of wages.

In particular, this paper uses the same dataset employed by Lee (2009), who develops an intuitive trimming procedure for bounding average treatment effects of Job Corps program on participants' wages.[8] Similarly to Lee (2009), the present analysis abstracts from missing values due to interview non-response and attrition over time by only including individuals who had no missing values for the post-treatment variables: weekly earnings and weekly hours worked. Thus, the resulting sample size, N_{Lee}=9145, is smaller than the original NJCS sample size, N=15386. Due to both programmatic and research reasons, different subgroups in the population study had different probabilities of being

agencies include private nonprofit firms, private for-profit firms, state employment agencies, and the centers themselves (Schochet et al., 2001).

[8]For a description of Lee's (2009) trimming procedure refer to footnote 18 in section 4.1.

included in the research sample, and thus, subsequent analysis requires the use of design weights (Schochet, 2001).[9]

Summary statistics are presented in Table 1. Pretreatment variables in the dataset include: demographic 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% level of confidence. The resulting difference for post-treatment earnings across groups, also reported in penultimate column, is quantitatively equivalent and consistent with the previously reported 12% positive effect of JC on participants' earnings (Burghardt et al., 1999; Flores-Lagunes et al., 2009; Schochet et al., 2001). Results were also consistent with those obtained in previous studies when looking at the effect of JC on participants' weekly hours worked (Schochet et al., 2001).

3 The Sample Selection Problem and Identification of Treatment Effects

Assessing the impact of job training programs on participants' wages, as pointed out by Zhang et al., (2008) and Lee (2009), is distinct than assessing the program's impact on earnings. Notice that 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, however, can be interpreted as pure price effect, which is the focus in the present study since significant increases in wages can be directly related with the improvement of the participants' human capital due to the program, which is essential for individuals to boost their labor market opportunities. In particular, one of JC main goals is the enhancement of participants' human capital through academic and vocational training, and thus, a proper assessment requires focusing on the program's impact on wages.

[9]For example, OA agencies had struggle recruiting females for residential slots. Therefore, sampling rates to the control group were intentionally set lower in some areas, to overcome difficulties with unfilled slots. See Schochet (2001) for more details on reasons and calculation of design weights.

It is well known, however, that estimation of program's treatment effect on participants' wages is complicated due to the fact that we only observe the wages of those who are employed, which is often referred in the literature as the sample selection problem (Heckman, 1979). Formally, let's assume an experimental setting with N individuals and with only two observable causes denoted by τ_i, where τ_i=1 indicates that individual i has been randomly assigned to participate in the program (treatment group) and τ_i=0 denotes no participation (control group). Y_i, individual i's wage, is assumed to be a linear function[10] of the treatment indicator τ_i and a set of pretreatment characteristics x_{1i},

$$Y_i = \beta_1 \tau_i + \beta_2 x_{1i} + \mu_{1i} \tag{1}$$

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

$$S_i^* = \delta_1 \tau_i + \delta_2 x_{2i} + \mu_{2i}, \tag{2}$$

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 individuals self-select into working, i.e., S_i =1 when individual i's propensity to work is positive ($S_i^* \geq 0$).

Conventionally, point identification of the parameter of interest β_1, which is assumed to be constant for the entire population, requires strong assumptions such as joint independence of the errors (u_{1i}, u_{2i}) in the wage and employment equations (1) and (2),

[10]Linearity is assumed to simplify the exposition of the sample selection problem and the identification of treatment effects. However, the alternative non-parametric approach to address sample selection, which is the focus of this paper, does not impose linearity to identify bounds on treatment effect parameters. In fact, as shown below and in subsequent sections, the identification procedure discussed in this study makes no functional form assumptions.

respectively, and the regressors τ_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), 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 is in practice difficult (Angrist and Krueger, 1999; Angrist and Krueger, 2001).

An alternative approach to model sample selection suggests that treatment effect parameters can be bounded without making strong distributional assumptions or without relying on the validity of exclusion restrictions. Following a conservative general framework provided by Horowitz and Manski (2000)[11] (HM hereafter), bounds on treatment effects when data is missing due to a nonrandom process, such as the self-selection into not working ($S_i^* < 0$), can be constructed, provided that the outcome variable has a bounded support. These bounds are known in the literature as "worst-case" scenario bounds (HM).

For ease of explanation we now switch to a slightly different notation than the one introduced above, let's define the average treatment effect (ATE) using the potential outcomes framework (Rubin, 1974), as the following

$$ATE = E[Y_i(1) - Y_i(0)|X_i] = E[Y_i(1)|X_i] - E[Y_i(0)|X_i], \tag{3}$$

where $Y_i(0)$ and $Y_i(1)$ are the two potential wages for unit i under control (τ_i=0) and treatment (τ_i=1), respectively. For simplicity we are going to suppress the conditioning on X_i, where X_i=(x_{1i}, x_{2i}). Conditional on τ_i and the observed employment indicator S_i, the ATE in (3) can be written as:[12]

[11]In their paper 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. Their general framework, however, can be applied to continuous outcome variables, thus allowing to model sample selection in our setting (Lee, 2009).

[12]Notice that outcomes of individuals are compared by random assignment. In the presence of non-compliance, conditioning on the assigned treatment indicator τ_i the formulae for ATE is interpreted as the "intention to treat" (ITT).

$$
\begin{aligned}
ATE = E[Y_i|\tau_i = 1, S_i = 1]Pr(S_i = 1|\tau_i = 1)+ \\
E[Y_i(1)|\tau_i = 1, S_i = 0]Pr(S_i = 0|\tau_i = 1) \\
-E[Y_i|\tau_i = 0, S_i = 1]Pr(S_i = 1|\tau_i = 0)- \\
E[Y_i(0)|\tau_i = 0, S_i = 0]Pr(S_i = 0|\tau_i = 0)
\end{aligned} \tag{4}
$$

Examination of Equation (4) reveals that from the data we can identify all the conditional probabilities ($Pr(S_i|\tau_i)$), and also the expectations of 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, due to sample selection, from the experimental data is not possible to point identify $E[Y_i|\tau_i = 1, S_i = 0]$ and $E[Y_i|\tau_i = 0, S_i = 0]$. We can, however, impute "worst-case" scenario bounds on these unobserved quantities, provided that the support of the outcome lies in the interval (Y^{LB}, Y^{UB}), i.e., the missing outcomes $E[Y_i|\tau_i = 1, S_i = 0]$ and $E[Y_i|\tau_i = 0, S_i = 0]$ can take any value in the interval (Y^{LB}, Y^{UB}). The HM lower and upper bounds (LB^{HM} and UB^{HM} respectively) are calculated from the data as follow:

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

In the next sections we follow, in spirit, this general bounding approach and proceed by imposing more structure through the use of several assumptions in the context of the Principal Stratification framework (Frangakis and Rubin, 2002).

4 Principal Stratification Framework and Identification of Bounds on Treatment Effects

Flores and Flores-Lagunes (2010) (FF hereafter) employed the Principal Stratification (PS) framework (Frangakis and Rubin, 2002) to study the mechanisms or channels through which the treatment works. To accomplish their objective, FF decomposed the

ATE into net average treatment effect ($NATE$), defined as "the average potential outcome from a counterfactual treatment in which the effect of the original treatment on the mechanism variable is blocked minus the average potential outcome under control" and mechanism average treatment effect ($MATE$), which is equal to ATE-$NATE$. In their paper they used local net average treatment effects ($LNATE$) and local mechanism average treatment effects ($LMATE$), which are defined at the principal strata level, to derive informative non-parametric bounds for the population $NATE$ and $MATE$. As they pointed out, their approach for identification of $LNATE$ can be useful to study treatment effects when dealing with sample selection. In our particular application, treatment effects on wages are identified after controlling for the self-selection "mechanism", i.e., employment, which can be done for a specific "local" subpopulation comprised of individuals with defined wages, thus the focus here is on the local net average treatment effect for those who are always employed independent of treatment assignment.

FF approach followed in essence the same identification strategy employed by Zhang et al. (2008), who also used PS framework, and Lee (2009). FF's study, however, was not limited by a context where selection causes censoring of the outcome of interest, and therefore, they were able to derive non-parametric bounds for the population parameters, $NATE$ and $MATE$, which enables learning "how" the treatment affects the outcome. In our case, similarly to Zhang et al. (2008) and Lee (2009), the focus is on the average treatment effect on wages, which are censored when the individuals are not employed. Thus, the identification of the ATE on wages requires controlling for selection into employment (the mechanism), and it will become clear in this section that this is equivalent to focus on the $LNATE$ for a subpopulation for which the mechanism is not affected by the treatment. Mainly, this section is devoted to summarize FF's main identification results as well as to apply them to our particular application, which is the identification of informative non-parametric bounds for the treatment effect of JC on participants' wages.

PS framework introduced by Frangakis and Rubin (2002) allows for identification of average causal effects when controlling for a post-treatment variable that has been affected by treatment assignment. In the context of JC, this affected post-treatment variable is employment. Following FF terminology, this variable is referred to as a mechanism. It will become clearer in the next few paragraphs that whenever individuals belong to the same principal strata, which are constructed based on the potential values that the mechanism can take, which is a function of the treatment, comparisons between average outcomes by treatment assignment will have a causal interpretation since the strata an individual belongs to is not affected by treatment assignment.

Consistent with the notation introduced in the previous section, based on Equation (2), it is clear that the mechanism S_i, is affected by the treatment (τ_i). Hence, the mechanism, denoted as $S_i(\tau)$, has two potential values $S_i(0)$ and $S_i(1)$, when i is assigned to control and treatment, respectively. Given $S_i(\tau)$ potential values, FF defined the "composite" potential outcomes as $Y_i(\tau_i, S_i(\tau))$.[13] The potential outcome $Y_i(1, S_i(0))$[14] is used to define $NATE$, since it represents the potential outcome of individual i under treatment in which the effect that τ_i=1 has on the mechanism is controlled, such that $S_i(\tau)$=0. The ATE in (3), without the conditioning on X, can be decomposed using $Y_i(1, S_i(0))$ as follows:

If we write the $ATE = E[Y_i(1) - Y_i(1, S_i(0))] + E[Y_i(1, S_i(0)) - Y_i(0)]$, then the $NATE$ can be formally defined as:

$$NATE = E[Y_i(1, S_i(0)) - Y_i(0)], \tag{6}$$

and the $MATE = E[Y_i(1) - Y_i(1, S_i(0))]$. As mentioned at the beginning of this section, in our particular application (treatment effect of program participation on wages) discussion of the $MATE$ is out of the scope of the paper,[15] the focus instead is on $LNATE$, which will be formally discussed in subsequent paragraphs.

Basic principal stratification consists in partitioning individuals into groups based on the values that the mechanism vector $\{S_i(0), S_i(1)\}$ may take. Let $S_i(\tau)$ be binary. This is the case in the context of our application since $S_i(\tau)$ may only take a value of 0 if individual i is unemployed and 1 if employed. As a result, the four principal strata are:

[13]Notice that the potential outcomes $Y_i(1, S_i(1))$ and $Y_i(0, S_i(0))$ correspond to the conventional potential outcomes $Y_i(1)$ and $Y_i(0)$ used in Equation (3). Two more potential outcomes are generated from the "composite" process, $Y_i(1, S_i(0))$ and $Y_i(0, S_i(1))$, for more detail see FF (2010).

[14]The potential outcome $Y_i(1, S_i(0))$ represents the counterfactual used in the definition of $NATE$ given at the beginning of this section.

[15]Perhaps one may be interested in studying "how" the treatment works, in other words, understanding the mechanisms or channels through which the treatment affects participants' wages (Blanco, et al., 2011).

$$
\begin{aligned}
NN &= S_i(0) = 0, S_i(1) = 0 \\
EE &= S_i(0) = 1, S_i(1) = 1 \\
EN &= S_i(0) = 1, S_i(1) = 0 \\
NE &= S_i(0) = 0, S_i(1) = 1
\end{aligned}
\tag{7}
$$

In the context of JC, NN is the strata for those individuals who would be unemployed independent of treatment assignment, EE is the strata for those who would be employed independent of treatment assignment, EN represent those who would be employed if assigned to control, but not employed 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.

FF's first result states that the observed data (Y_i, τ_i, S_i) contains information on the counterfactual $Y_i(1, S_i(0))$ only for the subpopulation of individuals where treatment (τ_i) does not affect the mechanism (S_i), such that $S_i(0) = S_i(1)$ and $Y_i = Y_i(1, S_i(0))$. With this result in mind, it follows that from the data is possible to identify objects for certain strata. Let the "Local" $NATE$, or $LNATE$, be defined as the $NATE$ for a given principal strata in (7):

$$
LNATE_k = E[Y_i(1, S_i(0))|k] - E[Y_i(0)|k], fork = NN, EE, EN, NE \tag{8}
$$

In general the $LNATE_k$ in (8) is useful for analyzing treatment effects in the presence of self-selection. In particular, $LNATE_k$ in (8) will be used to analyze treatment effects of JC on participants' wages. The reason for using (8) is that wages are not defined for individuals when they are not employed, therefore, one has to focus on those individuals with defined wages independent of treatment assignment. This is the case for individuals that belong to the $k = EE$ strata. Notice that $k = EE$ corresponds to the strata for which the mechanism is not affected by treatment assignment. Another strata in which the mechanism is not affected by treatment would be $k = NN$, but we will not be able to compute $LNATE_{NN}$ since wages for this individuals are not defined in neither treatment arm ($k = NN$ is comprised of always unemployed individuals). Furthermore, within the EE strata, comparisons between average wages by treatment assignment will have a causal interpretation, since the strata an individual belongs to is not affected by treatment assignment. Intuitively, "estimation" of the parameter $LNATE_{EE}$ on wages controls for selection into employment.[16]

Note that, the $LNATE_{EE}$ parameter is equivalent to the ATE_{EE} parameter used by Zhang et al. (2008), and the ATE in Lee (2009). To see this, notice that from FF's first result $Y_i = Y_i(1, S_i(0))$, for $k = EE$, thus, $LMATE_{EE} = E[Y_i(1)|EE] - E[Y_i(1, S_i(0))|EE] = 0$ (the same applies when $k = NN$). Identification of the $LNATE_k$ parameter for k different than EE is not possible given that wages are not well-defined when individuals are unemployed.

4.1 Basic Assumptions and Identification of Bounds

Without enough assumptions, point identification of $LNATE_{EE}$ in the form of (8) is not possible since one does not observe the counterfactual $E[Y_i(1, S_i(0))|EE]$. Thus, the focus is on partial identification of $LNATE_{EE}$. To partially identify $LNATE_{EE}$, we start by making the following assumptions:

Assumption A: *Randomly Assigned Treatment.*

Assumption B: *Individual Level Monotonicity of* τ *on* $S(\tau)$*.*

Assumptions A and B correspond to assumptions A1 and A2 in FF, respectively. Assumption A corresponds implies independence of the errors in regressions (1) and (2) (u_{1i}, u_{2i}) and (τ_i, X). This commonly used assumption is ensured by the random assignment of treatment in the NJCS. The monotonicity assumption (B) is applied to the effect that the treatment has on the mechanism.[17] Specifically, monotonicity states that treat-

[16]Identification of $LNATE_{EE}$ using PS framework is similar to the estimation of Local average treatment effects using instrumental variables by Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996).

[17]Assumption B is also commonly invoked in the literature of imperfect compliance but is applied to the effect of an instrument on treatment status (Imbens and Angrist, 1994; and Angrist, Imbens and Rubin, 1996).

ment assignment can only affect the mechanism in one direction, $S_i(1) \geq S_i(0)$ for all i. Zhang et al. (2008) and Lee (2009) employed the monotonicity assumption, stated as how treatment affects selection into employment. In this particular application, sample selection, defined by employment status ($S_i(\tau)$), corresponds to the "so-called" mechanism variable. Therefore, up to this point, the approaches in Zhang et al. (2008), and Lee (2009) yield the same results as in FF.

Given the monotonicity assumption (B) of a non-decreasing effect of τ_i on S_i, one may rule out the existence of the principal strata defined as EN, comprised of individuals whose likelihood of employment was affected negatively given that they were assigned to the treatment group. In the context of Job Corps, individual level monotonicity is likely to hold since the program offers job search assistance to their participants. Unfortunately, Assumption B is not testable and a negative impact of treatment on employment can not be statistically ruled out. As shown in the last column of Table 3, Assumption B allows the identification of some members in EE and NN, they are defined by the observed groups (first column) with (τ_i, S_i) (0, 1) and (1,0), respectively. Furthermore, given assumptions A and B, the proportions of each principal strata in the population are point identified (Zhang et al., 2008; and FF, 2010). Let π_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 $\pi_{EN} = 0$.

From Table 3, we know that individuals in the observed group with $(\tau_i, S_i) = (0, 1)$ belong to the strata of interest EE (Last row and column). Therefore, from $LNATE_{EE} = E[Y_i(1, S_i(0))|EE] - E[Y_i(0)|EE]$ one can point identify the control $E[Y_i(0)|EE]$ by computing $E[Y_i|\tau_i = 0, S_i = 1]$. On the other hand, is not possible to point identify $E[Y_i(1, S_i(0))|EE]$, since the average outcome for individuals in the observed group with $(\tau_i, S_i) = (1, 1)$, contains units from two strata, EE and NE. With the known population proportions π_k, however, note that $E[Y_i|\tau_i = 1, S_i = 1]$ can be written as a weighted average between individuals in EE and NE:

$$E[Y_i|\tau_i = 1, S_i = 1] = \frac{\pi_{EE}}{(\pi_{EE} + \pi_{NE})} E[Y_i(1)|EE] + \frac{\pi_{NE}}{(\pi_{EE} + \pi_{NE})} E[Y_i(1)|NE] \quad (9)$$

Notice in (9) that the proportion of EE in the group $(\tau_i, S_i) = (1, 1)$ can be point identified as $\pi_{EE}/(\pi_{EE}+\pi_{NE})=p_{1|0}/p_{1|1}$. Therefore, following the identification results by Zhang et al. (2008), and FF (2010), which are the same as those reported in Lee (2009),[18] $E[Y_i(1)|EE]$ 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 for those in the observed group (τ_i, S_i)=(1, 1). In other words, we cannot identify which observations belong to $E[Y_i(1)|EE]$, the "infra-marginal" individuals, and which belong to $E[Y_i(1)|NE]$, the "marginal" individuals. But the "worst-case" scenario is that the largest values $(p_{1|0}/p_{1|1})$ of Y_i belong to the "infra-marginal" individuals. Thus, computing the expected value of Y_i after trimming the lower tail of the distribution of Y_i, in (τ_i, S_i)=(1, 1), by $1-(p_{1|0}/p_{1|1})$ yields an upper bound for the "infra-marginal" group (Lee, 2009). 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.

From FF proposition 1, $LNATE_{EE}$ has the following upper and lower bounds, UB_{EE} and LB_{EE}, respectively:

$$\begin{aligned} UB_{EE} &= E[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}] - E[Y_i|\tau_i = 0, S_i = 1] \\ LB_{EE} &= E[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}] - E[Y_i|\tau_i = 0, S_i = 1], \end{aligned} \tag{10}$$

where $y^{11}_{1-(p_{1|0}/p_{1|1})}$ and $y^{11}_{(p_{1|0}/p_{1|1})}$ denote the $1-(p_{1|0}/p_{1|1})$ and the $(p_{1|0}/p_{1|1})$ quantile of Y_i conditional on $\tau_i = 1$ and $S_i = 1$, respectively.

4.1.1 Estimation Using Basic Assumptions

The estimates of bounds in (10) have the following sample analog form:

[18]The identification procedure in Lee (2009), which uses a generalize sample selection model approach, is the same in nature to that in Zhang et al. (2008) and Flores and Flores-Lagunes (2010), both under a PS framework. Specifically, from the wage regression in (1) Lee noted that the observed population mean for the treatment group $E[Y_i|\tau_i = 1, S_i = 1]$ corresponds to a weighted average of "infra-marginal" individuals, whose employment is not affected by treatment assignment, and "marginal" individuals, who are induced by the treatment assignment to be selected into the sample; hence, resulting in Equation (9). From this point on, Lee (2009) calculates trimming proportions based on the same intuition provided in Zhang, et al. (2008) and FF (2010) for bounding $E[Y_i(1)|EE]$ in equation (9).

$$\widehat{UB_{EE}} = \frac{\Sigma_{i=1}^n Y_i \cdot \tau_i \cdot S_i \cdot 1[Y_i \geq \widehat{y_{1-\hat{p}}}]}{\Sigma_{i=1}^n \tau_i \cdot S_i \cdot 1[Y_i \geq \widehat{y_{1-\hat{p}}}]} - \frac{\Sigma_{i=1}^n Y_i \cdot (1-\tau_i) \cdot S_i}{\Sigma_{i=1}^n (1-\tau_i) \cdot S_i}$$
$$\widehat{LB_{EE}} = \frac{\Sigma_{i=1}^n Y_i \cdot \tau_i \cdot S_i \cdot 1[Y_i \leq \widehat{y_{\hat{p}}}]}{\Sigma_{i=1}^n \tau_i \cdot S_i \cdot 1[Y_i \leq \widehat{y_{\hat{p}}}]} - \frac{\Sigma_{i=1}^n Y_i \cdot (1-\tau_i) \cdot S_i}{\Sigma_{i=1}^n (1-\tau_i) \cdot S_i}, \tag{11}$$

where $\hat{p}$, the sample analog of $(p_{1|0}/p_{1|1})$, is used to pin down the quantiles ($\widehat{y_{1-\hat{p}}}$ and $\widehat{y_{\hat{p}}}$) of the treatment group outcome distribution (analogs to the quantiles $y^{11}_{1-(p_{1|0}/p_{1|1})}$ and $y^{11}_{(p_{1|0}/p_{1|1})}$ in (10), respectively), is calculated as follows:

$$\hat{p} = \frac{\Sigma_{i=1}^n (1-\tau_i) \cdot S_i}{\Sigma_{i=1}^n (1-\tau_i)} / \frac{\Sigma_{i=1}^n \tau_i \cdot S_i}{\Sigma_{i=1}^n \tau_i} \tag{12}$$

Up to this point, using basic assumptions A and B, our results are quantitatively equivalent to those reported by Lee (2009). Specifically, Lee (2009) calculates the trimming proportion by taking the treatment control difference in the proportion with non-missing outcomes and dividing by the proportion that is selected in the treatment group.[19] Afterwards, Lee uses this proportion to calculate the threshold quantiles to trim the data and compute bounds for the treatment effect, yielding the same bounds estimates as those in (11). Lee (2009) shows that bounds in (11) are sharp and asymptotically normal, which allows the computation of confidence intervals.

4.2 Narrowing Bounds: Weak Monotonicity Within and Across Strata

We now consider two assumptions that can help sharpen the bounds in (10). The first assumption is related to, but different from, Manski (1997) and Manski and Pepper (2000) "monotone treatment response". 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, our assumption is weaker since it allows some individual effects of the treatment on the outcome to be negative. This is accomplished by imposing monotonicity on the mean potential outcomes for those individuals in the EE strata. Notice our assumption is a subset of FF's Assumption B, who uses weak

[19] Notice, Lee's (2009) trimming proportion, which is equal to: $[\frac{\Sigma S \cdot \tau}{\Sigma \tau} - \frac{\Sigma S \cdot (1-\tau)}{\Sigma (1-\tau)}]/[\frac{\Sigma S \cdot \tau}{\Sigma \tau}]$ with Σ summing for the entire sample n, is equivalent to the expression for $\hat{p}$ in (12)

monotonicity of mean potential outcomes within each of the four stratum. Formally, we employ:

Assumption C: *Weak Monotonicity of Mean Potential Outcomes Within the EE Strata.*

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

In addition to the basic assumptions A and B, Assumption C implies that $LNATE_{EE} \geq 0$.[20] Therefore, the lower bound in (10) becomes: $max\{0, LB_{EE}\}$; while the upper bound remains unchanged. In the context of Job Corps, Assumption C is likely to hold given that participants are exposed to substantial academic instruction.[21] Thus, consistent with conventional human capital theories in economics, one would expect, on average, a non-negative effect of treatment (JC participation) on wages.

As pointed out by FF, a potentially unattractive feature of Assumption C is that it restricts the sign of the effect of interest. We now consider a second assumption that is available in the partial identification literature. Our assumption is related to, but different from Manski and Pepper (2000) “monotone instrumental variable”. Their assumption states that mean responses vary weakly monotonically across subpopulations defined by specific values of the instrument. In contrast, our assumption conditions mean responses on two of the basic principal strata defined by a specific value of the mechanism, i.e., $S(1)$. A formal statement of our assumption is as follows:

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

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

Assumption D is a subset of FF’s Assumption C. Although their approach is the basis for our analysis, their application does not deal with censored outcomes, and thus, their

[20]To see this, take the $LNATE_{EE}$ definition in (8), $E[Y(1, S(0))|EE] - E[Y(0)|EE]$, employing Assumption C, $E[Y(1, S(0))|EE] \geq E[Y(0)|EE]$, will result in $LNATE_{EE} \geq 0$.

[21]On average, JC participants can expect to receive about 440 hours of academic instruction (Schochet et al., 2001).

assumption also contemplates weak monotonic relations with respect to the NN strata. Our assumption is also related to the stochastic dominance assumption in Blundell et. al., (2007), Lechner and Melly (2010), and Zhang et. al., (2008). Their assumption is a special case of the "monotone instrumental variable" assumption, applied in settings with missing outcomes, and it relates to the stochastic dominance conditions (Manski, 2003). Similarly to the above mentioned literature, Assumption D formalizes the notion that the EE strata is likely to be comprise of more capable individuals than those belonging to the NE strata, since ability is positively correlated with labor market outcomes (e.g. wages, employment) one should expect potential outcomes for the EE strata to weakly dominate those for the NE strata. In our particular application, Assumption D is not directly testable since we are dealing with censored outcomes[22], however, in the estimation section we provide an indirect way of gauging its plausibility.

Employing assumptions A and B in addition to Assumption D results in tighter bounds. To illustrate this derivation consider the following: from (8), $LNATE_{EE} = E[Y_i(1, S_i(0))|EE] - E[Y_i(0)|EE]$, one can point identify the control $E[Y_i(0)|EE]$ by computing $E[Y_i|\tau_i = 0, S_i = 1]$ while $E[Y_i(1, S_i(0))|EE]$ is only partially identified. 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, $E[Y_i|\tau_i = 1, S_i = 1] = \frac{\pi_{EE}}{(\pi_{EE}+\pi_{NE})}E[Y_i(1)|EE] + \frac{\pi_{NE}}{(\pi_{EE}+\pi_{NE})}E[Y_i(1)|NE]$. After solving for $E[Y_i(1)|NE]$, substitute it into the inequality of Assumption C6, the result suggest that $E[Y_i(1)|EE] \geq E[Y_i|\tau_i = 1, S_i = 1]$, implying 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 (10) becomes: $E[Y_i|\tau_i = 1, S_i = 1] - E[Y_i|\tau_i = 0, S_i = 1]$.

4.2.1 Estimation Using Basic Assumptions and Assumptions C and D

When Assumption C is added to the basic assumptions (A and B), the upper bound estimate of (10) remains $\widehat{UB}_{EE}$ from (13). The estimate for the lower bound, however, is taken to be the *max* between zero and the lower bound estimated in (13). Formally, in addition to the basic assumptions, under weak monotonicity within the EE strata the lower bound sample estimate is:

[22]In a setting were outcome data is not censored, FF provided a formal test that requires the combination of several of their assumptions that can be use to falsify these assumptions.

$$\widehat{LB^c_{EE}} = max\{0, \widehat{LB_{EE}}\} \tag{13}$$

We now explore the tightening power of Assumption D, in combination with A and B. As with Assumption C, the upper bound estimate of (10) remains $\widehat{UB_{EE}}$ from (13). The effect of Assumption D is on the estimate for the lower bound, which has the following sample analog form:

$$\widehat{LB^d_{EE}} = \frac{\Sigma^n_{i=1} Y_i \cdot \tau_i \cdot S_i}{\Sigma^n_{i=1} \tau_i \cdot S_i} - \frac{\Sigma^n_{i=1} Y_i \cdot (1-\tau_i) \cdot S_i}{\Sigma^n_{i=1} (1-\tau_i) \cdot S_i} \tag{14}$$

Although Assumption D is not directly testable, we can compare average baseline characteristics across strata as an indirect way of gauging its plausibility. Intuitively, one should expect greater or equal average values of characteristics for the EE strata, follow by the NE, and lastly by the NN (i.e., a weakly monotonic rank across strata). We perform this indirect test in our empirical application (section 6).

The next section will summarize the results of using PS framework and our identifying assumptions to bound effects at different quantiles of the wage distribution.

5 Bounds on Quantiles of Treatment Effects

The principal stratification framework introduced, discussed, and implemented in the previous section, provides the basis for analyzing different quantiles in the distribution of effects that JC participation have on post treatment wages. Specifically, this section reports bounds for treatment effects on the $5^{th}, 10^{th}, ...,$ and 95^{th} percentile of participants' wages in week 208.

The parameter of interest in this particular section is the "Quantile Treatment Effect" (QTE), defined as the difference in quantiles between the treated and control groups' outcomes at a given quantile level "α" (Abadie, et. al., 2002; and Chernozhukov and Hansen, 2005). Conventionally, this difference is defined as long as the marginal distributions of potential outcomes are identified, in our particular application, however, marginal distributions of potential outcomes are only partially identified. Nevertheless, we follow the same conventional rationale and propose the estimation of bounds on the

QTE based on the partial identification results derived using PS.

Our work on QTE is closely related to two papers. First, Blundell, et. al., (2007) derived sharp bounds on the distribution of wages and the interquantile range, which is their measure to study income inequality in the U.K. for years 1978 to 2000. Their work builds on Manski (1994) and Manski and Pepper (2000). From this starting point, the "worst-case" bounds on the conditional quantiles (Manski, 1994), they imposed theoretical motivated restrictions to tighten bounds on quantiles. First they introduced positive selection into work, which is expressed as the stochastic dominance of employed individuals' wages on wages for the unemployed. Note that this is analogous to Assumption D in our paper. Second, Blundell, et al., (2007) considered exclusion restrictions that can be weakened with monotonicity.[23] Exclusions restrictions were shown to have tightening power, however, their use is out of the scope of our paper.

Blundell, et. al., (2007) were interested in population parameters and since wages are not defined for unemployed individuals, their procedure requires nonparametric estimation of employment probabilities and the observed wage distribution amongst workers conditional on a set of characteristics X. In contrast, our study focuses on analyzing QTE for a particular subpopulation with defined wages, the EE strata, and thus we do not rely on nonparametric estimates of employment probabilities and wage distributions. Even though, in our particular application we do not identify population parameters, when studying job training program effects our parameter is considered relevant for policy purposes. Note that we partially identify the effect of training on a population that accounts for 57% of the total sample, where 39% are individuals with undefined wages and the remainder 4% are individuals with defined wages dependent on being assigned to treatment ($\tau = 1$).

Second, Lechner and Melly (2010) use partial identification to bound wage effects of a German job training program. To bound QTE they consider a nonparametric version of the linear quantile regression (Koenker and Portnoy, 1987) to estimate the conditional distribution function, in doing so, they rely on the propensity score to reduce the dimensionality problem.[24] In contrast, due to the experimental nature of our data and

[23] In a different setting, Flores and Flores-Lagunes (2010b) imposes similar restrictions to derive bounds on local average treatment effects, using invalid instrumental variables.

[24] Another shortcoming in the application, not the methodology, in Lechner and Melly (2010) is data

our set of assumptions, identification and use of principal stratification allow us to rely on asymmetrically trimmed distributions (Lee, 2009), unconditional on covariates,[25] to estimate QTE. Therefore, we fully relax assumptions of unconfoundedness, traditionally seen as an all or nothing assumptions (Imbens and Wooldridge, 2009). In what follows we formally introduce the proposed methodology to bound QTE.

Let $\alpha \in (0,1)$ denote the *α-quantile* of the distribution of individuals' wages that belong to the EE principal strata $F(Y_i|EE)$. Following the same intuition for identification of the sample population quantity $E[Y_i|\tau_i = 0, S_i = 1]$ and the trimmed means $E[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}]$ and $E[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}]$, which are comprised of individuals that belong to EE, we propose the construction of bounds for the local quantile treatment effect $LQTE^{\alpha}_{EE}$ as follow:

Proposition 1 *Under assumptions A and B, then* $LB^{\alpha}_{EE} \leq LQTE^{\alpha}_{EE} \leq UB^{\alpha}_{EE}$*; where*

$$\begin{aligned} UB^{\alpha}_{EE} &= F_{\alpha}[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}] \\ &\quad - F_{\alpha}[Y_i|\tau_i = 0, S_i = 1] \\ LB^{\alpha}_{EE} &= F_{\alpha}[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}] \\ &\quad - F_{\alpha}[Y_i|\tau_i = 0, S_i = 1] \end{aligned} \tag{15}$$

Where $F_{\alpha}[\cdot]$ is the *α-quantile* of $F[\cdot]$. Analogous to (10), $F[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}]$ and $F[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}]$ correspond to the upper and lower bounding distributions of infra-marginals, i.e., those individuals that belong to EE in the observed group $(\tau_i, S_i) = (1,1)$.[26] As such, UB^{α}_{EE} is an upper bound of the difference in

driven. Basically, they don't have data on wages, and thus, use results of an intuitive decomposition of earnings to conclude about the effect of training on human capital. Specifically they decompose $E[Y(1)-Y(0)] = E[Y(1)-Y(0)|S(1) = 1]Pr(S(1) = 1) + (E[Y(0)|S(0) \leq S(1)])Pr(S(0) \leq S(1))$, where Y represents earnings, the rest is consistent with our notation. The first term is the effect on human capital and the second the effect on employment.

[25] Lee (2009) uses baseline characteristics to tighten bounds. The idea is to split the sample into mutually exclusive groups based on observed covariates and perform the analysis separately in each group.

[26] Recall in the case of bounds derived in (10), $E[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}]$ and $E[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}]$ represented the upper and lower bounds for infra-marginal individuals' means.

quantiles between the treated and control groups' defined outcomes at a given α-*quantile*. Similarly, LB_{EE}^{α} represents a lower bound for this difference.

5.1 Estimation Using Basic Assumptions

The estimates of bounds in (15) are obtained as follow:

$$
\begin{aligned}
\widehat{UB_{EE}^{\alpha}} &= \widehat{y_{\alpha}^{u}} - \widehat{y_{\alpha}^{c}} \\
\widehat{LB_{EE}^{\alpha}} &= \widehat{y_{\alpha}^{l}} - \widehat{y_{\alpha}^{c}}
\end{aligned}
\tag{16}
$$

,

where the α-*quantiles* for both marginal distribution are calculated as:

$$\widehat{y_{\alpha}^{bd}} = min\{y : \frac{\Sigma_{i=1}^{n}\tau_i \cdot S_i \cdot 1[Y^{bd} \leq y]}{\Sigma_{i=1}^{n}\tau_i \cdot S_i} \geq \alpha\},$$

with $bd = u, l$ for the upper and lower bounding distribution, respectively; and Y^{bd} represents the distributions $F[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y_{1-(p_{1|0}/p_{1|1})}^{11}]$ for $bd = u$ and $F[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y_{(p_{1|0}/p_{1|1})}^{11}]$ for $bd = l$, in (14). Similarly, the α-*quantiles* for the observed control group with $(\tau_i, S_i) = (0, 1)$, are calculated as:

$$\widehat{y_{\alpha}^{c}} = min\{y : \frac{\Sigma_{i=1}^{n}(1-\tau_i) \cdot S_i \cdot 1[Y^{c} \leq y]}{\Sigma_{i=1}^{n}(1-\tau_i) \cdot S_i} \geq \alpha\},$$

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

5.2 Identification and Estimation Using Assumptions C and D

Analogous to Section 4.2, we seek to tighten bounds in (15) by employing Assumptions C and D. These assumptions impose direct restrictions on the distributions of infra-marginal individuals allowing for a more accurate identification. Since section 4.2 contains a detail exposition of how these restrictions work in our application (and in general), the focus here is on the proposition, estimation and results that can be derived based on our framework and the set of assumptions. In particular, we propose bounds on the QTE as in (15) and estimate the difference in quantiles of the distributions of infra-marginal individuals and observed control outcomes at a given α-*quantile*, where the distribution of infra-marginals' outcomes has been further restricted by Assumption C or D.[27] Formally we have:

Proposition 2 *Under assumptions A and B, and C, then* $LB_{EE}^{\alpha\ c} \leq LQTE_{EE}^{\alpha} \leq UB_{EE}^{\alpha}$*; where*

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

Estimation in (16) will be slightly modified for the estimate of the lower bound $\widehat{LB_{EE}^{\alpha\ c}}$, and remains the same for the upper bound estimate $\widehat{UB_{EE}^{\alpha}}$. Specifically,

$$\widehat{LB_{EE}^{\alpha\ c}} = max\{0, \widehat{LB_{EE}^{\alpha}}\} \tag{18}$$

We now consider the identifying power of assumption D, and formally propose:

Proposition 3 *Under assumptions A and B, and D, then* $LB_{EE}^{\alpha\ d} \leq LQTE_{EE}^{\alpha} \leq UB_{EE}^{\alpha}$*; where*

$$LB_{EE}^{\alpha\ d} = F_{\alpha}[Y_i|\tau_i = 1, S_i = 1] - F_{\alpha}[Y_i|\tau_i = 0, S_i = 1] \tag{19}$$

As before estimation of the upper bound is given by $\widehat{UB_{EE}^{\alpha}}$, and the estimate for $LB_{EE}^{\alpha\ d}$ is given by:

$$\widehat{LB_{EE}^{\alpha\ d}} = \widehat{y_{\alpha}^{l\ d}} - \widehat{y_{\alpha}^{c}}, \tag{20}$$

where $\widehat{y_{\alpha}^{l\ d}} = min\{y : \frac{\Sigma_{i=1}^{n} \tau_i \cdot S_i \cdot 1[Y^t \leq y]}{\Sigma_{i=1}^{n} \tau_i \cdot S_i} \geq \alpha\}$, and Y^t represents the untrimmed distribution $F[Y_i|\tau_i = 1, S_i = 1]$.

[27] We could also combine the assumptions to obtain better results, but in our particular application this combination does not produce tighter bounds than those using D alone with the basic assumptions. In other words lower bounds under Assumption D are always the binding maximum.

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

In this section we empirically assess the effect of Job Corps training on wages using data from the National Job Corps Study. This substantive empirical analysis starts by imputing the HM "worst-case" bounds. Results from this general bounding approach are considered a benchmark from which we proceed by imposing more structure as previously discussed. After reporting the HM bounds (Section 6.1), we report bounds derived under the PS framework and the basic assumptions (A and B) in Section 6.2, and assumptions C and D in Section 6.3. Sections 6.4 and 6.5 illustrate the identifying power of bounds in Proposition 1 and 2, respectively.

6.1 Horowitz and Manski (HM) "worst-case" bounds

Table 2 reports the HM "worst-case" scenario bounds for the treatment effect of JC on log wages in week 208 after randomization. Similarly to Lee (2009), the variable wage was transformed to minimize the effect of outliers on the width of these bounds. Specifically, wages were split into 20 percentile groups, according to the 5^{th}, 10^{th},..., and 95^{th} percentile of wages, and individuals belonging to a particular group were assigned with the mean wage for that group. In addition, we also report these bounds using original wages. Original wages enable us to: measure the effect of Lee's (2009) "smoothing" of wages, and more importantly, take advantage of the original variation in wages to further analyze bounds of treatment effect on quantiles of their distribution.

Column 3 in Table 2 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} in (5), was calculated to be 0.90. As expected, the "smoothing" of wages has a large impact on the support of the outcome. From the last column, the upper and lower bounds of the support of original log wages are 5.99 and -1.55, respectively. Consequently, HM bounds' width for original log wages (6.244) is considerably larger than that for the transformed (1.548). Detailed calculations of all quantities needed to construct bounds in (5) are shown in the second column of Table 2. Despite large differences, evidence presented in Table 2 has the same qualitative implication about the HM bounds using both sets of wages, e.g., transformed and untransformed. Based on the calculated upper and lower HM bounds using transformed log wages (0.802 and -0.746, respectively) and original log wages (3.135 and -3.109, respectively), one is driven to conclude that these intervals are not informative in the context of JC, as they are as consistent with positive as they are with negative values. Nevertheless, HM "worst-case" bounds provide a useful starting point for the construction of tighter bounds. An alternative framework to identify more informative bounds is discussed in the following section, and will be used for the remainder of this analysis.

6.2 Results under Assumptions A and B

Table 4 reports results after constructing bounds estimates in (11) for $LNATE_{EE}$. Population quantities needed for the construction of these bounds under the PS framework using Lee's transformed wages are in column 3. Therefore, these results replicate those by Lee (2009). For example, Lee's trimming proportion $p = 0.068$ is equivalent to 1 minus the proportion of EE in Equation (9), which is estimated as $(1-\hat{p})$ from (12), and corresponds to the quantile of Y_i with a $ln(wage) = 1.636$. Lee's trimmed mean $E[Y|y > y_p] = 2.090$ corresponds to the expected value of Y_i for the 1-$(p_{1|0}/p_{1|1})$ fraction of the largest values of Y_i for those in the observed treatment group, $E[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}]$, which is estimated as $\frac{\Sigma^n_{i=1} Y_i \cdot \tau_i \cdot S_i \cdot 1[Y_i \geq \widehat{y_{1-\hat{p}}}]}{\Sigma^n_{i=1} \tau_i \cdot S_i \cdot 1[Y_i \geq \widehat{y_{1-\hat{p}}}]}$ from (11). Finally, under both procedures, the estimated upper bound $\widehat{UB_{EE}} = 0.093$ is computed as the difference between 2.090 and 1.997, where the latter quantity corresponds to the observed control $E[Y_i|\tau_i = 0, S_i = 1]$ in (10), estimated as $\frac{\Sigma^n_{i=1} Y_i \cdot (1-\tau_i) \cdot S_i}{\Sigma^n_{i=1} (1-\tau_i) \cdot S_i}$. A symmetric procedure yields an estimated lower bound $\widehat{LB_{EE}}$=-0.019 for the $LNATE_{EE}$. The width of these bounds is 0.112.

Table 4 also reports estimated bounds for $LNATE_{EE}$ using original wages. Population quantities needed for the construction of these bounds are in the 4^{rd} column. Unlike the HM "worst-case" bounds from the previous section, the bounding procedure in (10) does not depend on a bounded support, and thus, the effect of Lee's smoothing of wages is negligible. The estimated upper and lower bounds, $\widehat{UB_{EE}} = 0.099$ and $\widehat{LB_{EE}} = -0.022$, respectively, are slightly greater in magnitude that the estimated bounds using transformed wages. Consequently, estimated bounds' width, 0.121, is only slightly larger. Despite minor differences, the implication of the estimated bounds in (12) using both transformed and original wages is the same. One may note that in comparison to the HM bounds (Table 2), which are consistent with both large negative and positive treatment effects, bounds reported in Table 4 are narrower, and hence, more informative about the size and sing of the effect, i.e., compared to $\widehat{UB_{EE}}$, $\widehat{LB_{EE}}$ is negative but closer to zero, thus, these bounds are more consistent with positive effects.[28]

In both cases, transformed and original wages, we provide bootstrap standard errors (in parenthesis) for the estimated bounds.[29] Our standard errors are numerically equivalent to those reported in Lee (2009) (0.013 and 0.018 for the upper and lower bound, respectively), which he derived from the asymptotic normality of his bounds. In particular, standard errors reported in Table 4 give a sense of the accuracy of the estimated bounds, more importantly, they can be used to construct confidence intervals.

6.3 Results Adding Assumptions C and D

Columns 3 and 6 in Table 5 (subheading C) report results after tightening bounds in (11), with Assumption C, for transformed and original wages, respectively. Implementation of weak monotonicity of mean potential outcomes within the EE strata results in tighter bounds for the $LNATE_{EE}$ when compared to bounds for the ATE in Lee (2009), the difference is in the order of 17 percent for both transformed and untransformed wages. To see this, notice that compared to the negative lower bounds estimated in columns 2 and 5 (subheading A and B), which only employ the basic assumptions, the $\widehat{LB^c_{EE}}$ for both transformed and original wages, are zero due to Assumption C. Therefore, the estimated bounds' width is reduced from 0.112 to 0.093 and from 0.121 to 0.099 for transformed and original wages, respectively. Given that these bounds are truncated at zero, bootstrap standard errors (in parenthesis) employ the formula for truncated (at zero) normal

[28]These bounds are also useful to assert assumptions required for conventional point identification (discussed in previous section). Notice, the difference in means estimator 0.034 (2.031 - 1.997) is consistent with the estimated bounds on $LNATE_{EE}$, which suggests the following statistical test: Reject point identification assumptions if the point identified effect 0.034 lies out of bounds ($\widehat{UB_{EE}}, \widehat{LB_{EE}}$) (Manski, 2003).

[29]All standard errors for bounds not involving maximum operators are obtained with 5,000 bootstrap replications.

distribution (Cai et al., 2008). Unfortunately, these standard errors can not be used to compute confidence intervals, unlike those standard errors reported under assumptions A and B (subheading A and B), and assumptions A, B and D (subheading D).

Columns 4 and 7 in Table 5 (subheading D) report results after tightening bounds in (11), with Assumption D, for transformed and original wages, respectively. In this case implementation of weak monotonicity of mean potential outcomes across the EE and the NE strata results in tighter bounds for the $LNATE_{EE}$ when compared to bounds for the ATE in Lee (2009). The difference is in the order of 47 to 49 percent for transformed and untransformed wages, respectively. Notice, bounds estimates with Assumption D are also tighter than those estimated under Assumption C. Differentials are calculated by comparing the effect of Assumption D on the estimated bounds' width. For example, focusing on original wages, under Assumption D the $\widehat{LB_{EE}}$ increases from -0.022, using basic assumptions A and B, to $\widehat{LB^{c}_{EE}}$=0.037, resulting in estimated bounds' width reduction from 0.121 to 0.062.

Importantly, employing Assumption D enable us to estimate bounds that are informative about the sign of the effect of Job Corps training on participant wages, suggesting a positive change bounded from 0.037 to 0.099 percent (original wages). In contrast, the estimated bounds using basic assumptions, as those in Lee (2009), are not informative. Furthermore, compared to Assumption C, Assumption D does not restrict the sign of the effect to be positive.

As mentioned in section 4.2.1, we perform indirect test to gauge the plausibility of Assumption D, using baseline sample information on hourly wages, weekly earnings, and weekly hours worked, which are hypothesized to be related to better labor market outcomes (e.g., post-treatment wages). According to Assumption D, one should expect better characteristics for the EE strata, follow by the NE, and lastly by the NN (i.e., a weakly monotonic rank across strata). Ranking mean characteristics among EE and NN is straightforward since it only requires comparing the means between the observed groups (τ_i, S_i)=(0, 1) and (1, 0), respectively. Baseline hourly wages for the EE and NN strata are, respectively (standard errors in parenthesis): 3.49 (0.08) and 2.56 (0.05). Since the difference is statistically significant, so far there is no evidence against Assumption D. Similarly to FF, the mean for the NE strata can be written as a function of the population mean, the means of the EE and NN stratum and the population proportions, formally: $E[BW_i|NE] = \{E[BW_i] - \pi_{EE}E[BW_i|EE] - \pi_{NN}E[BW_i|NN]\}/\pi_{NE}$, where BW_i represent baseline average hourly wages. After computing $E[BW_i|NE]$ with the corresponding sample quantities the resulting mean (standard error), 3.94 (1.49), is not statistically significant from the means for the EE and NN. This result further suggests that there is no statistical evidence against Assumption D. Similar conclusions can be reached when considering baseline weekly earnings and hours worked.[30]

Following the analysis in Lee (2009), we also examine bounds at different time horizons (weeks 135, 180 and 208) and provide further evidence about the positive impact of Job Corps on participants' wages. In addition, we provide more evidence about the tightening power of Assumptions C and D. Figure 1 contains 6 different graphs depicting the weekly evolution of bounds on the treatment effect of interest. We contrasts results of estimated bounds on $LNATE_{EE}$ by transformed and original wages, graphs on the left and on the right, respectively; and by assumption, where basic assumptions A and B are depicted in the upper pair of graphs, Assumption C in the middle graphs, and Assumption D in the bottom graphs. Given that in both sets of wages (transformed and original) the estimated results are similar, our focus for this visual analysis is on original wages (graphs on the right).

Lower bounds using basic assumptions A and B are negative for weeks 135, 180 and 208, ranging from -0.033 to -0.007, the estimated values in weeks 180 and 135, respectively. These results are quantitatively similar to the results reported in Lee (2009) (which are reported in the upper right graph). As noted by Lee (2009), these bounds are more consistent with positive effects, upper bounds range from 0.084 to 0.099, the respective values for weeks 135 and 208. Implementation of Assumption C results in tighter bounds, which are depicted in the middle graphs. Note that Assumption C restricts the lower bounds, in all weeks analyzed, to be equal to zero. As previously discussed upper bounds remain unchanged across assumptions. Finally, after implementing Assumption D, bounds become fully informative about the sign of the effect of Job Corps on wages, for all weeks

[30]Baseline weekly earnings are ranked as follows: for the EE and NN strata 119.46 (2.63) and 89.19 (1.91), respectively, and 198.97 (77.12) for the NE stratum. Weekly hours worked at the baseline are ranked 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.

considered the lowest lower bound observed is 0.030, which corresponds to the effect on week 180. This last result indicates that the impact of Job Corps on wages is no less than 3.0 percent in the course of weeks considered.

6.4 Quantile Treatment Effect Results under Proposition 1

Table 6 reports the estimated bounds in (16) for the $LQTE^{\alpha}_{EE}$ using Lee's (2009) transformed wages. The *α-quantiles* studied correspond to the 20 percentiles in column 1, i.e., α=(0.05, 0.10,...,0.90, 0.95). Column 2 contains the respective *α-percentiles* of the distribution $F[Y_i|\tau_i = 0, S_i = 1]$ of wages for employed individuals in the control group. Columns 3 and 4 report *α-percentiles* of the distributions $F[Y_i|\tau_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}]$ and $F[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}]$, respectively. Notice that, in columns 2, 3 and 4, wages, as one may expect, are strictly increasing with α.[31] The last 3 columns report, respectively, the upper and lower bounds estimates for the $LQTE^{\alpha}_{EE}$, and the width of these bounds, computed as the difference $\widehat{UB^{\alpha}_{EE}} - \widehat{LB^{\alpha}_{EE}}$. Results are documented in the following paragraph.

The highest upper bound of 0.203 is observed at the 0.05 percentile; subsequent percentiles' upper bounds remain positive with values between 0.049 and 0.111, at the 0.60 and 0.65 percentiles, respectively. The upper bound becomes zero at the 0.80 percentile, spikes up to 0.127 at the subsequent percentile and returns to zero in percentiles 0.90 and 0.95. At the median the upper bound is 0.098, which is close to the upper bound for the mean ($\widehat{UB_{EE}}$=0.093) reported in Table 4. Also in percentile 0.05, the lower bound reaches a maximum of 0.105. The effect on the 0.95 percentile is the only one consistent with negative effects of Job Corps participation on wages, the lower bound is -0.365. With a lower bound of 0.041, median bounds are informative about the sign of the effect of JC participation on wages, which is not the case for the mean reported in Table 4 ($\widehat{LB_{EE}}$=-0.019). Interestingly, lower bounds at several percentiles were zero; this result can be attributed to Lee's (2009) smoothing of wages. Recall, these transformed

[31] Chernozhukov and Hansen (2005), who proposed a model for instrumental quantile treatment effects estimation, assume that the potential outcomes are strictly increasing in *α-quantiles*. We have yet to test the implications of such assumption in our in our model, i.e., monotonicity in the local quantile treatment response function.

wages are the mean value within the percentile range of wages they belong to (e.g. wages between the 0.05 and 0.10 percentiles are equal to the mean value of original wages between those percentiles), and thus, values of wages from the "worst-case" scenario trimmed distribution $F[Y_i|\tau_i = 1, S_i = 1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}]$ and wages for the observed control distribution $F[Y_i|\tau_i = 0, S_i = 1]$ are likely to overlap when computing $LQTE^{\alpha}_{EE}$. A better assessment of the effect of Lee's smoothing is conducted after contrasting these results with those using original wages (Table 7).

Table 7 also reports the estimated bounds in (16) for the $LQTE^{\alpha}_{EE}$, but using original wages. Notice that Lee's (2009) smoothing of wages has large impacts on the lower bound estimates of the $LQTE^{\alpha}_{EE}$, this can be seen by comparing column 6 in Tables 6 and 7. Specifically, lower bounds with zero effect of JC participation are not predominant when using original wages, around 60 percent reduction of zero effect lower bounds relative to those reported using transformed wages. Using original wages is advantageous in the sense that it allow us to exploit the original variation on wages, thus resulting in a more credible analysis of QTE. In what follows the analysis is performed using original wages.

Figure 2 contains a graph depicting the upper and lower bounds, based on results reported in Table 7. After taking advantage of the original variation in wages, we note that for higher quantiles of the distribution (higher than 0.75 percentile) the effect of JC on wages may be negative, the estimated lower bounds for the $LQTE^{\alpha}_{EE}$ range from -0.003 to -0.200 for percentiles 0.85 and 0.95. This 20 percent reduction in wages due to JC participation at the 0.95 percentile is about 9 times more negative than the lower bound for the mean effect reported in Table 4. In contrast, for quantiles below the median, bounds for the treatment effect of interest are positive. More importantly, for the lowest quantiles studied, 0.05, 0.10, 0.15 percentiles, the upper bounds for the effect of JC on wages are larger relative to the rest of the distribution, these values are 0.161, 0.102, and 0.087, respectively. At the median, bounds ($\widehat{UB^{0.5}_{EE}} = 0.081$ and $\widehat{LB^{0.5}_{EE}} = 0.030$) are tighter and informative, i.e., consistent with positive effects of JC on wages, relative to bounds for the mean effect reported in Table 4 ($\widehat{LB_{EE}} = -0.022$ and $\widehat{UB_{EE}} = 0.099$).

We would like to highlight the importance of these empirical results as they provide the basis for policy implication discussion in the last section. In summary, according to the derived bounds for the $LQTE^{\alpha}_{EE}$, the effect of JC on participants' wages' distribution is heterogeneous, this is evident given that, in general, bounds contain positive and larger effects at percentiles lower than the median than those higher, which may contain negative effects as they approach the endpoint of the distribution.

6.5 Quantile Treatment Effect Results under Proposition 2

Estimated bounds for the $LQTE^{\alpha}_{EE}$ are reported in Table 8. Consistent with previous results (section 4.2.1), the restrictive nature of Assumption C (heading Monotonicity within strata) affects the the lower bounds (subheading LB) on the last five quantiles analyzed, note that these lower bounds under the basic assumptions A and B in column 3 are negative. Results for the remainder quantiles (from 0.05 to 0.70 percentiles) do not change compared to those calculated assuming A and B (Basic A and B), and thus, we can draw the same conclusions as before (those for Table 7).

Estimated bounds for the $LQTE^{\alpha}_{EE}$ employing weak monotonicity across strata (D plus the basic assumptions A and B) are reported under columns 8 and 9 in Table 8. Consistent with previous results (also in section 4.2.1), compared to bounds estimated using Assumption A, B and C, bounds estimates employing A, B and D are tighter and more informative in every percentile analyzed. These estimated results can be seen clearer in the graphical analysis in Figure 3, where Assumption D yields tighter bounds (UB and LB-D for upper and lower, respectively), followed by C (UB and LB-C) and lastly by Basic Assumptions (UB and LB).

Importantly, we find that the program's impact on lower quantiles of the distribution is higher, with the highest impact being in the 0.05 percentile, where the positive effect on wages is bounded between 0.084 and 0.161. At the median, the effect is bounded between 0.042 and 0.081, which is similar but tighter than the corresponding results for the mean. Furthermore, the effects at other conditional quantiles of the distribution of wages do not exceed bounds between 0.022 and 0.067. These results are encouraging with regard to the effectiveness of Job Corps on participant's wages, and provide new insights about the policy-relevant question of whether Job Corps has a higher impact on the more disadvantaged participants, i.e., those individuals in the lower tail of the wage distribution.

7 Concluding Remarks and Implications

In this paper, we empirically assess the effect of training on wages using data from the National Job Corps Study, a randomized evaluation of the U.S. Job Corps, the nation's largest and most important job training program targeting disadvantaged youths. In accomplishing this objective we make two important contributions. The first one, is a substantive empirical analysis of the effect of the Job Corps training program on participants' wages. Results derived in our empirical application provide evidence to answer a policy relevant question about the impact of Job Corps on more disadvantage participants, and hence its effectiveness. With legislation seeking to cut federal expending, positive evidence is particularly important for this federally funded program. Importantly, data to derive our results come from the first nationally representative experimental evaluation of an active labor market program (Schochet et al., 2008), and thus implications can be generalized, with confidence, to Job Corps at a national level.

The second contribution, methodological in nature, is that we extend recent partial identification results of 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). This strategy allows constructing informative nonparametric bounds for the causal effect of interest under weaker assumptions than those conventionally used for point identification of treatment effects in the presence of sample selection. In addition to providing bounds on average effects, we propose bounds on quantile treatment effects of the program on participants' wages. Importantly, these bounds allow analyzing the heterogeneity of this effect on different points of the participants' post-training wage distribution, a feature that is not capture when analyzing mean impacts (Bitler et al., 2006).

When only considering mean impacts, our bounds are tighter and more informative about the sign of the effect of training on wages relative to those in Lee (2009). Our results indicate that the Job Corps program has a positive average effect on participants' wages measured 208 weeks after random assignment that is bounded between 3.4 and 9.3 percent. Similarly to Lee (2009), we conclude that the program can be view as a human capital investment given that these bounds are roughly consistent with point estimates reported in the literature of returns to schooling (Card, 1999).

The proposed quantile model allows characterizing the impact heterogeneity of Job Corps training on different points of the participants' wage distribution. Impacts on lower quantiles of the distribution of wages are higher, with the highest impact being in the 5^{th} percentile where a positive effect on wages is bounded between 8.4 and 16.1 percent. At the median, the effect is bounded between 4.2 and 8.1percent, which is similar but tighter than the corresponding results for the mean. Furthermore, the effects at other conditional quantiles of the distribution of wages do not exceed bounds between 2.2 and 6.7 percent.

These results are encouraging with regard to the effectiveness of Job Corps on participant's wages, and provide new insights about the policy-relevant question of whether Job Corps has a higher impact on the more disadvantaged participants, i.e., those individuals in the lower tail of the wage distribution. In other words, it is now evident that the effect of Job Corps is twofold; first, it has a positive impact across the studied distribution of wages (20 percentiles), second, the program has an effect of wage compression within disadvantage groups. To our knowledge, the latter effect has not been previously identified, and thus it sheds light on the effectiveness of Job Corps at a new, important level.

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Table 1. Summary statistics, by treatment status, NJCS.

Row #	Variable	Control: Proportion nonmissing	Control: Mean	Control: S.D.	Program: Proportion nonmissing	Program: Mean	Program: S.D.	Difference: Difference	Difference: S.E.
1	Female	1.00	0.458	0.498	1.00	0.452	0.498	-0.006	0.010
2	Age	1.00	18.351	2.101	0.98	18.436	2.159	0.085	0.045
3	White	1.00	0.263	0.440	1.00	0.266	0.442	0.002	0.009
4	Black	1.00	0.491	0.500	1.00	0.493	0.500	0.003	0.010
5	Hispanic	1.00	0.172	0.377	1.00	0.169	0.375	-0.003	0.008
6	Other race	1.00	0.074	0.262	1.00	0.072	0.258	-0.002	0.005
7	Never married	0.98	0.916	0.278	0.98	0.917	0.275	0.002	0.006
8	Married	0.98	0.023	0.150	0.98	0.020	0.139	-0.003	0.003
9	Living together	0.98	0.040	0.197	0.98	0.039	0.193	-0.002	0.004
10	Separated	0.98	0.021	0.144	0.98	0.024	0.154	0.003	0.003
11	Has a child	0.99	0.193	0.395	0.98	0.189	0.392	-0.004	0.008
12	# of child	0.99	0.268	0.640	0.98	0.270	0.650	0.002	0.014
13	Education	0.98	10.105	1.540	0.98	10.114	1.562	0.009	0.033
14	Mother's ed.	0.81	11.461	2.589	0.82	11.483	2.562	0.022	0.060
15	Father's ed.	0.61	11.540	2.789	0.62	11.394	2.853	-0.146	0.075
16	Ever arrested	0.98	0.249	0.432	0.98	0.249	0.432	-0.001	0.009
	household income:								
17	<3,000	0.65	0.251	0.434	0.63	0.253	0.435	0.002	0.011
18	3,000 - 6,000	0.65	0.208	0.406	0.63	0.206	0.405	-0.002	0.011
19	6,000 - 9,000	0.65	0.114	0.317	0.63	0.117	0.321	0.003	0.008
20	9,000 - 18,000	0.65	0.245	0.430	0.63	0.245	0.430	0.000	0.011
21	>18,000	0.65	0.182	0.386	0.63	0.179	0.383	-0.003	0.010
	Personal income								
22	<3,000	0.92	0.789	0.408	0.92	0.789	0.408	-0.001	0.009
23	3,000 - 6,000	0.92	0.131	0.337	0.92	0.127	0.334	-0.003	0.007
24	6,000 - 9,000	0.92	0.046	0.209	0.92	0.053	0.223	0.007	0.005
25	>9,000	0.92	0.034	0.181	0.92	0.031	0.174	-0.003	0.004
	At baseline								
26	Have a job	0.98	0.192	0.394	0.98	0.198	0.398	0.006	0.008
27	Months employed	1.00	3.530	4.238	0.60	3.596	4.249	0.066	0.089
28	Had a job	0.98	0.627	0.484	0.98	0.635	0.482	0.007	0.010
29	Earnings	0.93	2810.482	4435.616	0.94	2906.453	6401.328	95.971	118.631
30	Usual hrs/week	1.00	20.908	20.704	0.61	21.816	21.046	0.908	0.437
31	Usual weekly earnings	1.00	102.894	116.465	0.97	110.993	350.613	8.099	5.423
	After random assignment								
32	Week 52 weekly hrs.	1.00	17.784	23.392	1.00	15.297	22.680	-2.487	0.482
33	Week 104 weekly hrs.	1.00	21.977	26.080	1.00	22.645	26.252	0.668	0.547
34	Week 156 weekly hrs.	1.00	23.881	26.151	1.00	25.879	26.574	1.997	0.551
35	Week 208 weekly hrs.	1.00	25.833	26.250	1.00	27.786	25.745	1.953	0.544
36	Week 52 weekly earnings	1.00	103.801	159.893	1.00	91.552	149.282	-12.249	3.238
37	Week 104 weekly earnings	1.00	150.407	210.241	1.00	157.423	200.266	7.015	4.297
38	Week 156 weekly earnings	1.00	180.875	224.426	1.00	203.714	239.802	22.839	4.855
39	Week 208 weekly earnings	1.00	200.500	230.661	1.00	227.912	250.222	27.412	5.127
	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.
All proportions of nonmissing, estimated mean values, and standard deviations (S.D.) for pre and post-treatment variables were the same as those reported in Lee (2009).

Table 2. Bounds on treatment effects for week 208 ln(wage) using bounds of support (Horowitz and Manski, 2000).

	Quantity in eq. (5)	Transformed wages	Original wages
Bounds on Support of wages			
5^{th} percentile mean wage		2.46	4.77
95^{th} percentile mean wage		15.96	14.00
Y^{LB}	Y^{LB}	0.90	-1.55
Y^{UB}	Y^{UB}	2.77	5.99
Control group			
Observations		3599	3599
(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			
Observations		5546	5546
(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 3. Observed groups based on treatment and employment indicators (τ_i, S_i) and PS mix within groups.

Groups by observed (τ_i, S_i)	PS	PS (individual monotonicity)
(0,0)	*NN* and *NE*	*NN* and *NE*
(1,1)	*EE* and *NE*	*EE* and *NE*
(1,0)	*NN* and *EN*	*NN*
(0,1)	*EE* and *EN*	*EE*

Notes: PS stands for principal strata.

Table 4. Bounds on treatment effects for ln(wage) in week 208 using principal stratification (PS).

	PS framework	Transformed wages	Original wages
Control group			
Number of observations		3599	3599
(ii)Proportion of nonmissing	$p_{1\|0} = Pr(S_i = 1\| \tau_i = 0)$	0.566	0.566
(iii)Mean ln(wage) for employed	$E[Y_i \mid \tau_i=0, S_i=1]$	1.997	1.991
Treatment group			
Number of observations		5546	5546
(v)Proportion of nonmissing	$p_{1\|1} = Pr(S_i = 1\| \tau_i = 1)$	0.607	0.607
Mean ln(wage) for employed	$E[Y_i \mid \tau_i=1, S_i=1]$	2.031	2.028
p= [(v)-(ii)]/(v)	$1 - p_{1\|0} / p_{1\|1}$	0.068	0.068
p^{th} quantile	$y^{11}_{1-(p_{1\|0}/p_{1\|1})}$	1.636	1.639
(ix)Trimmed mean: $E[Y\|y>y_p]$	$E[Y_i \mid \tau_i=1, S_i=1, Y_i \geq y^{11}_{1-(p_{1\|0}/p_{1\|1})}]$	2.090	2.090
$(1-p)^{th}$ quantile	$y^{11}_{(p_{1\|0}/p_{1\|1})}$	2.768	2.565
(xi)Trimmed mean: $E[Y\|y<y_{1-p}]$	$E[Y_i \mid \tau_i=1, S_i=1, Y_i \leq y^{11}_{(p_{1\|0}/p_{1\|1})}]$	1.978	1.969
Effect			
Upper bound	UB_{EE} = (ix)-(iii)	0.093	0.099
		(0.014)	(0.014)
Lower bound	LB_{EE} = (xi)-(iii)	-0.019	-0.022
		(0.018)	(0.016)
Width	$UB_{EE} - LB_{EE}$	0.112	0.121

Notes: PS stands for principal stratification methodology, as used in Flores and Flores-Lagunes (2010). In parenthesis are standard errors computed as described in footnote 31.

Table 5. Bounds on treatment effects for ln(wage) in week 208 using principal stratification (PS) and assumptions A and B, C, and D.

	Transformed wages			Original wages		
Assumption:	A and B	C	D	A and B	C	D
Control group						
Number of observations	3599	3599	3599	3599	3599	3599
(ii)Proportion of nonmissing	0.566	0.566	0.566	0.566	0.566	0.566
Mean ln(wage) for employed	1.997	1.997	1.997	1.991	1.991	1.991
Treatment group						
Number of observations	5546	5546	5546	5546	5546	5546
(v)Proportion of nonmissing	0.607	0.607	0.607	0.607	0.607	0.607
Mean ln(wage) for employed	2.031	2.031	2.031	2.028	2.028	2.028
p= [(v)-(ii)]/(v)	0.068	0.068	0.068	0.068	0.068	0.068
pth quantile	1.636	1.636	1.636	1.639	1.639	1.639
Trimmed mean: $E[Y\|y>y_p]$	2.090	2.090	2.090	2.090	2.090	2.090
1-p quantile	2.768	2.768	2.768	2.565	2.565	2.565
Trimmed mean: $E[Y\|y<y_{1-p}]$	1.978	1.978	1.978	1.969	1.969	1.969
Effect						
Upper bound	0.093	0.093	0.093	0.099	0.099	0.099
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Lower bound	-0.019	0.000	0.034	-0.022	0.000	0.037
	(0.018)	(0.011)	(0.011)	(0.016)	(0.012)	(0.012)
Width	0.112	0.093	0.059	0.121	0.099	0.062

Notes: Assumption A and B for randomized treatment and individual level monotonicity. Assumptions C and D correspond to monotonicity within and across strata, respectively. In parenthesis are standard errors, (described in footnote 31). Standard errors' computations under Assumption C follow Cai et al. (2008).

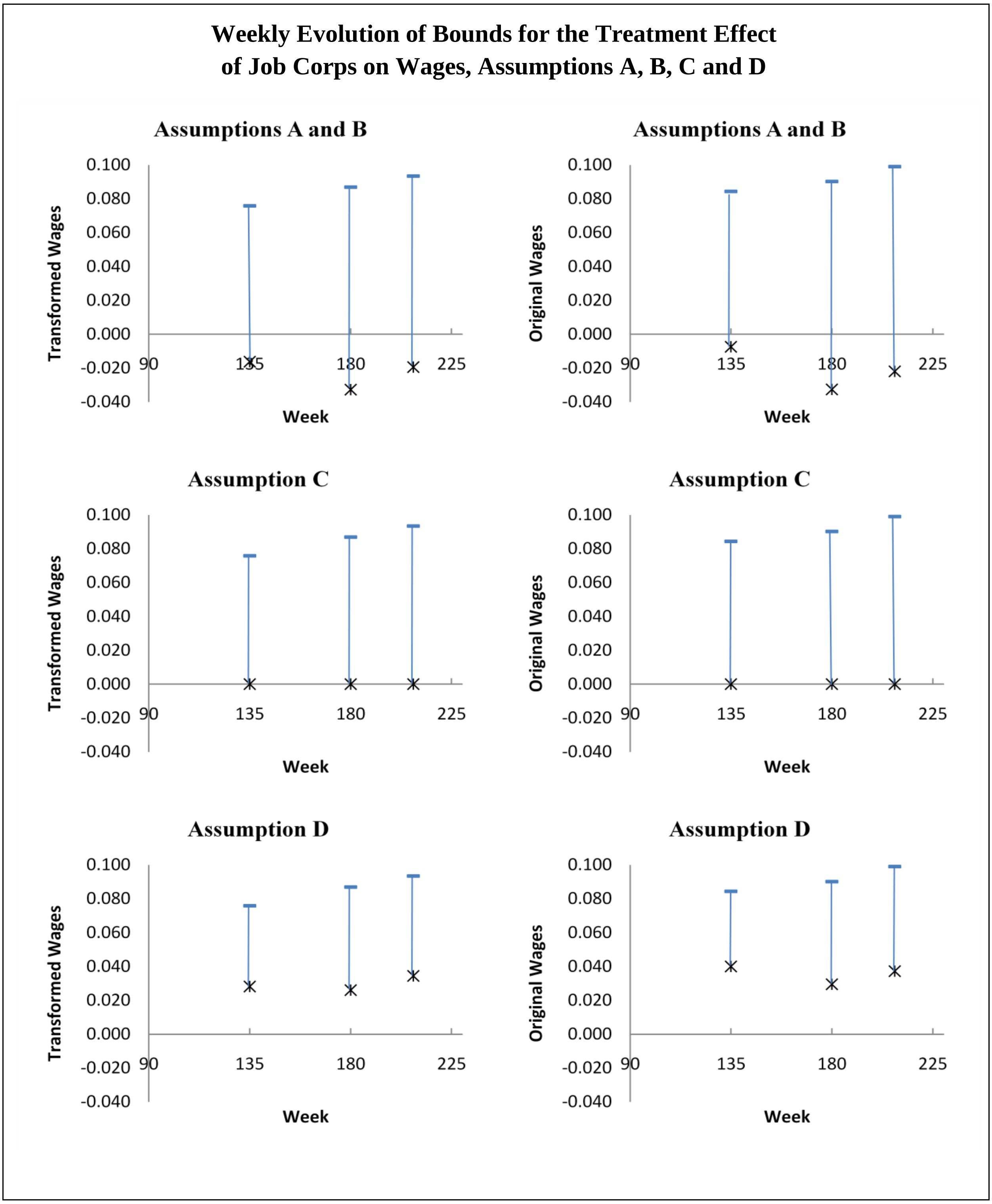

Figure 1. Bounds of Job Corps Effects by Week after Random Assignment.

Notes: *Y* axis is the effect on log wages.

Assumption A and B for randomized treatment and individual level monotonicity. Assumptions C and D correspond to monotonicity within and across strata, respectively.

Table 6. Bounds on quantiles of the distribution of transformed ln(wages) in week 208, using PS framework and Assumptions A and B.

α-percentile	α-control	α-$F[Y\|y>y_{1-p}]$	α-$F[Y\|y<y_p]$	Upper bound	Lower bound	Width
0.05	1.499	1.701	1.604	0.203 (0.062)	0.105 (0.077)	0.097
0.10	1.660	1.740	1.660	0.080 (0.026)	0.000 (0.014)	0.080
0.15	1.701	1.789	1.740	0.087 (0.014)	0.039 (0.021)	0.049
0.20	1.740	1.824	1.789	0.085 (0.029)	0.049 (0.028)	0.036
0.25	1.789	1.866	1.789	0.077 (0.014)	0.000 (0.015)	0.077
0.30	1.824	1.900	1.824	0.075 (0.023)	0.000 (0.023)	0.075
0.35	1.866	1.942	1.866	0.076 (0.007)	0.000 (0.013)	0.076
0.40	1.900	1.975	1.942	0.075 (0.027)	0.043 (0.026)	0.033
0.45	1.942	2.016	1.942	0.074 (0.007)	0.000 (0.013)	0.074
0.50	1.975	2.073	2.016	0.098 (0.026)	0.041 (0.023)	0.057
0.55	2.016	2.073	2.016	0.057 (0.013)	0.000 (0.023)	0.057
0.60	2.073	2.122	2.073	0.049 (0.011)	0.000 (0.011)	0.049
0.65	2.073	2.184	2.122	0.111 (0.038)	0.049 (0.032)	0.062
0.70	2.122	2.184	2.122	0.062 (0.023)	0.000 (0.026)	0.062
0.75	2.184	2.277	2.184	0.092 (0.004)	0.000 (0.014)	0.092
0.80	2.277	2.277	2.277	0.000 (0.015)	0.000 (0.046)	0.000
0.85	2.277	2.403	2.277	0.127 (0.060)	0.000 (0.061)	0.127
0.90	2.403	2.403	2.403	0.000 (0.116)	0.000 (0.061)	0.000
0.95	2.768	2.768	2.403	0.000 (0.000)	-0.365 (0.087)	0.365

Notes: α-*control* corresponds to the *α-percentile* of $F[Y_i \mid \tau_i=0, S_i=1]$.
α-$F[Y|y>y_{1-p}]$ and α-$F[Y|y<y_p]$ correspond to the α-percentile of the distributions $F[Y_i \mid \tau_i=1, S_i=1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}]$ and $F[Y_i \mid \tau_i=1, S_i=1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}]$, respectively.
The upper bound UB^{α}_{EE}= α-$F[Y|y>y_{1-p}]$ - α-*control*. The lower bound LB^{α}_{EE}= α-$F[Y|y<y_p]$ - α-*control.*
The width in the last column is computed as UB^{α}_{EE}- LB^{α}_{EE}.
In parenthesis are bootstrap standard errors, computed as described in footnote 31.

Table 7. Bounds on quantiles of the distribution of original ln(wages) in week 208, using PS framework and Assumptions A and B.

α-percentile	α-control	α-$F[Y\|y>y_{1-p}]$	α-$F[Y\|y<y_p]$	Upper bound	Lower bound	Width
0.05	1.526	1.686	1.594	0.161 (0.055)	0.068 (0.062)	0.092477
0.10	1.648	1.749	1.658	0.102 (0.024)	0.011 (0.011)	0.090972
0.15	1.705	1.792	1.706	0.087 (0.010)	0.001 (0.013)	0.085782
0.20	1.749	1.820	1.792	0.071 (0.022)	0.043 (0.020)	0.028330
0.25	1.792	1.872	1.792	0.080 (0.014)	0.000 (0.007)	0.080043
0.30	1.833	1.902	1.833	0.070 (0.022)	0.000 (0.013)	0.069526
0.35	1.872	1.946	1.872	0.074 (0.007)	0.000 (0.008)	0.074108
0.40	1.910	1.974	1.940	0.065 (0.022)	0.030 (0.020)	0.034106
0.45	1.946	2.015	1.946	0.069 (0.006)	0.000 (0.008)	0.068993
0.50	1.973	2.054	2.002	0.081 (0.022)	0.030 (0.019)	0.051643
0.55	2.015	2.079	2.019	0.065 (0.007)	0.005 (0.016)	0.059961
0.60	2.062	2.110	2.079	0.048 (0.019)	0.018 (0.016)	0.030772
0.65	2.079	2.141	2.092	0.062 (0.013)	0.012 (0.017)	0.049378
0.70	2.140	2.197	2.140	0.057 (0.010)	0.000 (0.012)	0.057159
0.75	2.197	2.251	2.175	0.054 (0.012)	-0.023 (0.019)	0.076770
0.80	2.251	2.303	2.225	0.051 (0.018)	-0.027 (0.029)	0.077962
0.85	2.303	2.369	2.300	0.067 (0.021)	-0.003 (0.022)	0.069914
0.90	2.439	2.485	2.327	0.046 (0.030)	-0.111 (0.040)	0.157629
0.95	2.608	2.669	2.408	0.061 (0.036)	-0.200 (0.051)	0.260884

Notes: Same notes as in Table 6 apply to Table 7. The difference is that the outcome, Y_i, corresponds to original ln(wages) rather than transformed ln(wages). In parenthesis are bootstrap standard errors, computed as described in footnote 31.

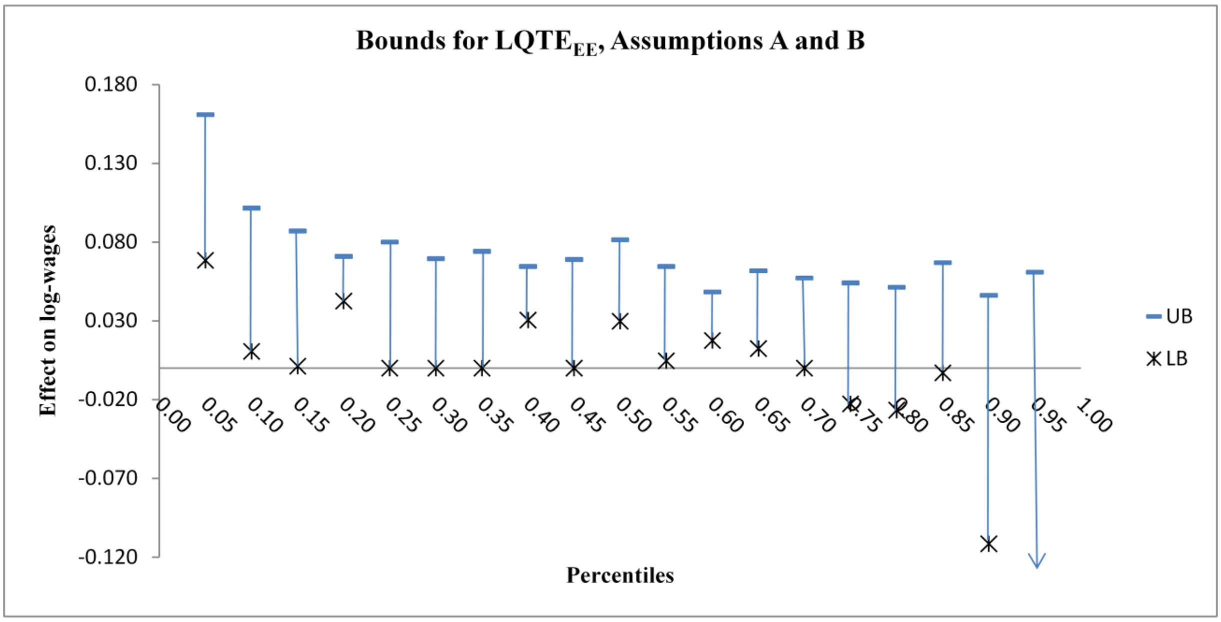

Figure 2. Bounds on quantiles of the distribution of original ln(wages)in week 208, using PS framework and assumptions A and B.

Table 8. Bounds on quantiles of the distribution of original ln(wages) in week 208, using PS framework and assumptions A and B, C, and D.

	Basic A and B			Monotonicity within strata			Monotonicity across strata		
α-percentile	UB	LB	Width	UB	LB	Width	UB	LB	Width
0.05	0.161	0.068	0.092	0.161	0.068	0.092	0.161	0.084	0.077
	(0.055)	(0.062)		(0.055)	(0.044)		(0.055)	(0.054)	
0.10	0.102	0.011	0.091	0.102	0.011	0.091	0.102	0.011	0.091
	(0.024)	(0.011)		(0.024)	(0.007)		(0.024)	(0.012)	
0.15	0.087	0.001	0.086	0.087	0.001	0.086	0.087	0.027	0.060
	(0.010)	(0.013)		(0.010)	(0.008)		(0.010)	(0.013)	
0.20	0.071	0.043	0.028	0.071	0.043	0.028	0.071	0.043	0.028
	(0.022)	(0.020)		(0.022)	(0.016)		(0.022)	(0.015)	
0.25	0.08	0.000	0.080	0.08	0.000	0.080	0.08	0.025	0.055
	(0.014)	(0.007)		(0.014)	(0.044)		(0.014)	(0.012)	
0.30	0.07	0.000	0.070	0.07	0.000	0.070	0.07	0.039	0.030
	(0.022)	(0.013)		(0.022)	(0.007)		(0.022)	(0.013)	
0.35	0.074	0.000	0.074	0.074	0.000	0.074	0.074	0.035	0.039
	(0.007)	(0.008)		(0.007)	(0.005)		(0.007)	(0.013)	
0.40	0.065	0.030	0.034	0.065	0.03	0.034	0.065	0.036	0.028
	(0.022)	(0.020)		(0.022)	(0.016)		(0.022)	(0.014)	
0.45	0.069	0.000	0.069	0.069	0.000	0.069	0.069	0.035	0.034
	(0.006)	(0.008)		(0.006)	(0.005)		(0.006)	(0.011)	
0.50	0.081	0.030	0.052	0.081	0.029	0.053	0.081	0.042	0.039
	(0.022)	(0.019)		(0.022)	(0.016)		(0.022)	(0.014)	
0.55	0.065	0.005	0.060	0.065	0.005	0.060	0.065	0.065	0.000
	(0.007)	(0.016)		(0.007)	(0.010)		(0.007)	(0.010)	
0.60	0.048	0.018	0.031	0.048	0.018	0.031	0.048	0.022	0.026
	(0.019)	(0.016)		(0.019)	(0.012)		(0.019)	(0.017)	
0.65	0.062	0.012	0.049	0.062	0.012	0.049	0.062	0.061	0.001
	(0.013)	(0.017)		(0.013)	(0.011)		(0.013)	(0.009)	
0.70	0.057	0.000	0.057	0.057	0.000	0.057	0.057	0.035	0.022
	(0.010)	(0.012)		(0.010)	(0.004)		(0.010)	(0.014)	
0.75	0.054	-0.023	0.077	0.054	0.000	0.054	0.054	0.038	0.016
	(0.012)	(0.019)		(0.012)	(0.003)		(0.012)	(0.015)	
0.80	0.051	-0.027	0.078	0.051	0.000	0.051	0.051	0.051	0.000
	(0.018)	(0.029)		(0.018)	(0.005)		(0.018)	(0.019)	
0.85	0.067	-0.003	0.07	0.067	0.000	0.067	0.067	0.049	0.018
	(0.021)	(0.022)		(0.021)	(0.001)		(0.021)	(0.018)	
0.90	0.046	-0.111	0.158	0.046	0.000	0.046	0.046	0.03	0.016
	(0.030)	(0.040)		(0.030)	(0.001)		(0.030)	(0.033)	
0.95	0.061	-0.200	0.261	0.061	0.000	0.061	0.061	0.031	0.030
	(0.036)	(0.051)		(0.036)	(0.001)		(0.036)	(0.034)	

Notes: In parenthesis are standard errors, computed as described in footnote 31. Standard errors' computations under Monotonicity within strata (Assumption C) follow Cai et al. (2008).

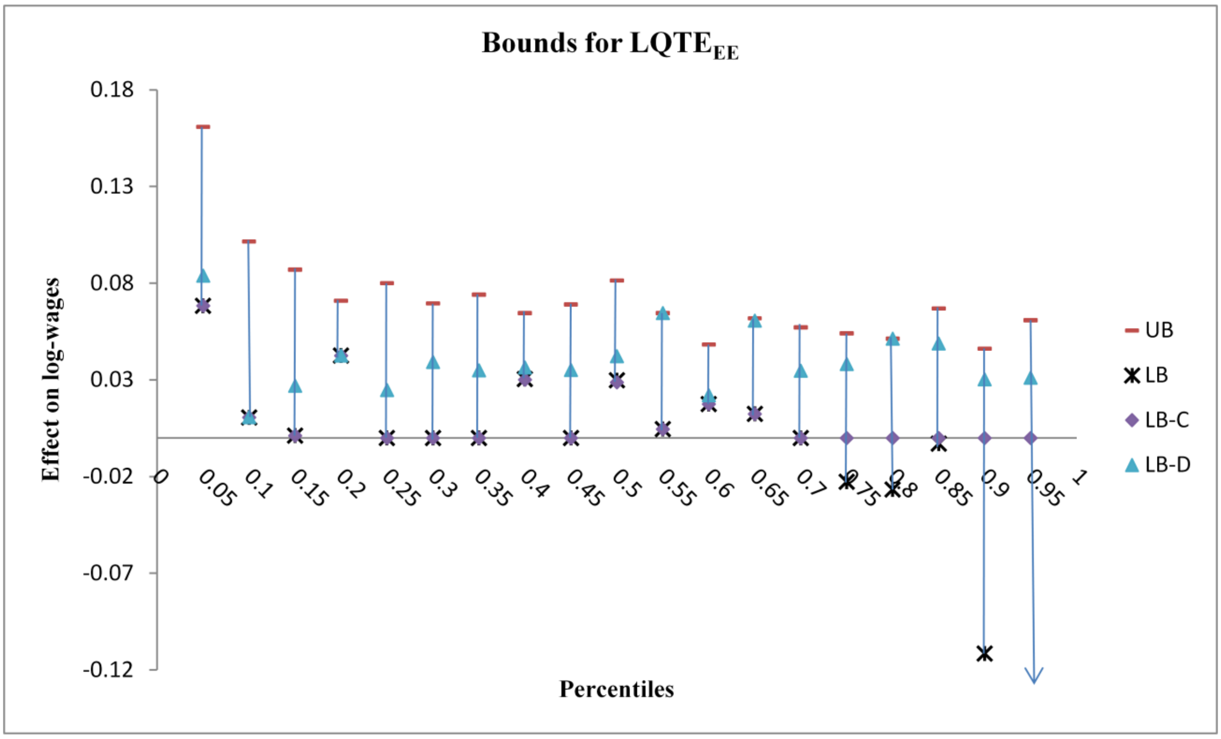

Figure 3. Bounds on quantiles of the distribution of original ln(wages)in week 208, using PS framework under assumptions A and B, C, and D.