Bounds on Treatment Effects in the Presence of Sample Selection and Noncompliance: The Wage Effects of Job Corps

Xuan CHEN
School of Labor and Human Resources, Beijing, Renmin University of China (xchen11@ruc.edu.cn)

Carlos A. FLORES
Department of Economics, California Polytechnic State University at San Luis Obispo, San Luis Obispo, CA 93407 (cflore32@calpoly.edu)

Randomized and natural experiments are commonly used in economics and other social science fields to estimate the effect of programs and interventions. Even when employing experimental data, assessing the impact of a treatment is often complicated by the presence of sample selection (outcomes are only observed for a selected group) and noncompliance (some treatment group individuals do not receive the treatment while some control individuals do). We address both of these identification problems simultaneously and derive nonparametric bounds for average treatment effects within a principal stratification framework. We employ these bounds to empirically assess the wage effects of Job Corps (JC), the most comprehensive and largest federally funded job training program for disadvantaged youth in the United States. Our results strongly suggest positive average effects of JC on wages for individuals who comply with their treatment assignment and would be employed whether or not they enrolled in JC (the “always-employed compliers”). Under relatively weak monotonicity and mean dominance assumptions, we find that this average effect is between 5.7% and 13.9% 4 years after randomization, and between 7.7% and 17.5% for non-Hispanics. Our results are consistent with larger effects of JC on wages than those found without adjusting for noncompliance.

KEY WORDS: Instrumental variables; Nonparametric partial identification; Principal stratification; Program evaluation; Training programs.

1. INTRODUCTION

Randomized and natural experiments are commonly used in economics and other fields to estimate the effect of programs and interventions. Even when employing experimental data, assessing the impact of a treatment is often complicated by two critical identification problems: sample selection and noncompliance with the assigned treatment. Our leading example in the article is the evaluation of the effect of the Job Corps (JC) training program on wages using data on individuals who were randomly assigned to participate or not in the program. In this case, the sample selection issue arises because wages are only observed for those who are employed, with the employment decision itself being potentially affected by the program. The noncompliance problem appears because some treatment group individuals did not enroll in the program, while some of the control individuals did enroll. In this article, we derive nonparametric bounds for average treatment effects in settings where both identification problems are present, and employ these bounds to empirically assess the effect of JC on its participants’ wages.

Job Corps (JC) is the most comprehensive and largest federally funded job training program in the United States for disadvantaged youth between the ages of 16 and 24. It provides academic, vocational, and social skills training, among many other services (e.g., health care and job search assistance), at more than 120 centers nationwide. A typical JC student lives at a local JC center for 8 months and receives about 1100 hr of academic and vocational instruction, which is equivalent to approximately 1 year of high school (Schochet, Burghardt, and Glazerman 2001). Assessing the effect of this and similar programs on wages is of great importance to policy makers. Most of the econometric evaluations of training programs, however, focus on their impact on total earnings, which are the product of the hourly wage and the hours worked. As discussed by Lee (2009), focusing only on total earnings fails to answer the relevant question of whether the programs lead to an increase in participants’ wages (e.g., through human capital accumulation), or to an increase in the probability of being employed (e.g., through job search assistance services) without any increase in wages.

Standard approaches for point identifying (i.e., theoretically learning the true parameter values in infinite samples) treatment effects in the presence of sample selection require strong parametric assumptions or the availability of a valid instrument (e.g., Heckman 1979). In settings where an instrument is unavailable, a recent literature has focused on bounding or partially identifying these effects under relatively mild assumptions. Part of this literature uses principal stratification (Frangakis and Rubin 2002), which provides a framework for studying causal effects when controlling for a variable that has been affected by the treatment (in our setting, the employment decision). Principal stratification compares individuals within subpopulations (called principal strata) whose individuals share the same...
and show that, under some assumptions, IV estimators point identify the average treatment effect for those who comply with their treatment assignment (the compliers). We use principal stratification to address sample selection and noncompliance simultaneously, and derive bounds for the average effect of participating in a training program on wages for the stratum of always-employed compliers. This stratum consists of those who comply with their treatment assignment and would be employed whether or not they enrolled in the program. Analogous to the cases analyzed in Imbens and Angrist (1994), AIR, ZRM, and Lee (2009), this is the only stratum for which wages are observed for individuals who enrolled and did not enroll in the program. In our application, this is the largest stratum (about 40% of the population).

Principal stratification has often been used to address a single posttreatment complication. While it is our understanding that this is one of the first papers deriving bounds for treatment effects within this framework accounting for more than one identification problem, there are a few papers that employ this framework to point identify treatment effects in the presence of multiple complications. For example, Mattei and Mealli (2007) addressed noncompliance, sample selection, and missing outcomes (dropout) in randomized experiments using a parametric Bayesian approach, and employed their methods to evaluate the effects of a new teaching program on breast self-examination. A related article that is particularly relevant in our setting is the one by Frumento et al. (2012), who performed a likelihood-based analysis of the effects of JC on employment and wages simultaneously addressing noncompliance, sample selection, and missing outcomes due to nonresponse. They stratified the population based on the potential values of the compliance behavior and employment status to address noncompliance and sample selection, and to address the missing-outcome problem they assumed that the probability that the outcome is missing for a given individual is random conditional on a set of observable characteristics. Under some parametric assumptions, they point identified the effect of JC on wages for the always-employed compliers. Our article complements the work by Frumento et al. (2012) by constructing nonparametric bounds for the effect of JC on wages based on an alternative set of assumptions. In the empirical section of the article, we also present results that account for missing values due to nonresponse by using weights constructed based on nonpublic use data that account for sample design and nonresponse.

The contribution of this article is two-fold. First, we add to the partial identification literature by deriving nonparametric bounds for average treatment effects in the presence of sample selection and noncompliance. More generally, our bounds can be used in settings where two identification problems are present (e.g., sample selection and an endogenous treatment) and there is a valid IV to address one of the problems. For example, when assessing the effect of military service on future health using the Vietnam-era draft lottery as an IV to address the endogeneity of the decision to serve in the military (e.g., Angrist, Chen, and Frandsen 2010), our results could be used to bound the average effect for those who enrolled in the military because of the draft lottery (compliers) and would live at the time the outcome is measured regardless of their veteran status. Second, we contribute to the literature on the evaluation of JC (Schochet,
population. For each unit $i$, Section 3 empirically analyzes the wage effects of JC using the econometric framework and the partial identification results. Under our monotonicity and mean dominance assumptions, we find that the average effect of JC on wages for the always-employed compliers is between 5.7% and 13.9% 4 years after randomization, and between 7.7% and 17.5% for non-Hispanics.

The article is organized as follows. Section 2 presents the econometric framework and the partial identification results. Section 3 presents a simulation study based on our data which analyzes the performance of our bounds, especially in cases when our assumptions are violated. Section 5 provides our conclusions.

2. ECONOMETRIC FRAMEWORK

2.1 Setup, Principal Strata, and Parameter of Interest

Assume we have a random sample of size $n$ from a large population. For each unit $i$ in the sample, let $Z_i = z \in \{0, 1\}$ indicate whether the unit was randomly assigned to the treatment group ($Z_i = 1$) or to the control group ($Z_i = 0$), let $D_i = d \in \{0, 1\}$ indicate whether individual $i$ actually received the treatment ($D_i = 1$) or not ($D_i = 0$), and let $S_i = s \in \{0, 1\}$ be a posttreatment sample selection indicator for whether the latent outcome variable $Y_i^*$ is observed ($S_i = 1$) or not ($S_i = 0$). In addition, let $D(z)$ denote the potential compliance behavior as a function of the treatment assignment, and let $S(z, d)$ and $Y^*(z, d)$ denote the potential values of the selection indicator and the latent outcome, respectively, as a function of the treatment assignment ($z$) and the treatment received ($d$). We observe $(Z_i, D_i(Z_i), S_i(Z_i, D_i(Z_i)))$ for all units, whereas the observed outcome is $Y_i = Y_i^*(Z_i, D_i(Z_i))$ if $S_i = 1$ and is missing if $S_i = 0$. Our notation implicitly imposes the stable unit treatment value assumption (SUTVA) (Rubin 1978, 1980, 1990), which implies that there are no different versions of the treatment and there is no interference between individuals.

In our application, $Z_i$ specifies whether individual $i$ was randomly assigned to participate or not in JC, $D_i$ denotes whether the individual actually enrolled in JC, $S_i$ specifies whether individual $i$ is employed or not, and $Y_i^*$ is the offered market wage. There is disagreement in the literature on whether it is possible to define wages for individuals who are unemployed. For example, ZRM, Zhang, Rubin, and Mealli (2009) and Frumento et al. (2012) argued that wages are well-defined only for those who are employed and, thus, did not consider parameters or assumptions involving wages for the unemployed (which are never observed). Here, we follow Blundell et al. (2007) and Lee (2009), and interpret $Y^*$ for unemployed individuals as the wages rejected by those who do not take up employment. However, our main results are unaffected by the way we treat $Y^*$ when $S = 0$ because our parameter of interest considers only individuals who would be employed under both treatment arms.

To simplify notation, in what follows we omit the subscript $i$ unless deemed necessary. We address noncompliance by using randomization into JC ($Z$) as an IV for JC enrollment ($D$). As in AIR, we impose the following assumptions:

**Assumption 1** (Randomly Assigned Instrument). $(Y^*(z, d), S(z, d), D(z))$ is independent of $Z$ for all $z, d \in \{0, 1\}$.

**Assumption 2** (Exclusion Restriction of $Z$). $Y^*(z, d) = Y^*(z', d) = Y^*(d)$ and $S(z, d) = S(z', d) = S(d)$ for all $z, d \in \{0, 1\}$.

**Assumption 3** (Nonzero Average Effect of $Z$ on $D$). $E[D(1) − D(0)] \neq 0$.

Assumption 2 states that any effect of the IV ($Z$) on the potential outcomes $Y^*$ and on the potential sample selection indicator $S$ must be via an effect of $Z$ on the treatment $D$. In our application, it requires that randomization affects potential wages and employment only through its effect on JC enrollment. Assumption 2 allows us to write the potential variables $Y^*(z, d)$ and $S(z, d)$ as a function of the treatment $d$ only. Assumption 3 requires the IV (randomization) to have a nonzero average effect on the probability of actually receiving treatment (JC enrollment). Note that we require $Z$ to be a valid IV for both $Y^*$ and $S$.

We derive bounds for the effect of JC enrollment on wages accounting for sample selection and noncompliance within a principal stratification framework (Frangakis and Rubin 2002). This framework is useful for studying causal effects when controlling for intermediate variables that have been affected by the treatment assignment ($Z$). The basic principal stratification with respect to $Z$ is a partition of the population into groups whose members share the same potential values of the intermediate variable under each value of $Z$. Since membership to a particular stratum is not affected by treatment assignment, comparisons of potential outcomes within a given stratum yield causal effects.

The intermediate variables we want to control for are the compliance behavior ($D$) and the sample-selection indicator ($S$). In our setting, the principal strata are defined by the joint potential values of $(D(z = 0), D(z = 1)) \times (S(z = 0), S(z = 1))$. We define the following subpopulations: $a = \{i : D_i(0) = D_i(1) = 1\}$, the “always-takers”; $n = \{i : D_i(0) = D_i(1) = 0\}$, the “never-takers”; $c = \{i : D_i(0) = 0, D_i(1) = 1\}$, the “compliers”; $d = \{i : D_i(0) = 1, D_i(1) = 0\}$, the “defiers”; as well as $EE = \{i : S_i(0) = S_i(1) = 1\}$, the “always-employed,” those who would be employed regardless of treatment assignment; $NN = \{i : S_i(0) = S_i(1) = 0\}$, the “never-employed,” those who would be unemployed regardless of treatment assignment; $NE = \{i : S_i(0) = 0, S_i(1) = 1\}$, those who would be employed only if assigned to the treatment group; $EN = \{i : S_i(0) = 1, S_i(1) = 0\}$, those who would be employed only if assigned to the control group. In total, we have 16 strata: $\{a, n, c, d\} \times \{EE, NN, NE, EN\}$. These strata are similar to those in Frumento et al. (2012), and they result from combining the strata employed in AIR to account for noncompliance with those used in ZRM to address sample selection (see also Mattei and Mealli 2007).
An important characteristic of principal strata is that they are latent subpopulations, meaning that, in general, we cannot observe to which stratum each individual belongs. Thus, additional assumptions are usually imposed to identify effects of interest by reducing the number of strata that exist in the population. Assumption 2 implies that the strata \( aNE, aEN, nNE, \) and \( nEN \) do not exist because, for these four strata, there exists an effect of the treatment assignment (\( Z \)) on employment (\( S \)) that is not through JC enrollment (since for them \( D(1) = D(0) \)), which contradicts the exclusion restriction of \( Z \). We also impose the following assumption, also used by AIR, which further reduces the number of existing strata.

**Assumption 4.** (Individual-Level Monotonicity of \( D \) in \( Z \)).
\[
D_i(1) \geq D_i(0) \text{ for all } i.
\]

Assumption 4 rules out the existence of defiers, thus eliminating the strata \( dEE, dNN, dEN, \) and \( dNE \). In our application, it assumes no individual would enroll in JC only if assigned to the control group. Assumption 4 implies that the average effect of \( Z \) on \( D \), which is point identified, is nonnegative. In the absence of sample selection, Imbens and Angrist (1994) and AIR showed that under Assumptions 1 through 4 IV estimators point identify the average treatment effect of \( D \) on \( Y^* \) for the compliers. If sample selection is present, however, those assumptions are not enough to point identify an average effect of \( D \) on \( Y^* \).

In this article, our parameter of interest (also considered by Frumento et al. 2012) is the average effect for the intersection of the subpopulation AIR focused on when accounting for noncompliance with the one ZRM and Lee (2009) focused on when addressing sample selection. More specifically, we focus on the average treatment effect of enrolling in JC (\( D \)) on latent wages (\( Y^* \)) for the “always-employed compliers” (i.e., the \( cEE \) stratum):
\[
\Delta = E[Y^*(1) - Y^*(0)|cEE].
\]

The focus is on the effect for the \( cEE \) stratum because, from the definition of the different strata, this is the only stratum for which wages are nonmissing for individuals who enrolled and did not enroll in JC (after imposing Assumption 4). Hence, the effect for this stratum is the only one that is well-defined whether or not we are willing to define wages for unemployed individuals, and is also the only one for which bounds can be derived without imposing assumptions on wages that are never observed in our setting. From the perspective that wages are well-defined only for employed individuals (e.g., ZMR), a causal effect of JC on wages is well-defined only for \( EE \) individuals (e.g., \( NE \) individuals’ wages are missing under the control treatment and their effect is thus undefined). Even if one assumes wages are well-defined for the unemployed (as we do), wages are never observed in at least one treatment arm for non-\( EE \) individuals. Moreover, among the \( EE \) individuals, wages are observed under enrollment and nonenrollment in JC only for compliers (e.g., \( nEE \) members’ wages under enrollment are never observed). Thus, bounding effects for strata other than the \( cEE \) stratum requires assumptions about quantities that are never observed regardless of treatment assignment, and there is disagreement in the literature on whether such assumptions can be made. For example, the original principal stratification framework of Frangakis and Rubin (2002) only employs potential outcomes that can be observed in the data depending on the treatment assignment, while other authors (e.g., Flores and Flores-Lagunes 2010, 2013; Chen, Flores, and Flores-Lagunes 2012; Huber and Mellace 2013) also use a priori counterfactual outcomes that are never observed regardless of treatment assignment (e.g., the \( nEE \) members’ wages if we could “force” them to enroll in JC) and thus are “entirely hypothetical” (Rubin 1990). Following the principal stratification literature, in the next two sections we avoid that type of assumptions to derive our bounds on (1). Finally, note that the stratum \( cEE \) can also be interpreted as those compliers who would be always employed regardless of treatment receipt, since for compliers \( Z = D \).

### 2.2 Bounds Under an Additional Monotonicity Assumption

We impose the following assumption.

**Assumption 5 (Individual-Level Monotonicity of \( S \) in \( D \)).**
\[
S_i(1) \geq S_i(0) \text{ for compliers}.
\]

Assumption 5 states that there is a nonnegative effect of \( D \) on \( S \) for every complier. It differs from the monotonicity assumption used by ZRM and Lee (2009) in that monotonicity is imposed in the actual treatment received (rather than in the treatment assigned) and only for compliers (rather than for all individuals). A testable implication of Assumption 5 is that the average effect of \( D \) on \( S \) for compliers, which is point identified under Assumptions 1 through 4, is nonnegative. Individual-level monotonicity assumptions are common in the partial identification literature (e.g., Manski and Pepper 2000; Flores and Flores-Lagunes 2013). In our application, Assumption 5 states that there is a nonnegative effect of JC on employment for every complier. While this assumption is weaker than that in ZRM and Lee (2009) (as it only applies to compliers), it is not innocuous because it is imposed at the individual level and thus it requires that no individual complier has a negative effect of JC on employment. We discuss potential threats to the plausibility of this assumption in Section 3.2.

From the strata remaining after imposing Assumptions 1 through 4, Assumption 5 rules out the existence of the \( cEN \) stratum. Hence, under these five assumptions there are seven strata in the population: \( aEE, aNN, nEE, nNN, cEE, cNN, \) and \( cNE \). The relationship between these seven strata and the observed groups defined by the values of \( \{Z, D(Z), S(Z, D(Z))\} \) is given in Table 1. This table illustrates that the \( cEE \) stratum is the only one for which wages are observed under both \( D = 0 \) and \( D = 1 \).

Let \( \pi_k \) denote the proportion of stratum \( k \) in the population, and let \( p_{d|z} \equiv Pr(D = d, S = s|Z = z) \) and \( q_{d|z} \equiv Pr(S = s|Z = z) \). Under Assumptions 1 through 5, we can identify \( \pi_k \) for all strata as:
\[
\pi_{aNN} = p^{1010}, \quad \pi_{aEE} = p^{1110}, \quad \pi_{nNN} = p^{1001}, \quad \pi_{nEE} = p^{1101}, \quad \pi_{cEE} = p^{1101} - p^{1001}, \quad \pi_{cNN} = p^{1001} - p^{1010}, \quad \text{and } \pi_{cNE} = q_{1|1} - q_{1|0}.
\]

Similarly, letting \( \overline{Y}_d^{\pi_1} \equiv E[Y|Z = z, D = d, S = s] \), we can write the mean outcomes for the observed cells with \( S = 1 \) as a function of mean potential outcomes for different strata as:
\[
E[Y^*(1)|aEE],
\]
\[ \overline{Y}^{101} = E[Y^*(0)|nEE], \]
\[ \overline{Y}^{001} = E[Y^*(0)|cEE] \frac{\pi_{cEE}}{\pi_{cEE} + \pi_{aEE}} \]
\[ + E[Y^*(0)|nEE] \frac{\pi_{nEE}}{\pi_{cEE} + \pi_{nEE}}, \]  
(2)

and
\[ \underline{Y}^{111} = \frac{\pi_{cEE} \cdot E[Y^*(1)|cEE]}{\pi_{cEE} + \pi_{cNE} + \pi_{aEE}} + \frac{\pi_{cNE} \cdot E[Y^*(1)|cNE]}{\pi_{cEE} + \pi_{cNE} + \pi_{aEE}} \]
\[ + \frac{\pi_{aEE} \cdot E[Y^*(1)|aEE]}{\pi_{cEE} + \pi_{cNE} + \pi_{aEE}}. \]  
(3)

In addition to \( E[Y^*(1)|aEE] \) and \( E[Y^*(0)|nEE], \) \( E[Y^*(0)|cEE] \) is also identified, from (2), by
\[ E[Y^*(0)|cEE] = \frac{p_{01|1}}{p_{01|1} - p_{01|0}} \overline{Y}^{101} - \frac{p_{01|0}}{p_{01|1} - p_{01|0}} \underline{Y}^{101}. \]  
(4)

Therefore, one of the terms of \( \Delta \) in (1) is point identified. However, the term \( E[Y^*(1)|cEE] \) is not point identified because two of the conditional means in (3) are not point identified. Next, we construct bounds for \( E[Y^*(1)|cEE] \) and, thus, \( \Delta. \)

In the absence of noncompliance, ZRM and Lee (2009) constructed bounds for the nonpoint identified mean of \( Y^*(1) \) for always-employed (EE) individuals from a cell containing only two strata. To illustrate the idea behind their trimming bounds, suppose there were no individuals in the \( aEE \) stratum \( (\pi_{aEE} = 0), \) so that the cell \( \{Z = 1, D = 1, S = 1\} \) had only two strata, \( cEE \) and \( cNE. \) Then, \( E[Y^*(1)|cEE] \) would be bounded from above (below) by the mean of \( Y \) for the fraction \( \pi_{cEE}/(\pi_{cEE} + \pi_{cNE}) \) of the largest (smallest) values of \( Y \) for those individuals in that cell. A key difference between the bounds derived in those papers and ours is that in our setting the bounds for \( E[Y^*(1)|cEE] \) are derived from a cell containing three strata.

Although \( \overline{Y}^{111} \) is a weighted average of the mean of \( Y^*(1) \) for three strata (Equation (3)), \( E[Y^*(1)|aEE] \) is point identified. To motivate how we construct our bounds, we think of the problem as finding “worst-case” scenarios for \( E[Y^*(1)|cEE] \) subject to the constraint that \( \overline{Y}^{111} = E[Y^*(1)|aEE]. \) To bound \( E[Y^*(1)|cEE], \) we solve the unconstrained problem first and check whether the value of \( E[Y^*(1)|aEE] \) implied by this solution can satisfy the constraint, in which case the unconstrained solution is the solution to the constrained problem. If not, we impose the constraint first and then obtain the solution to the constrained problem.

To describe our bounds, let \( Y_{r}^{111} \) be the \( r \)th quantile of \( Y \) in the cell \( \{Z = 1, D = 1, S = 1\}, \) and let \( \overline{Y}(Y_{r}^{111} \leq Y \leq Y_{r}^{111}) \equiv E[Y|Z = 1, D = 1, S = 1, Y_{r}^{111} \leq Y \leq Y_{r}^{111}] \) be the mean of the outcomes between the \( r \)th and \( r \)th quantiles of \( Y \) in that cell. Suppose we want to derive the lower bound for \( E[Y^*(1)|cEE] \).

We first consider the problem without the constraint. In this case, we may apply the trimming procedure in ZRM and Lee (2009) and bound \( E[Y^*(1)|cEE] \) from below by the expected value of \( Y \) for the \( \pi_{cEE}/p_{11} \) fraction of the smallest values of \( Y \) in the cell \( \{Z = 1, D = 1, S = 1\}, \) or, \( \overline{Y}(Y \leq Y_{111}^{111}/p_{11}), \) where \( p_{11} \equiv \pi_{cEE} + \pi_{cNE} + \pi_{aEE}. \) Next, we check whether this solution is consistent with the constraint \( \overline{Y}^{101} = E[Y^*(1)|aEE]. \) To do this, we construct the “worst-case” scenario lower bound for \( E[Y^*(1)|aEE], \) call it \( \underline{aEE}, \) implied by the unconstrained solution by placing all the \( aEE \) individuals at the bottom of the remaining observations in the cell \( \{Z = 1, D = 1, S = 1\}. \) This yields \( \underline{aEE} = \overline{Y}(Y_{111}^{111}/p_{11} \leq Y \leq Y_{111}^{111}/(\pi_{cNE}/p_{11})); \) otherwise, the unconstrained solution is inconsistent with \( \overline{Y}^{011} = E[Y^*(1)|cEE]. \) Intuitively, having \( \underline{aEE} \leq \overline{Y}^{011}, \) the unconstrained solution is consistent with the constraint and the lower bound for \( E[Y^*(1)|cEE] \) is \( \overline{Y}(Y \leq Y_{111}^{111}/p_{11}); \) otherwise, the unconstrained solution is inconsistent with \( \overline{Y}^{011} = E[Y^*(1)|aEE]. \) Thus, if \( \underline{aEE} > \overline{Y}^{101}; \) the lower bound for \( E[Y^*(1)|cEE] \), call it \( LY_{1,cEE}, \) is derived from the equation:
\[ \overline{Y}(Y \leq Y_{111}^{111}/(\pi_{cNE}/p_{11})) = \frac{\pi_{cEE}}{\pi_{cEE} + \pi_{aEE}} \cdot LY_{1,cEE} \]
\[ + \frac{\pi_{aEE}}{\pi_{cEE} + \pi_{aEE}} \overline{Y}^{111}, \]  
(5)

where \( \overline{Y}(Y \leq Y_{111}^{111}/(\pi_{cNE}/p_{11})) \) is the mean of \( Y \) for the \( 1 - (\pi_{cNE}/p_{11}) \) fraction of the smallest values of \( Y \) in the cell \( \{Z = 1, D = 1, S = 1\}. \)

Note that if \( \underline{aEE} \leq \overline{Y}^{101}, \) the lower bound derived from (5) will be smaller than the lower bound derived without using the information on \( E[Y^*(1)|aEE], \) \( \overline{Y}(Y \leq Y_{111}^{111}/p_{11})). \) Intuitively, if \( \overline{Y}^{101} = E[Y^*(1)|aEE] \) is very large, it is impossible that all \( aEE \) individuals are at the bottom \( 1 - (\pi_{cNE}/p_{11}) \) fraction of the smallest values of \( Y \) in the cell \( \{Z = 1, D = 1, S = 1\}, \) and the lower bound derived from (5) will be lower than \( \overline{Y}(Y \leq Y_{111}^{111}/(\pi_{cNE}/p_{11})). \) In general, we can write the lower bound for \( E[Y^*(1)|cEE] \) as the maximum of those two lower bounds.

The upper bound for \( E[Y^*(1)|cEE] \) is derived in a similar way by placing the observations in the corresponding strata in the upper part of the distribution of \( Y \) in the cell \( \{Z = 1, D = 1, S = 1\}. \) The bounds for \( E[Y^*(1)|cEE] \) are combined with the

| Table 1. Observed groups and principal strata |
|---------------------------------------------|
| Z = 0                                      |
| D   | S | 0 | cNE, cNN, nNN | aNN | cEE, nEE | aEE |
| 0   |   | 1 |                |     |          |     |
| 1   |   |   |                |     |          |     |

| Z = 1                                      |
| D   | S | 0 | nNN | cNN, aNN | cEE, cNE, aEE |
| 0   |   | 1 |     |          |              |
| 1   |   |   |     |          |              |
Proposition 1. If Assumptions 1 through 5 hold, then $L_{cEE} \leq \Delta \leq U_{cEE}$, where

\[ L_{cEE} = LY_{1,cEE} - \left( \frac{\pi_{001}}{\pi_{010} - \pi_{011}} + \frac{\pi_{101}}{\pi_{010} - \pi_{011}} \right), \]

\[ U_{cEE} = UY_{1,cEE} - \left( \frac{\pi_{001}}{\pi_{010} - \pi_{011}} + \frac{\pi_{101}}{\pi_{010} - \pi_{011}} \right), \]

\[ LY_{1,cEE} = \max \left\{ \left( \frac{\pi_{111}}{\pi_{010} - \pi_{011}} \right), \left( \frac{\pi_{111}}{\pi_{010} - \pi_{011}} \right) \right\}, \]

\[ UY_{1,cEE} = \min \left\{ \left( \frac{\pi_{111}}{\pi_{010} - \pi_{011}} \right), \left( \frac{\pi_{111}}{\pi_{010} - \pi_{011}} \right) \right\}, \]

\[ \alpha_{cEE} = \frac{\pi_{cEE}}{\pi_{111}} = \frac{\pi_{010} - \pi_{011}}{\pi_{111}}, \quad \text{and} \]

\[ \alpha_{cNE} = \frac{\pi_{cNE}}{\pi_{111}} = \frac{q_{11} - p_{11}}{\pi_{111}}. \]

Proof. See the online Appendix.

2.3 Bounds Under Mean Dominance

We now consider an assumption that narrows the bounds presented in Proposition 1.

Assumption 6. (Mean Dominance). $E[Y^*(1)|cEE] \geq E[Y^*(1)|cNE]$.  

Assumption 6 comes from the notion that some strata are likely to have more favorable characteristics and thus better potential outcomes than others. In our application, this assumption states that the mean potential wage under treatment of the always-employed compliers ($cEE$ stratum) is greater than or equal to that of those compliers who would be employed only if they enrolled in JC ($cNE$ stratum). Assumption 6 implies a positive correlation between employment and wages, which is supported by standard models of labor supply. ZRM and Huber and Mellace (2013) considered stochastic-dominance versions of Assumption 6 that require weakly dominance at any rank of the potential outcome distribution, instead of only at the mean. Stochastic dominance is much stronger than needed for our purposes.

In practice, the bounds under both Assumptions 5 and 6 may be preferred over those under only Assumption 5 because they are narrower, in which case it is important to have a sense about the plausibility of Assumption 6. Although it is not directly testable, it is possible to get indirect evidence about its plausibility by comparing the average baseline characteristics of the $cEE$ and $cNE$ strata that are closely related to the outcome of interest (e.g., prerandomization outcome values). Assumption 6 is less likely to hold if these comparisons suggest that the $cNE$ stratum has better characteristics at the baseline than does the $cEE$ stratum. Under Assumptions 1 through 5 the average baseline characteristics of all the strata are point identified from the observed average baseline characteristics of the observed groups $\{Z, D, S\}$ in Table 1, as each of these eight observed means is a weighted average of the mean baseline characteristics of the seven strata (see, for reference, Equations (2) and (3)), with the weights being also point identified. Since the number of moment conditions implied by these equations is greater than the number of parameters, in our application we use generalized method of moments (GMM) to estimate the mean baseline characteristics of all seven strata. GMM minimizes a squared Euclidean distance of the sample analogues of the moment conditions to their population value of zero (e.g., Newey and McFadden 1994). We provide further details in the online Appendix.

Assumption 6 tightens the bounds in Proposition 1 by increasing the lower bound on $E[Y^*(1)|cEE]$. Similar to Equation (3), we can write

\[ \bar{Y}^{11} = \frac{(\pi_{cEE} + \pi_{cNE}) \cdot E[Y^*(1)|cEE, cNE]}{\pi_{cEE} + \pi_{cNE} + \pi_{aEE}} + \frac{\pi_{aEE} \cdot E[Y^*(1)|aEE]}{\pi_{cEE} + \pi_{cNE} + \pi_{aEE}}, \]

where the stratum proportions and $E[Y^*(1)|aEE]$ are point identified. Assumption 6 implies $E[Y^*(1)|cEE] \geq E[Y^*(1)|cEE, cNE]$, which provides a lower bound for $E[Y^*(1)|cEE]$ that is greater than or equal to the one in Proposition 1. Proposition 2 presents bounds for $\Delta$ under Assumptions 1 through 6.

Proposition 2. If Assumptions 1 through 6 hold, then $L_{cEE} \leq \Delta \leq U_{cEE}$, where $U_{cEE}$ is as given in Proposition 1 and $L_{cEE}$ equals:

\[ L_{cEE} = LY_{1,cEE} - \left( \frac{\pi_{001}}{\pi_{010} - \pi_{011}} \right), \]

\[ U_{cEE} = UY_{1,cEE} - \left( \frac{\pi_{001}}{\pi_{010} - \pi_{011}} \right), \]

with

\[ LY_{1,cEE} = \frac{\pi_{111}}{\pi_{010} - \pi_{011}}, \]

\[ UY_{1,cEE} = \frac{\pi_{111}}{\pi_{010} - \pi_{011}}. \]

Proof. See the online Appendix.

2.4 Remarks

Remark 1. It is possible to construct bounds on $\Delta$ without Assumption 5, in which case the stratum $cEN$ is not ruled out and appears in the observed cells $\{Z = 0, D = 0, S = 1\}$ and $\{Z = 1, D = 1, S = 0\}$ in Table 1. Although the proportions of the strata $aEE$, $aNN$, $nEE$, and $nNN$ are still point identified, neither the proportions of the strata $cEE$, $cNN$, $cNE$, and $cEN$ nor the term $E[Y^*(0)|cEE]$ is now point identified. To construct bounds for $\Delta$ in this case, we can combine our approach in Section 2.2 with that followed by Zhang and Rubin (2003), ZRM, Imai (2008), and Huber and Mellace (2013)
in a setting without noncompliance. The main idea for constructing their bounds on the average effect for the EE stratum is to consider “worst-case” scenarios that are consistent with the possible values $\pi_{EE}$ can take based on the data, with the bounds being obtained at the minimal value $\pi_{EE}$ can take (Huber and Mellace 2013). In our setting, the worst-case scenarios for $\Delta$ occur when $\pi_{EE}$ is at its minimum value that is consistent with the data. The minimum possible value of $\pi_{EE}$, calculated from the cells in Table 1, is $\pi_{EE} \geq \max(0, p_{010} - p_{011} + p_{101})$. As in Lee (2009), the bounds on $\Delta$ are well-defined only if $\pi_{EE} > 0$, which implies $\pi_{EE}$ is minimized at $p_{010} - p_{011} + p_{101}$. Given this lower bound for $\pi_{EE}$, the same approach as in Section 2.2 can be used to derive bounds on $\Delta$ by constructing bounds for $E[Y^*(1)|cEE]$ and $E[Y^*(0)|cEE]$. However, these bounds will be wider than those in Proposition 1 and may be uninformative in practice, as illustrated by Blanco, Flores, and Flores-Lagunes (2013) in a setting without noncompliance.

Remark 2. In the absence of Assumptions 5 and 6, the lower bound for $\Delta$ in Proposition 2, $\bar{T}_{cEE}$, provides information for another parameter of interest: $\beta = E[Y^*(1) - Y^*(0)|cEE, cNE]$, the average effect of $D$ on $Y^*$ for the $cEE$ and $cNE$ strata. $\bar{T}_{cEE}$ gives a lower bound for $\beta$ under Assumptions 1 through 4 and the following assumption:

Assumption 5'. $E[Y^*(0)|cEE, cEN] \geq E[Y^*(0)|cEE, cNE]$.

This assumption states that the mean of $Y^*(0)$ (i.e., the latent wage under no enrollment in JC) for compliers who would be employed if they did not attend JC ($cEE$ and $cEN$) is greater than or equal to that for compliers who would be employed if they did attend JC ($cEE$ and $cNE$). This assumption exploits the positive correlation between employment and wages implied by standard models of labor supply, as the $cEN$ are employed under the control treatment but the $cNE$ are not. Note that in this case one needs to be willing to define wages for the unemployed and make assumptions about never-observed counterfactual wages.

To see why $\bar{T}_{cEE}$ provides a lower bound for $\beta$, note that $E[Y^*(1)|cEE, cEN]$ is point identified from (6) and equals $\bar{Y}_{1,cEE}$ (defined in Proposition 2), while Assumption 5' implies that an upper bound for $E[Y^*(0)|cEE, cNE]$ is given by $E[Y^*(0)|cEE, cEN]$, which is point identified as $E[Y^*(0)|cEE, cEN] = (\bar{Y}_{010|} - \bar{Y}_{011|})/(p_{010} - p_{011})$ from

$$\bar{Y}_{010|} = \frac{(\pi_{cEE} + \pi_{cEN}) \cdot E[Y^*(0)|cEE, cEN]}{\pi_{cEE} + \pi_{cEN} + \pi_{nEE}} + \frac{\pi_{nEE} \cdot E[Y^*(0)|nEE]}{\pi_{cEE} + \pi_{cEN} + \pi_{nEE}}.$$

(7)

In practice, if selected compliers under control ($cEE$ and $cEN$ strata) have more favorable characteristics than selected compliers under treatment ($cEE$ and $cNE$ strata), we would expect the former group to be more likely to experience better outcomes than the latter in the absence of treatment. Thus, indirect evidence about the plausibility of Assumption 5' can be obtained by comparing $E[X|cEE, cEN]$ and $E[X|cEE, cNE]$, where $X$ denotes relevant baseline characteristics (e.g., prerandomization outcomes). Similarly, it is important to have a sense of the type of individuals who may belong to the $cEN$ stratum. In our application these could be compliers who, for example, if they enrolled in JC, would pursue an associate or other academic degree after leaving JC and would appear as unemployed at the time the outcome is measured, while, if not enrolled in JC, they may have no other choice but to work to cover their basic needs (JC also provides room, board, and a stipend) and are able to find a job.

In sum, in applications where Assumption 5 is difficult to justify, Assumption 5' may be an attractive alternative. Moreover, the mixture of the strata $cEE$ and $cNE$ seems to be an interesting subpopulation, since these are individuals who would comply with the treatment assignment and be employed if they attended JC.

2.5 Estimation and Inference

The bounds derived in Sections 2.2 and 2.3 contain minimum (min) and maximum (max) operators, which create problems for standard estimation and inference procedures. Sample analog estimates of those bounds tend to be tighter than the true bounds due to the concavity (convexity) of the min (max) function, and the asymptotic distribution of the bound estimators is usually unavailable. Hirano and Porter (2012) showed that for parameters that are nonsmooth functionals of the underlying data distribution, such as those having min or max operators, there exist no locally asymptotically unbiased estimators and no regular estimators. A growing literature has focused on developing valid estimation and inference procedures for this type of bounds (e.g., Tamer 2010). We use the methodology proposed by Chernozhukov, Lee, and Rosen (2013) (hereafter CLR) to obtain confidence regions for the true parameter value and half-median unbiased estimators for the lower and upper bounds. The half-median-unbiasedness property means that the upper bound estimator exceeds the true value of the upper bound with probability at least one half asymptotically, while the reverse holds for the lower bound.

To briefly describe CLR’s procedure, let the bounds for a parameter $\theta_0$ (e.g., $\Delta$) be given by $[\theta_0', \theta_0'']$, where $\theta_0' = \max_{v \in \theta_0} \{\theta(v)\}$ and $\theta_0'' = \min_{v \in \theta_0} \{\theta(v)\}$. CLR refer to $\theta(v)$ and $\theta_0(v)$ as bounding functions. In our setting, $v$ indexes the bounding functions, while $m'$ and $m''$ give, respectively, the number of terms inside the max and min operators. For example, the upper bound for $\theta_0 = E[Y^*(1)|cEE]$ in Proposition 1, $\bar{Y}_{1,cEE}$, can be written as $\theta_0' = \min_{v_0 \in \theta_0} \{\theta_0(v)\}$, with $\theta_0'(1) = \bar{Y}(Y \geq y|\pi_{a,EE})$ and $\theta_0''(2) = \bar{Y}(Y \geq y|\pi_{a,EE}) - \bar{Y}_{011|\pi_{010|} - \pi_{011|\pi_{010} - \pi_{011}}}$. In our case, sample analog estimators of the bounding functions are known to be consistent and asymptotically normally distributed, as they are simple functions of conditional probabilities, means, and trimmed means (Lee 2009; Newey and McFadden 1994).

CLR address the issues related to estimation and inference for the bounds $[\theta_0', \theta_0'']$ by employing precision-corrected estimates of the bounding functions before applying the min and max operators. The precision adjustment consists of adding to each estimated bounding function its pointwise standard error times an appropriate critical value, $\kappa(p)$, so that estimates with higher standard errors receive larger adjustments. Depending on the
choice of $\kappa(p)$, it is possible to obtain confidence regions for either the identified set or the true parameter value, and half-median unbiased estimators for the lower and upper bounds. More specifically, the precision-corrected estimator of the upper bound $\hat{\theta}_u^p$ is given by

$$\hat{\theta}_u^p(p) = \min_{v \in \mathcal{V}_u} \left[ \hat{\theta}_u^v(v) + \kappa_{\hat{V}_u}(p)s_u(v) \right],$$

(8)

where $\hat{\theta}_u^v(v)$ is the sample analog estimator of $\theta_u^v(v)$ and $s_u(v)$ is its standard error. CLR compute the critical value $\kappa_{\hat{V}_u}(p)$ based on simulation methods and a preliminary estimator $\hat{V}_n^u$ of $\mathcal{V}_u = \arg\min_{v \in \mathcal{V}_u} \theta_u^v(v)$. Intuitively, $\hat{V}_n^u$ selects those bounding functions that are close enough to binding to affect the asymptotic distribution of the estimator of the upper bound. A precision-corrected estimator of the lower bound $\hat{\theta}_l^p$ is obtained in a similar way. Further details on the CLR procedure and our implementation steps are provided in the online Appendix.

3. THE WAGE EFFECTS OF JOB CORPS

3.1 Data and Preliminary Analysis

We employ data from the National Job Corps Study (NJCS), a randomized experiment funded by the U.S. Department of Labor to evaluate the effectiveness of JC. Eligible individuals who applied to JC for the first time between November 1994 and December 1995 (80,833 individuals) were randomly assigned into a treatment group and a control group. Individuals in the control group (5977) were embargoed from the program for 3 years, while those in the treatment group (74,856) were allowed to enroll in JC. The research sample (15,386), which consisted of all control individuals plus 9409 randomly selected individuals from the treatment group, was interviewed at random assignment and at 12, 30, and 48 months after random assignment.

The specific sample we employ is based on the one used by Lee (2009). This sample contains only individuals with non-missing values for weekly earnings and hours worked for every week after randomization (9145 individuals). We add to our dataset enrollment information at week 208 (i.e., 48 months) after randomization. This is a binary indicator for whether or not the individual was ever enrolled in JC during the 208 weeks after random assignment. We drop 55 observations from Lee’s sample due to the missing enrollment variable, resulting in our final sample of 9090 individuals: 3599 in the control group and 5491 in the treatment group. Wages at week 208 are obtained by dividing weekly earnings by weekly hours worked at that week, where wages being missing is equivalent to weekly hours worked being equal to zero. We regard an individual as unemployed when the wage is missing, and regard her as employed otherwise. We use the NJCS design weights throughout our analysis because some subpopulations were randomized with different, but known, probabilities (Schochet, Burghardt, and Glazerman 2001).

Our definition of enrollment differs from that in Frumento et al. (2012), who considered enrollment within the first 6 months after randomization. As a result, the specific effect in (1) we estimate in this application differs from theirs because we estimate the effect for the $cEE$ stratum of ever participat-
Table 2. Summary statistics of selected baseline variables

|                          | Entire sample |               |               | Non-Hispanics |               |               |
|--------------------------|---------------|---------------|---------------|---------------|---------------|---------------|
|                          | Z = 1         | Z = 0         | Diff. (Std. Err.) | Z = 1         | Z = 0         | Diff. (Std. Err.) |
| Female                   | 0.454         | 0.458         | −0.004 (0.011) | 0.454         | 0.454         | −0.010 (0.012)   |
| Age at baseline          | 18.44         | 18.35         | 0.087* (0.046) | 18.44         | 18.34         | 0.096* (0.050)   |
| White, non-Hispanic      | 0.265         | 0.263         | 0.002 (0.009)  | 0.319         | 0.318         | 0.001 (0.011)    |
| Black, non-Hispanic      | 0.494         | 0.491         | 0.003 (0.011)  | 0.595         | 0.593         | 0.002 (0.012)    |
| Hispanic                 | 0.169         | 0.172         | −0.003 (0.008) | —             | —             | —              |
| Never married            | 0.917         | 0.916         | 0.002 (0.006)  | 0.926         | 0.924         | 0.002 (0.006)    |
| Has children             | 0.635         | 0.627         | 0.008 (0.010)  | 0.642         | 0.627         | 0.015 (0.011)    |
| Education (years of schooling) | 3.603 | 3.530         | 0.074 (0.091)  | 3.654         | 3.512         | 0.143 (0.100)    |
| Earnings, prev. yr.      | 2911.0        | 2810.5        | 100.56 (117.58)| 2900.3        | 2794.7        | 105.57 (106.34)  |

At Baseline:

|                          |               |               |
|--------------------------|---------------|---------------|
| Have job                 | 0.198         | 0.192         | 0.007 (0.009) |
| Had job, prev. yr.       | 0.635         | 0.627         | 0.008 (0.010) |
| Months employed, prev. yr.| 3.603        | 3.530         | 0.074 (0.091) |
| Earnings, prev. yr.      | 2911.0        | 2810.5        | 100.56 (117.58) |
| Number of observations   | 5491          | 3599          | Total: 9090   |

NOTE: Z is an indicator for whether the individual was randomly assigned to participate or not in JC. ** and * denote difference is statistically different from 0 at 5% and 10% level, respectively. Computations use design weights.

3.2 Assessment of Assumptions

Assumption 1 holds by design, and the results in Table 3 suggest that Assumption 3 is satisfied. Assumption 2 states that JC affects wages and employment only through its effect on JC enrollment. This assumption seems plausible and has been used before in the JC literature (e.g., Schochet 2001; Schochet, Burghardt, and Glazerman 2001; Burghardt et al. 2001; Frumento et al. 2012). However, there could be threats to its validity. For example, the offer of a JC slot could affect the job search behavior of some individuals by increasing their reservation wages relative to what they would had been without the offer.
option to enroll in JC. Although this type of responses could affect the labor market outcomes in the short run, it seems reasonable to assume that treatment assignment has a negligible effect on the long run outcomes we consider through channels other than JC enrollment (see Schochet, Burghardt, and Glazerman 2001 and Frumento et al. 2012 for further discussion on the plausibility of this assumption in JC). Assumption 4 (no defiers) has also been used in the JC literature and appears plausible, as it seems highly unlikely that an individual would enroll in JC only if they were denied access to it.

Assumption 5 states that there is a nonnegative effect of JC on employment for every individual complier at week 208. This assumption seems plausible given the objectives and services provided by JC (e.g., academic and vocational training, job search assistance). Its testable implication that the LATE of JC on employment for compliers is nonnegative is soundly supported by the data, as shown in Table 3. Nevertheless, there are two potential threats to the validity of Assumption 5. First, individuals who enroll in JC may be less likely to be employed while undergoing training than those who do not enroll (usually called the “lock-in” effect; van Ours 2004). Second, trained individuals may raise their reservation wages because of the JC training, which may lead them to reject some job offers that they would otherwise accept if they had not received training. Both potential threats, however, are likely to become less relevant in the long run, as trained individuals are no longer “locked-in” away from employment, and individuals who chose to remain unemployed in the short run because of raising their reservation wages find jobs in the long run. Consistent with this view, Schochet, Burghardt, and Glazerman (2001) and Lee (2009) found negative effects of JC on employment in the short run, and positive effects in the long run. Thus, we focus our analysis on wages at week 208 after randomization, which is the latest wage measure available in the NJCS.

The likelihood-based analysis by Frumento et al. (2012) suggested that there may be a positive proportion of compliers in the population for whom JC has a negative effect on employment at week 208, even though this proportion falls over time after randomization. Hence, to further increase the plausibility of Assumption 5 in our application, we also consider a sample that excludes Hispanics. Hispanics were the only demographic group in the NJCS for which negative but statistically insignificant effects of JC on employment and earnings were found (Schochet, Burghardt, and Glazerman 2001), and therefore, Assumption 5 may not be appropriate for them. Schochet, Burghardt, and Glazerman (2001) found that the different results for Hispanics (relative to other groups) could not be explained by differences in features such as baseline characteristics, degree attainment, enrollment duration, or characteristics of the centers attended, whereas Flores-Lagunes, Gonzalez, and Neumann (2010) found that the lack of effect for Hispanics can be partly attributed to the higher local unemployment rates they face, and to the greater negative impact they experience from these unemployment rates. The last set of columns in Tables 2 and 3 present summary statistics and preliminary effects for the non-Hispanics sample (7529 individuals). As expected, the estimated ITT and LATE effects of JC on labor outcomes for non-Hispanics are stronger than those for the entire sample.

Assumption 6 states that the mean potential outcome under treatment of the always-employed compliers (cEE stratum) is greater than or equal to that of individuals who would be employed only if they enrolled in JC (cNE stratum). As discussed in Section 2.3, to shed light on the plausibility of this assumption we compare average baseline characteristics of the cEE and cNE strata that are likely to be highly correlated to wages at week 208 for the entire and non-Hispanics samples (the results are provided in the online Appendix). Focusing on the non-Hispanics sample we find that, relative to individuals in the cNE stratum, individuals in the cEE stratum are more likely to be male and white, to have never been arrested at the baseline, and to have better labor market outcomes the year before randomization. These differences, however, are not statistically different from zero. We conclude from these results that the data do not provide indirect evidence against Assumption 6, and that the point estimates of the differences suggest that the assumption is plausible.

In sum, while not innocuous, Assumptions 1 through 6 appear reasonable in our application.

3.3 Empirical Results

3.3.1 Bounds on ITT Effect. We start this section by bounding the average effect of being allowed to enroll in JC on wages (ITT effect) for the individuals who would always be employed regardless of treatment assignment, and then we bound the average effect of JC enrollment on wages for those compliers who would always be employed regardless of treatment receipt (Δ in (1)). The first parameter, which ignores noncompliance, is the one considered by ZRM and Lee (2009). In their setting, the principal strata are EE, NE, E, and ENE, where the last stratum is ruled out by assuming monotonicity of S in Z.

Table 4 presents estimation results for the average ITT effect of JC on ln(wage) for always-employed individuals (EE stratum). The first column of Table 4 presents results for our entire sample. The estimated proportion of the EE stratum in the population is 56.6%. Under the monotonicity of S in Z assumption, the estimated lower and upper bounds for the ITT effect of JC on ln(wage) for the EE stratum are −0.022 and 0.100, respectively. These results are very similar to those obtained by Lee (2009) (they are not exactly the same because he uses a transformation of the wages—see footnote 13 in that paper—and we drop 55 observations from his sample due to missing enrollment information). As noted by Lee (2009), although the bounds include zero, they rule out plausible negative effects. Moreover, these lower bounds are implicitly based on the extreme and unintuitive assumption that wages are perfectly negatively correlated with the probability of being employed. This is contradicted by standard models of labor supply, in which individuals with higher wages are also more likely to be employed.

The last part of Table 4 presents bounds on the ITT effect when adding the mean dominance assumption that the mean potential wage under z = 1 of the EE stratum is greater than or equal to that of the NE stratum. This assumption can be seen as a way to rule out the implausible extreme case mentioned above by implying a positive correlation between wages and employment. In this case, the estimated lower bound on the ITT effect of JC on ln(wage) for the EE stratum is 0.038, which rules out negative effects. Table 4 also presents 95% confidence intervals, which are based on the results by Imbens and Manski (2004)—since these bounds do not involve min or max
operators—and asymptotically cover the true parameter value with 0.95 probability. As above, while the confidence intervals do not rule out negative effects under the monotonicity assumption, they imply a positive effect once the mean dominance assumption is added. This illustrates the identifying power of this additional assumption.

The second column of Table 4 presents results for non-Hispanics, for whom the monotonicity assumption of $S$ in $Z$ is more plausible. In general, the lower and upper bounds under the two sets of assumptions are larger for non-Hispanics than for the entire population. Under the monotonicity and mean dominance assumptions, the estimated lower and upper bounds on the ITT effect of JC on $\ln(wage)$ for the $EE$ stratum are 0.050 and 0.119, respectively, and the 95% confidence interval is $[0.029, 0.144]$.

### 3.3.2 Bounds on $\Delta$

Table 5 shows the estimation results for the average effect of JC enrollment on $\ln(wage)$ for always-employed compliers ($\Delta$ in (1)). This table shows the estimated stratum proportions, relevant quantities used in estimating the bounds, half-median unbiased estimates of our bounds, and CLR

### Table 4. Bounds for the ITT effect of JC on $\ln(wage)$ for the $EE$ stratum

|                         | Entire sample | Non-Hispanics |
|-------------------------|---------------|---------------|
|                         |               |               |
| Stratum proportions: Always-employed $(EE)$ | 0.566** (0.009) | 0.559** (0.009) |
|                         | 0.393** (0.007) | 0.391** (0.007) |
| Employed only if assigned to program $(NE)$ | 0.041** (0.011) | 0.050** (0.012) |
| $E[Y^*(Z = 0)|EE]$ | 1.991** (0.009) | 1.977** (0.010) |
| Proportion of $EE$ in cell $[Z = 1, S = 1]$ | 0.932** (0.017) | 0.918** (0.019) |
| **Bounds under monotonicity:** |               |               |
| Lower bound for the ITT effect for $EE$ stratum | $-0.022 (0.016)$ | $-0.018 (0.017)$ |
| Upper bound for the ITT effect for $EE$ stratum | 0.100** (0.014) | 0.119** (0.015) |
| Imbens and Manski 95% confidence interval | $[-0.048, 0.123]$ | $[-0.047, 0.144]$ |
| **Bounds under monotonicity and mean dominance:** |               |               |
| Lower bound for the ITT effect for $EE$ stratum | 0.038** (0.012) | 0.050** (0.013) |
| Upper bound for the ITT effect for $EE$ stratum | 0.100** (0.014) | 0.119** (0.015) |
| Imbens and Manski 95% confidence interval | $[0.019, 0.123]$ | $[0.029, 0.144]$ |

### Table 5. Bounds for the effect of JC on $\ln(wage)$ for the $cEE$ stratum

|                         | Entire sample | Non-Hispanics |
|-------------------------|---------------|---------------|
| $\pi_{cEE}$ | 0.016** (0.002) | 0.018** (0.002) |
| $\pi_{nEE}$ | 0.158** (0.005) | 0.160** (0.005) |
| $\pi_{cEE}$ | 0.391** (0.010) | 0.381** (0.011) |
| $\pi_{nEE}$ | 0.041** (0.011) | 0.050** (0.012) |
| $\pi_{cNN}$ | 0.028** (0.003) | 0.030** (0.003) |
| $\pi_{nNN}$ | 0.104** (0.004) | 0.104** (0.005) |
| $\pi_{cNN}$ | 0.261** (0.007) | 0.258** (0.008) |
| $\pi_{cNE}$ | 0.0872** (0.023) | 0.8494** (0.025) |
| $E[Y^*(1)|aEE]$ | 2.033** (0.059) | 2.016** (0.061) |
| $E[Y^*(0)|nEE]$ | 2.033** (0.016) | 2.033** (0.017) |
| $E[Y^*(0)|cEE]$ | 1.972** (0.015) | 1.952** (0.016) |
| $\Pi_{Y}^{1}_{Y_{1}}$ & $\leq y_{1}^{11}$ | 2.429** (0.066) | 2.376** (0.057) |
| $\Pi_{Y}^{1}_{Y_{1}}$ & $\leq y_{1}^{11}$ | 1.676** (0.034) | 1.703** (0.035) |

**Bounds under monotonicity (Proposition 1):**

|                         | Entire sample | Non-Hispanics |
|-------------------------|---------------|---------------|
| $[LY_{1|cEE}, UY_{1|cEE}]$ | [1.951, 2.102] | [1.938, 2.113] |
| CLR 95% confidence interval | (1.921, 2.128) | (1.907, 2.140) |
| $[L_{cEE}, U_{cEE}]$ | [-0.022, 0.130] | [-0.014, 0.161] |
| CLR 95% confidence interval | (-0.061, 0.168) | (-0.057, 0.201) |

**Bounds under monotonicity and mean dominance (Proposition 2):**

|                         | Entire sample | Non-Hispanics |
|-------------------------|---------------|---------------|
| $[LY_{1|cEE}, UY_{1|cEE}]$ | [2.027, 2.102] | [2.026, 2.113] |
| CLR 95% confidence interval | (2.011, 2.129) | (2.008, 2.141) |
| $[L_{cEE}, U_{cEE}]$ | [0.055, 0.130] | [0.074, 0.161] |
| CLR 95% confidence interval | (0.023, 0.170) | (0.039, 0.202) |
| Number of observations | 9090 | 7529 |

**NOTE:** Outcome is measured at week 208 after randomization. Numbers in parentheses are standard errors. ** denotes estimate is statistically different from 0 at 5% level. Computations use design weights. Standard errors are calculated by a 5000-repetition bootstrap. Numbers in square brackets are half-median unbiased estimates of the bounds, and the numbers below them are CLR 95% confidence intervals, which contain the true value of the parameter with a given probability (see Section 2.5 for details). The definitions of $\alpha_{c,EE}$ and $\alpha_{c,NE}$ are provided in Proposition 1.
Tables 4 and 5, the positive region covered by the bounds on the effect of JC enrollment on wages for the JC on ln(wage) for the non-Hispanics, 0\(\Delta_1\) through 6 (Proposition 2). The first column presents the results for the entire sample, and the second shows the results for non-Hispanics. For both samples, the largest stratum is the \(cEE\) stratum, with an estimated proportion of almost 40%, while the stratum of always-employed always-takers (\(aEE\)) is the smallest one. All the estimated stratum proportions in Table 5 are statistically different from zero.

For the entire sample, the estimated lower and upper bounds on \(\Delta\) under Proposition 1 are \(-0.022\) and 0.130, respectively, while the corresponding numbers for non-Hispanics are \(-0.014\) and 0.161. Given the weak effects of JC on labor market outcomes for Hispanics, it is not surprising that the bounds for non-Hispanics cover a larger positive region than those for the entire population. For both samples, the estimated lower and upper bounds are larger than the corresponding bounds for the ITT effect in Table 4, especially the upper bound (e.g., for non-Hispanics, 0.119 versus 0.161, or a 35.3% increase). From Tables 4 and 5, the positive region covered by the bounds on the effect of JC enrollment on wages for the \(cEE\) stratum is larger than the positive region covered by the bounds on the ITT effect of JC on wages for the \(EE\) stratum. This suggests that the effects of JC on wages obtained by Lee (2009) were conservative, since the effect was weakened by noncompliance to the assigned treatment.

As in Lee (2009), we are unable to rule out a zero effect of JC on wages using only the monotonicity assumption of the effect of JC on employment. However, as before, our lower bounds are implicitly constructed under the implausible worst-case scenario of a perfect negative correlation between employment and wages, which is contradicted by standard economic models. The mean dominance assumption we use rules out this implausible extreme case and helps to increase the lower bound and to narrow the bounds. The last part of Table 5 shows results when the monotonicity and mean dominance assumptions are both used. In this case, the estimated lower bounds on the average effect of JC on ln(wage) for the \(cEE\) stratum are 0.055 and 0.074 for the entire and non-Hispanics samples, respectively, and the CLR-based 95% confidence intervals for the true value of \(\Delta\) exclude zero for both samples. Thus, under all six assumptions, our results imply positive average effects of JC on wages for the \(cEE\) stratum in both the entire and non-Hispanics samples. These results illustrate the identifying power of Assumption 6, and also reinforce the notion that the ITT effects of JC on wages are likely to be lower than the effect of JC enrollment on wages. Finally, note that, as discussed in Remark 2, in the absence of Assumption 5 the lower bound for \(\Delta\) in Proposition 2 can be interpreted as the lower bound for \(\beta = E[Y^*(1) - Y^*(0)|cEE, cNE]\) under Assumptions 1 through 4, and 5'.

### Table 6. Bounds for the effect of JC on ln(wage) for \(cEE\) adjusting for nonresponse

|                     | Entire sample       | Non-Hispanics       |
|---------------------|---------------------|---------------------|
| \(\pi_{cEE}\)      | 0.391** (0.009)     | 0.381** (0.010)     |
| \(\alpha_{cEE}\)   | 0.877** (0.022)     | 0.851** (0.024)     |

**Bounds under monotonicity (Proposition 1):**

\[\{L_{1,cEE}, U_{1,cEE}\}\]

CLR 95% confidence interval

\([-0.028, 0.117]\]

CLR 95% confidence interval

\((-0.065, 0.153)\]

**Bounds under monotonicity and mean dominance (Proposition 2):**

\[\{\tilde{L}_{1,cEE}, \tilde{U}_{1,cEE}\}\]

CLR 95% confidence interval

\[(2.013, 2.128)\]

CLR 95% confidence interval

\[(0.044, 0.117)\]

Number of observations

10520

8701

NOTE: Outcome is measured at week 208 after randomization. Numbers in parentheses are standard errors. ** denotes estimate is statistically different from 0 at 5% level. Computations use weights accounting for sample design, interview design and interview nonresponse. Standard errors are calculated by a 5000-repetition bootstrap. Numbers in square brackets are half-median unbiased estimates of the bounds, and the numbers below them are CLR 95% confidence intervals, which contain the true value of the parameter with a given probability (see Section 2.5 for details). The definition of \(\alpha_{cEE}\) is provided in Proposition 1.
average effect of JC enrollment on wages 4 years after random assignment for the always-employed compliers.

Finally, we compare these results to the ones in Frumento et al. (2012), who point identified the average effect of JC enrollment on wages, adjusting for sample selection, noncompliance, and missing outcomes, by imposing a different set of assumptions from the two sets we consider. Employing a different sample from ours, their results imply an effect on ln(wage) of about 0.038 for the cEE stratum. This point estimate is consistent with the estimated bounds for this effect presented in Table 6 for the entire population under our Assumptions 1 through 5 and, although it is below the estimated lower bound under Assumptions 1 through 6 (0.044), it is inside the corresponding 95% confidence interval. Thus, their point estimate of the effect of JC on wages for the cEE stratum is consistent with our bounds adjusting for nonresponse.

3.4 Conclusions From Empirical Analysis

We draw the following conclusions from our empirical analysis. First, our results strongly suggest a positive average effect of participating in JC on wages 4 years after random assignment for the always-employed compliers. For non-Hispanics, for whom the monotonicity assumption on the effect of JC on employment is more likely to hold, our estimated bounds under Assumptions 1 through 6 in Table 5 imply that participating in JC increases the average wage (as opposed to ln(wage)) of the always-employed compliers by between 7.7% and 17.5%. This evidence suggests that JC has an effect on participants’ earnings not only by increasing their probability of being employed but also by increasing their wages, which is most likely a consequence of their human capital accumulation during enrollment in JC.

Second, our analysis suggests that the results from the studies of the ITT effects of JC on wages in Lee (2009) and Blanco, Flores, and Flores-Lagunes (2013) are conservative because non-compliance is likely to dilute the effect of JC enrollment on wages. In particular, we find that for the two samples we consider, regardless of whether or not we employ Assumption 6, the positive region covered by the bounds on the effect of JC enrollment on wages for the cEE stratum is larger than the positive region covered by the bounds on the ITT effect of JC on wages for the EE stratum. This is consistent with the results presented in Section 3.1, which show that the LATE estimates of the effects of JC on other labor market outcomes not suffering from sample selection are larger than the corresponding ITT estimates. This conclusion is also consistent with that by Frumento et al. (2012), who found that their point estimate of the effect of JC enrollment on wages at week 208 for the cEE stratum is larger than the point estimate of the corresponding ITT effect for the EE stratum in Zhang, Rubin, and Mealli (2009).

4. SIMULATION

This section presents a simulation study aimed at shedding light on the performance of our bounds when some of our assumptions are violated, and when all our assumptions hold. In particular, we use two simulation designs to analyze violations (or near violations) of: (1) Assumption 5 (no cEN stratum); (2) Assumption 6 (mean dominance); (3) both Assumptions 5 and 6; (4) Assumption 3 (E[D(1) − D(0)] ̸= 0); and, (5) Assumption 4 (no defiers).

In order for our simulation designs to be close to situations found in empirical research, they mimic the characteristics of our entire sample (described in Section 3.1). Both of our designs share the following characteristics. Each simulated sample contains 9090 observations, with each observation receiving a randomly assigned value of Z = 1 with probability 5491/9090. The individual’s membership to principal strata is drawn from a uniform distribution, where the fraction of each stratum in the population is explained below. The observed treatment and employment statuses, D and S, are jointly determined by the membership to principal strata and Z. The individuals’ wages in each of the strata follow a lognormal distribution with variance equal to 0.2, and means equal to E[Y*(1)|cEE] = 2.04, E[Y*(0)|cEE] = 1.97, E[Y*(1)|aEE] = 2.035, E[Y*(0)|aEE] = 2.035, and E[Y*(0)|cEN] = 2. Thus, the true effect Δ is equal to 0.07. Those parameter values are chosen to be close to their corresponding estimated values, and to our estimates of Y^*_{nn}, based on our original sample (e.g., see Table 5).

The first design is used to analyze violations to Assumptions 5 and 6. It sets the true values of the stratum proportions to be close to their corresponding estimated values (and to our estimates of p_{ds}) based on our original sample as: π_{aNN} = 0.030, π_{aEE} = 0.015, π_{aNE} = 0.155, π_{cEE} = 0.395 − π_{cEN}, π_{cNN} = 0.26 − π_{cEN}, π_{cNE} = 0.04 + π_{cEN}. The total proportion of compliers (π_c) equals E[D(1) − D(0)] = 0.695, as in our original sample, and π_{cEN} ̸= 0 without Assumption 5. Finally, the individuals’ wages in the cEN stratum follow a lognormal distribution with mean E[Y*(0)|cEN] = 2 and variance 0.2.

Simulation 1 considers violations to Assumption 5, while mean dominance holds with E[Y*(1)|cEE] − E[Y*(1)|cNE] = 0.16 (chosen to be consistent with our original estimate of Y^*_{nn}). Figure 1(a) shows the average of the estimated lower and upper bounds over 1000 replications for values of π_{cEN} between 0 and 0.26 (outside this range some stratum proportions are negative). To give a sense about the variability of the estimated bounds, Figure 1(a) also reports the 95th percentile of the estimates of U_{cEE} from the 1000 replications, and the corresponding 5th percentiles of the estimates of L_{cEE} and U_{cEE}. For example, 95% of the estimates of L_{cEE} lie above the plotted squares, while 95% of the estimates of U_{cEE} lie below the plotted circles. For clarity, we omit the 5th and 95th percentiles for the upper and lower bounds, respectively (they are provided in the online Appendix).

Figure 1(a) reports that the means of the estimated bounds decrease as π_{cEN} increases. This happens because the estimates of E[Y*(0)|cEE] increase while the estimated bounds on E[Y*(1)|cEE] decrease. As π_{cEN} increases, the fraction of cEE individuals in the cell {Z = 0, D = 0, S = 1} falls while the fraction of cEN individuals increases. Since in our design the mean of Y*(0) for the cEN stratum is larger than that of the cEE stratum, Y^*_{nn} and thus our estimate of E[Y*(0)|cEE] increase (see Equation (4)). Something similar occurs in the
cell \( \{Z = 1, D = 1, S = 1\} \), where the fraction of \( cEE \) individuals decreases while the fraction of \( cNE \) individuals increases (since \( \pi_{cNE} = 0.04 + \pi_{cEN} \)), which combined with the mean of \( Y^*(1) \) being larger for the former than the latter individuals results in smaller bounds for \( E[Y^*(1)|cEE] \). The true effect is above the mean estimates of the lower bounds over the entire range of \( \pi_{cEN} \), and is above the mean estimates of \( U_{cEE} \) when \( \pi_{cEN} > 0.16 \).

Figure 1(b) shows the percentage of times out of the 1000 repetitions that the true effect is within the estimated bounds. For Proposition 1, this number gradually declines as \( \pi_{cEN} \) increases. For Proposition 2, it starts lower than for Proposition 1 because the true value of \( T_{cEE} \) when \( \pi_{cEN} = 0 \) is close to \( \Delta \). The estimated bounds cover \( \Delta \) in both propositions at least 80% of the time when \( \pi_{cEN} < 0.1 \). When \( \pi_{cEN} = 0 \) and all our assumptions hold, the means of the simulated bounds are \([-0.027, 0.136]\) in Proposition 1 and \([0.054, 0.136]\) in Proposition 2, which are similar to the empirical results for the entire sample in Table 5.

Simulation 2 considers violations of Assumption 6 when \( \pi_{cEN} = 0 \). Figure 2(a) presents results for different values of \( E[Y^*(1)|cEE] - E[Y^*(1)|cNE] \), where mean dominance is violated for negative values of this difference. As this difference increases, the means of the estimated bounds fall because larger values of the \( cNE \) members’ outcomes in the cell \( \{Z = 1, D = 1, S = 1\} \) are being replaced with smaller values, which results in smaller estimated bounds on \( E[Y^*(1)|cEE] \) (here the stratum proportions and \( E[Y^*(1)|cEE] \) are held fixed). While the true effect is within the mean estimated bounds in Proposition 1 over the entire range considered, it is below the mean of the estimates of \( L_{cEE} \) when Assumption 6 is violated. The reason is that, by construction, if this assumption is violated then \( E[Y^*(1)|cNE] \) provides an upper (rather than a lower) bound for \( E[Y^*(1)|cEE] \), and thus \( L_{cEE} \) provides an upper bound for \( \Delta \). Figure 2(b) reports that \( \Delta \) is within the estimated bounds in Proposition 1 in almost every replication. In contrast, the percentage of times \( \Delta \) is within the estimated bounds in Proposition 2 increases gradually as the values of \( E[Y^*(1)|cEE] - E[Y^*(1)|cNE] \) increase. At zero, the percentage is about 50%. However, note that the 5th percentile of the estimates of \( L_{cEE} \) is below the true value of \( \Delta \) for an important range of negative values of that difference.

Simulation 3 considers the case when both Assumptions 5 and 6 fail. Figure 3 shows plots similar to those in Figure 2 when \( \pi_{cEN} \) equals 0.05, 0.10 and 0.15. As \( \pi_{cEN} \) increases in Figures 3(a), 3(b), and 3(c), \( \Delta \) is within the mean estimated bounds in Proposition 1 over a shrinking range of \( E[Y^*(1)|cEE] - E[Y^*(1)|cNE] \). The intersection of the mean of the estimates of \( T_{cEE} \) with \( \Delta \) moves slightly leftward as \( \pi_{cEN} \) increases. In contrast to Figure 2(b), Figures 3(d), 3(e),
and 3(f) display a bell shape of the percentage of times \( \Delta \) is within the estimated bounds, with the bell covering a shrinking area as \( \pi_{cEN} \) increases. This implies that when Assumption 5 is violated, larger values of \( E[Y^*(1)|cEE] - E[Y^*(1)|cNE] \) tend to have a lower probability that the estimated bounds cover the true effect. The percentages for Proposition 1 in those figures are above those for Proposition 2, with the bell shape being centered at zero. Instead, the center for Proposition 2 is in the positive region and moves slightly left as \( \pi_{cEN} \) increases.

Next we examine the performance of our bounds when the commonly used IV Assumptions 3 and 4 are violated or close to be violated. To be able to vary the proportion of defiers (\( \pi_d \)) and the strength of the IV (i.e., the value of \( E[D(1) - D(0)] \)), we use a second design. Let \( \pi_n \) and \( \pi_d \) denote the proportions of never and always takers, respectively. Based on our original sample, the ratios between different strata are set as follows: \( \pi_n/\pi_a = 5.7, \pi_{aEE}/\pi_a = 0.3, \pi_{nEE}/\pi_a = 0.6, \pi_{cEE}/\pi_c = 0.559, \pi_{cNE}/\pi_c = 0.059, \pi_{dEE}/\pi_d = 0.5, \) and \( \pi_{dEN}/\pi_d = 0.1 \). The specific stratum proportions are obtained by setting the values of \( E[D(1) - D(0)] \) and \( \pi_d \) (since \( \pi_c = E[D(1) - D(0)] + \pi_d \)), where Assumption 5 implies \( \pi_{dNE} = 0 \). The individuals’ wages in the strata follow a lognormal distribution with variance 0.2 and means: \( E[Y^*(1)|dEE] = 2.02, E[Y^*(0)|dEE] = 1.99, E[Y^*(1)|dEN] = 1.85, \) and \( E[Y^*(1)|cNE] = 1.88 \) (the means of the other strata are the same as before).

Note that, since we use IV methods to address noncompliance, our bounds are likely to be affected by a weak IV, that is, one for which \( E[D(1) - D(0)] \) is close to 0. The reason is that the term \( \pi_{cEE} = p_{01|0} - p_{01|1} \), which is no greater than \( E[D(1) - D(0)] \), appears in the denominator in several of the expressions in Propositions 1 and 2 (see also, e.g., Equation (4)), and thus values of \( p_{01|0} - p_{01|1} \) close to 0 can lead to large variance and instability in our estimated bounds. The trimming proportions \( \alpha_{cEE} \) and \( \alpha_{cNE} \) are also negatively affected by small values of \( \pi_{cEE} \) and \( \pi_{cNE} \), leading to wider trimming bounds. Moreover, weak IVs are known to exacerbate the bias of IV estimators coming from the presence of defiers (AIR). In our setting, we can write the right-hand side of Equation (4) as \( E[Y^*(0)|cEE] \) plus a bias term equal to (details provided in the online Appendix): \( \pi_{dEE}(E[Y^*(0)|cEE] - E[Y^*(0)|dEE])/(\pi_{cEE} - \pi_{dEE}) \). A weak IV implies a small value of the denominator and thus a greater bias coming from the presence of defiers. As a result, our simulation when Assumption 4 fails considers different IV strengths.

Simulation 4 analyzes cases when Assumption 4 holds but Assumption 3 is violated or close to be violated. Figure 4 shows results when we vary the proportion of compliers (\( \pi_c \)), which equals \( E[D(1) - D(0)] \) when \( \pi_c = 0 \), from 0 to 0.7. This range covers the estimated value of \( \pi_c \) in our original sample (0.695). Because of the large variation in our estimated bounds when \( \pi_c \) is very close to zero, we present the results in two sets of graphs. The first set varies \( \pi_c \) from 0 to 0.05, and the second varies \( \pi_c \) from 0.05 to 0.7 (note the change of scale in the y-axis of those figures). For reference, we refer to these two cases as “weak IV” and “stronger IV,” respectively. Figure 4(a) shows that there is substantial variation and instability in our estimated bounds when \( \pi_c \) is close to zero. Moreover, the closer \( \pi_c \) is to zero, the more likely we are to have replications in which the estimated lower bound is above the estimated upper bound or to have some negative estimated stratum proportions (not shown in figures). The poor performance of our bounds in such cases...
is also reflected in Figure 4(c) with a very small probability that the estimated bounds cover the true effect. Importantly, Figures 4(b) and 4(d) show that, as \( \pi_c \) moves away from zero, the performance of our bounds improve significantly. Note also from Figures 4(c) and Figures 4(d) that, except for very low values of \( \pi_c \), the percentage of times \( \Delta \) is within the estimated bounds in Proposition 1 is greater than that for the estimated bounds in Proposition 2.

Simulation 5 analyzes the case when Assumption 4 is violated for different values of \( E[D(1) - D(0)] \): 0.695, 0.4, and 0.02. For reference, we label these cases strong, moderate, and weak IV, respectively. Figure 5 shows results for those three cases.
as we vary \( \pi_d \) (whose possible values depend on \( E[D(1) - D(0)] \)). Figure 5(a) reports that the true effect is within the mean estimated bounds in Proposition 2 for \( \pi_d \leq 0.15 \) with a strong IV, and for \( \pi_d \leq 0.1 \) with a moderate IV. For the strong and moderate IV cases, the true effect is within the mean estimated bounds in Proposition 1 for all the values of \( \pi_d \) considered in each case. As before, Figure 5(c) shows that there is a lot of instability in our bounds when the IV is weak. The graphs at the bottom of Figure 5 show that the percentage of times the estimated bounds cover the true effect decreases as \( \pi_d \) increases. They also illustrate the importance that the strength of the IV has on the performance of our bounds, as the percentages decrease dramatically as the IV becomes weaker.

In general, we conclude from the simulation exercises above that our bounds seem to be relatively robust to small violations of Assumptions 5 and 6. Our results also imply that the strength of the IV affects the sensitivity of our bounds to Assumption 4: with a strong IV (which is the case in our JC application) our bounds are relatively robust to the presence of defiers. The simulations also illustrate an important tradeoff between the bounds in Propositions 1 and 2: while the estimated bounds in Proposition 2 are narrower than those in Proposition 1, the probability that the estimated bounds in Proposition 2 cover the true effect is usually lower, especially when some of our assumptions are violated. This is an important consideration when employing our bounds in practice. Finally, our results show that our bounds perform poorly when the IV is weak, thus warning researchers against their use in such situations. This is not surprising given the poor performance of standard IV estimators with weak IVs (e.g., Staiger and Stock 1997).

5. CONCLUSION

We derive nonparametric bounds for average treatment effects in the presence of both sample selection and noncompliance under relatively weak assumptions, and employ these bounds to empirically assess the wage effects of participating in Job Corps (JC). The results from a simulation study suggest that our bounds are relatively robust to small violations of our assumptions, but warn against their use when the instrument employed to address noncompliance is weak.

The first contribution of the article is to extend the bounds derived by Zhang, Rubin, and Mealli (2008) and Lee (2009), which address only sample selection, to settings where noncompliance is also present. More generally, our bounds can be used in settings where two identification issues are present and there is an IV to address one of them. For example, our methods could be applied to analyze time-to-event outcomes (which are usually right censored) when another identification issue is present. In the context of JC, an important question is whether enrollment in JC has an effect on its participants’ unemployment spells, which are right censored because not all individuals find employment before the end of the study. In this case, our methods would provide information about the effect of JC on unemployment spells for those compliers who would always find a job before the end of the study whether or not they enrolled in JC. Moreover, the bounds developed by Zhang, Rubin, and Mealli (2008) and Lee (2009) are becoming increasingly used in the partial identification literature to address other problems. For instance, Huber and Mellace (2011) employed those bounds to test implications of the exclusion restriction assumption in just-identified models, while Flores and Flores-Lagunes (2010) used those bounds to construct bounds on direct and indirect effects. The bounds derived in this article can be employed to extend the results in those papers to address more than one complication. In addition, the methods herein can be combined with those in Huber and Mellace (2013) to construct bounds on the average effect for subpopulations other than the \( \text{eEE} \) stratum (e.g., for the treated and selected individuals, who have \( D = 1 \) and \( S = 1 \)).

The second contribution of the article is to study the wage effects of JC. Our results strongly suggest a positive average effect of participating in JC on wages 4 years after randomization for the always-employed compliers. The results also point to larger positive wage effects of JC than those found without adjusting for noncompliance in Lee (2009) and Blanco, Flores, and Flores-Lagunes (2013). Our findings suggest that JC has positive effects not only on the employability of its participants but also on their wages, implying that JC is likely to have positive effects in their human capital. Therefore, it is very important to consider the potential benefits of JC and other training programs on wages when evaluating their effects.

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