The Effects of Job Corps Training on Wages of Adolescents and Young Adults

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Schochet et al. (2001) report an extensive set of findings from an experimental evaluation of Job Corps, America’s largest job training program targeting youths. One of the interesting findings is that the impact of Job Corps on post-program earnings differs markedly between younger (aged 16-19) and older (aged 20-24) participants. The older group experienced larger average effects on weekly earnings four years after randomization (27 percent) than the younger group (13 percent).\footnote{These effects were computed by us with a sample that differs from that used by Schochet et al. (2001) in two aspects. First, we exclude the group of Hispanics for reasons described in the next section; second, we combine their 16-17 and 18-19 groups into a single group to simplify the subsequent analysis. None of these sample differences results in significant qualitative differences in the estimates.} Schochet et al. (2008) point out that these disparate effects are consistent with the 10 percent higher length of enrollment and larger gains in academic and vocational training by older youth in the program. These explanations point towards a larger human capital (HC) accumulation by the older group. The evaluation based on weekly earnings, however, does not directly assess whether the program had a differential effect on the wages of these subgroups, which would correspond more closely with the notion of differential HC accumulation. If the disparate effects on earnings are mainly due to the differential accumulation of HC, it is expected that the difference in the impact of JC on wages between those two groups is substantially higher than the one on earnings. If corroborated, this can have important implications for how the program could be improved to better serve younger participants. Unfortunately, estimation of the program’s effect on wages is not straightforward due to sample selection: wages are observed only for those individuals who are employed.

We empirically assess the effect of Job Corps (JC) on wages for two groups of eligible participants: adolescents (16 to 19 years of age) and young adults (20 to 24 years of age). We employ recently developed nonparametric bounds for average and quantile treatment effects that account for sample selection, which typically require weaker assumptions than those conventionally employed for point identification (e.g., Lee, 2009). Two monotonicity
assumptions are employed. The first states individual-level weak monotonicity of the effect of the program on employment. This assumption was also used by Lee (2009) and Zhang et al. (2008) to bound average wage effects. The second assumption is on mean potential outcomes across strata, which are subpopulations defined by the potential values of the employment indicator as a function of treatment assignment. Bounds on quantile treatment effects, which employ a similar set of assumptions, are based on results by Imai (2008) and Blanco, Flores and Flores-Lagunes (2012, BFF hereafter).

Our empirical results suggest that JC has a positive average effect on wages four years after randomization for both groups. The average effect, which is statistically significant for both groups, is bounded between 5 and 10.4 percent for adolescents, and between 4 and 12.3 percent for young adults. Our estimated bounds for this effect are similar and relatively tight for both groups, implying that the difference in impacts from HC accumulation are not dramatically larger than the difference in earnings impacts. In particular, they imply that the average wage effect of JC for young adults is at most $2.5 (=12.3/5)$ times higher than that of adolescents, which is not sharply different from the $2.1 (=27/13)$ times higher effect in average earnings for young adults relative to that of adolescents. We also characterize the heterogeneous impact of JC training at different points of the wage distribution and note that statistically significant quantile treatment effects are bounded between 1.9 and 20 percent for adolescents and between 5.1 and 13 percent for young adults. For the former group, we also note that the statistical significant effects are evident across the majority of the quantiles analyzed, while for the latter the statistically significant effects are concentrated in the middle of their wage distribution.

1 Job Corps and the National Job Corps Study

JC is America’s largest and most comprehensive education and job training program. It is federally funded and currently administered by the US Department of Labor. With a yearly cost of about $1.5 billion, JC enrollment ascends to 100,000 students (US Department
of Labor, 2010). The program’s goal is to help disadvantaged youths, ages 16 to 24, improve the quality of their lives by enhancing their labor market opportunities and educational skills set. Eligible participants receive academic, vocational, and social skills training at over 123 centers nationwide, where they typically reside.

During the mid nineties, the US Department of Labor funded the National Job Corps Study (NJCS) to determine the program’s effectiveness. A sample of individuals from nearly all JC’s outreach and admissions agencies located in the 48 continuous states and the District of Columbia was randomly assigned to treatment and control groups. From a randomly selected research sample of 15,386 first time eligible applicants, approximately 61 percent were assigned to the treatment group (9,409) and 39 percent to the control group (5,977), during the sample intake period from November 1994 to February 1996. After recording their data through a baseline interview, a series of follow up interviews were conducted at weeks 52, 130, and 208 after randomization (Schochet et al., 2001). Due to the presence of non-compliance (e.g., about 27 percent of treatment-group individuals did not enroll into the program), the average and quantile effects we estimate below have the interpretation of “intention to treat” effects.

Our sample is restricted to individuals who have non-missing values for weekly earnings and weekly hours worked for every week after random assignment. Our sample does not include Hispanics, since, as discussed in detail by BFF, the individual-level monotonicity assumption we employ below is likely not satisfied by this group, and bounds that do not use this assumption are largely uninformative. Our non-Hispanic sample contains 7,573 individuals. Finally, we employ the NJCS design weights throughout the analysis, since different subgroups in the population had different probabilities of being included in the research sample.

2 Bounds on Average Treatment Effects

Let the potential employment values be $S_i(0)$ and $S_i(1)$ when $i$ is assigned to control
\((T_i = 0)\) and treatment \((T_i = 1)\), respectively. We can partition the population into four subpopulations (strata) based on the values of \(\{S_i(0), S_i(1)\}\). Similar to Lee (2009), Zhang et al. (2008), and BFF, we focus on the average effect of a program on wages for individuals with \(\{S_i(0) = 1, S_i(1) = 1\}\), i.e., those who would be employed regardless of treatment status \((EE\) stratum hereafter). This stratum is the only one for which wages are observed under both treatment arms, and thus fewer assumptions are required to construct bounds for its effects.\(^2\) The average treatment effect for this stratum is:

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

where \(Y_i(1)\) and \(Y_i(0)\) are the potential wages for unit \(i\) under treatment \((T_i=1)\) and control \((T_i=0)\), respectively. In addition to the assumption of a randomly assigned treatment (Assumption 1), which holds by design, we maintain:

**Assumption 2. Individual-Level Weak Monotonicity of \(S\) in \(T\): \(S_i(1) \geq S_i(0)\) for all \(i\).**

This assumption states that treatment assignment weakly affects selection in one direction, effectively ruling out those with \(\{S_i(0) = 1, S_i(1) = 0\}\).\(^3\)

Assumptions 1 and 2 allow the point identification of \(E[Y_i(0)|EE]\) in (1) as \(E[Y_i|T_i = 0, S_i = 1]\) since those control individuals with observed wages belong to the \(EE\) stratum. However, it is not possible to point identify \(E[Y_i(1)|EE]\), since the observed group with \((T_i, S_i) = (1, 1)\) is a mixture of individuals from two strata, \(EE\) and those with \(\{S_i(0) = 0, S_i(1) = 1\}\), i.e., those who are employed only if treated \((NE\) hereafter). Nevertheless, \(E[Y_i(1)|EE]\) can be bounded. The proportion of \(EE\) individuals in \((T_i, S_i) = (1, 1)\) can be point identified as \((p_{10}/p_{11})\), where \(p_{st} = Pr(S_i = s|T_i = t)\) for \(t, s = 0, 1\). Therefore, \(E[Y_i(1)|EE]\) can be bounded from above by the expected value of \(Y_i\) for the \((p_{10}/p_{11})\)

\(^2\)In our application, the \(EE\) stratum is the largest, accounting for about 60 percent of the sample.

\(^3\)Lee (2009), Zhang et al. (2008), and BFF employed this assumption, and similar assumptions are widely used in the instrumental variable (Imbens and Angrist, 1994) and partial identification literatures (Manski and Pepper, 2000; Bhattacharya et al., 2008; Flores and Flores-Lagunes, 2010). A testable implication of Assumption 2 (Imai, 2008) holds in the context of JC. See BFF for more details.
fraction of the largest values of $Y_i$ in the observed group $(T_i, S_i)=(1, 1)$. In other words, the upper bound is obtained under the scenario that the largest $(p_{1|0}/p_{1|1})$ values of $Y_i$ belong to the EE individuals. An analogous procedure using the smallest values of $Y_i$ yields a lower bound for $E[Y_i(1)|EE]$. Bounds for $(\Pi)$ are obtained by combining the bounds for $E[Y_i(1)|EE]$ with the point identified term $E[Y_i(0)|EE]$. Lee (2009) shows that these trimming bounds are sharp.

In addition, we consider the following assumption.

**Assumption 3.** Weak Monotonicity of Mean Potential Outcomes Across the EE and NE Strata: $E[Y(1)|EE] \geq E[Y(1)|NE]$.

Intuitively, this assumption formalizes the notion that the EE stratum is likely to be comprised of more “able” individuals than those belonging to the NE stratum. Since “ability” is positively correlated with labor market outcomes, one would expect wages for the individuals who are employed regardless of treatment status (the EE stratum) to weakly dominate on average the wages of those individuals who are employed only if they receive training (the NE stratum).\[4\] Adding Assumption 3 implies $E[Y_i|T_i = 1, S_i = 1] \leq E[Y_i(1)|EE]$. Thus, the lower bound for $ATE_{EE}$ becomes: $E[Y_i|T_i = 1, S_i = 1] - E[Y_i|T_i = 0, S_i = 1]$. Imai (2008) shows that these bounds are sharp.

Lee (2009) shows that the trimming bounds under Assumptions 1 and 2 can be narrowed using a pre-treatment covariate $X$. Formally, let $X$ take values on $\{x_1, \ldots, x_J\}$. By the law of iterated expectations, we can write the non-point identified term in $(\Pi)$ as: $E[Y_i(1)|EE] = EX\{E[Y_i(1)|EE, X_i = x_j]|EE\}$. It is straightforward to construct bounds on the terms $E[Y_i(1)|EE, X_i = x_j]$ for the different values of $X$ by implementing the trimming bounds on $E[Y_i(1)|EE]$ within cells with $X_i = x_j$, which are then averaged over cells. The result

\[4\]In the context of JC, BFF indirectly gauged the plausibility of Assumption 3 by comparing the average of pre-treatment covariates that are highly correlated with wages between the EE and NE strata. They find support for this assumption.
that these bounds are narrower follows from the properties of trimmed means, and thus it
is applicable only to bounds that involve trimming.

3 Bounds on Quantile Treatment Effects

Imai (2008) extended the results presented in the previous section to construct bounds
on quantile treatment effects (QTE). The parameters of interest are differences in the
quantiles of the marginal distributions of the potential outcomes for the EE stratum; more
specifically, define the $\alpha$-quantile effect for the EE stratum as:

$$QTE_{EE}^\alpha = F_{Y_{i(1)}|EE}^{-1}(\alpha) - F_{Y_{i(0)}|EE}^{-1}(\alpha),$$

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

Similar to the $ATE_{EE}$ case, under Assumptions 1 and 2 the last term in (2) is point
identified from the cumulative distribution function (CDF) of individuals’ wages conditional
on $(T_i, S_i) = (0, 1)$, say $F_{Y_{i|T_i=0,S_i=1}}(\cdot)$, while the first term is partially identified by trimming
the CDF of $Y_i$ in $(T_i, S_i) = (1, 1)$ based on the proportion $(p_{1|0}/p_{1|1})$.

The trimming bounds for $QTE_{EE}$ can be tightened by strengthening Assumption 3. Let
$F_{Y_{i(1)}|EE}(\cdot)$ and $F_{Y_{i(1)}|NE}(\cdot)$ denote the CDFs of $Y_i(1)$ for individuals who belong to the EE
and $NE$ strata, respectively:

**Assumption 4. Stochastic Dominance:** $F_{Y_{i(1)}|EE}(y) \leq F_{Y_{i(1)}|NE}(y)$, for all $y$.

This assumption directly imposes restrictions on the distribution of potential outcomes
under treatment for individuals in the $EE$ stratum, which results in a tighter lower bound.

Adding Assumption 4 results in sharp bounds (Imai, 2008), where the lower bound is now
the untrimmed difference: $F_{Y_{i|T_i=1,S_i=1}}^{-1}(\alpha) - F_{Y_{i|T_i=0,S_i=1}}^{-1}(\alpha)$.

BFF proposed a way to use a pre-treatment covariate $X$ taking values on $\{x_1, \ldots, x_J\}$
to narrow the trimming bounds on $F_{Y_{i(1)}|EE}^{-1}(\alpha)$ and, thus, on $QTE_{EE}^\alpha$. The idea is similar
to that in Lee (2009), although the non-linear form of the quantile function $F_{Y_{i(1)}|EE}^{-1}(\alpha)$
prevents from directly using the law of iterated expectations. To circumvent this difficulty,
they employ the CDF of $Y_i(1)$ for the stratum $EE$ at a given point $\bar{y}$, $F_{Y_{i(1)|EE}(\bar{y})}$, and
write it as the mean of an indicator function, which allows the use of iterated expectations.

Using this insight, one can write: \( F_{Y_i(1)|EE}(\tilde{y}) = E[1[Y_i(1) \leq \tilde{y}]|EE] = E_X\{E[1[Y_i(1) \leq \tilde{y}]|EE, X_i = x_j]|EE\}. \) Using the indicator \( 1[Y_i(1) \leq \tilde{y}] \) as the outcome instead of \( Y_i(1) \) allows the application of the trimming bounds from Section 2 within cells with \( X_i = x_j \).

This approach is applied to several values of \( \tilde{y} \) to construct bounds for the entire CDF \( F_{Y_i(1)|EE}(\tilde{y}) \). These bounds are then inverted to obtain bounds on the quantiles of interest.

The formal expressions for the bounds described herein, along with greater detail about their implementation, can be found in BFF.

4 Empirical Results

The outcome we consider is log wages at week 208 after randomization. We present results based on imposing all our assumptions and use earnings in the year prior to randomization (which is highly correlated with the outcome) as a covariate \((X)\) to narrow the bounds. For each age-group, we split the sample into 3 groups based on the values of \( X \), each containing roughly the same number of observations. Then, bounds were computed for each group and averaged across groups, weighting by \( \Pr(X = x_j|EE) \).

Table 1 reports the estimated bounds on the \( ATE_{EE} \) for the two age-groups of interest: adolescents and young adults. For adolescents the average effect of JC on wages is bounded between 0.05 and 0.104, while for young adults is bounded between 0.04 and 0.123. Bounds for the latter group allow for slightly larger effects but also for slightly smaller effects; overall the difference in the bounds’ width between groups is not large. The bottom row of the table reports a 95 percent confidence interval for the true parameter value (Imbens and Manski, 2004, IM hereafter). These intervals suggest that the average wage effect is statistically different from zero for both groups.

The estimated bounds on \( QTE_{EE}^{\alpha} \) along with their corresponding IM confidence intervals, are shown in Figure 1.\(^5\) Similar to the estimated bounds on the \( ATE_{EE} \) in Table 1, the

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\(^5\) The complete numerical results are shown in the Appendix.
estimated bounds on $QTE_{EE}^{\alpha}$ do not include zero for all the quantiles considered. However, based on the IM confidence intervals, some of the quantile treatment effects are not statistically different from zero. The estimated bounds on $QTE_{EE}^{\alpha}$ for adolescents statistically exclude zero for the majority of analyzed quantiles, except for the effects at the following quantiles: 0.15, 0.25, 0.45, 0.85, and 0.90. An interesting trend is observed for the group of young adults, where the statistically significant quantile treatment effects are concentrated around the center of their wage distribution (except for quantile 0.55). Statistically significant quantile treatment effects are bounded between 1.9 and 20 percent for adolescents and between 5.1 and 13 percent for young adults.

5 Conclusions

We empirically assess the effect of JC on wages for adolescents (16 to 19 years of age) and young adults (20 to 24 years of age). These two groups were found to have disparate program effects on weekly earnings by Schochet et al. (2001) and Schochet et al. (2008), which were conjectured to be due to a differential accumulation of human capital (HC) within the program. One way to assess the validity of this conjecture is to estimate the effect of the program on wages, which are more closely related to HC improvements. If the disparate earnings effects are mainly due to the differential accumulation of HC, we would expect to see larger differences in the estimated wage effects between the two groups. The estimated nonparametric bounds for average and quantile treatment effects of JC on wages that account for selection into employment suggest that, while these effects are positive for both groups, the difference in rewards from HC accumulation between the two groups is not dramatically different from the difference in earnings rewards. An implication is that differential HC accumulation may not completely explain the previously found differential effect on earnings and that, as a result, the differential effect of the program on the employment probability of these two groups may also play a significant role. Given the importance of improving the effectiveness of the program on all subgroups it serves, additional research shedding light on
these issues is needed.

REFERENCES


Table 1. Bounds on average treatment effect of the EE stratum for log wages at week 208, by age groups, under assumptions 1, 2, and 3, using earnings in the year prior to randomization as a covariate to narrow the bounds.

<table>
<thead>
<tr>
<th></th>
<th>Adolescents (aged 16-19)</th>
<th>Young adults (aged 20-24)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Upper bound</td>
<td>0.104</td>
<td>0.123</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.025)</td>
</tr>
<tr>
<td>Lower bound</td>
<td>0.050</td>
<td>0.040</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>Width</td>
<td>0.054</td>
<td>0.083</td>
</tr>
<tr>
<td>95 percent Imbens and</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Manski (2004) confidence interval</td>
<td>[0.024, 0.133]</td>
<td>[0.004, 0.164]</td>
</tr>
<tr>
<td>Sample size</td>
<td>5505</td>
<td>2068</td>
</tr>
</tbody>
</table>

Note: Bootstrap standard errors in parentheses (based on 500 replications).

Figure 1. Bounds and 95 percent Imbens and Manski (2004) confidence intervals for QTE of the EE stratum by age groups, under Assumptions A, B and D, using earnings in the year prior to randomization as a covariate to narrow the bounds. Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical lines.