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WHO BENEFITS FROM JOB CORPS? A DISTRIBUTIONAL ANALYSIS OF AN ACTIVE LABOR MARKET PROGRAM

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SUMMARY

Using recently developed econometric techniques to estimate quantile treatment effects (QTE) and experimental data, we examine the impact of Job Corps on earnings distribution. Our results indicate a great deal of heterogeneity in the effects of Job Corps. The QTEs show an increasing pattern along the earnings distribution, with much more pronounced differences at the upper quantiles for males, whites, and ages 20–24. Moreover, we find the QTEs to be very small at quantiles below the median for males, ages 16–17 and 18–19, and non-resident students. We propose strong economic conditions and skill hypotheses to explain the heterogeneity observed over the earnings distribution. Copyright © 2013 John Wiley & Sons, Ltd.

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1. INTRODUCTION

Evaluating the effectiveness of active labor market programs has been of great interest to both economists and policymakers. Since the seminal paper by Ashenfelter (1978), researchers have utilized various methods and several different data types to estimate the causal effects of these programs on labor market outcomes. The evaluations were predominantly limited to estimating the mean impact (Heckman *et al*., 1999).[1] In the last decade or so, however, the program evaluation literature has witnessed a surging interest in the effects at the distributional level. This trend follows from the fact that many interesting questions regarding the political economy of any program and the distribution of its benefits require knowledge of the distribution, and relying solely on the mean impact may mask important features of the big picture. The most common way to address distributional concerns is to estimate mean impacts for judiciously chosen subgroups of the population. This approach, however, does not reveal much if the heterogeneity stems from intra-group variation rather than inter-group variation (Bitler *et al*., 2006, 2008).

In one of the pioneering studies examining heterogeneous response to active labor market programs, Heckman *et al*. (1997) estimate the distributional impacts of a job training scheme funded under Title II-A of the US Job Training Partnership Act (JTPA) and find strong evidence in favor of impact heterogeneity along the earnings distribution. Similarly, Abadie *et al*. (2002) and Frölich and Melly (2010, 2013) analyze the same program in an instrumental variable framework and find non-negligible differences in the effects along the distribution. Furthermore, the distributional analysis of program impacts is not limited to job training. In a recent paper, Bitler *et al*. (2006) find considerable heterogeneity in the program response for the Job First welfare waiver in Connecticut. Bitler *et al*. (2008) use a

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[1] Indeed, almost all of the papers surveyed in Heckman *et al*. (1999) focus only on the mean impact either for all participants or for a given demographic subgroup (see, for example, LaLonde, 1986; Heckman and Hotz, 1989; Dehejia and Wahba, 1999; Smith and Todd, 2001, 2005).

similar analysis for estimating the distributional impact of the Self Sufficiency program in Canada and once again find evidence for significant heterogeneity. Friedlander and Robins (1997) analyze the impact of employment training in early welfare reform experiments on distribution of earnings, while Koenker and Bilias (2001) examine the impact of a re-employment bonus on unemployment durations.

Considering the significant heterogeneity in the impacts of active labor programs, as well as welfare programs, throughout the distribution, it is worth noting that Job Corps, the USA's largest and most costly active labor market program targeting at-risk youth, has not attracted much attention in the distributional sense.[2] Job Corps was established in 1964 under the Economic Opportunity Act with the purpose of providing assistance to disadvantaged youths aged 16–24. Since its establishment, Job Corps has provided comprehensive services to more than 2.6 million at-risk young people and helped prepare them for the labor market. In 2008, Job Corps enrolled more than 60,000 new students nationwide at a cost of more than $1.5 billion (Job Corps Annual Report, 2008). Given the longevity of the program and it is importance to at-risk young adults and society in general, only a few studies examine the effectiveness of Job Corps even at the mean level (see, for example, Flores-Lagunes *et al*., 2010; Lee, 2009; Mallar *et al*., 1982; Schochet *et al*., 2001, 2008).[3]

In this paper, using experimental data from the follow-up surveys of applicants, we contribute to the research on Job Corps by examining the program's impact on earnings distribution. To do so, we utilize two recently developed econometric techniques. The first one follows Firpo (2007) and implements a two-step estimation. In the first step, we estimate propensity scores to adjust for differences in the baseline covariates of Job Corps eligible applicants.[4] We then solve two separate minimization problems: one for the treatment group and one for the control group. The difference in the solutions to these two problems yields the quantile treatment effect (QTE) for eligible Job Corps applicants—often referred to as the intention-to-treat (ITT) effect. The second technique uses the instrumental variables strategy proposed in Frölich and Melly (2010, 2013) to estimate the QTEs for actual Job Corps participants (compliers).[5] This strategy is an extension of Firpo (2007), where the consistency of the estimates is based on selection on observables. As discussed in more detail in section 3, Frölich and Melly (2010, 2013) incorporate an instrumental variable framework in the calculation of propensity score weights and use these weights in the second step to solve the minimization problems. To our knowledge, we are the first to employ an unconditional instrumental quantile estimation technique in the program evaluation literature. In addition to estimating the QTEs, we also perform tests of equality to summarize the comparison of earnings cumulative density functions (CDFs) between the treatment and control groups using the procedure proposed in Abadie (2002).

Our results indicate a great deal of heterogeneity in the effects of Job Corps across participants (and eligible applicants) such that the QTEs show an increasing pattern along the earnings distribution. This finding holds for the full sample and for almost all subgroups, with much more pronounced differences at the upper quantiles for males, whites, and ages 20–24. Moreover, we find the QTEs to be very small or exactly zero at quantiles below the median for males, ages 16–17 and 18–19.[6] We propose strong economic conditions and skill hypotheses as potential explanations for the ineffectiveness of the program at the lower quantiles. The former explanation hinges upon the strong economic performance

[2] In a recent working paper, Blanco *et al*. (2011) examine the effects of Job Corps on wages by constructing bounds throughout the entire wage distribution.

[3] This is partly because it is difficult to obtain a credible control group for the participants since Job Corps is a highly targeted program.

[4] We opt to include the baseline covariates to control for sample/survey design, as well as to increase efficiency, as we explain in more detail in Section 2.3.

[5] It is important to note that this paper only examines the QTEs. Quantiles or other features of the treatment effect distribution is beyond the scope of this paper.

[6] We must note that zero QTEs (and standard errors) for the lower tail of the earnings distribution in our results primarily reflect the fact that some members of treatment and control groups are unemployed in the second year of the post-program period.

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observed in the era of the experiment (late 1990s) in the USA, while the latter follows from the relatively low skill endowment for some subgroups. Finally, with the exception of Hispanics, the equality of the earnings CDFs between the treatment and the control units is rejected for all subgroups. Several robustness checks support our findings. These findings have significant policy implications and we briefly discuss these implications in concluding the paper.

2. BACKGROUND AND RESEARCH SAMPLE

2.1. The Job Corps Program

Job Corps was established in 1964 under the Economic Opportunity Act with the purpose of providing assistance to disadvantaged youths aged 16–24. It is an intense program with more than 110 centers throughout the USA and offers academic, vocational, and social training, as well as health care, counseling, and job placement services. To be eligible for Job Corps, applicants must meet several criteria including, but not limited to, age, poverty status, citizenship, need for additional education and training, and mental stability. Outreach and admissions agencies (e.g. non-profit firms, state employment agencies) conduct the screening and recruitment process for Job Corps. Once found eligible, the youths are assigned to Job Corps centers, most of which are operated by private contractors. With the help of counselors at the centers, participants develop individualized, self-paced programs based on their needs (i.e. remedial education) and preferences (i.e. vocational training). Each year, Job Corps serves around 60,000 new enrollees for an average of 8–9 months, at a total cost of more than $1.5 billion (Job Corps Annual Report, 2008).

Schochet *et al*. (2008) summarize three distinctive features of Job Corps compared to other active employment and training programs. First, the majority of the participants (over 80%) reside at a center while training. Second, Job Corps is more comprehensive and, unlike other Workforce Investment Act (WIA) training programs, it offers academic and vocational training along with a wide range of support services associated with residential living (e.g. health care).[7] Finally, Job Corps is administered by the Department of Labor through contracts with corporations and non-profit organizations, and inter-agency agreements with the US Department of Agriculture for the operations of training centers and not directly by local authorities, which brings greater uniformity to the program.[8] Of course, these distinctive features of Job Corps come at a significant cost. In 2007, Job Corps cost around $26,000 per participant. This cost is roughly ten times more than the cost of a typical WIA program for adults (Green Book, 2008).

2.2. Previous Literature

The early papers use non-experimental data to evaluate the impact of Job Corps training on earnings. Gay and Borus (1980) use data on the training cohorts of 1969–1972 Job Corps and find that Job Corps training was associated with positive earnings gains for black males, while the program had negative impacts on the earnings of black females, white males, and white females. Using the 1977 Job Corps cohort and focusing on the 4 years of post-program earnings data, Mallar *et al*. (1982) observe increased earnings for male participants of about $2000 per year. Needless to say, the use of non-experimental data raises some concerns regarding the validity of these findings.

[7] WIA, which was enacted in 1998, replaced JTPA with Title I of WIA, Workforce Investment Systems on 1 July 2000. See LaLonde (2003) for an excellent account of WIA and its differences from JTPA.

[8] For the administration of other training programs under WIA, the role of local authorities is much more pronounced. For example, under WIA's adult and youth training programs other than Job Corps, federal government allocates funds to states, which, in turn, allocate at least 85% of the funds to local workforce investment boards (Green Book, 2008).

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More credible evidence on the impact of Job Corps comes from the National Job Corps Study (NJCS), a randomized experiment conducted by Mathematica Policy Research. Schochet *et al.* (2008) report the impact on earnings based on the experimental data collected by Mathematica researchers. In the NJCS data, the estimated impact per Job Corps participant is $22 per week (or $1150 per year), which translates into a 12% earnings gain at the 48-month interview. Examination of the subgroups in the survey data also reveals significant inter-group variation. Specifically, the annual earnings gains are the largest for whites among racial groups and for the oldest youths among age groups. Moreover, earnings gains 4 years after randomization are greater for males than females. The authors also use two forms of administrative data on earnings: summary earnings records reported to Internal Revenue Service by employers and, for 22 states, unemployment insurance wages reported by employers to state agencies. It must be noted that the estimated program impact using these datasets is different from the program impact estimated from the survey data. In particular, the estimated earnings impact of Job Corps is smaller in the administrative data. Moreover, further examination of the mean impacts of Job Corps using the administrative data suggests that the earnings gains are not persistent except for the members of the oldest youth group, who seem to retain a significant portion of their gains between years 5 and 9 (1999–2003). However, Schochet *et al.* (2008, p. 1882) state that 'It is difficult to assess which data source provides more accurate information. Reported earnings levels for the Job Corps sample are nearly double in the survey data, suggesting that considerable amounts of earnings are not captured in the tax data. This pattern emerges across broad groups of youths defined by their demographic and job characteristics, and the undercount appears to be especially large for those in short-term casual jobs that offer low wages and few fringe benefits. On the other hand, survey-based earnings measures appear to be biased upward …'.

There are also a few other papers using the same publicly available NJCS data. Lee (2009) provides a bounding approach to deal with the potential sample selection issues in using wages and to estimate the average treatment effect. Lee (2009) finds a positive association between Job Corps enrollment and wages, and concludes that the earnings increases are not driven solely by higher labor force participation among Job Corps members. Flores-Lagunes *et al.* (2010) examine the impacts of Job Corps on Hispanics and incorporate the local labor market condition variables in explaining the (absence of) earnings gains for Hispanics from Job Corps. Finally, Flores *et al.* (2012) estimate a dose–response function to assess the effectiveness of Job Corps among participants by exploiting the different lengths of exposure to the training program (i.e. actual number of hours the participants received academic and vocational training). The authors find that the effects increase with the length of exposure.

2.3. Research Sample

As noted, the Department of Labor funded the NJCS to evaluate the effectiveness of Job Cops, which was carried out by Mathematica Policy Research. Individuals who applied to the program for the first time between November 1994 and February 1996, and who were found to be eligible (80,833 individuals), were randomly assigned to a control, treatment or a program non-research group. The control group of 5977 individuals and the treatment group of 9409 make up the research sample. Control group members were barred from enrolling in Job Corps for 3 years, while the treatment group could enroll in Job Corps. The remaining 65,497 eligible applicants were the non-research group: those who enrolled in Job Corps but were not followed for data collection.

The research sample (15,386 individuals) was interviewed just after the random assignment and then at 12, 30, and 48 months after randomization. Treatment group members in the NJCS typically enrolled in Job Corps soon after random assignment. The average waiting time was 1.4 months and almost all participants enrolled prior to the sixth month after randomization. Once in Job Corps, enrollees participated on average for 8 months, and almost all participants (92%) were out 2 years after random assignment. Therefore, the time span after the second year is considered to be the post-program period.

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The data for our study come from the public release of the research sample. [9] Following Schochet *et al*. (2008), we focus on the sample of youths who completed the 48-month interview (6828 treatments and 4485 controls). Our dependent variable is the average weekly earnings in year 4, the second year of the post-program period.

The sampling rates between the control and treatment groups in the NCJS differed for some subpopulation subgroups for programmatic and research reasons (Schochet, 2001). This difference calls for either use of the sample and survey weights or conditioning on the key features of the sample design in a regression framework. We choose the latter, which helps increase the precision of the estimates. In a randomized experiment setting, it is a well-known fact that controlling for the baseline characteristics does not affect the consistency of the treatment effect estimate; however, it helps increase efficiency (Frölich and Melly, 2013).[10]

Table I reports the summary statistics for the treatment and control group baseline characteristics used in estimations, as well as the differences in the means of these characteristics between the two groups. None of the pre-treatment differences are statistically significant. One other key insight that we observe from Table I is the presence of non-compliers; 28% of the treatment units did not participate in Job Corps and 1.1% of the control units participated in Job Corps before the 3-year embargo period ended.[11]

3. EMPIRICAL METHODOLOGY

Because we use different estimators to evaluate mean and distributional impacts of Job Corps, we utilize the potential outcomes framework often adopted in the treatment effect literature to be consistent. Consider a sample of N individuals indexed by $i = 1, \ldots, N$ and let Y_{1i} denote the earnings of individual i if randomly assigned to a treatment group of size N_1 (denoted by $Z_i = 1$), and Y_{0i} denote the earnings of individual i if randomly assigned to control group of size N_0 (denoted by $Z_i = 0$). The impact of Job Corps for eligible applicants is given by $\Delta_i = Y_{1i} - Y_{0i}$.Let also D_i denote the treatment participation indicator, which takes the value of one when individual i participates in Job Corps services and zero otherwise, Y_i^1 and Y_i^0 denote the earnings at each state (Y_i^d for $D_i = d,\ d \in \{0,1\}$) and $D_i(z)$ is the value of D_i when $Z_i = z$, $z \in \{0,1\}$ (i.e. reaction of D_i to an external intervention on Z_i).

3.1. Mean Treatment Effect

To initially examine Job Corps, we focus on the ITT effect:

$$\Delta = E(\Delta_i) = E(Y_{1i} - Y_{0i})$$

[9] Ideally, in addition to survey data, we would also like to use the administrative data on earnings, especially considering the differences in the mean impacts from two data sources. The administrative data also cover post-survey years which are significant for a longer-term analysis. Unfortunately,these data are not publicly available. Furthermore. after our correspondence with Mathematica, we found that in order to collect these administrative data Mathematica obtained legal memoranda of understanding (MOUs) with the social security administration and each of the 22 states where Unemployment Insurance (UI) data were collected. We have been told that these MOUs expired years ago and, to comply with the MOUs, the UI data were destroyed. We have been also told that even if we wanted to re-collect the administrative data, they could not give us social security numbers for the Job Corps sample owing to confidentiality issues.

[10] We also discuss the results with sampling weights in Section 4.3 of the paper.

[11] We also checked whether sample attrition due to missing earnings generates any threat to estimations. Specifically, we run a probit model of non-response indicator on Job Corps assignment along with baseline characteristics. The coefficient on Job Corps assignment is statistically insignificant. The full set of estimations is available online at http://faculty.bus.lsu.edu/oeren/Online_Appendix_JAE.pdf. Furthermore, Lee (2008), using sharp-bound analysis, states that there is no attrition bias in the research sample.

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Table I. Summary statistics

	Treatment		Control		Difference	
	Mean	SD	Mean	SD	Diff.	SD
Female	0.453	0.497	0.379	0.485	0.074	0.694
Age at application						
16–17	0.403	0.490	0.427	0.494	−0.024	0.695
18–19	0.316	0.465	0.310	0.462	0.006	0.655
20–24	0.279	0.449	0.261	0.439	0.018	0.627
Race						
White	0.262	0.440	0.265	0.441	−0.003	0.622
Black	0.494	0.500	0.487	0.499	0.007	0.706
Hispanic	0.171	0.377	0.175	0.380	−0.004	0.535
High school credentials at random assignment (1 = yes)	0.240	0.427	0.232	0.422	0.008	0.600
Lives with spouse/partner at random assignment (1 = yes)	0.065	0.246	0.060	0.239	0.005	0.342
Ever worked before random assignment (1 = yes)	0.799	0.400	0.789	0.407	0.010	0.570
Worked in the year prior to random assignment (1 = yes)	0.646	0.478	0.641	0.479	0.005	0.676
Had job at random assignment (1 = yes)	0.206	0.404	0.202	0.401	0.004	0.569
Months employed in the year prior to random assignment						
0–3 months	0.187	0.378	0.189	0.379	−0.005	0.535
3–6 months	0.280	0.436	0.273	0.432	0.007	0.613
9–12 months	0.178	0.371	0.178	0.371	0.000	0.524
Yearly earnings in the year prior to random assignment						
Less than 1000	0.105	0.307	0.109	0.312	−0.004	0.437
1000–5000	0.272	0.445	0.265	0.441	0.007	0.626
5000–10,000	0.137	0.344	0.132	0.338	0.005	0.482
10,000 or more	0.064	0.245	0.065	0.248	−0.001	0.348
Family on welfare when growing up (1 = yes)	0.202	0.401	0.193	0.395	0.009	0.562
Received AFDC in the year prior to random assignment (1 = yes)	0.316	0.464	0.300	0.458	0.016	0.651
Received food stamps in the year prior to random assignment (1 = yes)	0.450	0.497	0.440	0.496	0.010	0.702
Months in education/training in the year prior to random assignment	0.296	0.443	0.251	0.433	0.045	0.619
0–6 months	0.269	0.443	0.251	0.433	0.018	0.619
6–12 months	0.345	0.475	0.377	0.484	−0.032	0.678
Lived in public housing (1 = yes)	0.209	0.404	0.196	0.394	0.013	0.564
Poor health (1 = yes)	0.127	0.333	0.135	0.342	−0.008	0.477
Used hard drugs in the year prior to random assignment (1 = yes)	0.063	0.244	0.060	0.238	0.003	0.340
Used marijuana in the year prior to random assignment (1 = yes)	0.242	0.428	0.243	0.429	−0.001	0.605
Ever arrested (1 = yes)	0.239	0.426	0.252	0.434	−0.013	0.608
Lives in PMSA (1 = yes)	0.320	0.466	0.315	0.464	0.005	0.657
Lives in MSA (1 = yes)	0.467	0.498	0.461	0.498	0.006	0.704
Job Corps participation rate	0.720		0.011			
Sample size	6,372		4,223			

Note: Appropriate sample/survey weights utilized.

which can be estimated in various ways (see Imbens, 2004, for a review). Here we use the inverse probability weighted estimator of Horvitz and Thompson (1952). An estimate of Δ can be computed as follows:

$$\hat{\Delta} = \sum\nolimits_i \frac{1}{N}\left[\frac{Y_i Z_i}{P(X_i)} - \frac{Y_i(1-Z_i)}{(1-P(X_i))}\right] \tag{1}$$

where x is the set of pre-treatment variables and $P(X_i)$ is the propensity score $P(X_i) = P(Z_i = 1|X_i)$ (i.e. a logit or a probit model of assignment on pre-treatment variables). Conditional on random assignment and under the assumption of sufficient overlap between the distributions of the propensity scores across the $Z_i = 1$ and $Z_i = 0$ groups (typically referred to as the common support condition; see Dehejia and Wahba, 1999; Smith and Todd, 2005), it is conceivable to obtain a consistent estimate of the ITT effect.

The estimate presented in equation (1) makes no adjustment for participation in Job Corps services. However, as described above, ITT effects are diluted by non-compliance and, more importantly, policymakers are usually concerned with the impact of the program on the actual participants rather than just on eligible applicants. The impact of Job Corps on participants requires adjustment for

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non-compliance in the ITT effect and doing so yields the impact for the subpopulation of compliers. This estimate is given by

$$\hat{\Delta}_C = E\left(Y_i^1 - Y_i^0 \mid D_i(1) - D_i(0) = 1\right) = \frac{\sum_i \left(\frac{Y_i Z_i}{P(X_i)} - \frac{Y_i(1-Z_i)}{(1-P(X_i))}\right)}{\sum_i \left(\frac{D_i Z_i}{P(X_i)} - \frac{D_i(1-Z_i)}{(1-P(X_i))}\right)} \tag{2}$$

As apparent in equation (2), this is the propensity score weighted ITT effect divided by the propensity score weighted compliance rate of the treatment. Equation (2) is estimated using the procedure developed in Frölich (2007), where Z_i acts as an instrument for D_i. Note that this estimator identifies the Local average treatment effect (LATE) of Imbens and Angrist (1994).[12]

3.2. Distributional Approach

3.2.1. Quantile Treatment Effects

Focusing on the mean impact may mask meaningful, and policy-relevant, heterogeneity across the distribution. To examine such heterogeneity, we analyze the (unconditional) quantile treatment effects (QTE) for Job Corps eligible applicants, as well as for Job Corps participants.[13]

Let Y_0 and Y_1 denote two outcome variables to be compared; Y_0 (Y_1) may represent earnings for Job Corps control (treatment) group. $\{y_{0i}\}_{i=1}^{N_0}$ is a vector of N_0 observations of Y_0 (denoted by $Z_i = 0$); $\{y_{1i}\}_{i=1}^{N_1}$ is an analogous vector of realizations of Y_1 (denoted by $Z_i = 1$). Let $F_0(y) \equiv P[Y_0 < y]$ represent the cumulative density function (CDF) of Y_0; define $F_1(y)$ similarly for Y_1. The τth quantile of Y_0 is given by the smallest value y_0^τ such that $F_0\left(y_0^\tau\right) = \tau$; y_1^τ is defined similarly for Y_1. Using this notation, the QTE for quantile τ is given by $\Delta^\tau = y_1^\tau - y_0^\tau$, which is simply the horizontal difference between the CDFs at probability τ.[14] Estimation of the QTEs is complicated by the fact that one also needs to adjust for covariates. Following Firpo (2007) and as described above, we make this adjustment by the inverse propensity score weighting. Estimates, $\hat{\Delta}^\tau, \tau = 0.01, \ldots, 0.99$, are then solutions to the following minimization problem:

$$\begin{aligned} (\alpha, \Delta^\tau) &= \underset{\alpha, \Delta}{\operatorname{argmin}} \sum \omega_i \rho_\tau (Y_i - \alpha - Z_i \Delta) \\ \omega_i &= \frac{Z_i}{P(Z_i = 1|X_i)} + \frac{1 - Z_i}{1 - P(Z_i = 1|X_i)} \end{aligned} \tag{3}$$

[12] The LATE is identified under the following set of assumptions:

i. Independence of the instrument: $\left(Y_i^0, Y_i^1, D_i(1), D_i(0)\right) \perp Z_i | X_i$.
ii. Common support and existence of compliers: 0 < P(ZijXi) < 1 and P(Di(0) < D_i(1)) > 0.
iii. Monotonicity: P(Di(0) ≤ Di(1)) = 1.

The first assumption rules out a direct effect of Z on the outcome of interest. The second assumption ensures the common support condition and the existence of compliers. The monotonicity condition requires that any individual who would not have participated if assigned to the Job Corps would also have not done so if assigned to the control group. Even though the monotonicity assumption is not testable, we believe that it is likely to hold in the current context as non-compliance is the result of an individual's decision. Unlike example 2 in Imbens and Angrist (1994, p. 472), the participation decision is solely determined by random assignment. See also Bloom (1984) and Heckman et al. (1998) for different treatments of essentially the same estimator.

[13] See Abadie *et al*. (2002), Chernozhukov and Hansen (2006, 2008) and Firpo *et al*. (2009) for other approaches to examining heterogeneity along the distribution.

[14] It is important to note that the QTEs do not correspond to quantiles of the distribution of the treatment effect unless the assumption of *rank preservation* holds (Firpo, 2007). Absent this assumption, the QTE simply reflects differences in the quantiles of the two marginal distributions.

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where $\rho_\tau(u) = u \cdot \{\tau - 1(u < 0)\}$ and ω_i is the inverse propensity score weighting. As noted in Frölich and Melly (2013), equation (3) is a bivariate quantile regressor estimator with weights, and α is identified only from the $Z_i = 0$ observations and is numerically identical to $\hat{\alpha} = \underset{y_0}{\text{argmin}} \sum_{i:Z_i=0} \omega_i \rho_\tau(Y_i - y_0)$.In the same spirit, Δ^τ is identified only from the $Z_i = 1$ observations and is numerically identical to $\hat{\alpha} + \hat{\Delta}^\tau = \underset{y_1}{\text{argmin}} \sum_{i:Z_i=1} \omega_i \rho_\tau(Y_i - y_1)$.Estimates of Δ^τ using Job Corps eligibility as our treatment indicator provide us the QTEs for the ITT. To further examine the heterogeneity of Job Corps effects for the actual participants, we employ the estimator developed in Frölich and Melly (2013). Specifically, under LATE assumptions (see footnote 6), the QTE estimates for the subpopulation of compliers, $\hat{\Delta}_c^\tau, \tau = 0.01, \ldots, 0.99$, are solutions to the following minimization problem:

$$\left(\alpha_c, \Delta_c^\tau\right) = \underset{\alpha, \Delta}{\text{argmin}} \sum \omega_i^{\text{FM}} \rho_\tau(Y_i - \alpha - D_i \Delta) \tag{4}$$

$$\omega_i^{\text{FM}} = \frac{Z_i - P(Z_i|x_i)}{P(Z_i = 1|X_i)(1 - P(Z_i = 1|X_i))}(2D_i - 1)$$

where ω_i^{FM} is the modified inverse propensity score weighting in the QTE framework and Z_i acts as an instrument for D_i conditional on a set of pre-treatment variables. Once again, α is identified only from the $D_i = 0$ observations and is numerically identical to $\hat{\alpha}_c = \underset{y_0}{\text{argmin}} \sum_{i:D_i=0} \omega_i^{\text{FM}} \rho_\tau(Y_i - y_0)$. Likewise, Δ_c^τ is identified only from the $D_i = 1$ observations and is numerically identical to $\hat{\alpha}_c + \hat{\Delta}_c^\tau = \underset{y1}{\text{argmin}} \sum_{i:D_i=1} \omega_i^{\text{FM}} \rho_\tau(Y_i - y_1)$.

Before proceeding any further, it is important to note that the purpose of this paper is to estimate the QTEs (i.e. horizontal difference between the CDFs). Estimation of the quantiles of the treatment distribution requires further assumptions and detailed information on the joint distribution of the outcome variables.

3.2.2. Test of Equality

In addition to examining the QTEs, we test the joint null $H_0 \Delta^\tau = 0 \ \forall \tau \ \varepsilon \ (0,1)$ or equivalently $H_0 F_0 = F_1$, utilizing a two-sample Kolmogorov–Smirnov (KS) statistic (see, for example, Abadie, 2002). The test is based on the following KS statistic:

$$d_{\text{eq}} = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} sup|F_1 - F_0| \tag{5}$$

Specifically, our procedure calls for

i. obtaining the pre-treatment differences adjusted empirical CDFs for Y_0 and Y_1, defined as

$$\hat{F}_{jN_j}(y) = \frac{\sum_{i=1}^{N_j} \hat{\omega}_i \text{I}\left(Y_j \leq y\right)}{\sum_{i=1}^{N_j} \hat{\omega}_i} \quad j = 0, 1 \tag{6}$$

by computing the values of $\hat{F}_{0N_0}(y_k)$ and $\hat{F}_{1N_1}(y_k)$, where $\hat{\omega}_i$ is the inverse propensity score weight from equation (3), $\text{I}(\cdot)$ is an indicator function and y_k, $k = 1, \ldots, K$, denotes points in the support that are utilized ($K = 500$ in the application);

ii. and computing

$$\hat{d}_{\text{eq}} = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} \max_k \left\{ \left| \hat{F}_1(y_k) - \hat{F}_0(y_k) \right| \right\} \tag{7}$$

Inference for the test of equality of the distributions is conducted using the bootstrap procedure outlined in Abadie (2002). Specifically, we pool the two samples, resample (with replacement) from the combined sample, split the new sample into two samples, where the first N_0 represents Y_0 and the remainder represent Y_1, and compute the KS statistic. This process is repeated B times, and the p-value is given by

$$p - \text{value} = \frac{1}{B} \sum_{b=1}^{B} \mathrm{I}\left(\hat{d}^*_{\text{eq},b} > \hat{d}_{\text{eq}} \right) \tag{8}$$

The null hypothesis is rejected if the p-value is less than the desired significance level, say 0.10.

The CDFs in equation (6) and the test statistic from equation (8) depend on Job Corps eligibility. As for the distributional tests of the actual participants, we denote the CDFs of compliers as

$$\begin{aligned} F_0^c &= E[I\{(Y_0 \leq y\} | D_i(1) - D_i(0) = 1)] \\ F_1^c &= E[I\{(Y_1 \leq y\} | D_i(1) - D_i(0) = 1)] \end{aligned} \tag{9}$$

Under the LATE assumptions, Abadie (2002) shows that distributional tests conducted on the distributions of F_0^c and F_1^c are equivalent to tests conducted on F_0 and F_1. That is, the inference on test of equality using the CDFs for eligible participants is equally informative for the subpopulation of compliers. Therefore, in the results below, we report only the test statistics based on F_0 and F_1.

4. RESULTS

4.1. Mean Effects

To begin, we present the mean impacts of Job Corps on average weekly earnings in year 4 in Table II. In the first and second columns, the average treatment and control groups' earnings are displayed, while the third and fourth columns provide the estimated impacts on eligible applicants and on actual participants, respectively. Standard errors are given beneath each coefficient estimate. The estimates in the third and fourth columns are consistent with each other and, as expected, the LATE estimates are larger in magnitude.[15] Focusing on the last column of Table II and the full sample, the estimate for compliers indicates an average effect of \$21 and this effect translates into a 11% increase in the weekly earnings of those who participated in Job Corps.

The subsequent rows examine the effects of Job Corps on earnings based on gender, race, age at application, and residential designation. Our results are consistent with previous papers using the same data, which find that with the exception of Hispanics and those ages 16–17 and 18–19 at application, all subpopulations seem to benefit from Job Corps, on average. Flores-Lagunes *et al*. (2010) and Flores *et al*. (2012) link the puzzling negative effect observed for Hispanics to the differential local labor market unemployment rates they face. As for the subgroup of ages 18–19 at application, Schochet *et al*. (2001) attribute the small and insignificant impact to the unusually high employment and earnings level of the control group and not to the failure of Job Corps.

[15] There is full overlap in the estimated propensity scores across the Job Corps participants and the control units for the full sample and the subgroups. The density estimates of the propensity scores are available upon request.

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Table II. Mean impacts of Job Corps on weekly earnings

	Treatment group	Control group	Estimated impact per eligible applicant (SE)	Estimated impact per participant (SE)
Full sample (sample size:10,595)	209.13	192.63	14.74*** (3.64)	21.09*** (5.06)
Gender				
Males (sample size: 6104)	239.03	219.92	18.49*** (5.05)	25.66*** (6.78)
Females (sample size: 4491)	170.25	157.86	10.83** (5.07)	16.09** (7.42)
Race				
Whites (sample size: 2794)	260.58	227.64	30.88*** (7.52)	45.94*** (10.77)
Black (sample size: 5208)	183.04	164.23	16.58*** (4.92)	23.35*** (6.76)
Hispanic (sample size: 1834)	201.58	213.33	−9.40 (9.18)	−13.95 (12.83)
Age at application				
16–17 (sample size: 4374)	185.62	173.43	11.15** (5.30)	−6.27 (24.10)
18–19 (sample size: 3331)	209.00	204.37	1.46 (6.88)	−11.03 (9.90)
20–24 (sample size: 2890)	245.30	209.78	33.38*** (7.15)	50.04*** (10.68)
Residential designate				
Residential designate (sample size: 8638)	209.64	193.47	14.64*** (4.03)	20.51*** (5.45)
Non-residential (sample size: 1957)	206.08	187.82	16.77* (8.80)	28.98** (13.97)

Note: Asterisks indicate significance levels of: ***1%; **5%; and *10%. The impact per eligible applicant is obtained using inverse propensity score weighting, while the impact per participant is obtained using the estimation strategy proposed in Frölich (2007). The instrument for participation is Job Corps eligibility. Propensity score estimations control for the covariates described in Table I.

Comparing our mean findings with the existing studies, the results are almost identical in magnitude, with one exception. We did not find any positive impact of Job Corps participation on earnings for the youngest age group. This difference seems to arise because of the propensity score weighting in the LATE estimate. When we employ the linear IV estimation, the estimated effect on earnings turns out to be statistically significant, with a value of $16 for the youngest age group.[16]

Prior to continuing, two comments are warranted concerning the estimation strategy. First, our examination of the research sample indicates that roughly 18% (20%) of the treatment (control) group report zero earnings as their weekly earnings in year 4. The presence of mass zero points will be evident in our analysis below. This, however, is not likely to cause any problem in interpreting the distributional findings as the focus of our paper is the product of the price of labor and the labor supply (intensive and extensive margins). Second, as shown in Schochet *et al*. (2001) and Lee (2009), the Job Corps has a significant effect on the probability of being employed.[17] In this respect, isolating the wage (positive earnings) effects of the program without taking into account the sample selection

[16] Propensity score weighting also generates a similar problem for age group 18–19, for whom a positive and statistically insignificant ITT effect becomes a negative and statistically insignificant effect for compliers. When we use linear IV estimation, the effect for compliers is $3.

[17] We also estimated the impact of Job Corps on the probability of having a positive earning (extensive margin) in year 4 using equation (2). Job Corps participation increases this probability by around 2.5% and the effect is statistically significant. The impact of Job Corps on weekly hours of work (intensive margin) for the full sample is around 1.78 h, an increase of 6.7% relative to average weekly hours worked in year 4.

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problem in the QTE framework described above would be misleading. Even though the treatment and control groups are similar at the baseline, they may be systematically different conditional on employment. In this respect, a comparison of the wage rates across the treatment and control groups may not reflect the causal effect of the Job Corps. Indeed, Schochet *et al*. (2001, p. 136) state the fact that the wage estimates of Job Corps should not be interpreted as impact estimates.

4.2. Distributional Effects

The QTEs for actual participants are displayed in the panels of Figures 1–6 along with the mean treatment effect plotted as a horizontal dashed line (Appendix A, provides the QTEs for eligible Job Corps applicants). The 0-line is provided for reference. To facilitate comparison along the distribution, we also provide the selected QTE estimates in Table III. Results from tests of equality are given in Table IV. Standard errors for QTEs and tests of equality are all based on 500 bootstrap replications. Weights (ω and ω^{FM}) are obtained from a logit specification and for each bootstrap replication, we re-estimate the propensity score weights. Finally, as discussed above, the QTEs do not reflect the quantiles of the distribution of Job Corps assignment/participation effect.

4.2.1. Full Sample Analysis

Figure 1 presents the QTEs for the full sample of compliers and shows that the QTEs (and their standard errors) are exactly zero for the lower tail of the earnings distribution, primarily reflecting the fact that some treatment and control units are unemployed in the second year of the post-program period (non-impact of Job Corps on labor supply). Beginning with the 18th quantile, the QTEs become positive but until the 45th quantile they are sufficiently small and that zero effect is still within the 90% confidence interval. After that, however, all the estimates are statistically significant. Viewing the entire picture, we observe non-negligible heterogeneity in the QTEs and an increasing impact of Job Corps in the quantile index. Specifically, the selected QTE estimates from Panel A of Table III provide a Job Corps effect of \$16 at the 20th quantile, while the effect on earnings is \$31 at the 85th quantile. The range of point estimates for QTE is quite large [\$0, \$161] and is substantially larger than any conventional-level confidence interval range constructed around the mean effect: more than five times the

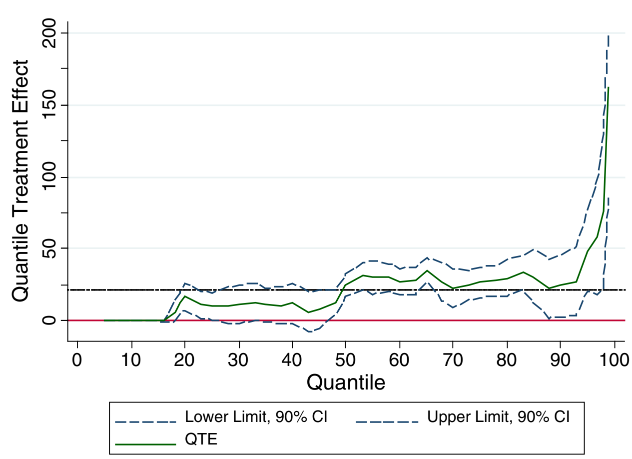

Figure 1. QTEs: full sample. The figure indicates the QTEs for actual participants. The horizontal dashed line is the mean effect. This figure is available in colour online at wileyonlinelibrary.com/journal/jae.)

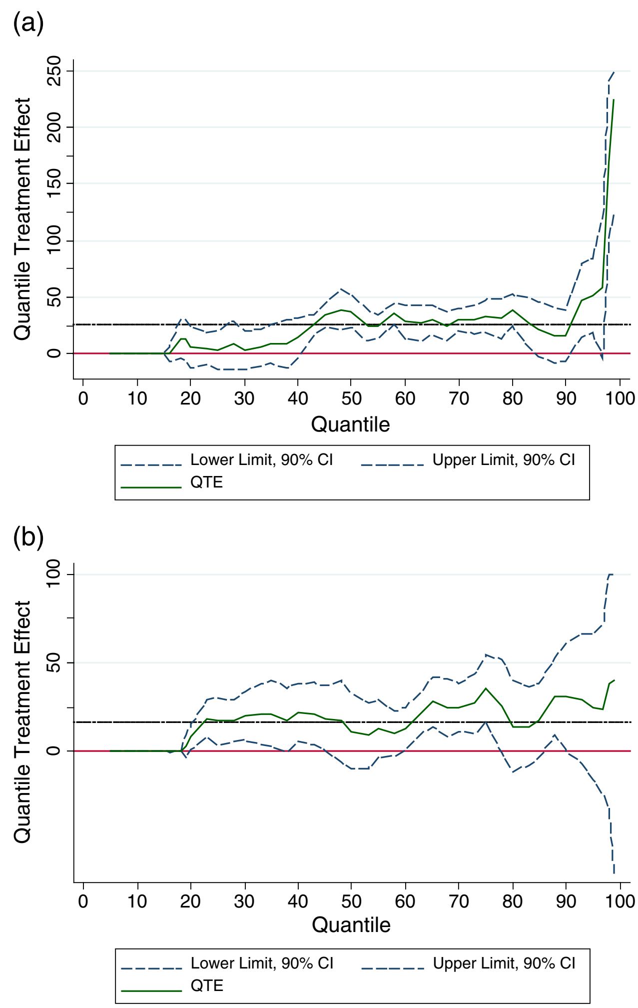

Figure 2. QTEs: gender: The figure indicates the QTEs for actual participants. The horizontal dashed lines are the mean effects: (a) Males; (b) females. This figure is available in colour online at wileyonlinelibrary.com/journal/jae.)

upper limit of the 90% confidence interval. The equality of CDFs between the treatment and the control groups is also rejected ($p = 0.00$, panel A of Table IV).

4.2.2. Subgroup Analysis

Gender. Our first set of subgroup results pertains to gender. Figure 2(a) presents the QTEs for males, while Figure 2(b) presents the estimates for females. Focusing on males, the QTEs reveal a pattern similar to that of the full sample. Specifically, Job Corps does not seem to be very effective at the lower tail of the distribution. Precision set aside and taken at face value, the QTEs are less than \$10 between the 15th and 40th quantiles. There is then a sudden jump, with the effects almost reaching to \$40 and, after that, the QTEs are usually uniform and statistically significant (Figure 2a and Table III panel B). For the top five quantiles, the QTEs indicate a statistically significant impact of above \$50. Comparing the upper quantiles with the lower ones shows that the difference is at least four times, and larger for some quantiles.

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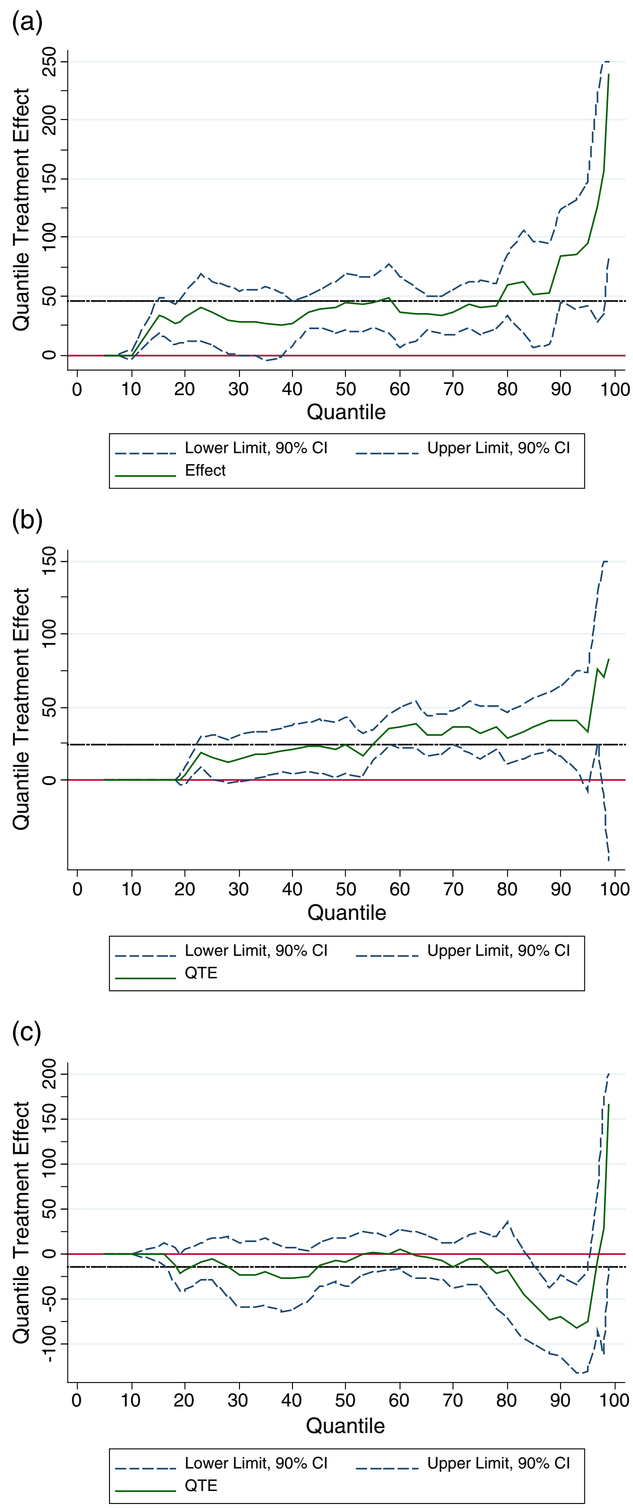

Figure 3. QTEs: race. The figure indicates the QTEs for actual participants. The horizontal dashed lines are the mean effects: (a) whites; (b) blacks; (c) Hispanics. This figure is available in colour online at wileyonlinelibrary.com/journal/jae.)

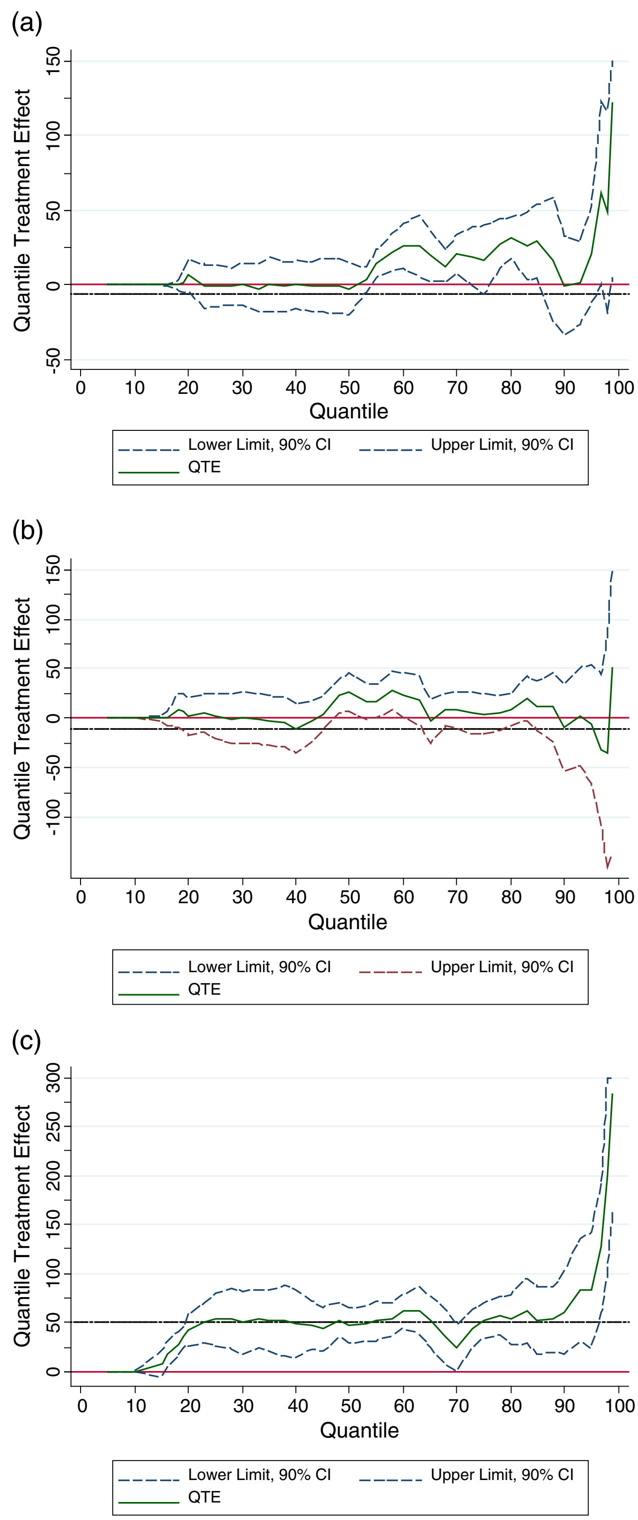

Figure 4. QTEs: age. The figure indicates the QTEs for actual participants: (a) age 16–17; (b) age 18–19; (c) age 20–24. This figure is available in colour online at wileyonlinelibrary.com/journal/jae.)

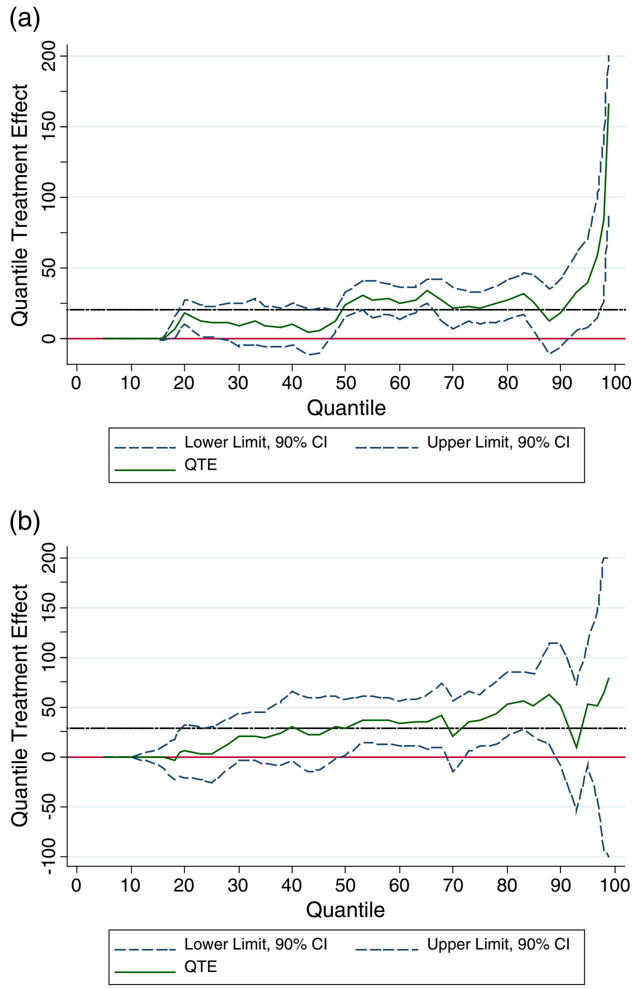

Figure 5. QTEs: residential designate. The figure indicates the QTEs for actual participants. The horizontal dashed lines are the mean effects: (a) residential; (b) non-residential. This figure is available in colour online at wileyonlinelibrary.com/journal/jae.)

Turning to females (Figure 2b), the QTEs are identically zero for roughly the bottom 15 quantiles, reflecting the fact that some female treatment and control units are unemployed after 4 years of random assignment. Unlike males, however, after the 15th quantile the impact is fairly uniform over the entire distribution. The QTEs evolve around \$20 and are statistically significant only between the 20th–45th and the 60th–80th quantiles. In other regions of the distribution, the 90% confidence interval for the QTEs includes zero. The range of point estimates for QTE is quite narrow [\$0, \$39].

For both males and females, the equality of CDFs between the treatment and control groups is rejected ($p = 0.00$).

Race. The next set of subgroup results pertains to race. Figure 3 and Table III panel C present the QTEs for whites, blacks, and Hispanics. Looking at Figure 3(a), we observe that the QTEs above the 10th quantile are all positive and precisely estimated for whites. The QTEs reveal virtually a constant impact of around \$30–\$40 between the 10th and 80th quantiles. Beginning with the 80th quantile, however, there is a jump and the QTEs reach \$60, and then we observe another jump at the 90th

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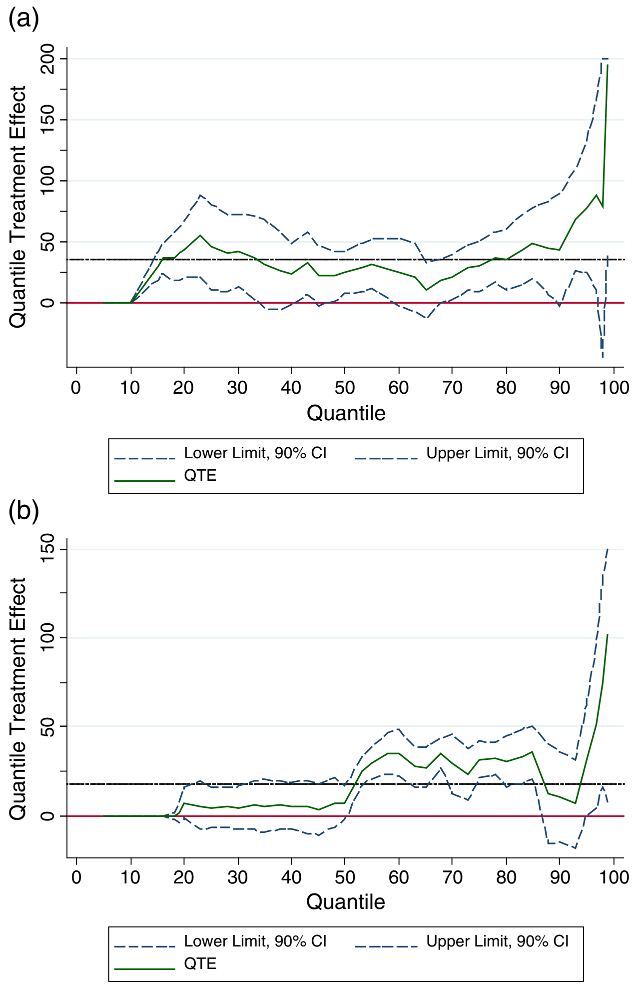

Figure 6. QTEs: high school credentials. The figure indicates the QTEs for actual participants. The horizontal dashed lines are the mean effects: (a) high school; (b) no high school. This figure is available in colour online at wileyonlinelibrary.com/journal/jae.)

quantile, reaching above \$85. Taking the control group weekly average income of \$227 for whites as a benchmark, the QTE at the 25th quantile translates into a Job Corps impact of 15%, while the QTE at the 90th quantile translates into a Job Corps impact of 37%, suggesting evidence for remarkable heterogeneity along the distribution. The equality of CDFs between the treatment and the control groups is also rejected ($p = 0.00$).

Turning to the QTEs for blacks, we once again observe an increasing pattern at the quantile index, although the increase is flatter compared to that of whites. The estimates are also predominantly significant (Figure 3b). The test of equality of CDFs is rejected.

Figure 3(c) presents the results for Hispanics. Consistent with the mean effect from Table II, the QTEs are all negative but statistically insignificant below the 85th quantile. Between the 85th and the 95th quantiles, the QTEs are negative and significant, suggesting an adverse effect of Job Corps participation on earnings. This finding should be taken with caution, though, as we show in the robustness section, it is sensitive to model specification. Moreover, we fail to reject the test of equality between the CDFs ($p = 0.27$).

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Table III. Selected quantile treatment effects of Job Corps on weekly earnings

	$\tau=5$	$\tau=15$	$\tau=20$	$\tau=50$	$\tau=80$	$\tau=85$	$\tau=95$
Panel A: Full sample							
Estimated impact per eligible applicant	0.00	0.00	10.02*	16.87*	21.96*	22.71*	28.44*
Estimated impact per participant	0.00	0.00	16.60*	24.63*	29.67*	30.93*	48.33*
Panel B: Gender							
Males							
Estimated impact per eligible applicant	0.00	0.00	5.59	27.11*	25.37*	10.81*	44.47*
Estimated impact per participant	0.00	0.00	5.94	37.90*	38.32*	22.56	51.53*
Females							
Estimated impact per eligible applicant	0.00	0.00	4.32	6.78	8.93	13.37	21.43
Estimated impact per participant	0.00	0.00	8.30*	11.18	14.21	17.28	25.11
Panel C: Race							
Whites							
Estimated impact per eligible applicant	0.00	21.29*	21.43*	27.44*	36.82*	39.11*	71.80*
Estimated impact per participant	0.00	32.97*	32.35*	44.93*	59.12*	51.53*	94.66*
Blacks							
Estimated impact per eligible applicant	0.00	0.00	0.00	14.36	21.01*	27.00*	23.86*
Estimated impact per participant	0.00	0.00	2.74	23.44*	28.52*	36.39*	32.70
Hispanics							
Estimated impact per eligible applicant	0.00	0.00	−16.17	−5.04	−9.53	−37.05	−53.19*
Estimated impact per participant	0.00	0.00	−17.89	−9.94	−17.76	−55.95*	−75.49*
Panel D: Age at application							
16–17							
Estimated impact per eligible applicant	0.00	0.00	2.88	−2.26	25.48*	24.17*	13.57
Estimated impact per participant	0.00	0.00	6.58	−2.53	31.56*	29.24*	20.58
18–19							
Estimated impact per eligible applicant	0.00	0.00	3.06	16.40	1.06	11.31	−2.69
Estimated impact per participant	0.00	0.00	2.16	26.27*	8.77	12.86	−5.89
20–24							
Estimated impact per eligible applicant	0.00	0.00	33.97*	37.88*	34.16*	40.53*	62.64*
Estimated impact per participant	0.00	8.08	42.22*	47.13*	52.70*	52.60*	82.49*
Panel E: Residential designate							
Residential							
Estimated impact per eligible applicant	0.00	0.00	12.88*	17.18*	19.48*	18.46*	30.72*
Estimated impact per participant	0.00	0.00	19.00*	24.21*	27.57*	26.00*	39.41*
Non-residential							
Estimated impact per eligible applicant	0.00	0.00	0.93	15.48	29.97*	40.06*	37.14
Estimated impact per participant	0.00	0.00	5.67	29.49*	52.93*	51.90*	52.39

Note: Asterisk indicates significance level of *10%. The weights for the QTEs are obtained via a logit specification using the covariates from Table I. See text for further details.

Age group. To further examine the potential heterogeneity in the impacts of Job Corps, we concentrate on age at application. Figure 4 and Table III panel D display the QTEs. For the youngest group (16–17), we observe the reflection of unemployment (zero earnings) for the bottom 18 quantiles. Between the 18th and the 53th quantiles, the QTEs are still either zero or marginally negative. For instance, the QTE at the 50th quantile is −\$2.53 for Job Corps participants (Table III Panel D). The effects from the 55th to the 85th quantile are positive and statistically significant. Above the 85th quantile, however, the QTEs turn out to be statistically not different from zero.

Turning to the mid-age group (18–19), consistent with the average effect from Table II, the QTEs are all insignificant and fluctuate around the line of zero (Figure 4b).

The next set of results pertains to the oldest age group (20–24). Figure 4(c) presents QTEs that are identically zero for the bottom 10 quantiles, reflecting the fact that for 10% of individuals both treatment and control units have zero earnings. There is then a sharp increase in the QTEs, reaching \$50 at the 25th quantile from \$8 at the 15th quantile. After this point, the QTEs show a uniform pattern up to the 90th quantile. We then observe a jump to a statistically significant QTE of \$80 or more. The range of point estimates for QTE is quite large [\$0, \$283]; this is more than four times the upper limit of the 90% confidence interval around the subgroup specific mean effect of \$50.

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Table IV. Distributional tests of Job Corps

Distribution	Test of equality
Panel A: Full sample	$p = 0.00$
Panel B: Gender	
Males	$p = 0.00$
Females	$p = 0.00$
Panel C: Race	
Whites	$p = 0.00$
Blacks	$p = 0.00$
Hispanics	$p = 0.27$
Panel D: Age at application	
16–17	$p = 0.01$
18–19	$p = 0.01$
20–24	$p = 0.00$
Panel E: Residential designate	
Residential	$p = 0.00$
Non-residential	$p = 0.01$

Note: Distributions adjusted for covariates using inverse propensity score weighting, where the covariates are described in Table I. See text for further details.

Table IV panel D shows that for all age groups the test of equality across the distributions of treatment and control groups is rejected.

Residential designate. As noted above, Job Corps offers residential living to applicants to provide a protective shield from their disadvantaged environment, to maximize the benefits of educational and vocational training. Although we do not have information on the actual residential status of all participants, the NJCS includes an indicator that is a prediction of outreach and admission (OA) counselors as to whether sample members would be assigned to a residential or a non-residential slot. About 12% of the enrollees in the NJCS were nonresidents.[18] Since the residential designation decision was determined prior to random assignment, a comparison of treatment and control units by residence is also plausible. To this end, the final set of results pertains to residential status of the students.

Figure 5(a) plots the QTEs for the residents. Between the 25th and 50th quantiles, the QTEs are uniform at around \$12 but are not statistically significant. For quantiles above the median, the estimates are somewhat larger in magnitude and gain statistical significance.

Turning to the results for non-residents, we observe a monotonic increasing pattern (Figure 5b). The QTEs begin with a negative (insignificant) −\$2 at the 18th quantile, reaching \$30 at the 50th quantile and peaking at around the 88th quantile, with a statistically significant QTE of \$63. The equality of the CDFs is rejected for each residential designation ($p = 0.00$).

4.3. Robustness Checks and Design Weighted QTEs

We undertake several sensitivity checks to examine the robustness of our results. First, we replace the average earnings in year 4 with the average weekly earnings from the last quarter (quarter 16) and rerun all the specifications. The results are virtually identical to those presented in the paper. Next, rather than a global logit, the propensity scores from the first stage for the QTEs are estimated with a local logit. The estimates are qualitatively and quantitatively very similar. The only exception is the negative significant impact for Hispanics observed at the upper quantile; the QTEs turn out to be statistically equal to zero if we employ a local logit in the first stage.

[18] Outreach and admission counselor projections of residential status seem to be very accurate. Schochet *et al*. (2001) show that about 98% of program group enrollees designated for residential slots actually enrolled in them and about 88% of program group enrollees designated for nonresidential slots actually enrolled in them.

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As described at the outset of the paper, we choose to condition on the pre-treatment variables in estimating the QTEs as this approach helps increase the efficiency of the QTEs (Frölich and Melly, 2013). The control variables used in the inverse propensity score weights hinge upon the key features of Job Corps sample design and are identical to the covariates used in regression estimates of Job Corps in Schochet (2001). Nevertheless, as an alternative, we also re-estimate all the QTEs in the absence of any controls, using sample design weights (DSGN_WGT, as described in Schochet, 2001). The QTEs from this exercise are very similar to those presented in the paper. Finally, we examine the possible asymptotic refinements of QTE results by using the analytical standard errors and then bootstrapping the *t*-statistics. The inference with this additional procedure leaves our results intact.[19]

4.4. Discussion

Stepping back and viewing the complete set of results, we have three key findings. First, there is a great deal of heterogeneity in the effects of Job Corps across both eligible applicants and participants, and relying solely on the mean effect seems to mask some remarkable pieces of the overall picture. The QTEs exhibit an increasing impact along the earnings distribution and the largest QTE for the full sample reaches $161 as opposed to the mean effect of $21. Second, extending the analysis to the subgroup mean effects does not reveal much with respect to heterogeneity, as it predominantly stems from intra-group variation. Specifically, we observe a difference of at least four times between the lower and upper quantiles for males. For whites, the QTEs indicate an impact of 15% on the 25th quantile, but an impact of 37% at the 90th quantile. Moreover, we do not observe any effect of Job Corps for quantiles below the median for ages 16–17 at application. In contrast, the QTEs for ages 20–24 range from $0 to $283 and are precisely estimated almost over the entire distribution. Third, with the exception of Hispanics, the equality of the earnings CDFs between the treatment and the control groups are always rejected.

Of course, a natural question to ask at this point is why we observe a small impact or non-impact of Job Corps at the bottom quantiles for some but not all subgroups. One possibility is Job Corps experiences. However, in terms of enrollment rates, duration of participation, as well as academic and vocational training, the differences across subgroups were generally small, which rules out the Job Corps experiences as a potential explanation.

Flores-Lagunes *et al*. (2010) attribute the absence of an average positive impact of Job Corps on Hispanics to the local labor market unemployment rates. Specifically, the higher local unemployment rates and their differential effects on Hispanics mitigate the potential gains from Job Corps. The authors argue that, had the differential effects of local unemployment rates been purged out, the average impact of Job Corps on Hispanics would converge to that of whites. In the current context, to serve as an explanation, the local unemployment rates must not only have differential effects for subgroups, but must also have varying effects on different parts of the earnings distribution.

Another potential explanation relies on the strong economic performance observed in the era of NJCS in the USA, with inflation and unemployment reaching in 1998 their lowest levels since the 1960s. The economic boom of the 1990s generated large improvements at the bottom of the earnings distribution. The findings from recent studies indicate that earnings and employment improvements for those with low skills are larger than for those with high skills in this period (Hoynes, 2000; Freeman and Rodgers, 1999; Katz and Krueger, 1999). Given that the control group has less training and lower skills than the treatment group by the post-program period, the strong economic conditions may have overshadowed the gains of Job Corps for the lower quantiles of the earnings distribution. To further explore this argument, we split the sample based on high school credentials at the time of Job Corps application and examine the QTEs across the earnings distribution. The greater skill homogeneity

[19] All of these additional estimates are available upon request.

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for high school graduates would lead one to expect large QTEs at the lower quantiles. In the same spirit, one would expect small QTEs at the lower quantiles for those who lack a high school credential. This assumption follows from the fact that the skill homogeneity argument is less likely to hold for this subgroup and that the strong economic conditions may have degraded the impact of Job Corps by favoring the less skilled control group more.

Figure 6 plots the QTEs. For those with a high school credential, the QTEs are large and statistically significant at the lower tail of the earnings distribution (Figure 6a). In contrast, for those without a high school credential, the QTEs are small and insignificant below the median. Even though a larger portion of Job Corps participants (compared to control group) without a high school degree at the baseline received a GED degree over the 4-year period, it may not be surprising not to see any effect below the median because the GED is a mixed signal that characterizes its recipients as smart but unreliable (Heckman and Rubinstein, 2001).[20]

A potentially complementary explanation to strong economic conditions is skill hypothesis. Taken together, our findings indicate more uniform QTEs for females, whites, and ages 20–24. The common feature among these subgroups is endowment with relatively higher skills (i.e. higher education). Therefore, it may simply be the case that Job Corps is not effective at all for those who are at the bottom of the skill distribution.

Finally, we must point out that our analysis and results are based on survey data collected 4 years after randomization. Owing to data restrictions and for confidentiality reasons, we were not able to use administrative data either for the same time period or the years following the collection of the last survey data. As we briefly mentioned in the Introduction, there are mainly two differences in the results based on the survey and administrative data. First, for the period covered by both datasets the impact on earnings of the Job Corps program is significantly smaller when administrative data are used. Second, for the period beyond the time frame covered by the survey data, using administrative data suggests that the effect of the program on earnings persists only for the older participants (aged 20–24). Considering the significant within-group heterogeneity we found in our analysis of the impact on earnings of the Job Corps program, conducting the same analysis with the administrative data is imperative and should be the focus of future research.

5. CONCLUSION

Each year, Job Corps serves around 60,000 new enrollees for an average of 8–9 months at a total annual cost of more than $1.5 billion. To evaluate the effectiveness of Job Corps, the Department of Labor funded a random assignment study carried out by Mathematica Policy Research: the National Job Corps Study (NJCS). Using the public release of NJCS data and recently developed estimation techniques for quantile treatment effects, we examine the impact of Job Corps across the earnings distribution. Our paper provides the estimates for the sample of eligible applicants and the sample of actual participants (compliers). We find a great deal of heterogeneity in the effects of Job Corps such that the QTEs show an increasing pattern along the earnings distribution. This finding holds for the full sample and almost all subgroups, with much more pronounced differences at the upper quantiles for males, whites, and ages 20–24. Moreover, unemployed treatment and controls units set aside, we find the QTEs to be very small or essentially zero at quantiles below the median for males, ages 16–17 and 18–19, and non-resident students. We propose two complementary hypotheses, namely the strong economic conditions and skill hypotheses, to explain the phenomenon at the lower quantiles.

From a policy point of view, significant and large estimates observed over almost the entire earnings distribution accompanied by the persistent long-run mean impacts of Job Corps for older youths (Schochet *et al.*, 2008) may suggest an older age criterion in Job Corps eligibility for efficient

[20] In a pursuit for additional insights in explaining the heterogeneity over the earnings distribution, we further broke down the subgroups as follows: (i) female with children; (ii) female without children; (iii) males on arrest experience (i.e. whether arrested or not). The findings from this exercise indicate very similar heterogeneous patterns to those presented in the paper for females with children and for males with or without any arrest records prior to assignment.

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allocation of resources. Alternatively, a restructuring of the Job Corps design may help in spreading the positive impacts to other subgroups and the lower tail of the earnings distribution. For instance, enhancing training by targeting to improve non-cognitive skills may help those at the bottom of the skill distribution (Eren and Ozbeklik, 2012; Lindqvist and Vestman, 2011). Moreover, our results suggest that the effectiveness of Job Corps may depend on the overall wellbeing of the economy, as previous research has shown that the success of active labor market programs may be strongly affected by the economic conditions at the time of the program and shortly after (Hotz *et al*., 2006; Lechner and Wunsch, 2009).

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APPENDIX: QTE$_S$ FOR ELIGIBLE JOB CORPS APPLICANTS

Panel A: Full Sample

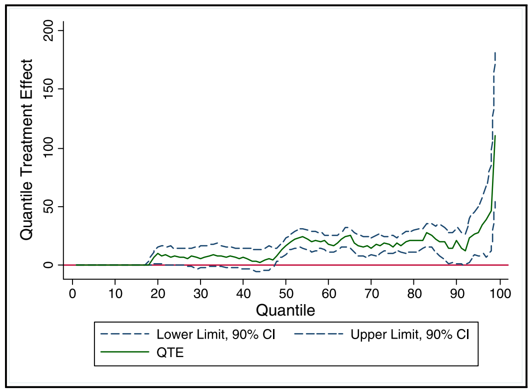

Figure A.1. QTEs-Full Sample: The panel indicates the QTEs for eligible Job Corps applicants.

Panel A: Males

Panel A: Females

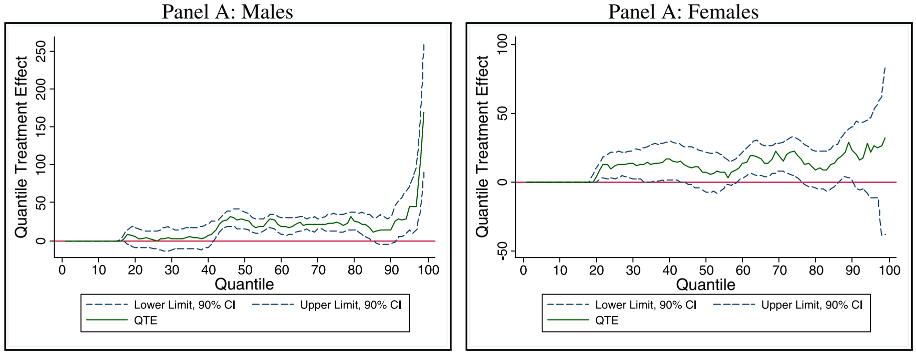

Figure A.2. QTEs-Gender: The panels indicate the QTEs for eligible Job Corps applicants.

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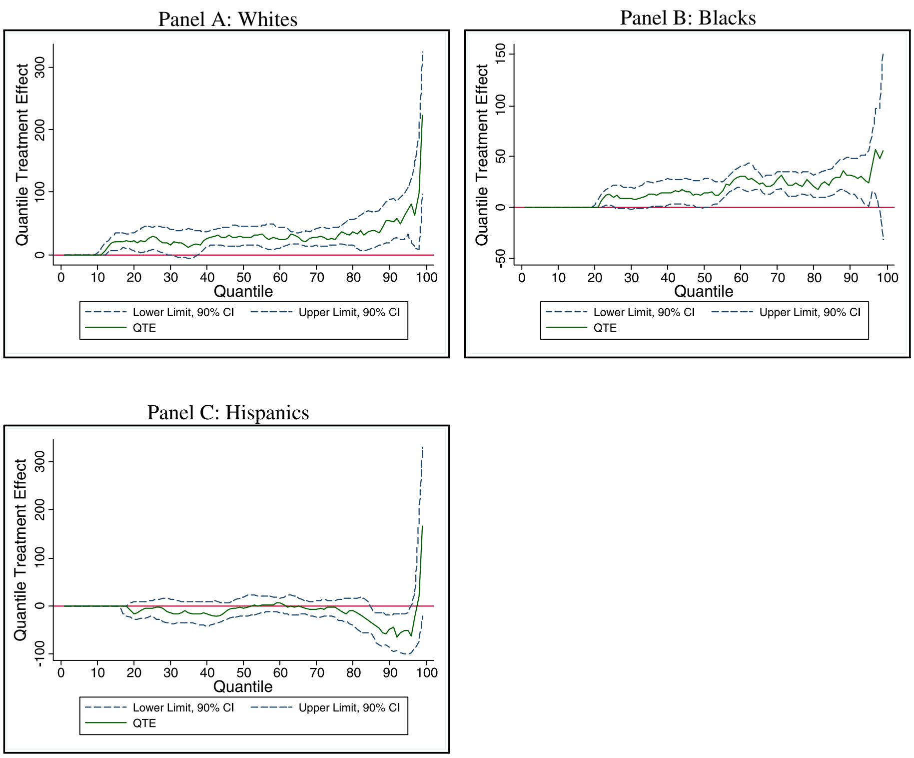

Figure A.3. QTEs-Race: The panels indicate the QTEs for eligible Job Corps applicants.

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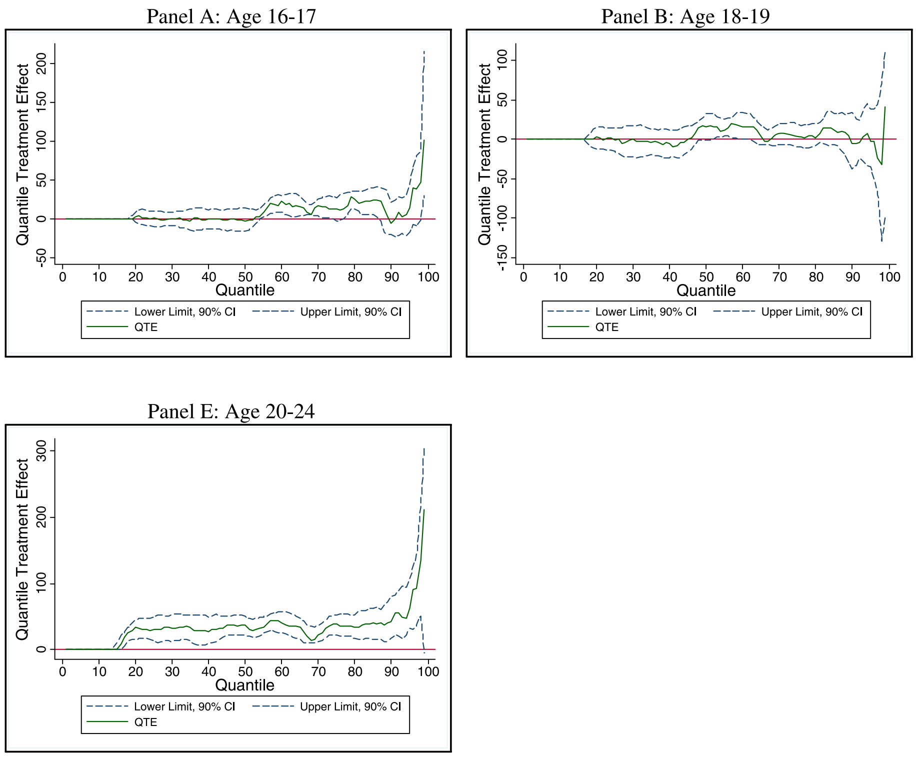

Figure A.4. QTEs-Age: The panels indicate the QTEs for eligible Job Corps applicants.

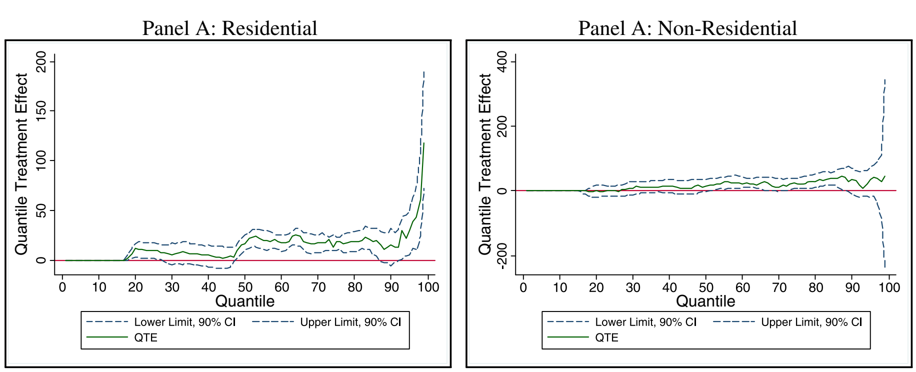

Figure A.5. QTEs-Residential Designate: The panels indicate the QTEs for eligible Job Corps applicants.

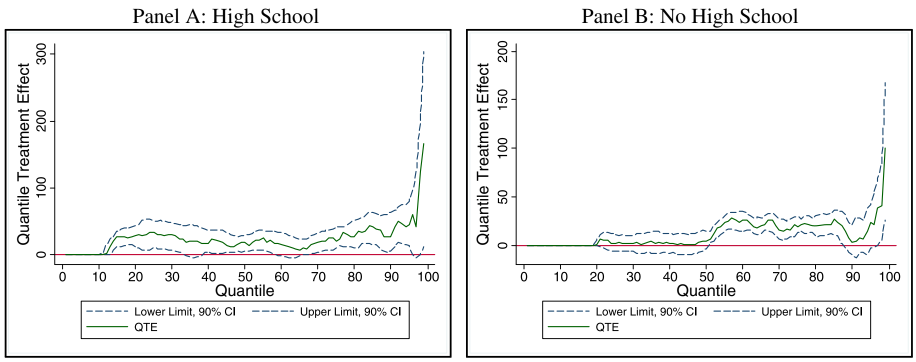

Figure A.6. QTEs-High School Credentials: The panels indicate the QTEs for eligible Job Corps applicants.