Does Youth Training Lead to Better Job Quality? Evidence from Job Corps*

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Abstract

Most of the economics literature uses monetary compensation as a proxy for job quality. Although active labor market programs generally aim to improve the future quality of life of participants, the extensive literature evaluating them typically focuses on employment and earnings. We analyze the causal effect of a U.S. job training program for youth—Job Corps—on future job quality. We define job quality by constructing a linear index that reduces a vector of job characteristics (e.g., fringe benefits) to a scalar quantity. Our job quality index is consistent with the view that workers evaluate a job as a bundle of attributes that may have some level of substitutability. Also, since our index is continuous, it permits us to evaluate the distributional impacts of Job Corps training. Given that the quality of a job is defined only for employed individuals, we address the selection into employment problem by estimating nonparametric bounds on the effects of Job Corps participation for the latent group of individuals that comply with their treatment assignment and would be employed regardless of Job Corps training. We find that Job Corps has substantial and statistically significant effects on the average quality of jobs attained that are bounded between 14 and 36 percent of a standard deviation in our job quality index. These effects are heterogeneous over the distribution of the job quality index. Females and older participants appear to experience stronger effects relative to males and younger participants, and this may be driven by experiencing greater access to certain fringe benefits (e.g., flexible work hours and child care).

JEL classification: J3, J32, J33, J38, J08.

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1 Introduction

By and large, most of the economics literature measures the desirability of jobs based on their monetary (wage) compensation. And while other aspects of jobs, such as working conditions or fringe benefits, have long been recognized as relevant (e.g., Rosen, 1986; Woodbury, 1983), the difficulty in measuring these other aspects typically relegates them from analyses. For instance, important strands of literature such as that documenting the role of the firm in wage differentials (e.g., Card et al., 2018) and the existence of "good" or "bad" jobs (e.g., Acemoglu, 2001; Green, 2015) largely focus on wage compensation. Yet, at least in the United States, non-wage compensation (fringe benefits) accounts for over 30% of total compensation of civilian workers (U.S. Bureau of Labor Statistics, 2019).[1] For the purposes of this paper, we refer to job quality as the combination of the wage compensation and the non-wage compensation of jobs.

Another important literature that employs wage compensation as the main measure of the desirability of jobs is that evaluating active labor market programs (ALMPs). The main labor market outcomes analyzed are employment, earnings, and job duration (for a survey see, for example, Heckman et al., 1999; Imbens and Wooldridge, 2009). At the same time, an often-stated or implicit goal of ALMPs is to improve the quality of participants' lives, such as in the program we analyze below. Undoubtedly, other aspects of jobs beyond the wage compensation contribute to that goal, such as health insurance, flexible work schedule, etc.

In this paper, we analyze the effects of an ALMP on the distribution of job quality, thereby providing insights into the often-neglected effects on non-monetary compensation. The ALMP we focus on is the Job Corps (JC) program, the U.S.'s main job training program for disadvantaged youth. We construct an index of job quality using principal components analysis based on the wage compensation and fringe benefits information on jobs available in the National Job Corps Study (NJCS). To evaluate the impact of any ALMP on job quality, one must deal with the familiar problem of selection into employment—job quality is only defined conditional on employment. To address this, we use non-parametric bounds that exploit the random assignment of eligible participants into JC (the cornerstone of the NJCS). Besides accounting for potential bias due to selection into employment, our methods also account for the non-compliance present in the NJCS, and allow us to study the distributional impacts on the job quality index through the estimation of bounds on the quantile treatment effects.

We are not the first to analyze a measure of quality of jobs. Some studies have analyzed different aspects

[1] This figure includes all workers in private nonfarm jobs. The fringe benefits include paid leave, supplemental pay, health insurance, retirement and savings, among others.

of job quality in the U.S. using monetary compensation as a proxy for quality (e.g., Bluestone and Harrison, 1988; Houseman, 1995; Farber, 1997; Acemoglu, 2001; Green, 2015; Card et al., 2018). We borrow from this vast literature the notion that the wage is an important dimension of job quality. Another literature indicates that an important dimension of job quality is the availability of fringe benefits, especially health insurance and retirement benefits (Woodbury, 1983; Farber, 1997; Kalleberg, et al., 2000; Kalleberg and Vaisey, 2005; Eriksson and Kristensen, 2014). An alternative measure of the quality of jobs, mostly followed in sociology and psychology, is to use the perceived level of job satisfaction reported by workers (e.g., Clark and Oswald, 1996; Muñoz de Bustillo, et al., 2011). We lack information on self-reported job satisfaction in our data and thus our measure does not account for this subjective indicator.

When different indicators of job quality are available, a common approach is to analyze job quality by separately focusing on the different dimensions or characteristics that contribute to job quality (e.g., Schochet et al., 2008; Andersson et al., 2016). While we also analyze separately each component of job quality available, our preferred set of results employs a unique linear index that reduces the vector of characteristics to a scalar quantity using principal components analysis (PCA). We present evidence suggesting that our index reliably bundles the different important aspects related to job quality. This measure has some desirable features. First, our constructed job quality index is consistent with the view that workers evaluate a job as a bundle of attributes that may have some level of substitutability (Rosen, 1986; Woodbury, 1983; Eriksson and Kristensen, 2014). Second, it is known that the construction of an index to measure a given concept (e.g., job quality) based on potentially error-ridden factors using PCA can ameliorate the consequences of measurement error (e.g., Cunha et al., 2010; Heckman et al., 2013). Third, using the job quality index we are able to perform distributional analysis as the index is nearly continuous. Lastly, by looking at a single summary measure we avoid concerns related to conducting multiple tests.

The Job Corps (JC) program is America's largest and most comprehensive education and job training program enrolling disadvantaged youth, ages 16 to 24, at no cost to them. Federal funds to run the program are around $1.6 billion (US Department of Labor, DOL, 2013). During 1995-96, the DOL funded the National Job Corps Study (NJCS) to determine the program's effectiveness. The main feature of the study was the random assignment of eligible participants to a treatment or control group (there was, however, non-compliance with treatment assignment of about 27 percent, mainly from treatment-group members who never enrolled in the program). In a landmark paper presenting impact findings from the NJCS, Schochet et al. (2008) found positive average impacts on a variety of important outcomes, including wages and some of the fringe benefits we consider. They point out, however, that findings on the latter set

of outcomes do not have a causal interpretation since they condition on employment, and employment is affected by training. In other words, their estimates are likely not causal due to selection into employment. In short, despite the availability of experimental data, since factors simultaneously affect both the outcome (e.g., fringe benefits) and its observability (e.g., employment), a comparison of treatment and control group averages produces a biased estimate of the average causal effect.

We assess the effect of JC on the job quality of eligible applicants by employing nonparametric bounds for average and quantile treatment effects that account for selection into employment. These bounds require weaker assumptions than those conventionally employed for point identification.[2] The two main assumptions we employ, and that are justified in section 4, are: individual-level weak monotonicity of the effect of the program on employment, and stochastic dominance of the outcome distribution across subpopulations (principal strata) defined by the potential values of the employment indicator as a function of treatment assignment. The bounds are for the causal effects defined for the subpopulation of individuals who would be employed regardless of the treatment. Since the job quality index is observed under both treatment arms only for this subpopulation, concentrating on it requires fewer assumptions for inference. In a first instance, we concentrate on bounding intention-to-treat (ITT) effects, in which case random assignment into JC is regarded as the treatment. Subsequently, we extend our analysis to also account for the noncompliance present in the NJCS. In this case, actual participation in JC is regarded as the treatment, and the subpopulation of focus consists of individuals that comply with treatment assignment and that would be employed regardless of random assignment. Both of the subpopulations above are important as judged by their size: ITT effects are estimated for a subpopulation that represents about 90 percent of those observed employed (63 percent of all eligible JC applicants), while the subpopulation corresponding to the effects of JC represents about 60 percent of those observed employed (42 percent of all eligible JC applicants).

Our results provide new and consequential evidence that JC positively affects the job quality of individuals that are employed regardless of participation in JC during the 16th quarter after random assignment. The estimated bounds indicate that the average treatment effect of JC is between 14 to 36 percent of a standard deviation in the job quality index for the sample of non-Hispanics (the reason we restrict analysis

[2]Point identification of treatment effects under selection into employment requires frequently strong distributional assumptions, such as bivariate normality (e.g., Heckman, 1979). Alternatives rely on exclusion restrictions (Heckman, 1990; Imbens and Angrist, 1994; Abadie et al., 2002), which require variables that determine selection into employment but do not affect the outcome (job quality). Finding exclusion restrictions in this context is difficult. For instance, it is well known that, when the wage rate is the outcome of interest, it is challenging to find plausible exclusion restrictions (Angrist and Krueger, 1999; Angrist and Krueger, 2001). The wage rate is an important component of job quality, and one can make the case that fringe benefits, another important component of our job quality measure, share this same characteristic of the wage.

to non-Hispanics is explained in sections 4 and 5). The 95 percent confidence interval on this effect exclude zero. Similar positive and statistically significant average treatment effects of JC on the job quality index are found for other demographic subsamples, where non-Hispanic females and older participants (young adults in their 20 to 24 years of age at baseline) show estimated bounds that are consistent with larger impacts relative to males and younger participants (teenagers in their 16 to 19 years of age at baseline). We also find suggestive evidence of heterogeneous impacts across the distribution of the job quality index, where estimated bounds, in general, show that the quantile treatment effects of JC on job quality are positive and statistically significant in most quantiles between the 30th and the 80th quantile. For example, estimated bounds on the 50th quantile for non-Hispanics indicate that the effect is between 19 and 54 percent of a standard deviation in the job quality index.

We see our analysis contributing to different strands of literature. First, our focus on the outcome of job quality as a bundle of job attributes is distinctive. Aspects of job quality are largely under-researched in economics and the program evaluation literature. Notable exceptions in the context of ALMPs are Ibarraran and Rosas Shady (2009), Attanasio et al. (2011), and Andersson et al. (2016). The first two studies, which focus on training programs in Latin American countries, use as measures of job quality whether the job obtained is in the formal sector and whether it has a contract. Ibarraran and Rosas Shady (2009) also consider whether the job provides health insurance. They both find positive and statistically significant effects of job training on these outcomes. Andersson et al. (2016) use the characteristics of the firms of employment as an outcome and find moderate (and statistically significant) positive effects for training participants under the US Workforce Investment Act (WIA).[3] In contrast to those studies, we use and summarize information on a variety of self-reported fringe benefits that are provided at the firms where participants are employed. Also, we explicitly deal with the problem of selection into employment. Second, our comprehensive analysis of average and distributional impacts of the JC program on job quality complements the work of Schochet et al. (2008) and many other studies that have evaluated the JC program (e.g., Zhang et al., 2008; Lee, 2009; Flores-Lagunes et al., 2010; Blanco et al., 2013, 2013b; Chen and Flores, 2015; Blanco, 2017).

Finally, we see our work contributing to the literature on good jobs (see e.g., Farber, 1997; Acemoglu, 2001; Green, 2015). Good jobs—which are characterized by a high wage—coexist in the economy with

[3] Andersson et al. (2016) use administrative data from two states that is merged with Longitudinal Employer-Household Dynamics (LEHD) data to access firm characteristics. Examples of the outcomes they consider are firm size, whether the firm has high turnover, and whether the firm fixed effect available in the LEHD (based on estimates using matched employer-employee data by Abowd and Kramarz, 2002) is above the in-sample median.

bad jobs.[4] This is consistent with evidence using employer-employee data (e.g., Card et al., 2018 and references therein) in which, conditional on worker fixed effects, firm (of employment) fixed effects explain a considerable portion of wage differentials (up to 20 percent). An important question in this literature is which workers are able to obtain those good jobs. One interpretation of our results is that an intervention that is equivalent to one year of high school (Schochet et al., 2001) appears to give access to good jobs to some disadvantaged, low-wage individuals.

In the next section we briefly discuss about the JC program and the data employed in our analysis. In Section 3 we implement PCA to construct the job quality index and provide an assessment of the proposed measure. We discuss the nonparametric bounds that allow us to estimate causal effects of interest in the presence of sample selection in Section 4. We present results in Section 5 and conclude in Section 6.

2 Job Corps and Data on Job Quality Correlates

The JC program was established in 1964 under the Economic Opportunity Act, and today operates under the provisions of the Workforce Innovation and Opportunity Act (WIOA), signed in 2014. The program is administered by the US Department of Labor (DOL) through a national and six regional offices. The JC is America's largest and most comprehensive education and job training program, at no out-of-pocket cost. Participants are selected based on several criteria, including age 16 to 24, legal US residency, economically disadvantage status, living in a disruptive environment, in need of additional education or training, and be judged to have the capability and aspirations to participate in JC (Schochet et al., 2001). The goal of the JC program is to provide services that will help disadvantaged young people improve the quality of their lives and enhance their labor market opportunities.

JC services are delivered in three different stages: outreach and admissions, center operations, and placement. Outreach and admissions is in charge of disseminating information about the program and determining eligibility of applicants. Once eligibility has been determined, the agency will assign participants to a JC center. In a typical year, about 60,000 eligible youths enroll in one of the 123 JC centers located nationwide. One unique feature of the JC program is that almost 90% of participants reside in a center while training. The typical participant receives intensive vocational and academic instruction, in addition to a variety of other services, including counseling, social and residential skills training, and health edu-

[4] Green (2015) provides a formal definition of good jobs. An implication of his definition is that the employer-employee match results in a surplus and part of this surplus is captured by the worker.

cation. To help participants finding jobs or pursuing additional training, in the last stage of the program participants are provided with placement services.[5] In contrast to other federally funded programs, JC offers more comprehensive services, which drive the cost of the program to over $1.6 billion per year (DOL, Office of Inspector General report in 2013), making it the U.S.'s largest job training program.

Due to its importance, size and nature of funding, the evaluation of the JC effectiveness has been of public interest. During the mid nineties, the DOL funded the National Job Corps Study (NJCS) to determine the program's effectiveness. The main feature of the NJCS was its random assignment. First, applicants were determined to be eligible for program participation from nearly all JC's outreach and admissions agencies, located in the 48 contiguous states and the District of Columbia.[6] Second, Mathematica Policy Research, Inc. conducted the random assignment of individuals to treatment and control groups. From a randomly selected research sample of 15,386 first-time eligible applicants, 9,409 were assigned to the treatment group and the remainder 5,977 to the control group, during the sample intake period from November 1994 to February 1996. After recording their data through a baseline interview for both treatment and control groups, a series of follow up interviews were conducted at 12, 30, and 48 months after randomization (Schochet et al., 2001).

Following Schochet et al. (2008), the NJCS sample we employ is initially restricted to individuals who completed the 48-month interview. This initial sample consists of 11,313 individuals: 6,828 and 4,485 in the randomized treatment and control groups, respectively. We additionally restrict focus on a sub-sample that excludes Hispanics. The reason is that this group experienced a negative (but statistically insignificant) effect of the program on employment. Since one of our main assumptions is that the program has a (weakly) positive, individual-level effect on employment, we aim to increase its plausibility through this sample restriction.[7] Similar sample restrictions were adopted by Blanco et al. (2013), Chen and Flores (2015), and Blanco et al. (2020). The Internet Appendix C shows that, based on key observable characteristics, the non-Hispanic sample differs little relative to the initial sample. From Table 1, it can be seen that the non-Hispanic sample consists of 8,929 individuals, with 5,384 of them randomly assigned to treatment and 3,545 to the control group. Characteristics of first time eligible JC participants summarized in Table 1 clearly indicate that the program serves disadvantaged youth. For example, on average, the majority of these youths are non-White (68%), have low levels education (less than 20% are high school

[5]See Blanco (2017) for an analysis of benefits from job placement services in the context of the JC program.

[6]This aspect of the NJCS makes it the only nationally representative (of the 48 contiguous states and the District of Columbia) social experiment conducted in the U.S.

[7]Bounds disposing of this assumption can be constructed and estimated for Hispanics. However, the resulting bounds are typically wide and thus nearly uninformative, as reported by Blanco et al. (2013).

graduates), have high levels of unemployment (35%), and earned less than $3,000 on the year prior to random assignment. In addition, a significant proportion of youths have had an arrest (26%) and received food stamps in the year prior to randomization (45%). In these calculations and throughout the analysis, we employ the NJCS design weights since different subgroups in the population had different probabilities of being included in the research sample (for details on the NJCS design weights see Schochet, 2001).

Table 2 reports, for the non-Hispanic sample and by treatment assignment, the average hourly wage and the proportions of individuals that reported having fringe benefits in the most recent job during quarter 16 after random assignment. For reference, the first row reports the proportion of individuals employed. It shows that there is a statistically significant difference of 3.2 percentage points (pp) in the employment rate between individuals in the treatment and control groups. This indicates that random assignment had a significant impact on employment—the variable that determines the observability of job characteristics. In addition to the hourly wage, the indicators for having health insurance, paid vacation, and retirement or pension benefits were considered by Schochet et al. (2008). Estimates in Table 2, which are obtained using only employed individuals as in Schochet et al. (2008), suggest that employed individuals in the treatment group earn more per hour than those in the control group ($7.51 compared to $7.19) and the difference ($0.32) is statistically significant at conventional levels. Similarly, employed individuals in the treatment group are 4, 2.6 and 5.3 pp more likely to have health insurance, paid vacation and retirement or pension benefits, respectively, than those in the control group. In our analysis below, we include additional indicators of fringe benefits available in the NJCS that are summarized in the lower panel of Table 2. Employed individuals assigned to treatment are statistically significantly more likely to have paid sick leave (by 3.5pp), child care assistance (by 1.5pp), dental plan (by 3.8pp) and tuition aid (by 2.1pp) than those in the control group. Meanwhile, the differences across treatment and control groups in the proportion of individuals with flexible hours and employer-provided transportation benefits are positive but not statistically significant.[8]

As pointed out in Schochet et al. (2008), the estimated differences in job characteristics reported in Table 2 are only suggestive and do not have a causal interpretation since they are conditional on being employed in quarter 16 after randomization. The reported statistically significant effect of random assignment on employment in quarter 16 (first row of Table 2) implies that sample selection bias is likely present in these estimates, since random assignment (a strong determinant of JC participation) simultaneously affects both

[8] In the Internet Appendix C, we report a comparison of the figures reported in Table 2 with the corresponding figures for the full sample that includes Hispanics. The magnitudes of the differences across treatment and control individuals are very similar, with the non-Hispanic sample exhibiting slightly higher differences relative to those in the full sample.

the job characteristics and their observability through employment status. The sample selection problem arises even as the intervention is randomized, and thus we must address this problem to claim causal interpretation. For the same reason, the sample selection problem also affects the estimation of effects on our proposed measure of job quality, since it is only observed for the employed. After discussing the construction of our measure of job quality in the next section, Section 4 will describe the empirical strategy we follow to conduct causal inference on the causal effects of interest while accounting for this sample selection.

3 Constructing a Job Quality Index

The economic literature supports the notion that workers evaluate a job as a bundle of attributes that may have some level of substitutability (Rosen, 1986; Woodbury, 1983; Eriksson and Kristensen, 2014). Important attributes include monetary compensation (wages) and fringe benefits (e.g., Woodbury, 1983; Farber, 1997; Eriksson and Kristensen, 2014). Ideally, to construct an index based on individual indicators of job quality, one would like to weigh variables based on theoretical considerations. However, given the lack of consensus information—theoretical or empirical—about how workers value each of the available indicators, the approach we follow is pragmatical in nature. We employ principal components analysis (PCA) to construct an empirical index of job quality that bundles the hourly wage with the available indicators of fringe benefits (those summarized in Table 2).[9] As a supplemental exercise, we also employ PCA to construct an alternative index of job quality that bundles only the available indicators of fringe benefits. The purpose of the latter exercise will be discussed in detail in Section 5.5. The PCA approach is known and widely implemented in the analysis of various important economic questions as well as in other disciplines. To our knowledge, however, this is the first study using PCA to construct an index of job quality, and to use the index to evaluate the impact of an ALMP.[10]

Several important studies in economics have employed factor and/or principal components analysis to accomplish similar tasks, that is, to produce a single interpretable measure from a set of proxies for the latent variable of interest. Examples are Black and Smith (2006) and Dillon and Smith (2017), who employ PCA to aggregate measures of college quality to construct an index. Cawley et al. (2001) use PCA to

[9] As discussed in Black and Smith (2006), this particular interpretation is appealing due to its conceptual simplicity and ease of interpretation.

[10] A couple of recent studies worth mentioning include, Cassells et al. (2018), who employ PCA to construct an index of job lack-of-quality, i.e., job precariousness, and Ribar and Wooden (2019), who employ Item Response Theory to construct an index of job quality. Both of these studies are focused on analyzing the evolution of job quality in the Australian labor market.

construct a measure of cognitive ability from the Armed Services Vocational Aptitude Battery (ASVAB) test score matrix. Heckman et al. (2013) employ factor analysis to construct indices for low-dimensional dimensions of skills from multiple available measures in the Perry Preschool Program. In those sample studies, the constructed index or indices are employed as explanatory variables. In our context, we use PCA to construct an index of job quality that is employed as an outcome (dependent variable) to learn about the impact that JC has on latent job quality.

We start with a set of J correlated variables denoted by X_j ($j = 1, ..., J$), that represent the hourly wage and the indicators of fringe benefits. As shown in the set of equations in (1), PCA employs a linear weighted combination of the J variables after being normalized to construct different uncorrelated components PC_j.

$$
\begin{aligned}
PC_1 &= w_{11}X_1 + ... + w_{1J}X_J \\
&... \\
PC_J &= w_{J1}X_1 + ... + w_{JJ}X_J,
\end{aligned}
\tag{1}
$$

where the weights w are obtained from the eigenvectors of the covariance matrix.[11] The first principal component (PC_1) explains the largest possible variation in the original data, followed by an orthogonal second component that would explain the remaining maximum variance, and so on. The variance of each principal component is the eigenvalue for the respective eigenvector, and since the sum of eigenvalues across principal components is equal to the number of variables employed, one can compute the total amount of variation explained by each component after dividing the eigenvalue by J.

Table 3 summarizes the results of the PCA used to construct the main index of job quality that we employ in subsequent analyses. This index corresponds to the first principal component estimated with the available hourly wage and nine indicators of fringe benefits for the individual's most recent job in quarter 16 after randomization. The top panel of Table 3 presents the eigenvalues and covariance explained of the first three estimated principal components. The first noteworthy feature is the large difference in eigenvalue value and the percentage of covariance explained in the original variables by the first principal component relative to higher principal components. The first principal component, which constitutes our job quality index, has an eigenvalue of 4.37 and explains about 43.7% of the covariance, while the corresponding values for the second principal component are 1.12 and 11.2%. This result primarily motivates our decision to focus on the first principal component as a measure of job quality. Moreover, Horn's parallel analysis

[11] It should be noted that estimates of principal components are obtained for each observation. Here, we have omitted an individual observation subscript for simplicity.

procedure (Horn, 1965) for selecting the number of principal components supports this decision as it strongly suggests retaining the first component.[12] Table 3 also reports estimated scoring factors for the job quality index (i.e., the first principal component). That these scoring factors are positive is indicative of a positive correlation between the variables considered and the job quality index. Looking at the nature of fringe benefits available in the data, this speaks to the desirability of jobs that come with such benefits.

In Tables 4 and 5 we further analyze the job quality index. For instance, in Table 4 we separate individuals into four mutually exclusive groups defined by quartiles of the distribution of the job quality index. Thus, all individuals in the group with the lowest values of the job quality index, shown under the column labeled "Up to Q1", have an index value that is less than the first quartile, followed by those with index values between the first and second quartile ("Q1 to Q2"), and so on. For each group, we compute the mean or proportion for each of the variables used in the construction of the job quality index.[13] From Table 4, it is evident that, as we consider quartiles with higher values of the job quality index across the columns, there is a monotone increase in the proportions of all of the variables that make up the index (with the exception of the variable employer-provided transportation) and also the average hourly wage. These results (i.e., average wage and fringe benefits increasing with job quality) is consistent with a high correlation between the job quality dimensions captured by monetary compensation and fringe benefits.

In Table 5, we present suggestive evidence about the relationship between the job quality index and selected post-treatment variables and demographic characteristics. Focusing on the top panel, we observe that average job quality is larger in the occupations classified as clerical, mechanics and other, while the average job quality is lower in the services, sales, construction, private household, and agriculture, which is ranked as the occupation with the worst average job quality. For individuals that had access to JC, their average job quality is larger if they received job placement services. The average job quality is much larger for individuals employed full-time (working at least 35 hours in a week) relative to those employed part-time. Males have a higher average job quality when compared to females. Average job quality is larger for women with children, followed by men without children, while the worst average job quality is observed for women without children.

In the bottom panel of Table 5, we further explore the relationship between the job quality index

[12]For example, adjusted eigenvalues after implementing Horn's test are 4.31 and 1.08 for the first and second principal component, respectively. While, strictly speaking, Horn's (1965) test suggests focusing on components with values larger than 1, and the second component is 1.08, we focus exclusively on the first component since the scoring factors for the second principal component change sign, suggesting that it does not have a straightforward interpretation. This is in contrast to the scoring factors for the first component, shown in Table 3, which are all positive.

[13]Our analysis in Table 4 is in the spirit of Filmer and Pritchett (2001), who employed PCA to construct an index of wealth using asset ownership variables in the context of India.

and selected post-treatment variables and demographics within a multivariate regression framework. The relationship between job quality and the local unemployment rate is negative and statistically significant at a 5 percent level.[14] Utilizing placement services is positively associated with job quality, and this relationship is statistically significant. Part-time employment is negatively related to job quality and this relationship is also statistically significant. Other statistically significant coefficients are obtained for the female indicator and its interaction with having children, so relative to males without children, being a woman with children is associated with higher levels of job quality, while females without children are associated with lower ones. Finally, we also confirm the previous descriptive results regarding differences in average levels of job quality by occupation (conditional on the other regression covariates) given that we observe that some differences amongst occupations are statistically significant. In summary, the evidence presented in Tables 4 and 5 suggests that our index indeed measures latent job quality. Furthermore, our measure of job quality conforms to the intuition that higher values of the index are related to higher values of desirable attributes of a job in terms of the hourly wage and fringe benefits. Lastly, we employ this measure to summarize descriptively the relationship between job quality and other relevant variables.

4 Methodology

Having constructed a job quality index, we aim to use it in evaluating whether JC training had an impact on job quality. In estimating impacts of JC on job quality, we need to tackle two main issues, one fundamental and one related to the nature of the NJCS data. The first, fundamental issue is a consequence of only being able to observe the measure of job quality for individuals that are employed during the 16th quarter after random assignment. The randomized nature of the data available in the NJCS is not directly helpful with this issue since employment is affected by the training program. Our approach, then, is to estimate nonparametric bounds on the causal parameters of interest in the presence of selection into employment. The main advantage in this approach is that we are able to rely on arguably weaker assumptions relative to other methods that rely on constant effects, distributional assumptions, or exclusion restrictions.[15] This approach also allows us to analyze distributional impacts of JC through the estimation of quantile treatment effects (defined below). In contrast, a perceived disadvantage of our

[14] To obtain the local unemployment rate for individuals in our sample, we matched restricted-use NJCS zip code data to the county-level unemployment rate in year 2000 from the Bureau of Labor Statistics' Local Area Unemployment Statistics program. These are the same local unemployment rates employed in Flores-Lagunes et al. (2010).

[15] Well-known techniques that achieve point identification are selection models (e.g., Heckman, 1979) and instrumental variable models (e.g., Heckman, 1990; Heckman and Smith, 1995; Imbens and Angrist, 1994; Angrist, Imbens and Rubin, 1996).

approach is that we undertake inference for a subpopulation consisting of those individuals that would be employed regardless of treatment assignment. This feature is in the same spirit as instrumental variables estimators under heterogeneous effects conducting inference for the "compliers" (e.g., Imbens and Angrist, 1994).

There is now a substantial literature that employs nonparametric bounds to estimate causal effects in the presence of sample selection, or more generally, missing data (Manski, 1997; Manski and Pepper, 2000; Zhang and Rubin, 2003; Imai, 2007, 2008; Blundell et al., 2007; Lee, 2009; Lechner and Melly, 2010; Bhattacharya et al., 2012; Flores and Flores-Lagunes, 2010, 2013; Chen and Flores, 2015; Frandsen and Lefgren, 2016; Blanco et al., 2020). Our approach builds upon Horowitz and Manski (2000), who proposed a general framework to construct bounds on treatment effects when data are missing due to a nonrandom process, such as selection into employment. These bounds only require the randomization of treatment assignment (and that the outcome has a bounded support), while we add two other assumptions to improve the informativeness (tightness) of the resulting bounds. The additional assumptions rely on a principal stratification framework (Frangakis and Rubin, 2002) and consist of an assumption of individual-level monotonicity of the treatment on employment, and stochastic dominance of the outcome distribution of a particular subpopulation (principal stratum) over another. They are formalized below. The same or similar nonparametric bounds have been advanced and used by, among others, Zhang and Rubin (2003), Imai (2007, 2008), Blundell et al. (2007), Lee (2009), Lechner and Melly (2010), Blanco et al. (2013), Chen and Flores (2015), and Blanco et al. (2020).

The second econometric issue we deal with is noncompliance with the randomized treatment assignment, which is a feature in the NJCS data. Noncompliance occurs because some individuals assigned to undergo JC training did not take it up, while some of those assigned not to undergo JC ended up taking it up. The extent of noncompliance in the NJCS is widely documented (e.g., Schochet et al., 2001) and it is in the order of 30 percent. One alternative in the presence of noncompliance is to focus on estimating bounds for intention-to-treat effects, that is, the effect of random assignment as opposed to JC training. In this case, the nonparametric bounds described above are applied using the random assignment indicator as the treatment. Such effects have the interpretation of the effect of the availability of JC on job quality. A second possibility is to directly address noncompliance by employing the randomized treatment assignment as an instrumental variable for JC training participation. This approach allows undertaking inference on the effect of actual JC training on the job quality for individuals who comply with their treatment assignment, known as compliers (Imbens and Angrist, 1994; Angrist et al., 1996). To do this, we extend

the nonparametric bounds that address selection into employment to also address noncompliance. These nonparametric bounds build upon the work of Imai (2007), Chen and Flores (2015), and Blanco et al. (2020). We present the details of these nonparametric bounds in the remaining of this section. Note that, while we describe the bounds in terms of job quality, the bounding technique can be applied in other contexts where sample selection and/or noncompliance are present.

4.1 Notation, Principal Stratification, Assumptions, and Causal Parameters

Let $Z_i = z \in \{0, 1\}$ indicate whether unit i is randomly assigned to receive JC ($Z_i = 1$) or to the group embargoed from JC ($Z_i = 0$). Let $T_i = t \in \{0, 1\}$ be an indicator of whether unit i actually received JC services ($T_i = 1$) or not ($T_i = 0$). Then, $Z_i = T_i$ if there were perfect compliance with treatment assignment. The observed job quality in the most recent job during quarter 16 after randomization (the outcome) for unit i is denoted by Y_i. We use $S_i = s \in \{0, 1\}$ to denote whether an individual has a job during quarter 16 after randomization ($S_i = 1$) or not ($S_i = 0$). Job quality (Y_i) is only observed for individuals who are employed during quarter 16 after randomization ($S_i = 1$).

Using the potential outcomes framework, let $T_i(z)$ denote the binary potential treatment received as a function of random assignment $Z_i = z$, that is, $T_i(1)$ and $T_i(0)$ represent the potential treatment (JC participation) status of unit i when randomly assigned to the treatment and control group, respectively. Then, the observed treatment receipt indicator is $T_i = Z_i T_i(1) + (1 - Z_i)T_i(0)$. Also, define the potential employment indicator as a function of random assignment as $S_i(z)$; and the composite potential employment indicator as a function of random assignment and JC participation as $S_i(z, t)$. Finally, let $Y_i(z)$ denote the potential job quality as a function of random assignment, and $Y_i(z, t)$ denote the composite potential job quality as a function of random assignment and actual JC participation. Note that all potential outcomes, $Y_i(z)$ and $Y_i(z, t)$, are defined only when the corresponding potential employment indicators, $S_i(z)$ and $S_i(z, t)$, equal 1. The causal parameters of interest, defined later in this section, are comparisons of these potential outcomes for specific subpopulations.

To address the two econometric issues, we employ principal stratification (Frangakis and Rubin, 2002). One example of principal stratification is the familiar use of random assignment as an instrument for endogenous JC participation to address noncompliance, following the framework of Imbens and Angrist (1994) and Angrist et al. (1996). Define the following latent subpopulations (principal strata) based on the potential treatment $T_i(z)$: the *compliers*, $c = \{i : (T_i(0), T_i(1)) = (0, 1)\}$; the *always-takers*,

$a = \{i : (T_i(0), T_i(1)) = (1,1)\}$; the *never-takers*, $n = \{i : (T_i(0), T_i(1)) = (0,0)\}$; and the *defiers*, $d = \{i : (T_i(0), T_i(1)) = (1,0)\}$. In general, principal stratification can be employed to analyze causal effects when post-randomization variables affected by random assignment need to be controlled. This is the case when addressing the selection into employment issue. The key insight is to obtain causal effects by comparing treated and non-treated individuals within principal strata, which are latent subpopulations whose membership is not affected by random assignment since their members have the same potential values of the post-randomization variable. In the previous stratification, the post-randomization variable is the endogenous compliance status with random assignment and the principal strata consists of the well-known compliance types. In the context of selection into employment, the principal strata is defined based on values of the potential employment indicator, $S_i(z)$: the *always-employed*, $EE = \{i : (S_i(0), S_i(1)) = (1,1)\}$; the *employed only if assigned to the treatment group*, $NE = \{i : (S_i(0), S_i(1)) = (0,1)\}$; the *never-employed*, $NN = \{i : (S_i(0), S_i(1)) = (0,0)\}$; and the *employed only if assigned to the control group*, $EN = \{i : (S_i(0), S_i(1)) = (1,0)\}$. To simultaneously address noncompliance and selection into employment, we combine the two previous principal stratifications, that is, $\{a, n, c, d\} \times \{EE, NE, NN, EN\}$, which yields 16 possible latent principal strata.

We present the assumptions employed in the bounds estimated below in two sets. The first set, consisting of five assumptions, allow the partial identification of the effects of interest, while the second set—presented later—are adopted to obtain tighter bounds. The first set is as follows:

Assumption 1 Stable Unit Treatment Value Assumption (Rubin, 1980).

Assumption 2 Randomized Treatment Assignment, $Z_i \perp\!\!\!\perp \{T_i(z), S_i(z), Y_i(z), S_i(z,t), Y_i(z,t)\}$ for $z, t \in \{0,1\}$.

Assumption 3 Nonzero Average Causal Effect of Z on T, $E[T_i(1) - T_i(0)] \neq 0$.

Assumption 4 Individual-level Monotonicity of T_i in Z_i, $T_i(1) \geq T_i(0)$ *for all* i.

Assumption 5 Exclusion Restriction of Z_i from (a) $Y_i(z,t)$: $Y_i(z,t) = Y_i(z',t)$; and (b) $S_i(z,t)$: $S_i(z,t) = S_i(z',t)$ for all $z, z', t \in \{0,1\}$ with $z \neq z'$.

Assumption 1 implies that the potential outcomes for each unit i are unrelated to the treatment status of other units and that there are no hidden versions of the treatment in question (Imbens and Rubin, 2015). An important consequence of this assumption is that general equilibrium effects are rule out, which in the context of JC is plausible since the program serves a comparatively small number of disadvantaged youth in each location served. Assumption 2 is satisfied by design in the context of the NJCS (see Schochet et

al., 2001). Assumption 3 requires Z_i to have a non-zero average effect on T_i. This is verifiable and it holds in the context of the NJCS. Assumption 4, also known as the "no defiers" assumption, states that there is no unit i that does the opposite of his/her random assignment. This assumption has been extensively employed in the literature analyzing the NJCS data (e.g., Schochet et al., 2001, 2008; Chen and Flores, 2015; Chen et al., 2018) since it is hard to argue that defiers would exist in the context of a population that has gone through the trouble of applying to certify themselves eligible for JC training.

Lastly, Assumption 5 is an extended exclusion restriction assumption on the potential values of the two post-treatment variables of interest, $Y_i(z,t)$ and $S_i(z,t)$. To facilitate referencing to them separately below, we refer to each of them as Assumption 5(a) and Assumption 5(b). Technically, each implies that any effect of the random assignment Z_i on the potential values of $Y_i(z,t)$ and $S_i(z,t)$ must be via the effect of Z_i on the actual treatment receipt T_i. This assumption has also been extensively employed in the literature analyzing the NJCS data (e.g., Schochet et al., 2001, 2008; Chen et al., 2018) in the context of labor market outcomes such as employment and earnings. Nevertheless, there are scenarios (such as when focusing on short-term labor market outcomes) under which this assumption may be violated (see the discussion in Chen et al., 2018). It is well known that, in the absence of selection into employment, Assumptions 1 to 5 point identify the average treatment effect for the subpopulation of *compliers*, $E[Y_i(z,1) - Y(z,0)|c]$ (Imbens and Angrist (1994) and Angrist et al., 1996). However, they are not able to point identify causal effects under selection into employment.

As mentioned before, the principal stratification that addresses both econometric issues consists of 16 principal strata. Imposing Assumption 4 eliminates *dEE, dNE, dNN and dEN* since these contain *defiers*, while Assumption 5(b) eliminates *aNE, aEN, nNE, and nEN* since for these strata there is an effect of the random assignment Z_i on the employment indicator S_i that does not work through the actual treatment receipt T_i (to see this, note that Assumption 5(b) implies $S_i(1) = S_i(0)$ for noncompliers *a, n*). Table 6 shows the mixture of the remaining 8 principal strata contained within observed cells defined by the values of Z_i, T_i and S_i, after imposing Assumptions 1 to 5.

Causal effects of actual JC participation on the outcome of interest are defined by comparing the potential outcomes $Y_i(z,1)$ and $Y_i(z,0)$ within principal strata, since the conditioning on principal strata allows causal interpretation while controlling for post-treatment variables. The first set of parameters we define are ITT effects that ignore noncompliance. For these effects, the subpopulation of focus is the *always-employed* regardless of treatment assignment (EE) (see Lee, 2009; Zhang et al., 2008; Blanco et al., 2013). The main reason is that this is the only stratum whose outcomes are defined under both random

assignment arms, and thus (point or partial) identification of effects for this stratum avoids assumptions required to extrapolate to the other subpopulations.[16] As shown in the next section, in our sample, the group of always-employed represents an estimated 63% of the population.

To formally define the causal parameters of interest, let $f_{k|z}$ denote the probability distribution function (pdf) of the potential outcome $Y_i(z)$ for stratum k, and let its $\alpha-$quantile be denoted by $q_{k|z}(\alpha) \equiv inf\{y : F_{k|z} \geq \alpha\}$ for $0 < \alpha < 1$, where $F_{k|z}$ denotes the cumulative distribution function (cdf) of $Y_i(z)$ for stratum k evaluated at y. The following are stratum-specific ITT and quantile ITT effects of random assignment on job quality measured 16 quarters after random assignment, which are defined for individuals that are always employed regardless of random assignment:

$$ITT_{EE} \equiv E[Y_i(1)|EE] - E[Y_i(0)|EE], \tag{2}$$

$$QITT^{\alpha}_{EE} \equiv q_{EE|1}(\alpha) - q_{EE|0}(\alpha). \tag{3}$$

The causal effects that also address noncompliance will focus on a different subpopulation. Note from Table 6 that there is only one principal stratum for which the potential job quality outcome is well-defined under both treatment arms. This is the cEE stratum, i.e., the *compliers* who are *always-employed* regardless of treatment assignment. For individuals in all other strata, at least one potential outcome under each treatment arm is undefined.[17] We thus concentrate on partial identification of causal effects for the subpopulation of compliers who are always-employed regardless of treatment assignment. This way we avoid potentially strong assumptions to extrapolate effects to other principal strata for which potential job quality is undefined at least under one treatment arm. As discussed in the next section, in our sample, the group of always-employed compliers represents an estimated 42% of the population, and it is the largest principal stratum. The causal parameters that simultaneously account for noncompliance and selection-into-employment are the following stratum-specific average and quantile treatment effects of JC participation on job quality measured 16 quarters after random assignment:

$$ATE_{cEE} \equiv E[Y_i(1)|cEE] - E[Y_i(0)|cEE], \tag{4}$$

$$QTE^{\alpha}_{cEE} \equiv q_{cEE|1}(\alpha) - q_{cEE|0}(\alpha). \tag{5}$$

[16] Partial identification results for the case of the ITT can be found in Blanco et al. (2013) and Internet Appendix A.

[17] For example, all individuals that belong to strata in the top row of Table 6 are not employed ($S_i = 0$) during quarter 16 after randomization, so their potential job quality is undefined.

4.2 Assumptions to Tighten the Nonparametric Bounds

In the rest of this section, we will focus on the construction of bounds on the effects that address both econometric problems, that is, effects (4) and (5). The bounds on the ITT effects ((2) and (3)) can be found in the Internet Appendix A. It is possible to construct bounds on the effects (4) and (5) under Assumptions 1 to 5.[18] However, these bounds are wide and this is a reason to entertain additional assumptions to tighten them. We employ two additional assumptions to tighten the bounds.

Assumption 6 Individual-level Monotonicity of S_i in T_i, $S_i(1) \geq S_i(0)$ for *compliers*.

Assumption 6 states that, for every *complier*, the effect of T_i on S_i is non-negative, which implies that the cEN stratum proportion is zero. It also implies that the effect of JC training on employment at the end of the 16th quarter after randomization is non-negative for every individual that complied with treatment assignment. Assumption 6 yields a testable implication in that the mean effect of JC on employment for compliers is non-negative, $E[S(1) - S(0)|c] \geq 0$, which is point identified under Assumptions 1 to 5 (this mean effect is shown to be positive and statistically significant in the following section).

At least two factors may cast a doubt on the plausibility of Assumption 6: the locked-in effect, which prevents trainees from being employed while undergoing treatment, and a potential increase in an individual's reservation wage. We argue that the relatively long period of time between random assignment and when the job quality outcome is measured (16 quarters) lessens the potential threats of those two factors. The reason is that, over time, most individuals would have finished undergoing training; and if those who potentially increase their reservation wage have not been able to find employment, they are likely to lower their reservation wage and take up employment. Similar assumptions have been used in the JC literature (e.g., Lee, 2009; Blanco et al., 2013a, 2013b; Chen and Flores, 2015; Blanco et al., 2020). In addition, since the group of Hispanics in the NJCS have been found to have a negative (but statistically insignificant) effect of JC on employment during quarter 16, we do not analyze this group.[19] A similar restriction was adopted by Blanco et al. (2013, 2013b), Chen and Flores (2015), and Blanco et al. (2020).

The second assumption we entertain to tighten the bounds on the causal effects of interest relies on imposing stochastic dominance of distributions of potential outcomes across different principal strata. Intuitively, this type of assumption can be employed when some principal strata are likely to have characteristics that make them more likely to have higher potential outcomes relative to others. Similar assumptions are

[18] The expressions for the bounds under Assumptions 1 to 5 are presented in Internet Appendix B.

[19] Internet Appendix I presents estimated bounds without Assumption 6 for the sample of Hispanics. As expected, these estimated bounds are wide.

common within the principal stratification literature (e.g., Zhang ad Rubin, 2003; Imai, 2007; Zhang et al., 2008; Blanco et al. 2013, 2013b; Chen and Flores, 2015; Blanco et al., 2020). Formally, we consider:

Assumption 7 Stochastic Dominance Across Strata: $F_{cEE|1}(y) \leq F_{cNE|1}(y)$ for all $y \in \mathcal{Y}$.

For bounding ATE_{cEE}, a weaker mean dominance version of Assumption 7 is employed, namely, $E[Y(1)|cEE] \geq E[Y(1)|cNE]$. Assumption 7 states that the distribution of the potential job quality under JC participation ($Y(1)$) for the cEE stratum stochastically dominates that of the cNE stratum. To think about the plausibility of this assumption, recall that the cEE stratum consists of compliers that would be employed during quarter 16 after random assignment regardless of treatment assignment. In turn, the cNE stratum consists of compliers who would be employed only if they participate in JC. Intuitively, the compliers that are always observed employed likely have characteristics (e.g., "ability") that result in better labor market outcomes (e.g., better job quality) relative to the compliers that "need" JC training to find employment during quarter 16 after randomization. Thus, this assumption implies a positive correlation between employment and job quality. Such positive correlation—i.e., positive selection into employment—is implied by standard models of labor supply when job quality is proxied by wages (e.g., Blundell et al., 2007).

While Assumption 7 is not directly testable since the outcome distributions of the principal strata involved are not point identified, indirect evidence about its plausibility can be obtained by analyzing the average pre-treatment characteristics of those strata. Desirable pre-treatment characteristics to analyze are those that are highly correlated with the outcome. This type of indirect assessment is common in the literature employing similar assumptions (e.g., Flores and Flores-Lagunes, 2010, 2013; Blanco et al., 2013, 2013b, 2020; Chen and Flores, 2015). To implement it, note that under Assumptions 1 to 6 average pre-treatment characteristics (X) for the cEE stratum are point identified as: $\frac{p_{01|0}\overline{X}^{001} - p_{01|1}\overline{X}^{101}}{p_{01|0} - p_{01|1}}$, where $p_{ts|z} \equiv Pr(T_i = t, S_i = s|Z_i = z)$ and $\overline{X}^{zts} = E[X|Z = z, T = t, S = s]$ for $z, t, s = \{0, 1\}$, where $\overline{X}$ is the average of a pre-treatment characteristic. Whereas the cNE stratum characteristics are not point identified, one can point identify average characteristics for the combined $\{cEE, cNE\}$ strata by: $\frac{p_{11|1}\overline{X}^{111} - p_{11|0}\overline{X}^{011}}{p_{11|1} - p_{11|0}}$. Then, given the implied ranking of strata in Assumption 7, one would expect to find evidence suggesting that the average baseline characteristics for the cEE stratum are more favorable than those for the $\{cEE, cNE\}$ strata, that is, $\frac{p_{01|0}\overline{X}^{001} - p_{01|1}\overline{X}^{101}}{p_{01|0} - p_{01|1}} \geq \frac{p_{11|1}\overline{X}^{111} - p_{11|0}\overline{X}^{011}}{p_{11|1} - p_{11|0}}$. We implement this indirect assessment in section 5.1.

4.3 Construction of the Nonparametric Bounds Under the Assumptions

In this section we present details of the partial identification strategy for the causal parameters of interest. We concentrate on the nonparametric bounds imposing all of the assumptions, while the details on bounds imposing subsets of assumptions can be found in Internet Appendix B. Let π_k denote the proportion of the principal stratum k in the population, where k represents any of the remaining 7 different strata in Table 6 after imposing Assumptions 1 to 6.[20] Let the conditional distribution of the observed outcome Y_i for units in cell $\{T_i = t, S_i = s, Z_i = z\}$ be denoted by $f_{ts|z}$. Under Assumptions 1 to 6, all 7 stratum proportions can be point identified as: $\pi_{aNN} = p_{10|0}$, $\pi_{aEE} = p_{11|0}$, $\pi_{nNN} = p_{00|1}$, $\pi_{nEE} = p_{01|1}$, $\pi_{cNN} = p_{10|1} - p_{10|0}$, $\pi_{cNE} = p_{00|0} - p_{00|1} - p_{10|1} + p_{10|0}$, and $\pi_{cEE} = p_{01|0} - p_{01|1}$. Their identification can be seen after some simple algebra from Table 6. Importantly, note that the proportion of the stratum of interest π_{cEE} is point identified.

Following Horowitz and Manski (1995) and Imai (2007), note that the distributions of observed outcomes in cells $\{T = 1, S = 1, Z = 1\}$ and $\{T = 0, S = 1, Z = 0\}$, denoted respectively by $f_{11|1}$ and $f_{01|0}$, are weighted averages of the distributions of potential outcomes for different strata (see Table 6), specifically:

$$f_{11|1} = \frac{\pi_{cEE}}{p_{11|1}} f_{cEE|1} + \frac{\pi_{cNE}}{p_{11|1}} f_{cNE|1} + \frac{\pi_{aEE}}{p_{11|1}} f_{aEE|1} \tag{6}$$

$$f_{01|0} = \frac{\pi_{cEE}}{p_{01|0}} f_{cEE|0} + \frac{\pi_{nEE}}{p_{01|0}} f_{nEE|0}, \tag{7}$$

where $p_{11|1} = \pi_{cEE} + \pi_{cNE} + \pi_{aEE}$ and $p_{01|0} = \pi_{cEE} + \pi_{nEE}$. Only one of the principal stratum distributions of potential outcomes are point identified in each of the expressions (6) and (7): $f_{aEE|1} = f_{11|0}$ and $f_{nEE|0} = f_{01|1}$, respectively. Rewriting equations (6) and (7) using these identified distributions, along with the point identified stratum proportions and their relationships, we obtain:

$$f_1 = \frac{\pi_{cEE}}{p_{11|1} - p_{11|0}} f_{cEE|1} + \frac{\pi_{cNE}}{p_{11|1} - p_{11|0}} f_{cNE|1} \tag{8}$$

$$f_0 = \frac{\pi_{cEE}}{p_{01|0} - p_{01|1}} f_{cEE|0} = f_{cEE|0}, \tag{9}$$

[20] Note that the assumptions that rule out certain principal strata, discussed above, imply that the corresponding stratum proportions are equal to zero in the population.

where

$$f_1 \equiv \frac{p_{11|1} f_{11|1} - p_{11|0} f_{11|0}}{p_{11|1} - p_{11|0}} \qquad \text{and} \qquad f_0 \equiv \frac{p_{01|0} f_{01|0} - p_{01|1} f_{01|1}}{p_{01|0} - p_{01|1}}. \tag{10}$$

Intuitively, equation (8) is obtained by subtracting—using the appropriate weights—the point identified distribution of potential outcomes for aEE from the the left-hand side of equation (6). Similarly, equation (9) is obtained by subtracting the point identified distribution of potential outcomes for nEE from the left-hand side of equation (7).[21] Importantly, (9) shows that $f_{cEE|0}$ is point-identified under Assumptions 1 to 6.

The construction of the nonparametric bounds we use employs trimming bounds (e.g., Lee, 2009). In a best-case (worst-case) scenario, $f_{cEE|1}$ in (8) will be bounded from above (below) by assuming that the largest (smallest) $\frac{\pi_{cEE}}{p_{11|1}-p_{11|0}}$-fraction of values of Y in the pdf f_1 belongs to the cEE stratum. The consequence of adding Assumption 7 (stochastic dominance between strata cEE and cNE), is that the distribution f_1 in (8) bounds from below the potential outcomes distribution under JC participation for the cEE stratum, i.e., $f_{cEE|1}$. The reason for this result is that f_1 in (8) contains a mixture of distributions for the two principal strata compared under Assumption 7. As a result, the trimming bounds are only employed to obtain an upper bound on $f_{cEE|1}$, since f_1 provides a lower bound. Therefore, under Assumptions 1 to 7, the upper and lower bounds on $f_{cEE|1}$ are contrasted with the point identified $f_{cEE|0}$ in (9) to compute the bounds for the parameters in equations (4) and (5).

Formally, let the α-quantile of f_1 and f_0 in equations (8) and (9) be denoted by $r_z(\alpha) \equiv \inf\{y : F_z(y) \geq \alpha\}$ for $0 < \alpha < 1$ and $z = 0, 1$. In addition, define the distribution $U_{\gamma|z}$ as:

$$U_{\gamma|z}[-\infty, y] \quad \equiv \quad \begin{cases} 0 & \text{, if } y < r_z(1-\gamma) \\ \frac{F_z(y)+\gamma-1}{\gamma} & \text{, if } y \geq r_z(1-\gamma), \end{cases}$$

where $\gamma \in (0, 1)$ is the trimming proportion used to construct trimming bounds on $f_{cEE|1}$, as described above. Under Assumptions 1 to 7, the sharp bounds are $LB \leq ATE_{cEE} \leq UB$, and $LB_\alpha \leq QTE^{\alpha}_{cEE} \leq$

[21] Equations (8) and (9) provide two additional testable implications, whereby f_1 and f_0 must be valid pdf's (i.e., non-negative and integrate to one).

UB_α, where:[22]

$$\begin{aligned}
LB &= \int y df_1 - \int y df_0 \\
UB &= \int y dU_{(p_{01|0}-p_{01|1})/(p_{11|1}-p_{11|0})|1} - \int y df_0 \\
LB_\alpha &= r_1(\alpha) - r_0(\alpha) \\
UB_\alpha &= r_1\left(1 - \frac{(1-\alpha)(p_{01|0}-p_{01|1})}{p_{11|1}-p_{11|0}}\right) - r_0(\alpha),
\end{aligned}$$

where the following terms are: $\int y df_0 = \frac{p_{01|0}\overline{Y}^{001} - p_{01|1}\overline{Y}^{101}}{p_{01|0}-p_{01|1}}$, $\int y df_1 = \frac{p_{11|1}\overline{Y}^{111} - p_{11|0}\overline{Y}^{011}}{p_{11|1}-p_{11|0}}$, and $\overline{Y}^{zts} = E[Y|Z = z, T = t, S = s]$ for $z, t, s = \{0, 1\}$.

4.4 Estimation and Inference

Estimation of all the bounds presented in Section 4.2 rely on sample analogs. We use the indicator function $1(Y_i \leq \tilde{y})$ and M different values of $\tilde{y}$ spanning the support of the observed outcome to estimate the conditional distribution of the observed outcome Y_i evaluated at the different values of $\tilde{y}$, e.g., $\hat{F}_{ts|z}(\tilde{y})$. The conditional distribution is used to obtain the sample analog versions of the distributions that are part of f_1 and f_0 in (10), namely, $f_{11|1}, f_{11|0}, f_{01|0}$, and $f_{01|1}$. We estimate the conditional probabilities $p_{ts|z}$ using sample analogs $\hat{p}_{ts|z}$:

$$\hat{p}_{11|1} = \frac{\sum_{i=1}^n T_i \cdot W_i \cdot Z_i}{\sum_{i=1}^n Z_i}, \qquad \hat{p}_{11|0} = \frac{\sum_{i=1}^n T_i \cdot W_i \cdot (1-Z_i)}{\sum_{i=1}^n (1-Z_i)},$$
$$\hat{p}_{01|0} = \frac{\sum_{i=1}^n (1-T_i) \cdot W_i \cdot (1-Z_i)}{\sum_{i=1}^n (1-Z_i)}, \qquad \hat{p}_{01|1} = \frac{\sum_{i=1}^n (1-T_i) \cdot W_i \cdot Z_i}{\sum_{i=1}^n Z_i}.$$

The estimates of the conditional distributions ($\hat{F}_{ts|z}(\tilde{y})$) and conditional probabilities ($\hat{p}_{ts|z}$) are then plugged into the distribution versions of f_1 and f_0 in (10) to construct estimates $\hat{F}_1(\tilde{y})$ and $\hat{F}_0(\tilde{y})$, specifically,

$$\hat{F}_1(\tilde{y}) = \frac{\hat{p}_{11|1}\hat{F}_{11|1} - \hat{p}_{11|0}\hat{F}_{11|0}}{\hat{p}_{11|1} - \hat{p}_{11|0}}, \qquad \hat{F}_0(\tilde{y}) = \frac{\hat{p}_{01|0}\hat{F}_{01|0} - \hat{p}_{01|1}\hat{F}_{01|1}}{\hat{p}_{01|0} - \hat{p}_{01|1}}.$$

Finally, the estimates $\hat{F}_1(\tilde{y})$, $\hat{F}_0(\tilde{y})$, and $\hat{p}_{ts|z}$ are used to compute the expected values and α-quantiles (obtained by inverting the corresponding distributions) needed to estimate the corresponding bounds.

[22] The sharpness of these bounds follows from existing results in Imai (2007, 2008) and Blanco et al. (2020).

Concerning statistical inference, we employ Imbens and Manski (2004; IM hereafter) confidence intervals.[23] These confidence intervals include the true value of the parameter of interest with a given probability. We employ bootstrapped standard errors to construct 95% IM confidence intervals, where, when the job quality index is the outcome, it is recomputed within the bootstrapped sample.

5 Results

We start by presenting some estimates aimed at gauging the plausibility of the assumptions employed for bounding the effects of interest. Second, we present estimated bounds for the average effect of JC eligibility (ITT) on job quality measured 16 quarters after random assignment for individuals that are always-employed regardless of random assignment (i.e., the EE stratum). These bounds address selection into employment but not noncompliance, but they represent effects for a larger subpopulation. Third, we present and discuss estimated bounds, under Assumptions 1 to 7, on the average effect of JC training on job quality for individuals that comply with their treatment assignment and are always employed regardless of treatment assignment (i.e., the cEE stratum).[24] Subsequently, we analyze the average effect of JC on each of the inputs that make up the job quality index. We do this by estimating bounds on the corresponding average effects. Finally, we complement the analysis on average effects by presenting and discussing estimated bounds on the quantile treatment effects of JC participation on the job quality index for the cEE stratum.

5.1 Assessment of Assumptions Employed

The plausibility of Assumptions 1 to 5 has been extensively discussed in the context of JC (e.g., Schochet et al., 2001; Schochet et al., 2008; Frumento et al., 2012; Chen and Flores, 2015; Chen et al., 2018; Blanco et al., 2020). Thus, we focus on assessing the plausibility of Assumptions 6 and 7. As discussed in Section 4.2, a testable implication of Assumption 6 is that $E[S(1) - S(0)|c] \geq 0$. The estimated average treatment effect of JC on employment during quarter 16 after randomization for the compliers (the so-called $LATE$) is positive (5.4pp) and statistically significant (p-value=0.001) for the sample of non-Hispanics. For other sub-samples of non-Hispanics for whom we will also discuss estimated bounds, the corresponding estimates

[23] The asymptotic normality of the bounds, combined with the fact that, by construction, the upper bounds are always larger than the lower bounds, justifies the use of Imbens and Manski's (2004) confidence intervals by Lemma 3 of Stoye (2009).

[24] The estimated bounds for the parameters of interest under other combinations of Assumptions are relegated to Internet Appendix G. The results from these other bounds are less informative relative to those employing Assumptions 1 to 7 presented here.

and levels of statistical significance are very similar (these estimates are reported in Internet Appendix E). As mentioned in section 4.2, the reason for focusing on samples that exclude Hispanics is related to the plausibility of Assumption 6 for Hispanics. The estimated $LATE$ on employment for Hispanics is negative (-3.16pp) and statistically insignificant (p-value=0.342): while not directly contradicting Assumption 6, the negative point estimate casts doubt on the validity of this assumption for this group.

Based on the indirect assessment of Assumption 7 explained in Section 4.2, we find support for this assumption since most of the estimated average pre-treatment characteristics positively correlated to labor market outcomes (education, employment and wages) are higher for the cEE stratum relative to the group consisting of the cEE and cNE strata.[25] For example, the cEE stratum has a higher proportion of GED completers (0.061 versus 0.043 and statistically significant), high school graduates (0.208 versus 0.204), higher wages (5.052 versus 5.014), and a higher proportion of individuals with working history (0.836 versus 0.829). Similar patterns are found for the other sub-samples of non-Hispanics. In sum, this indirect evidence is consistent with the validity of Assumption 7.

5.2 Estimated Bounds on the Intention-to-Treat Effects on the Job Quality for the Always-Employed

We start by estimating bounds on the average effect of Job Corp eligibility on job quality measured 16 quarters after randomization for individuals that are always employed regardless of treatment assignment (i.e., the EE stratum). While these are intention-to-treat effects (ITT), these effects are partially identified for a larger subpopulation (the always employed) that represents 63 percent of the non-Hispanic sample. The nonparametric bounds we employ for the ITT effects, which ignore non-compliance, are those in Imai (2008), Lee (2009), Zhang et al., (2008), and Blanco et al., (2013). These bounds, which are presented in Internet Appendix A, rely on a subset of assumptions relative to the bounds discussed in sections 4.1 and 4.2: Assumptions 1 and 2, and Assumptions 6 and 7.[26]

Estimated bounds on the average ITT effect on job quality, for the non-Hispanic sample and selected subsamples, are presented in Table 7. The subsamples considered are (non-Hispanic) Males, Females, Teenagers (between 16 and 19 years of age) and Young Adults (between 20 and 24 years of age). The job quality index is normalized to ease the interpretation of results. The top panel of Table 7 presents

[25] The corresponding estimated averages and their differences are reported in Internet Appendix Table E1.

[26] More precisely, Assumption 6 has to be strengthen slightly to include all individuals (not just the compliers), while Assumption 7 relates the always-employed stratum (EE) to the stratum of individuals who would be employed only if assigned to the treatment group (EN).

the estimated strata proportions, which in this case are only three given that the set of principal strata related to noncompliance is not employed. The always-employed (EE) is the largest stratum, accounting for an estimated 63.2 percent of all non-Hispanics. For the subsamples, the estimated proportion of always-employed range from 60.6 percent for females to 65.6 percent for the sample of young adults.

The subsequent panels in Table 7 report the estimated bounds within brackets and their 95% IM confidence intervals within parentheses. Focusing on non-Hispanics, the estimated lower bound for the ITT suggests that JC eligibility increases the job quality index by at least 10 percent of a standard deviation, with the estimated upper bound indicating that the ITT effect can be as large as 17.5 percent of a standard deviation in job quality. The respective IM confidence interval for the estimated bounds strongly rules out zero effects. Looking across the columns at the subsamples, we see that all estimated lower bounds are positive and that the estimated bounds' IM confidence interval rule out zero effects. The estimated bounds for males are smaller in magnitude. Interestingly, the estimated bounds for females are consistent with potentially larger JC eligibility impacts on job quality relative to males. This suggestive pattern between non-Hispanic females and males, which will be recurrent throughout the rest of our results, stands in stark contrast with previous findings using these same data, in which males exhibit larger labor market impacts than females (e.g., Schochet et al., 2001, 2008; Flores et al., 2010; Eren and Ozbeklik, 2014; Blanco et al., 2013). Importantly, none of the previous papers had considered labor market outcomes that included fringe benefits; they considered mainly employment, earnings, and wages. Nevertheless, we refer to the higher estimated impacts on females relative to males as merely suggestive given that the two sets of bounds overlap substantially. A similar pattern is drawn when comparing the subsamples of teenagers and young adults, where the estimated bounds for the relatively older participants are consistent with potentially larger impacts. Similarly, the latter comparison is also suggestive given the overlap in estimated bounds across subsamples.

The lower panel of Table 7 presents estimated bounds for the effect of JC availability on the (log) wage rate and selected components of our job quality index: three fringe benefits indicators that were also analyzed by Schochet et al. (2008), having access to sponsored child care, and flexibility in working schedule.[27] For non-Hispanics, the estimated bounds and 95% IM confidence intervals on the ITT effects on the log wage and the first three fringe benefit indicators rule out zero. While the estimated lower bounds are consistent with positive effects on child care access and flexible work hours, their corresponding 95% IM confidence intervals do not rule out zero effects. From the lower panel of Table 7, the effect on wages is

[27]The corresponding estimated bounds for all of the correlates that make up the job quality index are reported in Internet Appendix F.

at least 3.7 log points and could be as large as a 8.7 log points. Also the ITT effects on the availability on the first three fringe benefit indicators are at least 4pp (employer-provided health insurance), 2.3pp (paid vacation), and 5.1pp (retirement or pension benefits). The magnitude of the estimated lower bounds on the ITT effects is comparable to the point estimated ITT obtained when ignoring selection into employment (see Table 2); however, keep in mind that the latter applies to the full sample of non-Hispanics while the bounds are for the ITT effects on the always employed. The estimated bounds for the ITT effect on wages for the subsamples are very similar to the estimates for the sample of non-Hispanics. The one exception is the sample of young adults, where the estimated bounds' IM confidence interval does not rule out zero wage effects (this could be partly due to loss of power from a smaller sample). Interestingly, the estimated bounds for the ITT effects on the selected fringe benefits across subsamples are suggestive of heterogeneous effects. For instance, the estimated bounds are suggestively shifted to the right of the positive real line for the sample of females relative to males and for young adults relative to teenagers. Lastly, in contrast to the samples of non-Hispanics and males, the 95% IM confidence intervals for females rule out zero effects on child care and flexible working hours (the latter at a slightly less conservative confidence level of 94%).

5.3 Estimated Bounds for the Average Treatment Effects on the Job Quality for the Always-Employed Compliers

We now move on to analyze average treatment effects of actual JC participation on the job quality index. In accounting for noncompliance, our bounds make inference on the subpopulation of always-employed compliers. The estimates are presented in Table 8. From the estimated strata proportions in the top panel, we see that always-employed compliers (i.e., the cEE stratum) is the largest stratum, accounting for 42 percent of non-Hispanics (and about 59 percent of the compliers). For other subsamples, the always-employed compliers are also the largest stratum and represent between 45.6 percent (males) and 37.2 percent (females). We note that all the reported estimated strata proportions across samples are positive and highly statistically significant, which satisfies one of the testable implications of the assumptions from Section 4.2.

The estimated bounds on the JC participation effect on the job quality index are presented in the second panel of Table 8. Looking across columns, all estimated lower bounds across samples are positive and the bounds' 95% IM confidence interval rules out zero effects of JC participation on the job quality index. For non-Hispanic always-employed compliers, JC participation increases job quality by an average of at least a 14.4 percent of a standard deviation. From the estimated upper bound, this effect could be as high as

35.8 percent of a standard deviation in the job quality index. Relative to the estimated lower bound for the sample of non-Hispanics, larger magnitudes of the lower bound are estimated for females and young adults, and smaller for males and teenagers.

An interesting comparison is between males and females, since the estimated bounds for females are suggestive of larger average effects of JC participation on job quality. The estimated bounds for females indicate that the average effect of JC participation on job quality is between 23.7 and 48.7 percent of a standard deviation, whereas the corresponding impact for males is between 8.3 and 27.3 percent. The IM confidence intervals on this set of estimated bounds indicate that there is considerable overlap between them, implying that a formal statistical test would not reject the equality of their impacts. However, potentially larger effects of JC participation on job quality for females are intriguing considering that females have been found to place a relatively high value on certain fringe benefits (e.g., Currie and Chaykowski, 1992; Goldin, 2014; Lowen and Sicilian, 2009; Mas and Pallais, 2016; Wiswall and Zafar, 2018), which are inputs of our job quality index.

A second interesting comparison is between teenagers and young adults since the estimated bounds for the latter group are suggestive of relatively larger effects than those found for the former. Similar to the previous comparison, the contrast in estimates between teenagers and young adults is suggestive given the overlap in bounds and their respective IM confidence intervals. Regardless, the potential for larger impacts in the case of older participants is in line with the findings reported by Schochet et al., (2008) and Schochet (2018). Using restricted tax records, both studies noted that the older group experienced relatively larger impacts on earnings and employment, and that these impacts persisted over a longer period of time.[28] Next, we analyze hourly wages and the individual components of the job quality index.

The bottom panel of Table 8 presents estimated bounds on the average effect of JC participation on the hourly wage and selected fringe benefits for always-employed compliers. The full set of results are relegated to Internet Appendix G. The estimated bounds on wages suggests that JC participation increases average wages for non-Hispanics by at least 5.3 log points and at most 14.6 log points. Importantly, the bounds' 95% IM confidence interval exclude a zero effect. For the subsamples, the estimated bounds are similar in magnitude, except for young adults, whose estimated lower bound is smaller (3.2 log points) with a 95% IM confidence interval that does not exclude a zero effect. Focusing on the fringe benefit of health insurance, the estimated bounds are somewhat similar in magnitude across the non-Hispanics, males, and teenagers,

[28] Schochet et al. (2008) analyzed outcomes measured 5 to 9 years after randomization, while the report by Schochet (2018) focuses on a period 20 years after randomization.

for whom the lower bounds suggest that JC participation increases the availability of these important fringe benefits by at least 6.4pp (this figure is for non-Hispanics). For these groups, the 95% confidence intervals exclude zero. For the subsamples of females and young adults, the JC participation effects on health insurance are suggestive of potentially larger effects (at least 8.3pp based on the estimated lower bound). In the case of paid vacation, only the lower bounds' 95% IM confidence intervals for non-Hispanics, females and young adults rule out zero effects, where the estimated bounds are notably larger for the two subsamples. Zero effects are also ruled out in the analysis of retirement or pension benefits for all samples analyzed, except for males, whose estimated lower bound is positive and smaller.

In the case of child care benefits, while the estimated lower bounds are positive, the corresponding 95% IM confidence intervals do not rule out zero effects across all samples, with the notable exception of females. As for the impacts on flexible work hours, the estimated lower bounds for the average JC participation effect are positive for all samples except males. The estimated bounds' 95% IM confidence intervals do not rule out a zero effect, except for females and young adults. Once again, the estimated lower bounds are consistent with much larger effects on flexible work hours (over 8pp) in the case of females and young adults.

As before, the comparison between males and females in terms of fringe benefits is worth highlighting. First, we note that the magnitudes of the estimated lower bounds for females are larger in most cases than the upper bound estimates for males (the only exception is health insurance). Second, the estimated bounds' 95% IM confidence intervals for females rule out zero effects for all indicators of fringe benefits, while for males the only 95% IM confidence interval that rules out a zero effect is for health insurance. The other comparison worth highlighting is between teenagers and young adults, since young adults seems to benefit more, as suggested by the estimated bounds of the JC training effects on access to fringe benefits. In particular, we note that the estimated magnitudes for the lower bounds in the sample of young adults is almost as large as the estimated upper bounds for teenagers. Lastly, as it is typically the case when working with bounds, there is still a high degree of overlap in the sets of bounds' 95% IM confidence intervals contrasted, which prevents us from statistically concluding that the previous differences between males and females, and between teenagers and young adults, are statistically significant.

5.4 Estimated Bounds on the Quantile Treatment Effects on the Job Quality for the Always-Employed Compliers

An advantage of having constructed a continuous index of job quality is that it allows us to analyze effects of interest beyond the average impact. To this end, we estimate bounds for quantile treatment effects of JC participation on the job quality index for always employed compliers. Figure 1 summarizes graphically the estimated bounds for the sample of non-Hispanics.[29] In the figure, the estimated upper and lower bounds are denoted by a long dash, while their 95% IM confidence intervals are denoted by a short dash at the end of the dashed vertical lines.

The estimated lower bounds on all of the quantile treatment effects considered are non-negative, and the bounds show some noteworthy trends. First, the estimated bounds on quantiles of the job quality index below the 25th are considerably smaller in magnitude, and their 95% IM confidence intervals do not rule out a zero effect (with the exception of the 15th quantile). Second, the estimated lower bound at the median is indicative of a positive JC effect of at least a 18.6 percent of a standard deviation in job quality, which is somewhat larger than the estimated effect at the mean (at least 14.4 percent). However, the corresponding 95% IM confidence interval at the median does not rule out a zero effect (a 90% IM confidence interval does, however, rule out a zero effect). Third, most of the estimated lower bounds on quantiles that are between the 30th and the 90th quantile are indicative of important effects of JC on job quality. The estimated lower bounds at these quantiles range between 6 and 32 percent of a standard deviation in job quality, and the corresponding 95% IM confidence intervals rule out zero effects in all cases but two, the median and the 60th quantile (and for these two, zero effects are ruled out at the 90% level). In sum, the estimated bounds on the quantile treatment effects for non-Hispanics (always-employed compliers) beyond the 25th quantile are consistent with positive and statistically significant effects of JC participation on job quality. Taking at face value the evidence in Figure 1, JC training appears to have larger positive impacts on the quality of jobs for individuals who are beyond the 25th quantile of the job quality distribution.

The distributional analysis for the subsamples is presented in Figure 2. Consistent with the results for the sample of non-Hispanics, many of the estimated lower bounds for all subsamples below the 25th quantile of their job quality distribution are smaller, and their 95% IM confidence intervals include zero effects. The estimated bounds on quantile treatment effects for the samples of males and teenagers shown in Figures 2(a) and 2(c) are fairly similar to each other. For both subsamples, and in contrast to the

[29] All the numerical values for the construction of figures presented in the paper are presented in Internet Appendix G.

non-Hispanic sample, we observe that, while the estimated lower bounds are positive and sizable, they lack statistical significance in most quantiles between the 30th and the 60th. Moreover, only at three of the quantiles analyzed in the upper part of the distribution are the effects statistically significant (i.e., the corresponding 95% IM confidence intervals do not include zero).

The results for females, shown in Figure 2(b), stand in contrast with those for males, since their estimated bounds strongly suggest that females benefit more from JC participation relative to males throughout the job quality distribution. For females, all the estimated lower bounds for quantiles above the 25th quantile are positive and have 95% IM confidence intervals that exclude zero effects. Also, relative to males, females' estimated upper bounds for most of the quantiles analyzed are considerably larger in magnitude.

A similar contrast is evident between teenagers in Figure 2(c) and young adults in Figure 2(d). The group of young adults appear to benefit more from JC participation between the 15th and the 60th quantiles of the job quality distribution. In fact, their estimated bounds for quantiles around the median are similar in magnitude to those of the female sample. But unlike females, most of the estimated bounds' 95% confidence intervals in the upper part of the job quality distribution include zero. For females, in Figure 2(b), the effects at the median are bounded between 45 and 83 percent of a standard deviation in job quality, the highest among any group, while for young adults the effect at the median is bounded between a 38 and 68 percent of a standard deviation in job quality. In spite of all the suggestive comparisons of the quantile treatment effects across groups, we acknowledge that there is overlap in the 95% IM confidence intervals of the estimated bounds across these subsamples and quantiles. This implies that in most instances it is not possible to statistically rule out the equality of quantile treatment effects between the groups.

5.5 Additional Analysis and Discussion

The first analysis we conduct is an informal exercise aimed to help with the interpretation of the results in the previous subsections. Recall that the job quality index was standardized to have mean zero and standard deviation of one. Here, we exploit this standardization of the index to informally derive implications of the estimated magnitudes of the lower and upper bounds in terms of changes in the values of the correlates (fringe benefits) employed in the index construction. Specifically, we focus on the estimated lower and upper bounds for the average treatment effect of JC on the job quality index for the always-employed compliers. The estimated bounds on the average effect are converted into z-values using

the standard normal distribution. Adding this z-value to the median (0.5), provides a percentile of the job quality index distribution. Then, for each correlate in the index, we find the value it takes for the corresponding percentile of the job quality distribution. These values (one for each bound) allows us to compute the percentage change each represents relative to the value corresponding to the median of the job quality index distribution. In this way, we can provide an interpretation of the effects on the index as a function of the change in a fringe benefit of interest.

The results of this exercise are presented in Table 9. The first, second, and fourth columns present the median of the correlates included in the index (Median), the value of the average of the correlate that corresponds to the percentiles implied by the median plus the lower bound (Median + L.B.) and the median plus the upper bound (Median + U.B.), respectively. Columns 3 and 5 present the percentage change of (Median + L.B.) and (Median + U.B.) relative to the Median, respectively. These can be interpreted as percentage effects of JC participation on each of the correlates considered. Focusing on the first row in Table 9, the estimated lower bound for the average JC effect on the job quality index is consistent with an increase in wages of at least a 1.81 percent (third column) and at most 3.58% (fifth column), relative to the median wage. In addition to the increase in wages, the lower bound is also consistent with an increase in the probability of the job offering health insurance of at least 50.26 percent and at most 119%, relative to the probability of offering health insurance at the median (0.127). In general, the average effects of JC training on the job quality index are consistent with large increases (raging from 21 to 94 percent according to the estimated lower bounds) in paid vacation, pension or retirement benefits, paid sick leave, child care, dental plan, and tuition aid. Modest increases (less than 10 percent according to the estimated lower bounds) are observed in the access to the fringe benefits of flexible working hours and transportation.[30]

The second analysis we perform aims to assess how much the results of the previous sections are driven by the hourly wage. The job quality index we employed combines both the monetary (wages) and non-monetary (fringe benefits) aspects of jobs since both aspects are important dimensions of job quality. The monetary aspect has been studied extensively, while the non-monetary aspects of jobs are understudied but appear extremely important. A valid question concerns the extent to which the monetary aspect may be driving results. Thus, we analyze an alternative job quality index constructed with only non-monetary (fringe benefits) aspects of jobs. The estimated bounds for the causal parameters of interest when employing this alternative job quality index are presented in Internet Appendix H. In general, estimated results when we consider a job quality index that bundles only fringe benefits are very similar to the estimates presented

[30]We performed the same exercise as in Table 9 for the other subsamples. These results are reported in Internet Appendix G. Those results are largely consistent with our discussion of the estimated bounds for the group of non-Hispanics.

in the previous sections. For example, the PCA eigenvalue for the first principal component (the alternative index) is 4.3 and explains about 48 percent of the covariance among fringe benefits. The scoring factors in the alternative index are all positive, and thus, indicative of positive relations between the inputs (fringe benefits) that make up the index. Furthermore, an exercise similar to the one reported in Table 4 strongly suggests that as the alternative index of job quality increases in value so do the average wage, which is not included in this index, as well as the proportions of jobs with each of the fringe benefits. While the estimated bounds for the effects of JC participation on the alternative measure of job quality are somewhat different quantitatively, the qualitative patterns described in sections 5.3 and 5.4 are unchanged. For instance, the estimated bounds for the average JC effect on non-Hispanic always-employed compliers are 0.136 and 0.298 when employing the alternative job quality index with wages (versus 0.144 and 0.358 as reported in Table 8). If wages were driving the previous results, one would expect a significant change in the estimated bounds when the wage rate is not employed in the construction of the job quality index. In sum, when considering an alternative job quality index that excludes the hourly wage, similar conclusions are reached, implying that JC training does lead to significant increases in the presence of fringe benefits in the jobs obtained by individuals.

6 Conclusions

Most of the economic literature measures the desirability of jobs based on their monetary compensation. However, non-monetary compensation in the form of fringe benefits accounts for about a third of total job compensation in the U.S. At the same time, the literature examining the effects of active labor market programs (ALMPs) typically focuses on monetary compensation, while very few studies examine non-monetary aspects. Thus, comparatively few studies have analyzed non-monetary aspects of compensation and how workers may attain jobs with better fringe benefits.

In this paper, we analyze the effects of the Job Corps (JC) training program on the job quality of eligible participants. The JC is America's largest and most comprehensive education and job training program serving disadvantaged youth. Using data from a randomized evaluation of the JC, we employ principal components analysis to construct a single job quality index that effectively bundles information on the hourly wage and the availability of key fringe benefits. Even with the availability of random assignment, the estimation of causal effects on job quality is not straightforward due to the selection into employment problem that occurs after random assignment. The problem arises since there are unobserved factors

that simultaneously affect both job quality and its observability (determined by employment status). To deal with selection into employment—and the noncompliance with random assignment that also exists in the data—we use nonparametric bounds. These bounds require typically weaker assumptions than those conventionally employed by methods that lead to point identification. We estimate bounds for the average and quantile treatment effects on job quality using monotonicity and stochastic dominance assumptions that have empirical support in the data.

The estimated bounds provide new and important evidence about how an ALMP can positively impact the job quality of participants. Our results apply to non-Hispanic individuals who comply with their random assignment and are employed regardless of JC participation during quarter 16 after random assignment. This is an important latent group of eligible non-Hispanic JC applicants that accounts for 42 percent of non-Hispanics and about 60 percent of always-employed non-Hispanics. Our bounding results indicate that the average impact of JC participation on the job quality index of non-Hispanics is between 14 and 36 percent of a standard deviation in the index. Through an informal exercise, we show that such gains in job quality from JC training represent substantial positive changes in each of the fringe benefits that compose the index.

We find that the effects are heterogeneous across different subsamples of non-Hispanics. Females and young adults appear to experience larger impacts of JC training on their job quality, relative to males and teenagers, respectively. Notably, we find suggestive evidence that females that participate in JC experience more robust gains in fringe benefits that other literature has found to be important for this group, such as child care benefits and flexible work hours. We term the evidence about male-female and teenager-young adults differences suggestive given the difficulty of statistically rejecting the equality of parameters across groups when the effects are partially identified. We also find that the effect of JC participation is heterogeneous across the distribution of the job quality index, where estimated bounds suggest that quantile treatment effects are positive and significant beyond the 30th quantile for non-Hispanics as a whole. The estimated bounds on quantile treatment effects are also suggestive of larger effects on quantiles around the middle of the distribution for all groups. The estimated bounds on quantile treatment effects for females and young adults are also suggestive of larger impacts on most of the quantiles analyzed relative to males and teenagers, although, once again, it is not possible to conclude that their effects are statistically larger.

We believe that our results have a number of implications. First, we find interesting that a relatively small intervention such as JC—which has been documented to be equivalent, on average, to one year of regular high school—appears to allow disadvantaged youth to access jobs of significantly better quality.

This is particularly relevant given the literature documenting the existence of "good jobs" (e.g., Acemoglu, 2001; Green, 2015) likely offered by "good firms" (e.g., Abowd and Kramarz, 2002; Card et al., 2018). More research about the channels or reasons behind this apparent impact on job quality (e.g., partnership with firms, the signaling value of training) is needed. Second, our focus on both monetary (wages) and non-monetary (fringe benefits) compensation in the construction of a job quality index allows us to assess what may be lost when data limitations force researchers to focus only on monetary compensation, at least for the type of jobs that disadvantaged youths in our sample are able to access. From our study, it is clear that the overall patterns of impacts we obtain largely hold for wages (e.g., Blanco et al., 2013) as well as fringe benefits. Nevertheless, another relevant implication is that fringe benefits are an important part of worker compensation, and focusing only on wages likely misses relevant aspects of total compensation and job quality. Third, since most of the cost-benefit evaluations of ALMPs focus on labor market benefits such as employment and earnings, our study suggests that those evaluations likely miss other important benefits such as access to key fringe benefits in health/dental insurance, retirement benefits, etc. Indeed, for individuals with dependents, having access to health/dental insurance can generate additional benefits in the form of spillovers that should be considered in the evaluation of ALMPs.[31]

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[31] This would be the case, of course, if the alternative to employer-provided health/dental insurance is the absence of it.

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Table 1: Characteristics of Eligible Job Corps Applicants in the Sample of Non-Hispanics

Gender	
Male	0.566
Females without children	0.302
Females with children	0.129
Age at application	
16 to 17	0.420
18 to 19	0.315
20 to 24	0.265
Race and ethnicity	
White, non-Hispanic	0.325
Black, non-Hispanic	0.585
Other	0.090
Had a high school credential	
High school diploma	0.191
GED certificate	0.049
Lived in a metropolitan statistical area	0.749
Arrest history (self-reported)	
Ever arrested	0.261
Arrested for serious crime[a]	0.042
Received food stamps in the past year	0.451
Had a job in the past year	0.650
Average earnings in the past year ($)	2,897.61
Random assignment (individuals)	
Treatment	5,384
Control	3,545
Total Observations	8,929

Note: [a] Serious crimes includes aggravated assault, murder, robbery and burglary. Computations use design weights.

Table 2: Hourly Wage and Fringe Benefits in Quarter 16 after Randomization for Non-Hispanics, by Treatment Status

	Treatment	Control	Difference
Percent employed	0.711	0.678	0.032 ***
			(0.010)
Indicators in Schochet et al. (2008):			
Hourly wage	7.511	7.188	0.323 ***
			(0.086)
Health insurance	0.574	0.534	0.040 ***
			(0.013)
Paid vacation	0.628	0.602	0.026 **
			(0.012)
Retirement or pension benefits	0.490	0.437	0.053 ***
			(0.013)
Additional fringe benefit indicators:			
Paid sick leave	0.472	0.437	0.035 ***
			(0.013)
Child care	0.159	0.144	0.015 *
			(0.009)
Flexible work hours	0.578	0.565	0.013
			(0.013)
Transportation	0.195	0.192	0.003
			(0.010)
Dental plan	0.504	0.466	0.038 ***
			(0.013)
Tuition aid	0.285	0.263	0.021 *
			(0.011)

Note: *, **, and *** denote statistical significance difference in means, at a 90, 95 and 99 percent confidence level. Standard errors for the difference in means are reported in parentheses. Computations use design weights. As noted in Schochet et al. (2008), estimates under the column Difference do not have a causal interpretation since they are conditional on employment in quarter 16 after randomization.

Table 3: Principal Components Analysis and Scoring Factors of Variables used in the Computation of the Job Quality Index, Sample of Non-Hispanics

	Eigenvalues	Covariance explained (%)
Principal component 1	4.366	43.66
Principal component 2	1.123	11.23
Principal component 3	0.949	9.49

Scoring factors in the computation of the first principal component

Hourly Wage	0.144
Health insurance	0.407
Paid Vacation	0.390
Retirement or pension benefits	0.399
Paid sick leave	0.391
Child care	0.246
Flexible hours	0.144
Transportation	0.082
Dental plan	0.410
Tuition aid	0.307

Table 4: Average Hourly Wage and Proportions of Fringe Benefits within Quartiles of the Job Quality Index, Sample of Non-Hispanics

Quartile (Q):	Up to Q1	Q1 to Q2	Q2 to Q3	Q3 & up
Hourly Wage	5.655	7.438	7.618	8.122
Health insurance	0.000	0.263	0.885	0.906
Paid Vacation	0.000	0.407	0.897	0.934
Retirement or pension benefits	0.000	0.101	0.703	0.903
Paid sick leave	0.000	0.182	0.699	0.745
Child care	0.000	0.049	0.095	0.346
Flexible hours	0.207	0.604	0.638	0.728
Transportation	0.070	0.258	0.153	0.251
Dental plan	0.000	0.114	0.760	0.885
Tuition aid	0.000	0.115	0.222	0.565

Note: Computations use design weights.

Table 5: Further Descriptive Analysis of the Job Quality Index, Sample of Non-Hispanics

	Mean Job Quality	
Occupation		
Services	-0.195	
Sales	-0.251	
Construction	-0.102	
Private household	-0.250	
Clerical	0.282	
Mechanics	0.215	
Agriculture	-0.473	
Other	0.287	
Placement Services[a]		
Not received	0.016	
Received	0.076	
Employment type		
Full-time	0.101	
Part-time	-0.554	
Gender		
Males	0.021	
Females	-0.080	
Presence of Children		
Women with	0.056	
Women without	-0.137	
Men with	-0.063	
Men without	0.029	
Descriptive multivariate regression analysis		
Dependent variable: Job Quality	Coefficient	Standard error
Unemployment rate	-0.022	0.010 **
Placement Services[b]	0.102	0.040 **
Part-time employment	-0.582	0.035 ***
Female	-0.122	0.034 ***
Has children	-0.059	0.061
Female × has children	0.223	0.076 ***
Occupation[c]		
Sales	-0.020	0.050
Construction	-0.009	0.044
Private household	-0.001	0.058
Clerical	0.446	0.048 ***
Mechanics	0.296	0.048 ***
Agriculture	-0.340	0.090 ***
Other	0.402	0.048 ***
Constant	0.067	0.055

Note: *, **, and *** denote statistical significance difference in means, at a 90, 95 and 99 percent confidence level. Computations use design weights.

[a] Mean calculated conditional on receiving Job Corps training.

[b] Indicator = 1 if receiving placement services within JC, = 0 otherwise.

[c] Service is the omitted category.

Table 6: Pricipal Strata within Observed Cells Defined by Z_i, T_i and S_i

		$Z_i = 0$		$Z_i = 1$	
		$T_i =$		$T_i =$	
		0	1	0	1
$S_i =$	0	*cNN, cNE, nNN*	*aNN*	*nNN*	*cNN, cEN*†*, aNN*
	1	*cEE, cEN*†*, nEE*	*aEE*	*nEE*	*cEE, cNE, aEE*

†: Stratum cEN is ruled out by Assumption 6 (Section 4.2).

Table 7: Estimated Bounds on the Intention-to-Treat Effect of JC on Job Quality, Wages and Selected Fringe Benefits in Quarter 16 after Randomization for the Always Employed.

	Non-Hispanics	**Males**	**Females**	**Teenager** (age 16-19)	**Young Adults** (age 20-24)
Principal Stratum Proportions					
π_{EE}	0.632	0.653	0.606	0.623	0.656
π_{NN}	0.332	0.321	0.346	0.352	0.278
π_{NE}	0.036	0.026	0.048	0.025	0.066
Effects on the Job Quality Index:					
Bounds under Assumptions 1, 2, 6 and 7					
$[LB_{EE}, UB_{EE}]$	$[0.100, 0.175]$	$[0.062, 0.114]$	$[0.151, 0.254]$	$[0.077, 0.128]$	$[0.152, 0.296]$
(95% IM confidence intervals)	$(0.050, 0.239)$	$(0.001, 0.191)$	$(0.074, 0.356)$	$(0.020, 0.198)$	$(0.060, 0.415)$
Effects on Wages and Selected Fringe Benefits:					
Bounds under Assumptions 1, 2, 6 and 7					
Log wage					
$[LB_{EE}, UB_{EE}]$	$[0.037, 0.087]$	$[0.037, 0.078]$	$[0.036, 0.095]$	$[0.037, 0.077]$	$[0.031, 0.098]$
(95% IM confidence intervals):	$(0.020, 0.108)$	$(0.015, 0.106)$	$(0.008, 0.130)$	$(0.017, 0.105)$	$(-0.001, 0.136)$
Health insurance					
$[LB_{EE}, UB_{EE}]$	$[0.040, 0.068]$	$[0.033, 0.055]$	$[0.047, 0.083]$	$[0.036, 0.055]$	$[0.044, 0.102]$
(95% IM confidence intervals):	$(0.017, 0.097)$	$(0.004, 0.090)$	$(0.011, 0.128)$	$(0.010, 0.086)$	$(0.002, 0.155)$
Paid vacation					
$[LB_{EE}, UB_{EE}]$	$[0.023, 0.053]$	$[0.005, 0.027]$	$[0.046, 0.086]$	$[0.010, 0.029]$	$[0.051, 0.113]$
(95% IM confidence intervals):	$(0.000, 0.081)$	$(-0.023, 0.062)$	$(0.011, 0.131)$	$(-0.015, 0.059)$	$(0.013, 0.166)$
Retirement or pension benefits					
$[LB_{EE}, UB_{EE}]$	$[0.051, 0.076]$	$[0.039, 0.057]$	$[0.067, 0.099]$	$[0.038, 0.052]$	$[0.078, 0.133]$
(95% IM confidence intervals):	$(0.027, 0.102)$	$(0.011, 0.091)$	$(0.031, 0.141)$	$(0.012, 0.081)$	$(0.035, 0.186)$
Child care					
$[LB_{EE}, UB_{EE}]$	$[0.015, 0.022]$	$[0.006, 0.012]$	$[0.025, 0.033]$	$[0.009, 0.014]$	$[0.028, 0.043]$
(95% IM confidence intervals):	$(-0.001, 0.039)$	$(-0.015, 0.035)$	$(0.003, 0.056)$	$(-0.009, 0.032)$	$(-0.003, 0.077)$
Flexible work hours					
$[LB_{EE}, UB_{EE}]$	$[0.013, 0.039]$	$[-0.003, 0.016]$	$[0.035, 0.073]$	$[0.004, 0.020]$	$[0.037, 0.088]$
(95% IM confidence intervals):	$(-0.009, 0.066)$	$(-0.031, 0.048)$	$(-0.001, 0.117)$	$(-0.022, 0.052)$	$(-0.005, 0.138)$

Note: IM refers to the Imbens and Manski (2004) confidence intervals. These confidence intervals were computed using bootstrap standard errors from 1,000 replications.

Table 8: Estimated Bounds on the Average Treatment Effect of JC on Job Quality, Wages and Selected Fringe Benefits in Quarter 16 after Randomization for the Always-Employed Compliers.

	Non-Hispanics	**Males**	**Females**	**Teenager** (age 16-19)	**Young Adults** (age 20-24)
Principal Stratum Proportions					
π_{cEE}	0.420	0.456	0.372	0.430	0.393
π_{cNE}	0.062	0.058	0.068	0.056	0.077
π_{cNN}	0.235	0.230	0.240	0.254	0.182
π_{aEE}	0.007	0.008	0.005	0.007	0.006
π_{aNN}	0.004	0.004	0.004	0.004	0.005
π_{nEE}	0.181	0.158	0.210	0.156	0.249
π_{nNN}	0.092	0.085	0.101	0.094	0.088
Effects on the Job Quality Index:					
Bounds under Assumptions 1 to 7					
$[LB_{EE}, UB_{EE}]$	$[0.144, 0.358]$	$[0.083, 0.273]$	$[0.237, 0.487]$	$[0.104, 0.294]$	$[0.246, 0.534]$
(95% IM confidence intervals)	$(0.074, 0.455)$	$(0.000, 0.386)$	$(0.113, 0.664)$	$(0.026, 0.402)$	$(0.088, 0.756)$
Effects on Wages and Selected Fringe Benefits:					
Bounds under Assumptions 1 to 7					
Log wage					
$[LB_{EE}, UB_{EE}]$	$[0.053, 0.146]$	$[0.056, 0.133]$	$[0.049, 0.152]$	$[0.057, 0.145]$	$[0.032, 0.132]$
(95% IM confidence intervals):	$(0.027, 0.177)$	$(0.023, 0.173)$	$(0.008, 0.201)$	$(0.028, 0.179)$	$(-0.025, 0.198)$
Health insurance					
$[LB_{EE}, UB_{EE}]$	$[0.064, 0.146]$	$[0.052, 0.123]$	$[0.083, 0.180]$	$[0.055, 0.127]$	$[0.083, 0.208]$
(95% IM confidence intervals):	$(0.029, 0.193)$	$(0.010, 0.177)$	$(0.020, 0.264)$	$(0.016, 0.179)$	$(0.011, 0.306)$
Paid vacation					
$[LB_{EE}, UB_{EE}]$	$[0.038, 0.124]$	$[0.005, 0.078]$	$[0.088, 0.196]$	$[0.018, 0.094]$	$[0.090, 0.218]$
(95% IM confidence intervals):	$(0.002, 0.172)$	$(-0.038, 0.131)$	$(0.027, 0.277)$	$(-0.020, 0.145)$	$(0.017, 0.318)$
Retirement or pension benefits					
$[LB_{EE}, UB_{EE}]$	$[0.069, 0.141]$	$[0.041, 0.106]$	$[0.111, 0.198]$	$[0.051, 0.112]$	$[0.114, 0.223]$
(95% IM confidence intervals):	$(0.034, 0.185)$	$(-0.001, 0.158)$	$(0.049, 0.274)$	$(0.014, 0.158)$	$(0.035, 0.324)$
Child care					
$[LB_{EE}, UB_{EE}]$	$[0.024, 0.055]$	$[0.004, 0.035]$	$[0.055, 0.084]$	$[0.018, 0.044]$	$[0.041, 0.081]$
(95% IM confidence intervals):	$(-0.002, 0.084)$	$(-0.028, 0.071)$	$(0.017, 0.126)$	$(-0.010, 0.074)$	$(-0.019, 0.145)$
Flexible work hours					
$[LB_{EE}, UB_{EE}]$	$[0.034, 0.116]$	$[-0.001, 0.066]$	$[0.088, 0.197]$	$[0.019, 0.089]$	$[0.084, 0.187]$
(95% IM confidence intervals):	$(-0.001, 0.161)$	$(-0.044, 0.119)$	$(0.027, 0.282)$	$(-0.021, 0.141)$	$(0.007, 0.284)$

Note: IM refers to the Imbens and Manski (2004) confidence intervals. These confidence intervals were computed using bootstrap standard errors from 1,000 replications.

Table 9: Implied Changes in Hourly Wage and Proportions of Fringe Indicators from the JC Effects on the Job Quality Index.

Average/ Proportion at:	Median	Median +L.B.	**% Change**	Median +U.B.	**% Change**
Hourly Wage	6.517	6.635	1.81%	6.750	3.58%
Health insurance	0.127	0.191	50.26%	0.279	119.00%
Paid Vacation	0.197	0.259	31.37%	0.337	71.15%
Retirement or pension benefits	0.049	0.095	93.66%	0.158	223.41%
Paid sick leave	0.088	0.130	47.68%	0.191	117.60%
Child care	0.024	0.029	21.30%	0.037	56.79%
Flexible hours	0.399	0.424	6.29%	0.459	15.07%
Transportation	0.161	0.164	1.67%	0.167	3.97%
Dental plan	0.055	0.102	84.38%	0.180	226.17%
Tuition aid	0.055	0.071	28.77%	0.090	61.45%

Note: L.B. and U.B. stand for lower and upper bound, respectively. Estimated bounds implied changes in standard deviations are added to the median (z-value of zero), and at these cutoffs mean wage and proportions of fringe benefit indicators are calculated.

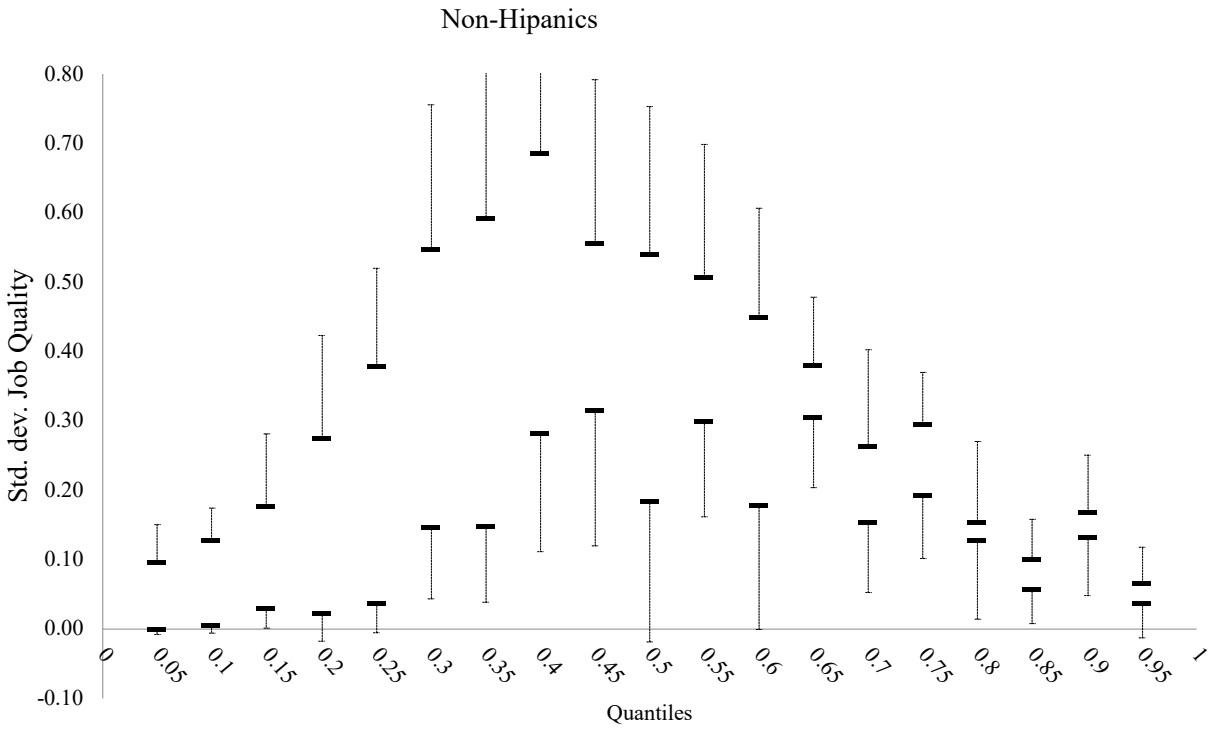

Figure 1: Estimated bounds on Quantile Treatment Effect of JC on job quality during quarter 16 after randomization for the *cEE* stratum under Assumptions 1 to 7, non-Hispanic sample. Upper and lower bounds are denoted by a long dash, while the 95 percent Imbens and Manski (2004) confidence intervals are denoted by a short dash at the end of the dashed vertical line.

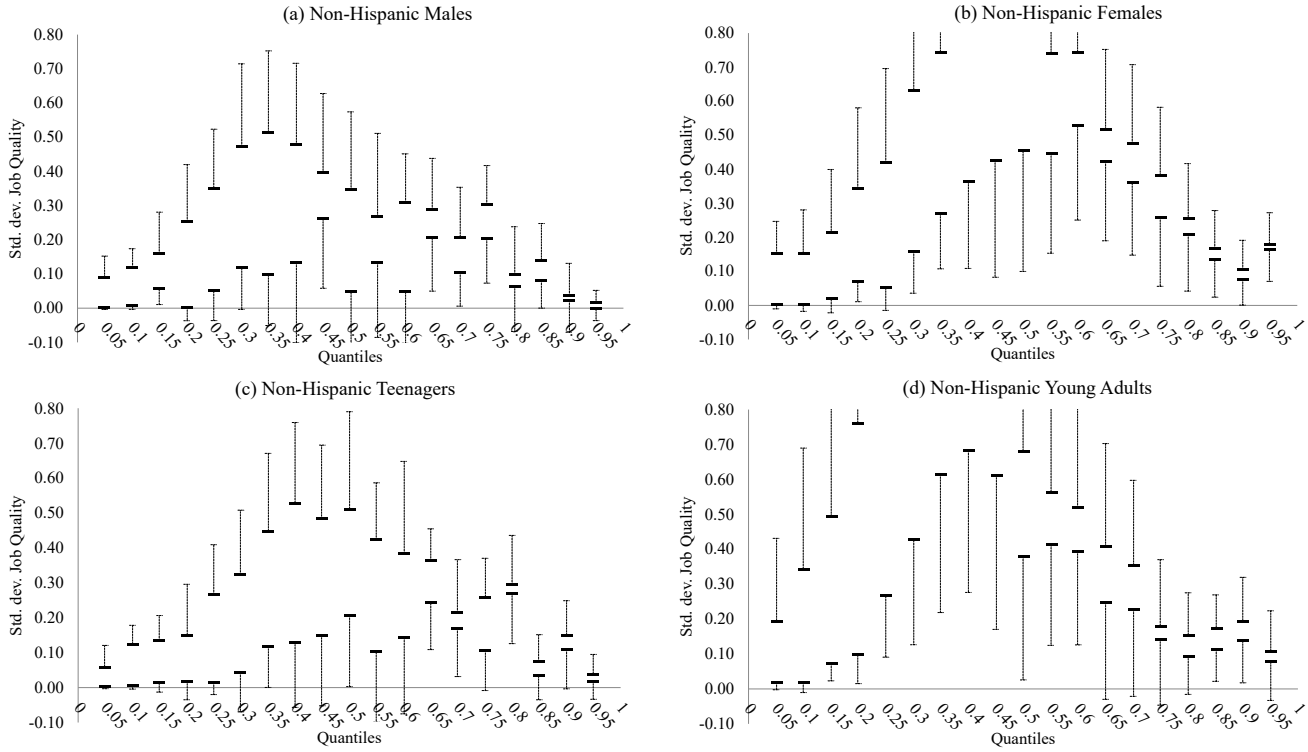

Figure 2: Estimated bounds on Quantile Treatment Effect of JC on job quality during quarter 16 after randomization for the *cEE* stratum under Assumptions 1 to 7, alternative demographic samples. Upper and lower bounds are denoted by a long dash, while the 95 percent Imbens and Manski (2004) confidence intervals are denoted by a short dash at the end of the dashed vertical line.