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

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Abstract

There exists an extensive literature evaluating the effects of public training programs on the employability and earnings of participants. At the same time, such public training programs generally aim to improve the future quality of life of participants. While earnings is an important factor in attaining a better quality of life, there are surely other contributing factors. In this paper, we analyze the causal effect of the availability of a U.S. 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 to a scalar quantity. In addition, we separately analyze several important characteristics of jobs. Our job quality index is consistent with the view that workers evaluate a job as a bundle of attributes that have some level of substitutability. Also, since our index is continuous, it permits us to evaluate the distributional impacts of training. Given that the quality of a job is defined only for employed individuals, we solve the selection-into-employment issue by estimating nonparametric bounds on the treatment effects of training for the latent subgroup of individuals that would be employed regardless of treatment assignment (a group that represents 65% of the population). An advantage of this approach is that it relies on relatively weak assumptions while partially identifying causal effects of training on job quality. We find that Job Corps has significant effects on average quality that are bounded between 10 and 18 percent of a standard deviation in our job quality index. The distributional analysis suggests that the effects are heterogeneous over the distribution of the job quality index. Finally, we also note that effects are heterogeneous across different demographic groups analyzed.

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1 Introduction

The literature evaluating active labor market programs (ALMP) has largely focused on the estimation of average effects on the "usual" labor market outcomes of interest, namely, earnings and employment (for a survey see, for example, Heckman et al., 1999; Imbens and Wooldridge, 2009). Lately, the focus has partly shifted towards the evaluation of distributional effects via quantile methods. The appeal of quantile based analysis stems from the fact that it sheds light on the impact heterogeneity across the distribution of the outcome of interest, and this information is essential to design better policy.[1] Also, policy designers will benefit from having information about how ALMPs impact other important outcomes. A largely under-researched outcome within the program evaluation literature is job quality. In this paper, we apply recent distributional analysis techniques to estimate causal effects of the availability of a job training program on the distribution of participants' job quality.

Studies that have analyzed changes in the composition of job quality in the US use monetary compensation as a proxy for quality (e.g., Bluestone and Harrison, 1988; Houseman, 1995; Farber, 1997; Acemoglu, 2001). We borrow from this vast literature the notion that the wage rate is an important dimension of job quality. Broad economic consensus indicates that another 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; and Kalleberg and Vaisey, 2005; Eriksson and Kristensen, 2014).[2] In contrast to studies that analyze the effects of job training on the different dimensions or characteristics that contribute to job quality (e.g., Schochet et al., 2008; Andersson et al., 2016), in our empirical application we employ a linear index that reduces the vector of job characteristics to a scalar quantity. Our quality index is then consistent with the view that workers evaluate a job as a bundle of attributes that have some level of substitutability (Rosen, 1986; Woodbury, 1983; and Eriksson and Kristensen, 2014). The methodology employed to construct our index of job quality is consistent with the

[1] Within the program evaluation literature, one could point out Heckman et al. (1997) as one of the first studies stressing the importance and explicitly analyzing the distributions of impacts. They present strong evidence about heterogeneous earnings impacts in their analysis of the employment and training programs funded by the National Job Training Partnership Act (JTPA).

[2] An alternative, mostly followed in sociology and psychology studies, is to use the perceived level of job satisfaction reported by workers (e.g., Clark and Oswald, 1996; Muñoz de Bustillo, et al., 2011). Unfortunately, we do not have information on self-reported job satisfaction in our data set.

proposed use of factor analysis to, for example, construct a reliable proxy for a latent measure of quality of college education to study its returns (Black and Smith, 2006), and to create low dimensional, interpretable, and informative characterizations of skills, that are causally impacted by an early childhood program to identify the causal effect of increases in these skills on future outcomes (Heckman et al., 2013).[3]

The program we analyze is the Job Corps (JC), America's largest and most comprehensive education and job training program enrolling disadvantaged youth, ages 16 to 24, at no out-of-pocket cost to them. Federal funds to run the program are around $1.6 billion (US Department of Labor, DOL, 2013), which makes its evaluation of public interest. 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.[4] In a landmark paper presenting impact findings from the NJCS, Schochet et al. (2008) found average positive impacts on a variety of important outcomes, including earnings and fringe benefits (e.g., health insurance). Findings on the latter set of outcomes, however, do not have a causal interpretation since their estimation was carried out using the subsample of individuals that found employment. Even with the availability of experimental data, when factors simultaneously affect both the outcome and its observability, a comparison of treatment and control group averages produces a biased estimate of the parameter of interest. This commonly found problem in applied econometrics is known as sample selection (Heckman, 1979).

Using data from the NJCS on the wage rate and availability of fringe benefits 16 quarters after random assignment, we employ principal components analysis to construct two indices for job quality. We present evidence suggesting that our index reliably bundles the different important aspects related to job quality. Then, we assess the effect of the availability of JC on each one of the separate components of job quality and on the

[3]Other examples within the literature analyzing the returns to education that have employed the related factor and principal components analysis include Carniero et al., 2003, Cawley et al., 2001, and other studies using principal components to measure different concepts of cognitive ability, constructed from the Armed Services Vocational Aptitude Battery test score administered to participants of the National Longitudinal Survey of Youth.

[4]There was, however, non-compliance with treatment assignment of about 27 percent, mainly from treatment-group members who never enrolled in the program. For this reason, we concentrate on intention-to-treat effects that exploit the random assignment and have the interpretation of the effect of the availability of the program.

proposed measure of job quality for eligible participants by employing recently developed nonparametric bounds for average and quantile treatment effects that account for sample selection. These bounds typically require weaker assumptions than those conventionally employed for point identification.[5] First, we employ individual-level weak monotonicity of the effect of the program on employment. This assumption has also been used by Zhang et al. (2008), Lee (2009), and Blanco, Flores and Flores-Lagunes (2013, BFF-L hereafter) to bound average wage effects. The second assumption is on mean potential outcomes across subpopulations defined by the potential values of the employment indicator as a function of treatment assignment. Similarly to Zhang et al. (2008), Lee (2009), and BFF-L (2013), our focus is on estimating bounds for the subpopulation of individuals who would be employed regardless of being assigned to participate in JC. Our job quality index is observed under both treatment arms only for this subpopulation, thus requiring fewer assumptions to construct bounds on their effect. Indeed, in our application this is an important subpopulation, accounting for about 65 percent of all eligible JC participants. Finally, the bounds on quantile treatment effects employ a similar set of assumptions and are based on results by Imai (2008).

Our estimated bounds provide new and important evidence about the positive and significant effects of the availability of JC on important components of job quality and the proposed summary measures of job quality, for individuals employed regardless of treatment assignment (during quarter 16 after random assignment). Under the preferred set of assumptions, we bound significant average treatment effects on the wage rate as in BFF-L, where the lower and upper bounds suggest that effects can be between 4 and 9 percent, and on the different fringe benefits considered, where relatively large impacts are observed in the availability of pension and retirement benefits (increasing by at least 5 percent). These estimates are obtained in a sample of non-Hispanics (for reasons described later), but we note that differences in the full sample analysis are negligible. Similarly, using our preferred index of job quality, we bound a significant

[5] Point identification of treatment effects under sample selection requires strong distributional assumptions that may not be satisfied in practice, 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 the sample (employment) but do not affect the outcome (job quality). Finding exclusion restrictions in our context is difficult. It is well known that in the case of employment and wages it is challenging to find plausible exclusion restrictions (Angrist and Krueger, 1999; Angrist and Krueger, 2001), and the wage rate is an important component of our job quality measure.

average treatment effect that is between 10 to 18 percent of a standard deviation in the job quality index. In addition, we find positive and significant average effects on the job quality for other sub-samples considered, where non-Hispanic females have estimated bounds consistent with larger impacts, and these are closely followed by the estimates for whites. Importantly, we find evidence of heterogeneous impacts across the distribution of the job quality index, where estimated bounds, in general, suggest that effects are positive and significant above the 0.4 quantile. We also report that estimated bounds are consistent with larger significant effects around the median for most samples: for example, most lower bounds oscillate between 25 and 40 percent of a standard deviation in job quality. Finally, relative to other demographic samples, we find that estimated bounds for non-Hispanic females are larger in magnitude in most of the quantiles of the job quality index distribution, reinforcing our findings based on bounds for average effects.

Our analysis contributes to the literature in at least three important ways. First, our focus on the outcome of job quality, as a bundle of job attributes, is unique. To our knowledge, this outcome is largely under-researched in the program evaluation literature, and we shed light on how the most important US job training program for youth impacts job quality. Related to the subject of job quality, a recent study by Andersson et al. (2016) finds moderately positive effects for participants that received training under the US Workforce Investment Act (WIA) on the outcome of firm (employer) quality. In contrast to the present study, they do not explicitly use information on the wage rate and the variety of fringe benefits that may be available at the firms where participants are employed. Instead, they rely on firm fixed effects and characteristics related to size, turnover, and industry changes to proxy for firm quality. They also do not deal with selection into employment in their analysis. Second, as previously discussed, the program evaluation literature has seen a shift in focus towards the evaluation of distributional effects, usually via quantile regression methods (e.g., Abadie et al., 2002; Chernozhukov and Hansen, 2005, 2006; Firpo, 2007; Frölich and Melly, 2013). We provide a comprehensive distributional analysis based on nonparametric bounds (e.g., Blundell et al., 2007; Imai, 2008; Lechner and Melly, 2010) that sheds light on the impact heterogeneity across the distribution of job quality. Finally, our comprehensive analysis of average and distributional impacts 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; BFF-L, 2013, 2013b; Chen and Flores, 2015; Blanco, 2017).

In the next section we briefly discuss the JC program and the data source employed in our analysis. In Section 3 we implement principal components analysis to construct our job quality indices and provide an assessment of the proposed measures. We describe the nonparametric bounds that allow us to estimate the causal effects of interest in the presence of sample selection in Section 4. We present our main 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 administer 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, offered at no out-of-pocket cost to participants. 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. For more information on eligibility criteria see 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. Most of these agencies are located in disadvantaged communities. 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 125 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 education. To help participants find jobs or pursue additional training, in the last stage of the program participants are provided with placement services.[6] In contrast to other federally funded programs, JC offers more comprehensive services, and thus, the cost of the program ascends to over $1.6 billion (DOL, Office of Inspector General report in 2013), making it the nation's largest job training program for youth.

Due to its importance, size, and nature of funding, the evaluation of the JC effectiveness is 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 study 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. 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 sample we employ is restricted to individuals who completed the 48 month interview. As shown at the end of column 1 in Table 1, the restriction results in a (full) sample of 11,313 individuals: 6,828 and 4,485 in the randomized treatment and control groups, respectively. Following some existing literature on the effects of the JC program, we consider a sub-sample that excludes Hispanics since that increases our confidence that the individual-level weak monotonicity assumption about the effect of the program on employment is satisfied.[7] The non-Hispanic sample, last column in Table 1, has a total of 9,351 individuals, with 5,653 of them

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

[7]All the assumptions we employ in our empirical analysis are formally presented and discussed in Section 4. A discussion about the potential caveat of considering Hispanics is presented in the results section.

randomly assigned to treatment and 3,698 to the control group. This represents 82% of the full sample. Characteristics of first time eligible JC participants summarized in Table 1 clearly indicate that the program serves disadvantaged youth. For example, on average these youths likely belong to a minority race or ethnic group, have low levels education, have high levels of unemployment and earned less than $3,000 during the year prior random assignment. In addition, a significant proportion of youths have had an arrest and received food stamps in the year prior randomization. A comparison of these average characteristics across the two columns in Table 1 also suggests that the differences in the main samples we consider are negligible. Finally, we employ the NJCS design weights throughout the analysis, since different subgroups in the population had different probabilities of being included in the research sample (for details on the NJCS design weights we employ see Schochet, 2001).

In Table 2, we compare average wages and proportions of individuals that reported having fringe benefits in the most recent job during quarter 16 after randomization, by treatment assignment. In addition to the wage rate, the indicators of having health insurance, paid vacation and retirement or pension benefits were considered in the analysis by Schochet et al. (2008). The first three columns of estimates suggest that employed individuals in the treatment group have higher wages than those in the control group ($7.52 compared to $7.29) and the difference ($0.23) is statistically significant at conventional levels. Similarly, employed individuals in the treatment group are 3, 2.2 and 4.7 percentage points more likely to have health insurance, paid vacation and retirement or pension benefits, respectively, than individuals in the control group. In our analysis we include additional indicators of fringe benefits and summarize them in the lower half of Table 2. In the full sample, employed individuals assigned to treatment are significantly more likely to have paid sick leave, child care assistance, dental plan and tuition reimbursement benefits than controls (a few percentage points in all cases), while differences across treatment and control group employed individuals in their proportion of individuals with flexible hours and employer-provided transportation benefits are positive but not statistically significant. We also include, in the last three columns of Table 2, the same information for the non-Hispanic sample. Other than noting that, in general, magnitudes for the differences across treatment and control group individuals are slightly higher in our non-Hispanic sample, these estimates have the same qualitative implications than those reported for the full sample, that is, employed individuals in the treatment group earn more and are somewhat more likely to receive fringe benefits than those in the control group.

As clearly pointed out by Schochet et al. (2008) all of these estimated differences do not have a causal interpretation since they are conditional on being employed in quarter 16 after randomization. As shown in the first row of Table 2, average employment in quarter 16 is affected by random assignment. Therefore, conditioning on employment likely introduces bias due to the well-known sample selection problem as the probability of observing the outcome is affected by the intervention. In the same way, this sample selection problem also affects our proposed measures of job quality, as they are only observed for those employed. In the next section, we describe the construction of our job quality measures and and summarize them. In Section 4, we describe the empirical strategy that allows us to estimate causal effects of interest in the present context.

3 Constructing a Job Quality Index

The economic literature supports the notion of analyzing a single measure that bundles important aspects of labor compensation, namely, wages and fringe benefits (e.g., Woodbury, 1983; Farber, 1997; Eriksson and Kristensen, 2014). The approach we follow is pragmatic in nature. We employ the principal components method to construct an index of job quality that bundles the wage rate and the available indicators of fringe benefits summarized in Table 2. The approach is widely implemented in the analysis of various important economic questions and in other disciplines.[8] To our knowledge, however, no other study within the program evaluation literature combines the availability of an experimental evaluation and the use of principal components to analyze effects on job quality accounting for selection into employment.[9] Our proposed job quality index

[8]As noted in Cawley et al., 2001, other variants of factor analysis, for example, principal factor and hierarchical principal factor, are known to produce virtually the same results produced by the related principal components analysis, as long as the first factor or component is the only one retained. A formal comparison and discussion of advantages and disadvantages of these related methods is presented in Jensen (1987).

[9]Many important studies analyzing different aspects of the returns to education have employed factor and/or principal components analysis to accomplish a similar task, that is, to produce a single interpretable measure from a set of several proxies for the latent variable of interest, for example, Black and Smith (2006), Cawley et al. (2001), Carniero et al. (2003), and Heckman et al. (2013). This last study

is analogous to an index of college quality proposed in the study of returns to quality of college education by Black and Smith (2006). Akin to their analysis, we adopt the idea of treating the observed job quality correlates (summarized in Table 2) as inputs into the production of a (latent) job quality measure.[10]

We start with a set of K correlated variables, i.e., the wage rate and indicators of fringe benefits. As shown in the set of equations in (1), principal components analysis employs a linear weighted combination of the K variables after being normalized, represented by X_k, to construct different uncorrelated components PC_k, with $k = 1, ..., K$.

$$
\begin{aligned}
PC_1 &= w_{11}X_1 + ... + w_{1K}X_K \\
&\;\;... \\
PC_K &= w_{K1}X_1 + ... + w_{KK}X_K,
\end{aligned}
\tag{1}
$$

where the weights w_{kk} 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 corresponds to 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 K.

Table 3 summarizes the principal components analysis used to construct two indices of job quality. Under the heading Index 1, we summarize the principal components estimated with the same set of variables analyzed in Schochet et al. (2008) plus the additional information on the availability of other fringe benefits and a control for the occupation in the most recent job in quarter 16 after randomization. Further, we separate the analysis for the full (top panel) and non-Hispanic samples (bottom panel). In both

also exploited the availability of a randomized evaluation of an important early childhood education program in the US, known as the Perry Preschool program. In contrast, our interest is on analyzing the effects of program availability on the proposed index, whereas these studies used their indices or low dimensional measures as explanatory variables.

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

[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.

samples we note a large difference in eigenvalues and the percentage of variation in the original variables (i.e., covariance) explained by the first principal component, relative to higher principal components. For example, the first principal component that constitutes Index 1 has an eigenvalue of 4.4 and explains about 40% of the covariance while the respective values for the second principal component are about 1.2 and 11%. Columns under the subheading Index 2 summarize results for principal components estimated with the same set of variables as in Index 1 but omitting the wage rate. An analysis of an index of job quality without wages allows us to learn about the relative importance of the employed fringe benefits.[12] Similar to the principal components analysis for Index 1, we note that the first principal component representing Index 2 has an eigenvalue of around 4.3 and explains close to 43% of the covariance while the second principal component's eigenvalue is 1.2 and the percentage of the covariance explained is about 12%. In both samples, it is important to highlight that the differences across indices are, to a large extent, negligible, and that in both cases Horn's parallel analysis (Horn, 1965) strongly favors retaining the first principal component to measure job quality. Table 3 also reports scoring factors for both indices. In general, these estimates are indicative of a positive correlation between the variables included in the construction of our job quality indices. Such positive correlation among inputs is desirable to facilitate the interpretation of these indices.

In Table 4, we further gauge the reliability of our two job quality indices. We separate individuals in 4 mutually exclusive groups defined by quartiles of the distribution of the respective job quality index. All individuals in the group with the lowest job quality index, 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, the second and third quartile, and above the third quartile, columns labeled "Q1 to Q2", "Q2 to Q3" and "Q3 & up", respectively. For each group, we compute the average and proportions for the variables employed as inputs of job quality. Focusing on the full sample (top panel), in the case of Index 1 (first 4 columns) there is a monotone increase in the average wage and the proportion of people with health insurance, paid vacation and retirement or pension benefits as we consider groups with higher values for the job

[12]For instance, by comparing the effects of the availability of JC on the two indices one can learn about the potential role that the wage rate has in driving results.

quality index. With the exception of employer-provided transportation, we also observe a monotone increase in the proportions of 5 additional variables measuring fringe benefits, which were also employed in the construction of Index 1. These full sample results for Index 1 are consistent with the non-Hispanic sample results for Index 1, as shown in the bottom panel of Table 4. In the last 4 columns, we report similar results in our analysis for Index 2. In both samples, with the exception of the variable employer-provided transportation, we report a monotonic and sizeable increase in the average wage and proportion of individuals with the different fringe benefits, as we consider groups with higher values for the job quality index. While Index 2 was constructed without using the wage rate, the evidence presented in Table 4 also suggests that both of our indices are internally coherent, robust and comparable across sub-samples.[13]

4 Bounding Treatment Effects

To deal with sample selection, point identification techniques employ distributional assumptions—such as selection models (Heckman, 1979)—or rely on the availability and validity of exclusion restrictions—such as instrumental variables models (Heckman, 1990; Heckman and Smith, 1995; Imbens and Angrist, 1994; Angrist, Imbens and Rubin, 1996). In contrast, we use an alternative approach that constructs bounds on the parameters of interest. Horowitz and Manski (2000; HM hereafter) proposed a general framework to construct bounds on treatment effects when data are missing due to a nonrandom process, such as self-selection into employment. The HM bounds are nonparametric and allow for heterogeneous effects, which departs from the usually employed assumption of constant effects over the population. These bounds only require randomization of treatment assignment and that the outcome has a bounded support. One caveat is that the HM bounds are often wide and uninformative about the sign of the effects of interest. In what follows, we build on this approach by imposing more structure through the use of assumptions that would typically be weaker than those needed for point identification. Below, we explicitly discuss the bounding technique in the context where job quality is the outcome. However, we note that the same technique will be applied to the analysis

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

of the individual inputs used in the construction of the job quality index.

4.1 Bounds on Average Treatment Effects

Table 5: Pricipal Strata within Observed cells defined by T_i and S_i.

		T_i = 0	T_i = 1
S_i	0	NN & NE	NN & EN
	1	EE & EN	EE & NE

Consider a random sample of size N from a large population. Let $T_i = z \in \{0,1\}$ indicate whether unit i was randomly assigned to the treatment group ($T_i = 1$) or to the control group ($T_i = 0$). Let the potential employment values be $S_i(0)$ and $S_i(1)$ when i is assigned to control ($T_i = 0$) and treatment ($T_i = 1$), respectively. We use principal stratification (Frangakis and Rubin, 2002) to define subpopulations based on values for the potential employment $\{S_i(0), S_i(1)\}$. In Table 5 we show how the population is partitioned into four subpopulations (strata): *always-employed*, $EE = \{i : S_i(0), S_i(1) = (1,1)\}$; *employed if assigned to the treatment group*, $NE = \{i : S_i(0), S_i(1) = (0,1)\}$; *never-employed*, $NN = \{i : S_i(0), S_i(1) = (0,0)\}$; and *employed if assigned to the control group*, $EN = \{i : S_i(0), S_i(1) = (1,0)\}$. Similar to Lee (2009), Zhang et al. (2008), and BFF-L, we focus on the average effect of a program on job quality for individuals that belong to the EE stratum, i.e., those who would be employed regardless of treatment assignment. This stratum is the only one for which the job quality index is observed under both treatment arms, and thus fewer assumptions are required to construct bounds for its effects. The average treatment effect for this stratum is:

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

where $Y_i(1)$ and $Y_i(0)$ are the potential job quality measures for unit i under treatment ($T_i = 1$) and control ($T_i = 0$), respectively. In addition to the assumption of a randomly assigned treatment (**Assumption 1**), which holds by design, we employ:[14]

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

[14] Unlike the HM bounds, the bounds we employ do not required a bounded support for the outcome.

This assumption states that treatment assignment weakly affects selection in one direction, effectively ruling out the stratum of *employed if assigned to the control group*, $EN = \{i : S_i(0), S_i(1) = (1, 0)\}$.[15]

Assumptions 1 and 2 allow the point identification of the term $E[Y_i(0)|EE]$ in (2) as $E[Y_i|T_i = 0, S_i = 1]$, since those are control individuals with observed job quality that belong to the EE stratum. This is clearly illustrated if one eliminates the EN stratum in Table 5 from the cell defined by $T_i = 0$ and $S_i = 1$ (lower left corner). However, it is not possible to point identify the treatment counterfactual $E[Y_i(1)|EE]$, since the observed group with $(T_i, S_i) = (1, 1)$ is a mixture of individuals from two strata, EE and those belonging to the stratum of *employed if assigned to the treatment group*, $NE = \{i : S_i(0), S_i(1) = (0, 1)\}$. Nevertheless, $E[Y_i(1)|EE]$ can be bounded. The proportion of EE individuals in $(T_i, S_i) = (1, 1)$ can be point identified as $(p_{1|0}/p_{1|1})$, where $p_{s|t} \equiv Pr(S_i = s|T_i = t)$ for $t, s = 0, 1$. Therefore, $E[Y_i(1)|EE]$ can be bounded from above (below) by the expected value of Y_i for the $(p_{1|0}/p_{1|1})$ fraction of the largest (smallest) values of Y_i in the observed group $(T_i, S_i) = (1, 1)$. In other words, the upper bound for the treatment counterfactual is obtained under the scenario that the largest $(p_{1|0}/p_{1|1})$ fraction of values of Y_i belongs to individuals from the EE stratum. Analogously, a lower bound for $E[Y_i(1)|EE]$ is obtained under the scenario where Y_i for individuals in the EE stratum are in the lowest $(p_{1|0}/p_{1|1})$ portion of the observed job quality distribution in $(T_i, S_i) = (1, 1)$. These bounds on the counterfactual $E[Y_i(1)|EE]$ and the point identified term $E[Y_i(0)|EE]$ are used to bound ATE_{EE} in (2). Formally, under Assumptions 1 and 2, the resulting upper (UB_{EE}) and lower (LB_{EE}) bounds for ATE_{EE} are:

$$
\begin{aligned}
UB_{EE} &= E[Y_i|T_i = 1, S_i = 1, Y_i \geq y^{11}_{1-(p_{1|0}/p_{1|1})}] - E[Y_i|T_i = 0, S_i = 1] \\
LB_{EE} &= E[Y_i|T_i = 1, S_i = 1, Y_i \leq y^{11}_{(p_{1|0}/p_{1|1})}] - E[Y_i|T_i = 0, S_i = 1],
\end{aligned} \tag{3}
$$

where $y^{11}_{1-(p_{1|0}/p_{1|1})}$ and $y^{11}_{(p_{1|0}/p_{1|1})}$ denote the $1 - (p_{1|0}/p_{1|1})$ and the $(p_{1|0}/p_{1|1})$ quantiles of the Y_i distribution conditional on $T_i = 1$ and $S_i = 1$, respectively. Lee (2009) shows that these bounds are sharp (i.e., there are no shorter bounds possible under the current assumptions).

[15]Lee (2009), Zhang et al. (2008), and BFF-L employed this assumption, and similar assumptions are widely used in the instrumental variable (e.g., Imbens and Angrist, 1994) and partial identification literature (Manski and Pepper, 2000; Bhattacharya et al., 2008; Flores and Flores-Lagunes, 2010).

In addition, we consider the following assumption to narrow the previous bounds.

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

Intuitively, this assumption formalizes the notion that the EE stratum is likely to be comprised of more "able" individuals than those belonging to the NE stratum. Since "ability" is positively correlated with labor market outcomes, one would expect the job quality index for the individuals who are employed regardless of treatment status (the EE stratum) to weakly dominate on average the job quality index of those individuals who are employed only if assigned to training (the NE stratum). Adding Assumption 3 implies $E[Y_i|T_i = 1, S_i = 1] \leq E[Y_i(1)|EE]$, that is, the tighter lower bound for the counterfactual $E[Y_i(1)|EE]$ is $E[Y_i|T_i = 1, S_i = 1]$. Thus, under Assumptions 1, 2 and 3, the upper bound (UB_{EE}) for ATE_{EE} is the same as in (3) and the tighter lower bound becomes: $E[Y_i|T_i = 1, S_i = 1] - E[Y_i|T_i = 0, S_i = 1]$. Imai (2008) shows that these bounds are sharp. See BFF-L (2013) for details about the estimation of bounds for the average treatment effect under Assumptions 1 through 3.

4.2 Bounds on Quantile Treatment Effects

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

$$QTE_{EE}^{\alpha} = F_{Y_i(1)|EE}^{-1}(\alpha) - F_{Y_i(0)|EE}^{-1}(\alpha), \tag{4}$$

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

Similar to the ATE_{EE} case, under Assumptions 1 and 2 the last term in (4) is point identified from the cumulative distribution function (CDF) of individuals' job quality conditional on $(T_i, S_i) = (0, 1)$, say $F_{Y_i|T_i=0,S_i=1}(\cdot)$, while the first term is partially identified by trimming the CDF of Y_i in $(T_i, S_i) = (1, 1)$ based on the proportion $(p_{1|0}/p_{1|1})$. Formally, under Assumptions 1 and 2, we partially identify QTE_{EE}^{α} as

$LB_{EE}^{\alpha} \leq QTE_{EE}^{\alpha} \leq UB_{EE}^{\alpha}$, with

$$
\begin{aligned}
UB_{EE}^{\alpha} &= F^{-1}_{Y_i|T_i=1,S_i=1,Y_i\geq y^{11}_{1-(p_{1|0}/p_{1|1})}}(\alpha) - F^{-1}_{Y_i|T_i=0,S_i=1}(\alpha) \\
LB_{EE}^{\alpha} &= F^{-1}_{Y_i|T_i=1,S_i=1,Y_i\leq y^{11}_{(p_{1|0}/p_{1|1})}}(\alpha) - F^{-1}_{Y_i|T_i=0,S_i=1}(\alpha),
\end{aligned}
\tag{5}
$$

where $F^{-1}_{Y_i|T_i=1,S_i=1,Y_i\geq y^{11}_{1-(p_{1|0}/p_{1|1})}}(\alpha)$ and $F^{-1}_{Y_i|T_i=1,S_i=1,Y_i\leq y^{11}_{(p_{1|0}/p_{1|1})}}(\alpha)$ correspond to the α-quantiles after trimming, respectively, the lower and upper tail of the distribution of Y_i in $(T_i, S_i) = (1, 1)$ by $1 - (p_{1|0}/p_{1|1})$, and thus they provide an upper and lower bound for the counterfactual $F^{-1}_{Y_i(1)|EE}(\alpha)$.

The trimming bounds for QTE_{EE} in (5) can be tightened by strengthening Assumption 3. Let $F_{Y_i(1)|EE}(\cdot)$ and $F_{Y_i(1)|NE}(\cdot)$ denote the CDFs of $Y_i(1)$ for individuals who belong to the EE and NE strata, respectively:

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

This assumption directly imposes restrictions on the distribution of potential outcomes under treatment for individuals in the EE stratum, which results in a tighter lower bound. Adding Assumption 4 results in sharp bounds (Imai, 2008), where the lower bound is now the untrimmed difference: $F^{-1}_{Y_i|T_i=1,S_i=1}(\alpha) - F^{-1}_{Y_i|T_i=0,S_i=1}(\alpha)$. For a detailed and formal discussion about the estimation of bounds on the quantile treatment effects, under Assumptions 1, 2, and 4, see BFF-L (2013).

5 Main Results

We present our main results by first discussing the estimated bounds for average treatment effects on each of the inputs used in the construction of our job quality indices, then we focus on the estimated bounds for average treatment effects on the proposed indices. Given that the wage rate is the only continuous input employed in the construction of our job quality measures, we complement the average effects analysis by presenting estimated bounds for the quantile treatment effects on wages. Finally, we close this section by presenting the estimated bounds for the quantile treatment effects of the availability of JC on the job quality of *always-employed* eligible participants.

5.1 Estimated Bounds for the Average Treatment Effects on the Inputs of Job Quality

The analysis for the full and non-Hispanic samples in Table 6 presents, under 2 sets of assumptions, the estimated bounds for the average effect of JC on wages and indicators of fringe benefits, which are important inputs of job quality as suggested in Tables 3 and 4. We highlight that these results are relevant for individuals who are employed, regardless of treatment assignment, in quarter 16 after the randomization, i.e., the EE stratum or *always-employed*. Therefore, it is of interest to estimate the size of that stratum relative to the full population. The estimated strata proportions vary slightly in the analysis of every input of job quality due to small differences in the amounts of missing information for these variables.[16] To avoid having an overwhelming amount of information in Table 6, we report the estimated strata proportions for the different inputs analyzed in the appendix Table A1. Here we only note that the EE stratum is consistently the largest one, accounting for more than two-thirds of the population. A discussion about the plausibility of the two monotonicity assumptions employed throughout our analysis is relegated to Section 5.2.

The full sample results are reported in the first column of estimates in Table 6, while the non-Hispanic sample results are reported in the last column. For each variable and set of assumptions (row header), we report the estimated bounds within brackets and their respective 95% Imbens and Manski (2004; IM hereafter) confidence intervals are reported within parentheses. Focusing on the full sample, under randomization and individual-level weak monotonicity of employment in JC assignment, Assumptions 1 and 2, the estimated bounds for the average treatment effects on the wage rate (in logs) for the *always-employed* do not rule out zero or a small negative effects. A similar qualitative result is estimated when analyzing the fringe benefits of having employer provided child care, flexible hours, and transportation. On the other hand, the lower bound is consistent with a positive impact on the availability of employer provided health insurance, paid vacation, retirement or pension benefits, paid sick leave, dental plan

[16]The proportions of missing information for the variables we employ vary from 0 to 0.09. Similarly to Schochet et al. (2008) and other studies on JC (Lee (2009), Zhang et al. (2009), Flores-Lagunes et al. (2010), Blanco et al. (2013, 2013b), and Blanco (2017)), we implicitly assume that values are missing completely at random.

and tuition aid. However, in all but one of these cases the positive lower bound is not statistically significant based on the 95% IM confidence intervals. The exception is retirement or pension benefits, where the positive and significant lower bound estimate is consistent with a relative increase of 2.7 percentage points and the upper bound suggest that the effect could be as large as 6.2 percentage points. While estimated results for the non-Hispanic sample are remarkably similar than those for the full sample, it is important to note that all the estimated upper bounds are slightly higher for the non-Hispanic sample and that no lower bound is positive and significant based on the IM confidence intervals.

The small and negative or positive but not statistically significant estimated lower bounds under Assumptions 1 and 2, reported in Table 6, can be interpreted as pointing toward positive effects (Lee, 2009). The reason is that under these assumptions the lower bound places the *always-employed* individuals (EE stratum) at the bottom of the observed outcome distribution in $(T_i, S_i) = (1, 1)$. While this mathematically identifies a valid lower bound, it implies a perfect negative correlation between employment and the important inputs of job quality (i.e., wages and fringe benefits). Such negative relationship may be regarded as implausible from the standpoint of standard models of labor supply. Indeed, one interpretation that can be given to Assumption 3 is that of formalizing this theoretical notion to tighten the lower bound. Results for the full sample show that most of the lower bounds for ATE_{EE} on the important inputs of job quality are now indicative of positive and significant effects after adding Assumption 3. For example, the significant effect on wages is at least 2.7 percent and could potentially be as large as a 6.7 percent. Similarly, the availability of employer provided health insurance, paid vacation, retirement or pension benefits, paid sick leave, child care, dental plan and tuition aid are also affected positively. The statistically significant lower bounds range from a 1.7 percentage point increase in employer provided child care to a 4.5 percentage points increase in retirement or pension benefits. On the other hand, the estimated lower bounds for the ATE_{EE} on the benefits of having flexible hours and employer provided transportation are positive but not statistically significant based on their IM confidence interval. Relative to results for the full sample, after adding Assumption 3, most of the estimated lower bounds for the non-Hispanic sample are slightly larger in magnitude, so both the lower and upper bounds are consistent with relatively larger impacts. For example, for non-Hispanics the ATE_{EE} on wages is bounded between 3.7 and 8.7 percent, and the effect on the important fringe benefit of having a retirement or pension benefit is bounded between 5.1 and 7.6 percentage points.[17]

5.2 Estimated Bounds for the Average Treatment Effects on Job Quality

The main analysis of average effects on our two proposed indices, for the full and non-Hispanic samples, is presented in Table 7. We remind readers that both indices have been normalized to ease interpretation of our estimates. Our focus is on individuals employed, regardless of treatment assignment, in quarter 16 after randomization. These *always-employed* account for about 64 and 63 percent of the entire full and non-Hispanic samples (first row). For both samples, the other estimated principal strata proportions indicate that the *never-employed* are the second largest group accounting for about 33 percent, followed by those who are *employed if assigned to treatment* with a small but statistically significant proportion. In the last four rows of Table 7 we report the estimated bounds within brackets with their respective 95% IM confidence intervals are reported within parentheses. Focusing on the full sample, under Assumptions 1 and 2, estimated lower bounds are positive for both indices considered, however, these bounds are not statistically different from zero based on the IM confidence interval. So one cannot rule out zero effects but positive effects can plausibly be as large as about 14 to 15 percent of a standard deviation in job quality, as suggested by the estimated upper bounds.

While the assumption of individual-level weak monotonicity of the effect of JC on employment (Assumption 2) is not testable, under certain circumstances it may be deemed dubious. For example, it has been documented that Hispanics in the NJCS exhibited negative (albeit not statistically significant) average effects of JC on both their employment and weekly earnings, while for the other groups these effects were positive and highly statistically significant (Schochet et al., 2001; Flores-Lagunes et al., 2010). This evidence

[17] Using a similar format than in Table 6, we present estimated bounds for various non-Hispanic demographic groups in the appendix Table A2. While some of the qualitative conclusions already discussed apply to other demographic groups, we highlight the fact that in many instances the estimated bounds are indicative of important heterogeneous effects across demographics. We further exploit this information in our discussion of estimated results in subsequent sub-sections of the paper.

casts doubt on the validity of Assumption 2 for Hispanics. Furthermore, Imai (2008) proposed implementing a testable implication of Assumption 2, based on the fact that Assumptions 1 and 2 imply (but are not implied by) $E(S_i|T_i = 1) - E(S_i|T_i = 0) \geq 0$. Note that $E(S_i|T_i = 1) - E(S_i|T_i = 0) = \pi_{NE}$, which is the proportion of *employed if assigned to treatment*. This proportion is positive and significant for the full and non-Hispanic samples (Table 7, third row), however, its estimate is negative and insignificant for Hispanics (not reported).[18] In the last 2 columns of Table 7 we report that our analysis for the non-Hispanic sample yields similar qualitative results than those observed for the full sample, that is, while the estimated lower bounds are positive, zero effects cannot be ruled out. On the other hand, the estimated magnitudes for the lower bound are slightly smaller for the non-Hispanic sample relative to the full sample, while the opposite is true for the estimated upper bounds.

In addition, we analyze the non-Hispanic groups of whites, blacks, females and males, and their estimates are presented in Table 8. First, we note that the proportions of the stratum of interest, the *always-employed*, varies from about 59 to 72 percent across demographic groups and indices. Whites have the largest proportion of *always-employed* while blacks have the lowest. In every case, our focus is on the largest and more important stratum. Second, consistent with the testable implication for Assumption 2 discussed above, we note that the estimated proportion of *employed if assigned to treatment*, π_{NE}, is positive and significant for all of these non-Hispanic samples. With the exception of the negative lower bound for blacks, bounds under Assumptions 1 and 2 are indicative of positive effects on both indices across demographics, however, based on the IM confidence intervals a zero effect cannot be ruled out.

Similar to our analysis of the inputs of job quality, under Assumptions 1 and 2, the lower bound places the *always-employed* at the bottom of the observed outcome distribution in $(T_i, S_i) = (1, 1)$, implying a perfect negative correlation between employment and job quality. As suggested by standard models of labor supply, we add Assumption 3 to tighten a lower bound that now rules out the previously implied perfect negative relation. Focusing on the last 2 rows of Table 7, in both main samples we note that the estimated lower bounds for the average effect of JC on the two job quality indices

[18] For similar reasons, the non-Hispanic group has been a main group analyzed by, for example, BFF-L (2013) and Chen and Flores (2015).

are now positive and statistically significant, based on the IM 95% confidence intervals. Bounds' estimated magnitudes are slightly larger for the non-Hispanic sample, relative to the full sample estimates. It is important to note that the estimated impact seen in both samples is economically important. For example, relative to *always-employed* non-Hispanics in the control group, JC eligibility significantly increases job quality by at least 10.03 percent of a standard deviation in Index 1, and the effect could be as large as 18 percent. Interestingly, there are no major differences between the estimated bounds for Index 1 and Index 2, where the latter does not consider wages. Estimated bounds for both indices are consistent with bounds presented in the analysis of the wage rate and the different fringe benefits (Table 6). One would expect a reduction in the estimated bounds if the wage rate is omitted from the job quality index, and indeed a slight reduction is estimated in general. Small reductions, we argue, are indicative of a high degree of complementarity between wages and the fringe benefits considered for these samples.

We turn our attention back to the analysis of other non-Hispanic demographics. The last two rows of Table 8 present estimated bounds after adding Assumption 3. All estimated lower bounds are now positive and statistically significant, based on the IM 95% confidence interval. Regardless of the index employed, it is interesting to note that the estimated bounds suggest that there is the potential for heterogeneous impacts across groups. For example, focusing on Index 1, estimated bounds for non-Hispanic females are consistent with larger impacts in job quality, relative to the non-Hispanic males, whose estimates are about 50 percent smaller in magnitude. As before, after removing wages from the job quality index, the striking differences between non-Hispanic females and males remain in our analysis of Index 2. Estimated bounds for both indices are consistent with increases in job quality that result from the relatively larger effects on the likelihood of having access to fringe benefits found in the non-Hispanic females sample (see appendix Table A2), while estimated bounds on wage effects are very similar across gender.[19] Regardless of the potential for large heterogeneous impacts, estimates for all the demographic groups analyzed are economically important.

Assumption 3 implies that the *always-employed*, who conform the EE stratum, possess traits that result in better labor market outcomes relative to the *employed if assigned to*

[19]This results is quite interesting considering that females have been found to value fringe benefits relatively more (e.g., Lowen and Sicilian, 2009; Currie and Chaykowski, 1992; and Goldin, 2014).

treatment that conform the NE stratum. While Assumption 3 is not directly testable, we exploit its implications and compare pre-treatment covariates correlated with job quality across the EE and NE strata. We selected the pre-treatment variables: earnings, whether the individual held a job, months employed (all three in the year prior to randomization), and education at randomization. The estimated differences between the average pre-treatment variables employed for the EE and NE strata were all positive, suggesting that Assumption 3 is plausible in our context.[20]

5.3 Estimated Bounds for the Quantile Treatment Effects on Wages

Given that the wage rate is the only continuous variable we employ as an input of job quality, we proceed to complement the analysis on average effects presented in Table 6 by reporting estimated bounds for quantile treatment effects on the log of wages for the *always-employed* (EE stratum), QTE_{EE}^{α}. Employing the preferred set of assumptions 1, 2 and 4, we present estimated bounds, with their corresponding IM confidence intervals, at several quantiles and for the different groups under analysis in Figure 1.[21]

Looking at the full sample estimated bounds for the QTE_{EE}^{α} in Figure 1(a), they are all positive in magnitude, however, in many of the quantiles below the median the lower bound is not statistically significant as suggested by the IM confidence intervals. Many of the quantiles above the median have a lower bound that is positive and statistically significant. Interestingly, the effect at the median is point identified and it suggests that the availability of JC has a positive and significant effect on wages for the *always-employed* of about 4.8 percent, which is closer to the upper bound reported in our analysis of the average treatment effects. Consistent with our analysis of average impacts, while the estimated bounds on QTE_{EE}^{α} for non-Hispanics in Figure 1(b) are generally more positive

[20] For details on how to implement the comparison of means across the EE and NE strata see BFF-L (2013). The table corresponding to this exercise can be found in the Internet Appendix.

[21] For simplicity, we only report estimates after employing the preferred set of assumptions, that is, after adding the stochastic dominance Assumption 4 to Assumptions 1 and 2. We would like to note that in the absence of the stochastic dominance all of the estimated lower bounds for the quantiles considered would either be negative or positive but statistically indistinguishable from zero. We conclude that adding Assumption 4 has significant identification power since many of the estimated lower bounds become positive and statistically significant as shown in Figure 1. The complete numerical results under Assumptions 1 and 2 and also under Assumptions 1, 2 and 4 are shown in the Internet Appendix.

relative to the full sample estimates, the difference in magnitudes is small enough that the same type of conclusions can be drawn. For example, 6.4 percent is the point identified estimate for the QTE_{EE}^{α} at the median in the sample of non-Hispanics. One notable difference, however, is that for non-Hispanics it is clearer that in most quantiles above the median the effect of JC is positive and significant, as suggested by the estimated lower bounds and their respective IM confidence intervals.

The results by race are shown in Figures 2(c) and 2(d). In contrast to the full, non-Hispanic and whites samples, where significant effects are found near the median and in higher quantiles of the wage distribution, most quantiles in the lower half of the wage distribution for the sample of blacks have lower bounds that are positive and significant, so JC availability has a positive and significant effect on wages throughout the distribution for this particular sample. Another interesting result that we can highlight is that in the sample of whites, relative to all other samples, the estimated bounds that partially identify positive and statistically significant effects tend to be larger in magnitude. Finally, Figures 1(e) and 1(f) show the estimated bounds for the QTE_{EE}^{α} and their corresponding 95% IM confidence intervals for non-Hispanic males and females, respectively. In all of the quantiles, in contrast to the other samples analyzed, the bounds for non-Hispanic males are very precisely estimated. Similarly to the estimates for the sample of blacks, in most of the quantiles in the lower half of the wage distribution for non-Hispanic males the effects of JC eligibility are positive and statistically significant as suggested by the lower bounds and their respective IM confidence intervals. Also, effects are clearly positive and statistically significant throughout the distribution of wages for non-Hispanic males. On the other hand, results for non-Hispanic females are more similar to those reported for whites, that is, estimated bounds' IM confidence intervals do not rule out zero effects in most quantiles below the median, while positive and significant effects are estimated in many of the quantiles beyond the median.

5.4 Estimated Bounds for the Quantile Treatment Effects on Job Quality

One advantage of having constructed a continuous index of job quality is that it allows us to analyze effects beyond the average impact by estimating bounds for quantile treatment effects for the *always-employed* group, that is, QTE_{EE}^{α}. For the different samples under analysis, Figures 2 and 3 are employed to summarize our estimated bounds, under assumptions 1, 2 and 4, at several quantiles of the distribution for Index 1 and Index 2, respectively.[22] To start summarizing some noteworthy trends, we focus on Figures 2(a) and 2(b), which present the estimated bounds for the QTE_{EE}^{α} on our preferred job quality Index 1 for the full and non-Hispanic samples. When excluding Hispanics from the full sample, estimated bounds shift towards slightly larger effects. In both samples the estimated bounds rule out statistically significant effects of JC eligibility in lower quantiles of the job quality distribution for the *always-employed*. Interestingly, in both samples the positive and statistically significant effects are at least larger than a 15 percent of a standard deviation in job quality between the 0.35 and the 0.75 quantile, and around the median some effects can be as large as 30 to 40 percent of a standard deviation in job quality. Relative to the estimated bounds for average effects, the significant effects at the median are about 50 percent larger in magnitudes for both these samples.

In general, while positive, the estimated lower bounds are not statistically significant in lower quantiles of Index 1 for the other demographic groups considered. Results for the samples of whites and blacks, Figures 2(c) and 2(d), are very similar; the only differences that we note are that in the upper tail of the distribution the estimated bounds are indicative of a positive and significant JC impact on the job quality index for blacks but not for whites, whose estimated lower bounds are not distinguishable from zero based on the IM confidence intervals. Also, the statistically significant bounds beyond the median are somewhat larger for whites. Results for non-Hispanic males, in Figure 2(e), are the most pessimistic about the JC effect on the distribution of job quality, where positive and significant results are only found in quantiles 0.45, 0.85 and 3 other quantiles between the median and the third quartile, with estimated magnitudes that are comparable to other previously reported. In contrast, non-Hispanic females, Figure 2(f), could potentially be the demographic benefiting more from JC, where all but one of the estimated bounds are positive and statistically significant beyond the first quartile of the

[22] Consistent with our analysis of QTE_{EE}^{α} on wages, here we also prefer to focus on presenting figures summarizing the estimated bounds for QTE_{EE}^{α} on the job quality indices after employing the preferred set of assumptions. We note that transitioning to bounds that add Assumption 4, to Assumptions 1 and 2, results in a tighter lower bound that, in many instances, becomes positive and statistically significant. As before, the complete numerical results for bounds under both sets of assumptions are relegated to the Internet Appendix.

job quality distribution, with notably larger magnitudes.

Figure 3 presents the estimated bounds for the QTE_{EE}^{α} on Index 2, where the wage rate was not considered as an input of job quality. Similar to the analysis of average effects, the differences in estimates found across Index 1 and Index 2 for the full and non-Hispanic samples are small, and thus, most of the conclusions drawn from the analysis of Index 1 apply to the analysis of Index 2. Thus, the results on Index 1 do not seem to be driven by wages. Nevertheless, some important points can be highlighted when contrasting both indices. Estimated bounds for the effects at the median are smaller in magnitudes when analyzing Index 2 for both samples. Relative to estimates for Index 1, we note that after excluding the wage rate, a couple of lower bounds for the effects of interest in lower quantiles of the job quality Index 2 are now positive and statistically significant. This is an interesting result given that the estimated lower bounds for the QTE_{EE}^{α} on wages were not statistically significant in lower quantiles of the distribution (see Figure 1).

We also observe a similar result for whites, note that in most lower quantiles, the effects of interest are positive and significant based on the lower bounds and their IM confidence intervals. Again, the result for whites is interesting when one considers that the analysis for wages indicated that there were no effects on lower quantiles. The results for blacks across indices are strikingly similar. On the other hand, we highlight that the effects in the lower quantiles of both job quality indices are not statistically significant, which is remarkable given that we bounded positive and significant effects in most lower quantiles of the wage distribution for blacks. The case of non-Hispanic males is peculiar since this is the only demographic considered whose estimated lower bounds for the QTE_{EE}^{α} on Index 2 decrease in magnitude in almost every quantile considered, relative to the estimated lower bounds for the QTE_{EE}^{α} on Index 1, leading to only 3 quantiles (out of 20) with statistically significant effects. Interestingly, most estimated bounds for the QTE_{EE}^{α} are indicative of relatively large and significant effects throughout the wage distribution for non-Hispanic males. Our distributional analysis of Index 2 is consistent with our analysis of Index 1 for non-Hispanic females. Relative to the other demographic groups, we consistently find that *always-employed* females have the potential for larger benefits at most points of the job quality distribution. The latter result is in contrast to our finding that estimated bounds for the QTE_{EE}^{α} on wages of non-Hispanic females do not rule out zero effects in most quantiles below the median.

Finally, we indirectly gauge the plausibility of Assumption 4 in a similar fashion as Assumption 3. We use the quintiles of each pre-treatment variable (same variables as those employed to support Assumption 3) to form 5 mutually exclusive groups. Then, within these groups we compute and test the difference in the average pre-treatment variables between the EE and NE strata. We do not find evidence against the stochastic dominance assumption for any of the samples analyzed. The results of this exercise can be found in the Internet Appendix.

6 Conclusions

We analyze 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 enrolling disadvantaged youth. Using data from a randomized evaluation of the JC, we employ principal components analysis to construct a single index that effectively bundles information on wages and the availability of fringe benefits. Even with the availability of random assignment of treatment, the estimation of causal effects on our job quality index is not straightforward due to the sample selection problem. In our context, sample selection arises since factors simultaneously affect both the index of job quality and its observability, in other words, the program has an effect on employment and we only observe the index for those employed at the end of the observation period (16 quarter after randomization). To deal with sample selection we employed recently developed nonparametric bounds for important treatment effects that account for sample selection. These bounds typically require weaker assumptions than those conventionally employed for point identification. For example, alternative estimators rely on difficult to find and validate exclusion restrictions. We estimate bounds for the average and quantile treatment effects on job quality using monotonicity and stochastic dominance assumptions that have empirical support in our main sample.

Our estimated bounds provide new and important evidence about the positive and significant effects of JC eligibility on the job quality of individuals who are employed regardless of treatment assignment during quarter 16 after randomization. This is the most important group, accounting for about 65 percent of the population. Under the preferred set of assumptions and index, bounds on average treatment effects are between 10 to 18 percent of a standard deviation in the job quality index for the non-Hispanics sample (full sample results are similar). Effects are somewhat heterogeneous across the different demographic groups analyzed. For example, non-Hispanic females have estimated bounds consistent with relatively larger impacts. The effect is also heterogeneous across the distribution of the job quality index, where estimated bounds suggest that effects are positive and significant beyond the 0.4 quantile. We note that estimated bounds are consistent with larger effects around the median for most samples. Finally, we report that estimated bounds for non-Hispanic females are larger in magnitude in many of the quantiles of the job quality index distribution, reinforcing our previous findings for the average effects. To the extent that there is overlap of individuals in low quantiles of the distributions of wages and job quality, the results suggests that while JC does not have a significant impact on wages in low paying jobs, it has an impact in the quality of the job that manifests through increases in the availability of fringe benefits.

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Table 1: Characteristics of Eligible Job Corps Applicants in the Full and Non-Hispanic Samples

	Full Sample	**Non-Hispanic Sample**
Gender	Proportion	Proportion
Male	0.56	0.57
Females without children	0.31	0.30
Females with children	0.13	0.13
Age at application		
16 to 17	0.42	0.42
18 to 19	0.32	0.32
20 to 24	0.27	0.26
Race and ethnicity		
White, non-Hispanic	0.27	0.33
Black, non-Hispanic	0.48	0.58
Hispanic	0.17	
Other	0.07	0.09
Had a high school credential		
High school diploma	0.19	0.19
GED certificate	0.05	0.05
Lived in a metropolitan statistical area	0.77	0.75
Arrest history (self-reported)		
Ever arrested	0.26	0.26
Arrested for serious crimes[a]	0.04	0.04
Received food stamps in the past year	0.46	0.45
Had a job in the past year	0.65	0.65
Average earnings in the past year ($)	2900.18	2889.24
Random assignment (individuals)		
Treatment	6828	5653
Control	4485	3698
Total Observations	11313	9351

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, by Treatment Status

	Full Sample			**Non-Hispanic Sample**		
	Treatment	Control	Difference	Treatment	Control	Difference
Percent employed	0.708	0.685	0.023 ***	0.711	0.678	0.032 ***
			(0.009)			(0.010)
Schochet et al. (2008) indicators:						
Wage	7.518	7.286	0.232 ***	7.511	7.188	0.323 ***
			(0.077)			(0.086)
Health insurance	0.571	0.541	0.030 ***	0.574	0.534	0.040 ***
			(0.012)			(0.013)
Paid vacation	0.627	0.606	0.022 *	0.628	0.602	0.026 **
			(0.011)			(0.012)
Retirement or pension benefits	0.483	0.436	0.047 ***	0.490	0.437	0.053 ***
			(0.012)			(0.013)
Additional fringe benefit indicators:						
Paid sick leave	0.472	0.442	0.030 ***	0.472	0.437	0.035 ***
			(0.012)			(0.013)
Child care	0.157	0.140	0.017 **	0.159	0.144	0.015 *
			(0.008)			(0.009)
Flexible hours	0.576	0.569	0.007	0.578	0.565	0.013
			(0.011)			(0.013)
Transportation	0.191	0.184	0.007	0.195	0.192	0.003
			(0.009)			(0.010)
Dental plan	0.498	0.470	0.027 **	0.504	0.466	0.038 ***
			(0.012)			(0.013)
Tuition aid	0.287	0.264	0.023 **	0.285	0.263	0.021 *
			(0.010)			(0.011)
Observations	6828	4485		5653	3698	

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 Indeces

Full Sample				
	Index 1		Index 2	
	Eigenvalues	Covariance explained (%)	Eigenvalues	Covariance explained (%)
Principal component 1	4.384	39.86	4.301	43.01
Principal component 2	1.188	10.80	1.184	11.84
Principal component 3	0.951	9.39	1.021	10.21

Scoring factors in the computation of the first principal component		
	Index 1	Index 2
Wage	0.155	
Health insurance	0.405	0.409
Paid Vacation	0.386	0.391
Retirement or pension benefits	0.396	0.401
Paid sick leave	0.387	0.392
Child care	0.245	0.251
Flexible hours	0.140	0.141
Transportation	0.083	0.083
Dental plan	0.407	0.411
Tuition aid	0.307	0.311
Occupation	0.104	0.102

Non-Hispanic Sample				
	Index 1		Index 2	
	Eigenvalues	Covariance explained (%)	Eigenvalues	Covariance explained (%)
Principal component 1	4.407	40.07	4.332	43.32
Principal component 2	1.186	10.78	1.176	11.76
Principal component 3	1.046	9.50	1.027	10.27

Scoring factors in the computation of the first principal component		
	Index 1	Index 2
Wage	0.147	
Health insurance	0.404	0.409
Paid Vacation	0.387	0.392
Retirement or pension benefits	0.397	0.401
Paid sick leave	0.388	0.393
Child care	0.246	0.251
Flexible hours	0.140	0.142
Transportation	0.085	0.085
Dental plan	0.407	0.411
Tuition aid	0.305	0.308
Occupation	0.107	0.104

Table 4: Mean and Proportions of Indicators within quartiles of Job Quality Indeces

Full Sample								
	Job Quality, Index 1				Job Quality, Index 2			
Quartile (Q):	Up to Q1	Q1 to Q2	Q2 to Q3	Q3 & up	Up to Q1	Q1 to Q2	Q2 to Q3	Q3 & up
Wage[a]	5.640	7.291	7.776	8.216	6.096	7.069	7.819	8.072
Health insurance	0.000	0.251	0.885	0.909	0.000	0.249	0.888	0.908
Paid Vacation	0.000	0.396	0.897	0.935	0.000	0.391	0.901	0.936
Retirement or pension benefits	0.000	0.098	0.686	0.892	0.000	0.103	0.676	0.896
Paid sick leave	0.000	0.186	0.692	0.745	0.000	0.180	0.696	0.746
Child care	0.000	0.046	0.091	0.340	0.000	0.043	0.093	0.342
Flexible hours	0.221	0.596	0.621	0.730	0.207	0.603	0.625	0.733
Transportation	0.059	0.262	0.143	0.242	0.053	0.272	0.132	0.247
Dental plan	0.000	0.106	0.755	0.879	0.000	0.108	0.756	0.877
Tuition aid	0.000	0.112	0.217	0.569	0.000	0.114	0.222	0.566
Non-Hispanic Sample								
	Job Quality, Index 1				Job Quality, Index 2			
Quartile (Q):	Up to Q1	Q1 to Q2	Q2 to Q3	Q3 & up	Up to Q1	Q1 to Q2	Q2 to Q3	Q3 & up
Wage[a]	5.656	7.317	7.696	8.111	6.099	7.084	7.722	7.992
Health insurance	0.000	0.251	0.890	0.904	0.000	0.256	0.893	0.902
Paid Vacation	0.000	0.396	0.899	0.931	0.000	0.398	0.902	0.932
Retirement or pension benefits	0.000	0.099	0.701	0.899	0.000	0.105	0.702	0.900
Paid sick leave	0.000	0.181	0.696	0.741	0.000	0.178	0.703	0.742
Child care	0.000	0.046	0.096	0.345	0.000	0.046	0.094	0.351
Flexible hours	0.214	0.603	0.622	0.727	0.207	0.602	0.641	0.726
Transportation	0.056	0.270	0.151	0.251	0.053	0.277	0.147	0.254
Dental plan	0.000	0.109	0.761	0.881	0.000	0.112	0.768	0.879
Tuition aid	0.000	0.115	0.216	0.564	0.000	0.118	0.221	0.565

Note: [a] the variable $Wage$ is omitted in the construction of Index 2. All other variables were used in the computation of both indeces. Computations use design weights.

Table 6: Bounds on the Average Treatment Effect of the EE Stratum's Log Wages and Indicators of Fringe Benefits in Quarter 16 after Randomization.

	Full Sample	Non-Hispanic Sample
Log wage		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[-0.005, 0.067]$	$[-0.006, 0.087]$
(95% IM confidence intervals)	$(-0.029, 0.090)$	$(-0.031, 0.108)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.027, 0.067]$	$[0.037, 0.087]$
(95% IM confidence intervals)	$(0.011, 0.090)$	$(0.020, 0.108)$
Health insurance		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.015, 0.049]$	$[0.018, 0.068]$
(95% IM confidence intervals)	$(-0.008, 0.073)$	$(-0.007, 0.097)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.029, 0.049]$	$[0.040, 0.068]$
(95% IM confidence intervals)	$(0.010, 0.073)$	$(0.017, 0.097)$
Paid vacation		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.006, 0.040]$	$[0.005, 0.053]$
(95% IM confidence intervals)	$(-0.015, 0.063)$	$(-0.020, 0.081)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.019, 0.040]$	$[0.023, 0.053]$
(95% IM confidence intervals)	$(0.000, 0.063)$	$(0.000, 0.081)$
Retirement or pension benefits		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.027, 0.062]$	$[0.025, 0.076]$
(95% IM confidence intervals)	$(0.003, 0.086)$	$(-0.003, 0.102)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.045, 0.062]$	$[0.051, 0.076]$
(95% IM confidence intervals)	$(0.025, 0.086)$	$(0.027, 0.102)$
Paid sick leave		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.012, 0.046]$	$[0.008, 0.058]$
(95% IM confidence intervals)	$(-0.011, 0.069)$	$(-0.018, 0.083)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.030, 0.046]$	$[0.034, 0.058]$
(95% IM confidence intervals)	$(0.010, 0.069)$	$(0.013, 0.083)$
Child care		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[-0.011, 0.022]$	$[-0.025, 0.022]$
(95% IM confidence intervals)	$(-0.036, 0.037)$	$(-0.052, 0.039)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.017, 0.022]$	$[0.015, 0.022]$
(95% IM confidence intervals)	$(0.002, 0.037)$	$(-0.001, 0.039)$
Flexible hours		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[-0.006, 0.025]$	$[-0.007, 0.039]$
(95% IM confidence intervals)	$(-0.028, 0.049)$	$(-0.031, 0.066)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.007, 0.025]$	$[0.013, 0.039]$
(95% IM confidence intervals)	$(-0.013, 0.049)$	$(-0.009, 0.066)$
Transportation		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[-0.018, 0.014]$	$[-0.034, 0.013]$
(95% IM confidence intervals)	$(-0.042, 0.030)$	$(-0.062, 0.031)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.008, 0.014]$	$[0.004, 0.013]$
(95% IM confidence intervals)	$(-0.007, 0.030)$	$(-0.013, 0.031)$
Dental plan		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.010, 0.045]$	$[0.013, 0.064]$
(95% IM confidence intervals)	$(-0.014, 0.069)$	$(-0.015, 0.092)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.027, 0.045]$	$[0.038, 0.064]$
(95% IM confidence intervals)	$(0.007, 0.069)$	$(0.014, 0.092)$
Tuition aid		
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.000, 0.032]$	$[-0.013, 0.035]$
(95% IM confidence intervals)	$(-0.025, 0.051)$	$(-0.040, 0.057)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.023, 0.032]$	$[0.022, 0.035]$
(95% IM confidence intervals)	$(0.005, 0.051)$	$(0.002, 0.057)$

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 7: Bounds on the Average Treatment Effect of the EE Stratum's Job Quality Indeces in Quarter 16 after Randomization.

	Full Sample		Non-Hispanic Sample	
Principal Strata				
Always employed, π_{EE}	0.639		0.631	
Never employed, π_{NN}	0.335		0.333	
Employed if treatment, π_{NE}	0.026		0.036	
	Index 1	Index 2	Index 1	Index 2
Bounds under Assumptions 1 and 2				
$[LB_{EE}, UB_{EE}]$	$[0.025, 0.147]$	$[0.022, 0.141]$	$[0.013, 0.179]$	$[0.009, 0.172]$
(95% IM confidence intervals)	$(-0.036, 0.203)$	$(-0.041, 0.196)$	$(-0.048, 0.248)$	$(-0.059, 0.235)$
Bounds adding Assumption 3				
$[LB_{EE}, UB_{EE}]$	$[0.092, 0.147]$	$[0.088, 0.141]$	$[0.103, 0.179]$	$[0.098, 0.172]$
(95% IM confidence intervals)	$(0.048, 0.203)$	$(0.043, 0.196)$	$(0.055, 0.248)$	$(0.050, 0.235)$

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: Analysis of Bounds for Average Treatment Effect on Job Quality Indeces of the EE Stratum, by Demographic Groups.

	Whites		**Blacks**		**Males**		**Females**	
Principal Strata								
Always employed, π_{EE}	0.715		0.586		0.652		0.606	
Never employed, π_{NN}	0.246		0.373		0.322		0.347	
Employed if treatment, π_{NE}	0.039		0.041		0.026		0.047	
	Index 1	Index 2	Index 1	Index 2	Index 1	Index 2	Index 1	Index 2
Bounds under Assumptions 1 and 2								
$[LB_{EE}, UB_{EE}]$	$[0.022, 0.186]$	$[0.017, 0.178]$	$[-0.002, 0.201]$	$[-0.006, 0.192]$	$[0.005, 0.122]$	$[0.000, 0.116]$	$[0.030, 0.254]$	$[0.026, 0.246]$
(95% IM confidence intervals)	$(-0.087, 0.286)$	$(-0.093, 0.277)$	$(-0.093, 0.286)$	$(-0.096, 0.277)$	$(-0.076, 0.201)$	$(-0.079, 0.192)$	$(-0.073, 0.355)$	$(-0.077, 0.346)$
Bounds adding Assumption 3								
$[LB_{EE}, UB_{EE}]$	$[0.113, 0.186]$	$[0.107, 0.178]$	$[0.105, 0.201]$	$[0.100, 0.192]$	$[0.068, 0.122]$	$[0.064, 0.116]$	$[0.149, 0.254]$	$[0.145, 0.246]$
(95% IM confidence intervals)	$(0.030, 0.286)$	$(0.026, 0.277)$	$(0.042, 0.286)$	$(0.037, 0.277)$	$(0.005, 0.201)$	$(0.003, 0.192)$	$(0.075, 0.355)$	$(0.071, 0.346)$

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

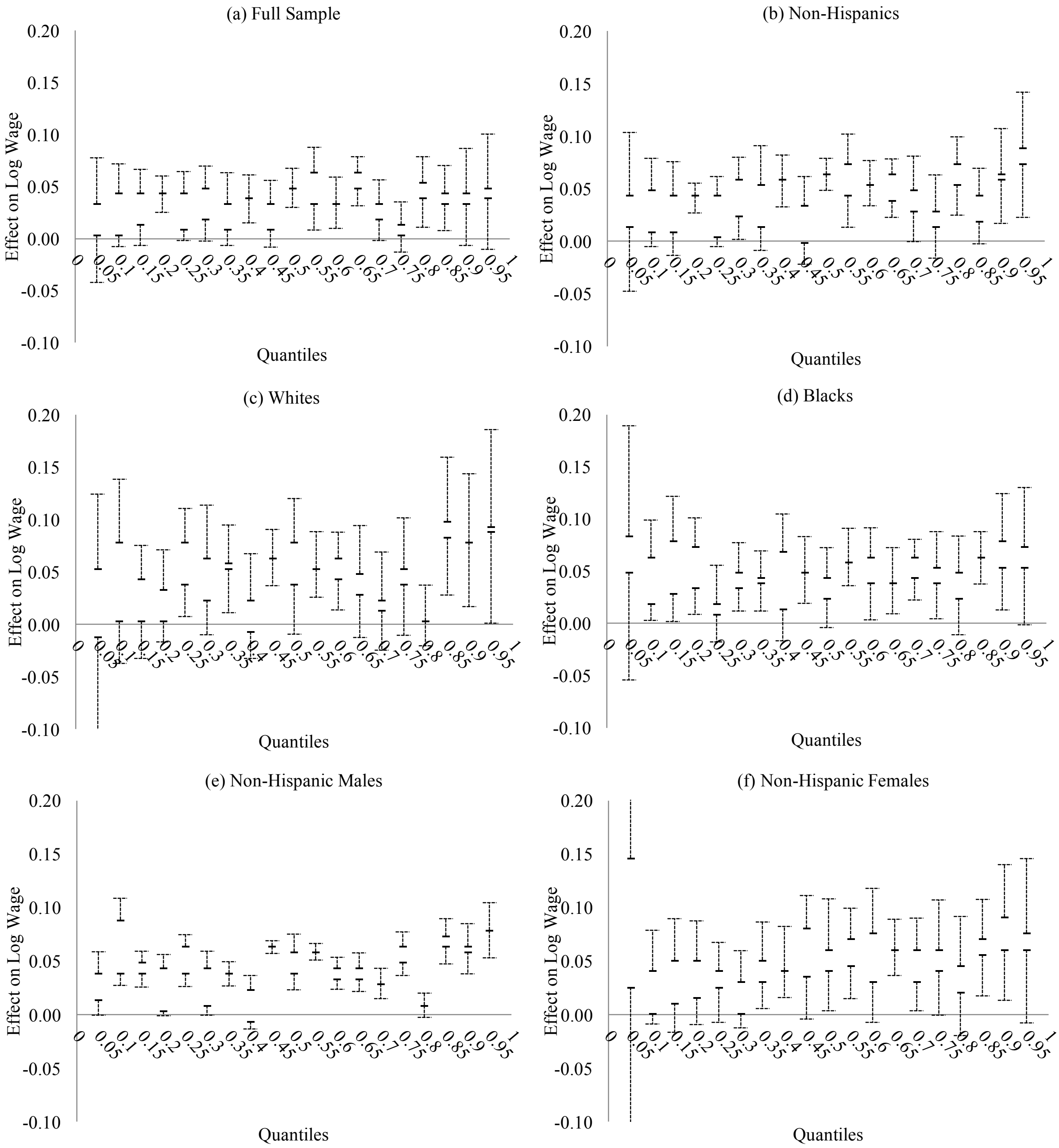

Figure 1. Bounds and 95 percent Imbens and Manski (2004) confidence intervals for QTE on the log of wages of the EE stratum by demographic groups, under Assumptions 1, 2 and 4. Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical lines.

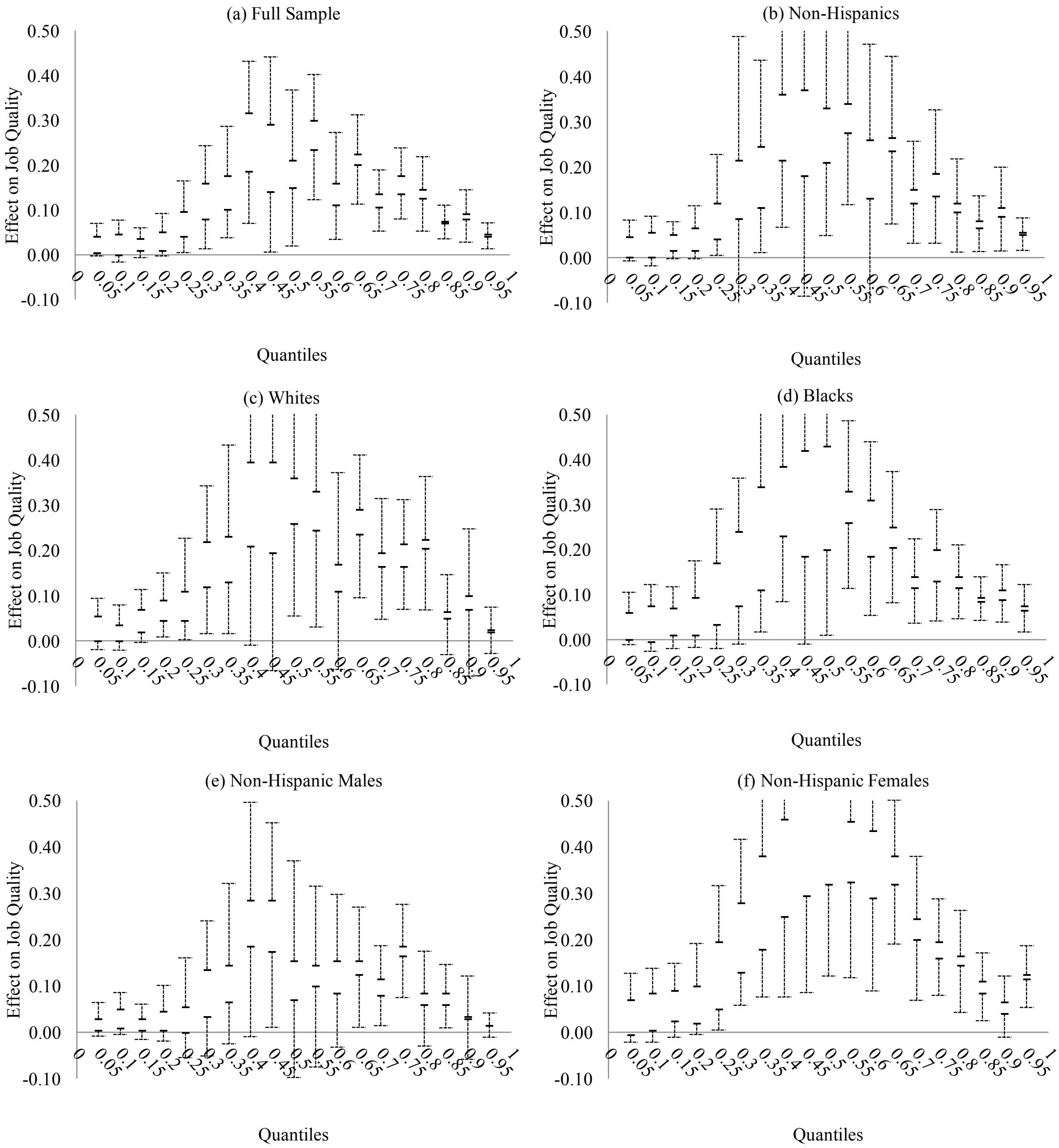

Figure 2. Bounds and 95 percent Imbens and Manski (2004) confidence intervals for QTE on job quality Index 1 of the EE stratum by demographic groups, under Assumptions 1, 2 and 4. Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical lines.

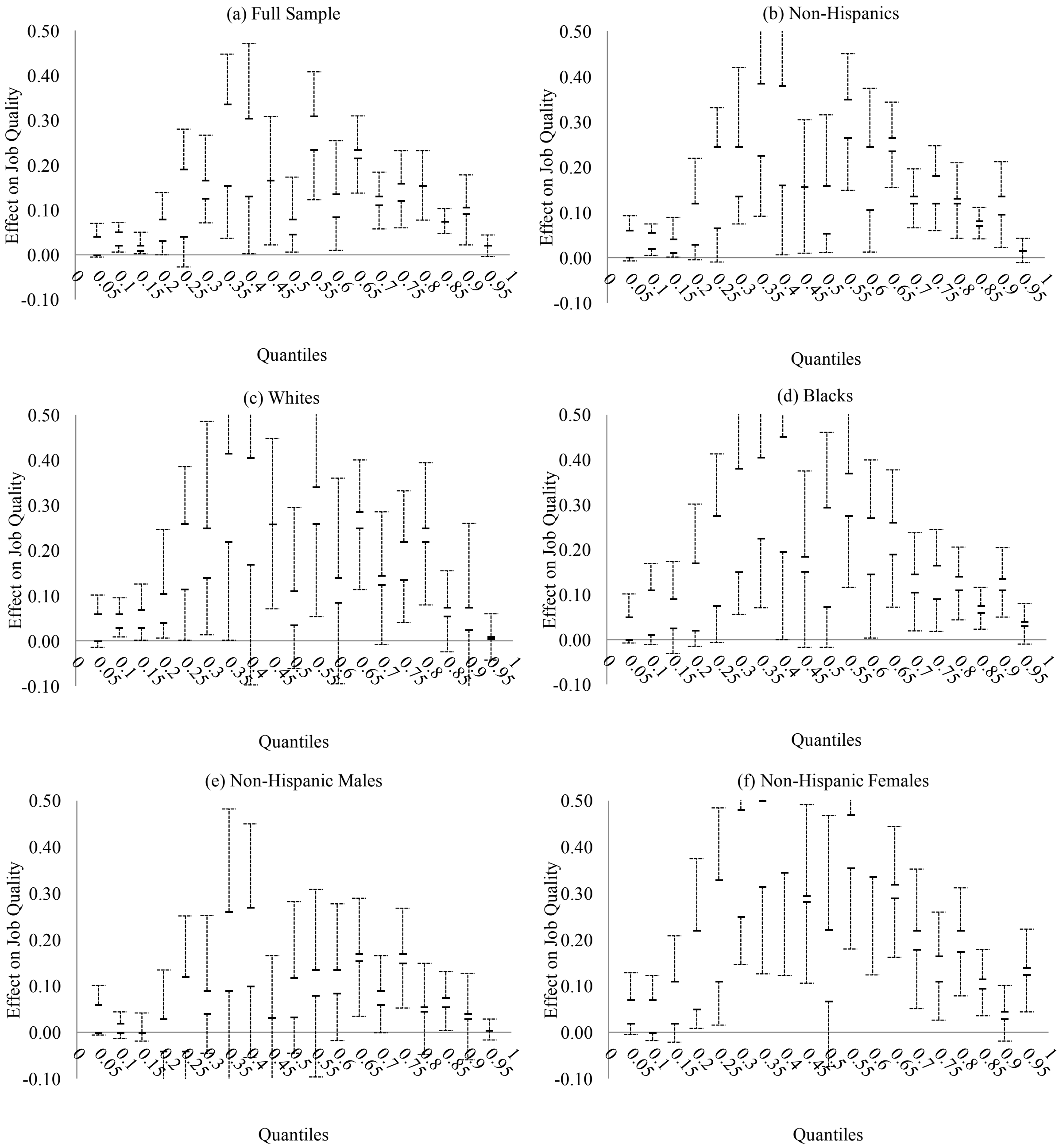

Figure 3. Bounds and 95 percent Imbens and Manski (2004) confidence intervals for QTE on job quality Index 2 (without wages) of the EE stratum by demographic groups, under Assumptions 1, 2 and 4. Upper and lower bounds are denoted by a short dash, while IM confidence intervals are denoted by a long dash at the end of the dashed vertical lines.

Appendix

Table A1: Estimated Principal Strata for the Analysis of Bounds on the Average Treatment Effect of the EE Stratum's Log Wages and Indicators of Fringe Benefits in Quarter 16 after Randomization by Demographic group.

Principal Strata	Full Sample	Non-Hispanic	Whites	Blacks	Males	Females
Log wage						
Always employed, π_{EE}	0.684	0.677	0.760	0.631	0.697	0.652
Never employed, π_{NN}	0.294	0.291	0.208	0.336	0.280	0.307
Employed if treatment, π_{NE}	0.022	0.032	0.032	0.033	0.023	0.042
Health insurance						
Always employed, π_{EE}	0.671	0.663	0.747	0.615	0.683	0.639
Never employed, π_{NN}	0.306	0.304	0.221	0.346	0.292	0.320
Employed if treatment, π_{NE}	0.023	0.033	0.032	0.039	0.026	0.041
Paid vacation						
Always employed, π_{EE}	0.682	0.675	0.758	0.628	0.695	0.651
Never employed, π_{NN}	0.295	0.292	0.209	0.337	0.281	0.308
Employed if treatment, π_{NE}	0.023	0.032	0.033	0.035	0.025	0.041
Retirement or pension benefits						
Always employed, π_{EE}	0.664	0.656	0.742	0.607	0.678	0.627
Never employed, π_{NN}	0.313	0.312	0.227	0.354	0.299	0.329
Employed if treatment, π_{NE}	0.023	0.033	0.031	0.039	0.023	0.044
Paid sick leave						
Always employed, π_{EE}	0.680	0.673	0.754	0.627	0.693	0.647
Never employed, π_{NN}	0.297	0.294	0.211	0.339	0.283	0.310
Employed if treatment, π_{NE}	0.023	0.033	0.036	0.034	0.024	0.043
Child care						
Always employed, π_{EE}	0.675	0.668	0.750	0.623	0.687	0.645
Never employed, π_{NN}	0.303	0.301	0.218	0.344	0.291	0.313
Employed if treatment, π_{NE}	0.022	0.031	0.032	0.033	0.022	0.042
Flexible hours						
Always employed, π_{EE}	0.685	0.677	0.760	0.631	0.697	0.653
Never employed, π_{NN}	0.294	0.292	0.209	0.336	0.280	0.308
Employed if treatment, π_{NE}	0.021	0.031	0.031	0.033	0.023	0.040
Transportation						
Always employed, π_{EE}	0.685	0.677	0.760	0.631	0.697	0.653
Never employed, π_{NN}	0.293	0.291	0.208	0.335	0.280	0.306
Employed if treatment, π_{NE}	0.022	0.032	0.032	0.034	0.024	0.041
Dental plan						
Always employed, π_{EE}	0.670	0.662	0.746	0.613	0.681	0.637
Never employed, π_{NN}	0.307	0.305	0.222	0.347	0.292	0.321
Employed if treatment, π_{NE}	0.023	0.034	0.032	0.040	0.026	0.042
Tuition aid						
Always employed, π_{EE}	0.680	0.672	0.755	0.626	0.691	0.647
Never employed, π_{NN}	0.299	0.296	0.210	0.343	0.285	0.312
Employed if treatment, π_{NE}	0.021	0.032	0.035	0.032	0.024	0.041

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 A2: Bounds on the Average Treatment Effect of the EE Stratum's Log Wages and Indicators of Fringe Benefits in Quarter 16 after Randomization by Demographic group.

	Whites	Blacks	Males	Females
Log wage				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[-0.005, 0.091]$	$[0.001, 0.088]$	$[0.001, 0.078]$	$[-0.012, 0.095]$
(95% IM confidence intervals):	$(-0.044, 0.129)$	$(-0.031, 0.116)$	$(-0.029, 0.106)$	$(-0.051, 0.130)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.038, 0.091]$	$[0.042, 0.088]$	$[0.037, 0.078]$	$[0.036, 0.095]$
(95% IM confidence intervals):	$(0.007, 0.129)$	$(0.019, 0.116)$	$(0.015, 0.106)$	$(0.008, 0.130)$
Health insurance				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.031, 0.074]$	$[0.012, 0.076]$	$[0.017, 0.055]$	$[0.018, 0.083]$
(95% IM confidence intervals):	$(-0.012, 0.118)$	$(-0.023, 0.114)$	$(-0.015, 0.090)$	$(-0.023, 0.128)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.049, 0.074]$	$[0.039, 0.076]$	$[0.033, 0.055]$	$[0.047, 0.083]$
(95% IM confidence intervals):	$(0.010, 0.118)$	$(0.009, 0.114)$	$(0.004, 0.090)$	$(0.011, 0.128)$
Paid vacation				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.033, 0.077]$	$[-0.005, 0.051]$	$[-0.008, 0.027]$	$[0.022, 0.086]$
(95% IM confidence intervals):	$(-0.005, 0.118)$	$(-0.039, 0.088)$	$(-0.039, 0.062)$	$(-0.018, 0.131)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.049, 0.077]$	$[0.016, 0.051]$	$[0.005, 0.027]$	$[0.046, 0.086]$
(95% IM confidence intervals):	$(0.014, 0.118)$	$(-0.014, 0.088)$	$(-0.023, 0.062)$	$(0.011, 0.131)$
Retirement or pension benefits				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.043, 0.085]$	$[0.019, 0.083]$	$[0.022, 0.057]$	$[0.029, 0.099]$
(95% IM confidence intervals):	$(-0.001, 0.129)$	$(-0.020, 0.120)$	$(-0.009, 0.091)$	$(-0.014, 0.141)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.064, 0.085]$	$[0.051, 0.083]$	$[0.039, 0.057]$	$[0.067, 0.099]$
(95% IM confidence intervals):	$(0.026, 0.129)$	$(0.020, 0.120)$	$(0.011, 0.091)$	$(0.031, 0.141)$
Paid sick leave				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.010, 0.058]$	$[0.012, 0.066]$	$[0.010, 0.045]$	$[0.007, 0.073]$
(95% IM confidence intervals):	$(-0.034, 0.099)$	$(-0.024, 0.102)$	$(-0.023, 0.077)$	$(-0.036, 0.115)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.036, 0.058]$	$[0.040, 0.066]$	$[0.028, 0.045]$	$[0.042, 0.073]$
(95% IM confidence intervals):	$(-0.002, 0.099)$	$(0.010, 0.102)$	$(0.000, 0.077)$	$(0.007, 0.115)$
Child care				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[-0.033, 0.010]$	$[-0.021, 0.032]$	$[-0.020, 0.012]$	$[-0.032, 0.033]$
(95% IM confidence intervals):	$(-0.075, 0.036)$	$(-0.058, 0.032)$	$(-0.052, 0.035)$	$(-0.078, 0.056)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.005, 0.010]$	$[0.023, 0.032]$	$[0.006, 0.012]$	$[0.025, 0.033]$
(95% IM confidence intervals):	$(-0.020, 0.036)$	$(0.000, 0.032)$	$(-0.015, 0.035)$	$(0.003, 0.056)$
Flexible hours				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.027, 0.069]$	$[-0.032, 0.021]$	$[-0.018, 0.016]$	$[0.012, 0.073]$
(95% IM confidence intervals):	$(-0.012, 0.109)$	$(-0.066, 0.058)$	$(-0.050, 0.048)$	$(-0.029, 0.117)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.045, 0.069]$	$[-0.010, 0.021]$	$[-0.003, 0.016]$	$[0.035, 0.073]$
(95% IM confidence intervals):	$(0.010, 0.109)$	$(-0.040, 0.058)$	$(-0.031, 0.048)$	$(-0.001, 0.117)$
Transportation				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[-0.019, 0.023]$	$[-0.047, 0.007]$	$[-0.016, 0.019]$	$[-0.063, 0.000]$
(95% IM confidence intervals):	$(-0.059, 0.054)$	$(-0.084, 0.031)$	$(-0.048, 0.044)$	$(-0.106, 0.023)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.014, 0.023]$	$[-0.003, 0.007]$	$[0.010, 0.019]$	$[-0.007, 0.000]$
(95% IM confidence intervals):	$(-0.015, 0.054)$	$(-0.025, 0.031)$	$(-0.014, 0.044)$	$(-0.029, 0.023)$
Dental plan				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[0.032, 0.075]$	$[0.007, 0.072]$	$[0.011, 0.050]$	$[0.015, 0.081]$
(95% IM confidence intervals):	$(-0.012, 0.116)$	$(-0.029, 0.108)$	$(-0.023, 0.084)$	$(-0.029, 0.125)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.054, 0.075]$	$[0.038, 0.072]$	$[0.030, 0.050]$	$[0.048, 0.081]$
(95% IM confidence intervals):	$(0.017, 0.116)$	$(0.008, 0.108)$	$(0.000, 0.084)$	$(0.011, 0.125)$
Tuition aid				
Bounds under Assumptions 1 and 2: $[LB_{EE}, UB_{EE}]$	$[-0.012, 0.035]$	$[-0.008, 0.043]$	$[-0.008, 0.027]$	$[-0.018, 0.046]$
(95% IM confidence intervals):	$(-0.055, 0.070)$	$(-0.046, 0.073)$	$(-0.042, 0.054)$	$(-0.063, 0.080)$
Bounds adding Assumption 3: $[LB_{EE}, UB_{EE}]$	$[0.022, 0.035]$	$[0.028, 0.043]$	$[0.017, 0.027]$	$[0.027, 0.046]$
(95% IM confidence intervals):	$(-0.013, 0.070)$	$(0.001, 0.073)$	$(-0.008, 0.054)$	$(-0.004, 0.080)$

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