Learning But Not Earning? The Value of Job Corps Training for Hispanic Youths[†]

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Abstract

The National Job Corps Study (NJCS) was a four-year longitudinal social experiment in which over 15,000 Job Corps eligible applicants were randomized into treatment and control groups. Using experimental estimators, Job Corps was found to have positive impacts in the weekly earnings of white and black youths 48 months after randomization, but not for Hispanic youths, a puzzling outcome that eluded explanation in the NJCS. This study considers explanations for why Job Corps does not increase the earnings of Hispanics in the NJCS. First, we show that the randomization in the NJCS did not create comparable treatment and control groups for Hispanics, possibly because Hispanics represent 20% of the entire randomized sample and are more concentrated in metropolitan areas. We then apply alternative estimators that adjust for systematic differences in pre-treatment observable and time-invariant unobservable characteristics of the Hispanic subsample, but still find statistically insignificant effects of Job Corps 48 months after randomization. Finally, we estimate the "net treatment effect" controlling for post-treatment experience to advance an explanation for why Job Corps *fails* to benefit Hispanics 48 months after randomization: non-treated Hispanics earn a significant amount of labor market experience during the study compared to treated Hispanics (and non-treated blacks and whites). This higher level of experience translates into higher earnings that Hispanic treated individuals are not able to overcome by the end of the study, despite having higher earnings growth in the 48-month period compared to whites and blacks (either treated or non-treated).

I. Introduction

It is well established that the lower than average educational attainment of Hispanics is the primary reason they earn less than non-Hispanic whites (Trejo, 1997; Gonzalez, 2002). Since only about half of Hispanics complete high school,[1] this lack of education more than likely prevents Hispanics from meeting the qualifications for many well-paying jobs with the potential for long-term earnings growth. The job prospects for young workers without a high school diploma or marketable skills are even bleaker. Young persons wishing to improve their human capital, but who have stopped their formal schooling, have limited options. It would seem that Hispanics and other disadvantaged youth would benefit from a job training program that provides educational and vocational training, provides job placement services, and also removes the adverse influence of the neighborhoods they live in. The Job Corps program differs from other job training programs because it targets youth trying to overcome these issues.

From the mid- to late-1990s, persons randomly selected into a Job Corps treatment and control group were tracked with a series of interviews 48 months after randomization. The National Job Corps Study (NJCS) evaluated the effectiveness of Job Corps across various dimensions, including employment and earnings, criminal behavior, and health outcomes.[2] With regards to earnings, the NJCS found that 48 months after randomization the treatment group earned a statistically significant 12% more than the control group (Burghardt, Schochet, McConnell, Johnson, Gritz, Glazerman, Homrighausen and Jackson, 2001). However, Hispanics in the treatment group earned 10% *less* (not statistically significant) than those in the control group 48 months after randomization.[3] In contrast, black and white treatment-group members

[1] Authors' calculations from the 2003 March Current Population Survey show that 56.4% of out-of-school Hispanics ages 16 and older graduated from high school, compared to 72.0% for non-Hispanics.
[2] The NJCS was sponsored by the Department of Labor in the late 1990s to assess the effectiveness and social value of the Job Corps program.
[3] This finding contrasts the findings of a previous Job Corps study concluding that in 1977 Hispanics that completed Job Corps had "significantly larger than average impacts" with regards to employment and earnings (Mallar, Kerachsky and Thornton, 1980, pg. 348).

experienced a statistically significant earnings increase of 14% and 24% with respect to their control group members. This was perhaps the most prominent "failure" of Job Corps and it could not be explained by individual and institutional variables, including the potential differences across Job Corps centers (Burghardt et al., 2001). Indeed, the NJCS found that Hispanics take advantage of their enrollment: Hispanics participated for a slightly longer period of time than non-Hispanics, and completed vocational, academic and other programs at similar levels than non-Hispanics (Burghardt et al., 2001).

Since Hispanics represent a significant and growing proportion of the U.S. population, and simultaneously disproportionately exhibit disadvantaged characteristics, it is important to understand the reasons behind this lack of impact. The objective of this paper is to explore two possible explanations for the lack of earnings gain for Hispanics in Job Corps. The first considers the possibility that social experiments may yield biased programmatic effects if certain assumptions underlying the experimental estimator are violated (see, for instance, Heckman, LaLonde and Smith, 1999). We present evidence that the randomization justifies the NJCS experimental (differences-in-means) estimator for whites and blacks but not for Hispanics, as randomization generated Hispanic treatment and control groups that are not statistically comparable. However, using alternative methods that adjust for systematic differences in pre-treatment observable and time-invariant unobservable characteristics to estimate the effect of Job Corps on Hispanics does not reverse the lack of program effects.

Our analysis of the data reveals that Hispanics that did not receive Job Corps training accumulate a large amount of labor market experience during the time of the study, compared to Hispanics that receive Job Corps and whites and blacks who did or did not receive it. For this reason, our second explanation focuses on this observable post-randomization labor market outcome that differentiates Hispanics from other groups. Specifically, estimating the "net treatment difference" (Rosenbaum, 1984), our analysis suggests that labor market experience gained during the time of the study completely explains the lack of effects of Job Corps on Hispanics 48 months after randomization.

Most labor market program evaluations in the literature generally focus on individuals that have been in the labor market for some time, sometimes explicitly avoiding the inclusion of youths (e.g., Heckman, Ichimura, Smith and Todd, 1998; Mueser, Troske and Gorislavsky, 2004), or do separate analyses for adults and youths (e.g., Heckman, Hohmann, Smith and Khoo, 2000; Heckman and Smith, 1999). This study adds to this literature by focusing on persons 16-24 (those eligible for Job Corps) and showing the importance of early labor market dynamics across ethnic groups. Specifically, since the majority of individuals do not have any (or very little) labor market experience, the actual labor market experience gained during the 48-month period of the study has a potential large impact on the earnings. This is exactly what we find in our data for Hispanics. This dynamic is particularly important in evaluating a program: the impact of a program will most likely take a longer time to realize since youths in the control group will accumulate more experience on average due to individuals in the treatment group spending time in the program. In other words, if early labor market experience is important, the effects of training programs on some youth may be noticed only in the medium- to long-term. We provide some informal evidence below that this might be the case for Hispanics in Job Corps.

Our paper is organized as follows. Section II describes the Job Corps program, the National Job Corps study (NJCS), and provides evidence that randomization did not create comparable treatment and control groups for Hispanics. Section III describes the restricted NJCS sample we use to obtain our alternative estimators of the average treatment effect on the treated

(*ATT*) that do not rely on the validity of randomization. These results are qualitatively similar to those of experimental estimators used in the NJCS, providing further credence to the lack of effects of Job Corps for Hispanics. In section IV we motivate and illustrate our main explanation for the lack of effects on Hispanics: the role of actual labor market experience gained during the follow-up period of the NJCS. Section V concludes and points to directions for future research.

II. The National Job Corps Study and the Randomization of Hispanics

A. The Job Corps Program

The Job Corps was created in 1964 as part of the War on Poverty under the Economic Opportunity Act, and has served over 2 million young persons ages 16-24. Currently, the Congressional mandate for Job Corps is derived from the Workforce Investment Act (WIA) of 1988 and administered by the Department of Labor's Employment and Training Administration.[4] The purpose of Job Corps is to provide low-skilled and less-educated young people with marketable skills to enhance their labor market outcomes. It does this by offering academic, vocational, and social skills training at over 115 centers throughout the country where nearly all students reside during training. The residential centers, which distinguish Job Corps from other job training and education programs, are run by private and not-for-profit groups or by the U.S. Department of Agriculture under contract with the Department of Labor. In addition to education and vocational training, Job Corps also provides health services and a stipend during program enrollment (Department of Labor, 1999; Schochet, Burghardt and Glazerman, 2001). Approximately 70,000 new students participate every year at a cost of about $1 billion, and the typical Job Corps student is a minority (70% of all students), 18 years of age, who has dropped out of high school (80%) and reads at a seventh grade level (Department of Labor, 1999).

[4] Job Corps operated under the Job Training Partnership Act from 1982 to July 2000, when it was replaced by Title I of the 1998 WIA.

Students are selected based on several criteria, including age (16-24), poverty status, residence in a disruptive environment, not on parole, being a high school dropout or in need of additional training or education, and citizen or permanent resident (Department of Labor, 1999; Schochet et al., 2001). Job Corps applicants become familiar with the program in various ways. The most important way is by word of mouth, with approximately 2/3 of applicants hearing about it from either friends or relatives (Schochet, 1998). Another 20% find out about Job Corps through direct mailings or from radio and television. The motivation for applying to Job Corps varies with age. In particular, the younger the applicant, the more likely he or she is interested in completing high school or GED degrees. Older applicants are less interested in general training, and instead want job training. Above all, they see Job Corps training as a means of finding employment since the majority has never held a full-time job (Schochet, 1998).

B. The National Job Corps Study

The data collected and used for this paper come from the National Job Corps Study (NJCS), a randomized experiment carried out during the mid- to late-1990s. The sampling frame for the NJCS consisted of first-time Job Corps applicants from nearly all outreach and admissions (OA) agencies—which are responsible for the recruitment and screening for Job Corps—in the 48 contiguous states and the District of Columbia.[5] Since all Job Corps training centers open in 1995 were part of the study, the NJCS (and the data used there) is based on a fully national sample. All pre-screened applications from November 1994 through December 1995 were forwarded by the OA agencies for random assignment. Each of the 80,833 eligible applicants were randomly assigned into control, program research (treatment), and program non-research groups during the sample intake period, between November 1994 through February

[5] The OA centers may include Job Corps centers, but they are also state employment agencies, profit and non-profit firms (Burghardt, McConnell, Meckstroth, Schochet, Johnson and Homrighausen, 1999).

1996. Approximately 7% of the eligible applicants was assigned to the control group (N = 5,977) while 12% was assigned to the program research group (N = 9,409). The remaining 65,497 eligible applicants were randomly assigned to a group permitted to enroll in Job Corps but were not part of the research sample.

Randomization took place *before* assignment to a Job Corps center. As a result, not all of those randomized into the research treatment group enrolled in Job Corps (73% of the treatment group enrolled in Job Corps). Meanwhile control group members were barred from enrolling in Job Corps for a period of three years. They could, however, enroll in other programs, some of which also offer job training and vocational opportunities which might be similar in nature or content as some of the Job Corps training. The control and treatment groups were tracked with a series of interviews immediately after randomization and continuing 12, 30, and 48 months after randomization. The outcomes at these points in time are the basis for the evaluation of Job Corps.

The implementation of the randomization was left to Job Corps staff and monitored by a process analysis using telephone surveys of Job Corps outreach and admission counselors, a mail survey of all Job Corps Centers, and visits to 23 centers (Johnson, Gritz, Jackson, Burghardt, Boussy, Leonard and Orians, 1999). The analysis indicated that randomization was successfully implemented for the most part. Less than 0.6% of eligible applicants were not assigned to their randomly selected groups. Furthermore, only 1.4% of control group members enrolled in Job Corps before the three-year embargo period had elapsed. The process analysis also concluded that the study should have had at most a modest effect on the program itself. For example, randomization had little effect on the activities of outreach counselors or the composition of youth interested in the program. Counselors also did not appear to devote any more time to finding alternative training for those assigned to the control group.

C. The NJCS Experimental Estimator and Findings

The original NJCS program evaluation is mostly based on a differences-in-means (or cross-section) estimator, modified to account for non-compliance: individuals in the treatment group who never enroll in Job Corps, and individuals in the control group that enroll in Job Corps before the three-year embargo (Schochet, 2001).

The randomization involved in the NJCS is used to justify the required assumption that, on average, individuals in the control group have the same treatment outcomes as those in the treatment group, if permitted to enroll, thus identifying the average treatment effect on the treated (*ATT*).[6] More specifically, let R_i be a binary variable indicating whether an eligible Job Corps applicant is randomly permitted to enroll in the program (R_i = 1) or prevented from enrolling (R_i = 0). Yet assignment to the treatment group ($R_i = 1$) does not rule out non-participation in Job Corps (D_i = 0) and vice-versa.[7] Therefore, the differences-in-means estimator employed in the original NJCS is modified by dividing it by the proportion of those individuals in the treatment group who enroll in Job Corps (P_{part}) minus the proportion of those individuals in the control group that enroll in Job Corps before the end of the three-year embargo (P_{cross}). Using this estimator, the effect of Job Corps on the "compliers" is:[8]

$$DM_{comp} = \frac{\overline{Y}(1)_{16} - \overline{Y}(0)_{16}}{P_{part} - P_{cross}}. \tag{1}$$

[6] The estimators we employ in this paper identify the average treatment effect on the treated (under different assumptions): $ATT = E[Y_i(1) - Y_i(0) \mid D_i = 1]$, where D_i is a binary variable that indicates whether the individual receives Job Corps training or not. For further details about the different estimators and their assumptions, see Heckman, LaLonde and Smith (1999).

[7] Approximately 27% of individuals in the treatment group (R = 1) never enrolled in Job Corps; while 1.4% of control group members (R = 0) receive training from Job Corps.

[8] This estimator assumes that the mean effect of Job Corps training is the same on those receiving it in both the treatment and control groups. Formally, $E[Y_i(1) \mid R_i = 1, D_i = 1] = E[Y_i(1) \mid R_i = 0, D_i = 1]$. We also note that this estimator is the local average treatment effect of Imbens and Angrist (1994) when the instrumental variable used is random assignment (R_i).

where $\bar{Y}(1)_{16}$ is the sample average of weekly earnings for individuals in the treatment group (*R* = 1) in quarter 16 and $\bar{Y}(0)_{16}$ is the sample average of weekly earnings for individuals in the control group (*R* = 0) in quarter 16.

The first row of Table 1 reports the original NJCS estimator. All the NJCS estimates for the entire sample are based on average weekly earnings in quarter 16; however, the estimates by race and ethnic group in the NJCS report employ average weekly earnings in year 4. Throughout this paper, we employ earnings in quarter 16 as our measure, but for comparison with the NJCS estimates by race, we also present in this table experimental estimators that use the average weekly earnings in year 4.[9] The NJCS estimates imply an overall gain of $22.1 per week, although it is not uniform across demographic groups: whites and blacks gain $46.2 and $22.8 per week, respectively, both statistically significant, while Hispanics show a statistically insignificant loss of $15.1. In the next section, we compare the NJCS estimates with similar estimates obtained using alternative estimators.

[TABLE 1 HERE]

D. Analysis of the Randomization

One of the main reasons why social experiments are employed is the notion that, because of randomization, the treatment and control group have the same distribution of observed and unobserved characteristics, and this allows the direct comparison between both groups (Smith, 2000).[10] Burghardt et al. (1999) describe the randomization design employed in the (NJCS),

[9] In the 48-month follow-up interview after randomization, respondents are asked about their weekly earnings during the 4th year after randomization as well as their weekly earnings during quarter 16 after randomization.

[10] Some other important assumptions are needed, such as the absence of an effect of randomization on the impact of participation. See Heckman, LaLonde and Smith (1999).

which was undertaken on the entire sample of Job Corps eligible applicants, without particular consideration for race or ethnicity.

1. Valid Control and Treatment Groups?

Given that the original randomization was applied to the entire sample and not explicitly to the different race and ethnic groups, it is an open question whether randomization created comparable experimental groups for them. The top panel of Table 2 takes the overall sample that responded to the baseline interview applied immediately after randomization, breaks it down by race/ethnicity and experimental group status (control or treatment), and compares their observable average characteristics. An important characteristic of the sampling design in the NJCS is that the sampling rate for females in the control group, who had a high likelihood of being residential students, was set lower because Job Corps officials were concerned that the study would cause slots for residential females to go unfilled given that they are difficult to recruit (Burghardt et al., 1999). For this reason, the figures in Table 2 and the original NJCS experimental estimator in the first row of Table 1 use sampling weights.

[TABLE 2 HERE]

The Hispanic control and treatment groups show more statistically significant differences in mean characteristics than any of the other two groups, which can be a result of representing only 20% of the entire randomized population and at the same time residing in large cities. They exhibit differences in the percentage of females, number of children, percent living in a PMSA, percent living in a MSA, percent unemployed at randomization, and percent employed at randomization. The other two groups show only two differences each that are barely statistically significant (z-statistic less than 1.86): number of children and average weekly pre-treatment earnings for whites, and age and percent who speak English as a native language for blacks. We regard this as evidence that the validity of randomization for Hispanics is doubtful, which would justify the use of alternative methods to try disentangling the reasons why Hispanics show no effects from Job Corps. We do this in section III below.

2. Are Hispanics Learning?

The bottom panel of Table 2 present means for selected variables at the end of the study, that is, the 48-month interview. The main conclusion to be drawn from this panel is that Hispanics in the treatment group attain degrees and diplomas from training in a similar rate as whites and blacks, implying that a lack of achievement is not the reason for the lack of experimentally estimated effects of Job Corps on Hispanics at the 48-month after randomization.

Rows 3 through 6 in the second panel of Table 2 show that Hispanics have a pattern of degree attainment very similar to that of whites and blacks. For all three groups, individuals have essentially the same highest grade completed across treatment and control groups, while individuals in the control group have a higher rate of high school completion relative to individuals in the treatment group. This is explained by the fact that Job Corps tend to steer participants toward the completion of GED and/or vocational diploma, resulting in individuals in the treatment group completing such degrees at a significantly higher rate than control-group individuals. Importantly, the rate of completion of those degrees by Hispanics is very similar to that of whites and blacks for both treatment and control groups.

Finally, rows 7 through 9 show some variables for which Hispanics significantly differ from whites and blacks. At the 48-month interview, Hispanics in the treatment and control groups have essentially the same rates of employment and weekly earnings in quarter 16, while whites and blacks in the treatment group have statistically significant gains in these two outcome variables. The last row shows one factor that we explore in detail below: only for Hispanics is it the case that there is a statistically significant difference in experience accumulated during the time of the study between control and treatment groups.

3. Some Consequences of the Randomization for Hispanics.

Probably the most labor-marker relevant dimension along which Hispanic treatment and control groups differ at baseline interview is the type of city of residence (PMSA, MSA or other). If this misalignment is responsible for the lack of experimentally-estimated effects of Job Corps on Hispanics, then we should expect to observe important differences for Hispanics but not for non-Hispanics across type of city. The original sample design randomly assigned 61% of the sample in the treatment group and 39% into the control group, so in order for randomization to be balanced for each group, we would expect these percentages to hold within each city type. Yet Table 3 shows that both the distribution and important outcomes of Hispanics vary by city type, while non-Hispanics generally have similar distribution and outcomes across city types. For example, the first row of the PMSA panel shows that treatment-group Hispanics are significantly under-represented in PMSAs (first panel) at 57%, which is statistically different than the expected 61% at the 10% level. At the same time, it is noteworthy that the distribution of white and black treatment and control group members is statistically consistent with the expected 61/39 ratio.

[TABLE 3 HERE]

The extent of difference in city-type distribution for Hispanics should be pointed out: nearly 90% of all Hispanics reside in either a PMSA or MSA, while blacks and whites are less likely to reside in the largest of cities (84 and 64 percent, respectively). The difference in geographic distribution can be summarized by the Duncan index of dissimilarity which, for each ethnic group *r*, is defined as

$$D_r = \frac{1}{2}\sum_{i=1}^{3}\left|c_{ri} - t_{ri}\right|, \tag{2}$$

where c_{ri} and t_{ri} is the proportion of control and treatment group members in city type *i*. This value is interpreted as the share of the treatment group that would need to move in order for both groups to have the same distribution across cities. As expected, Hispanic treatment and control group members are more unevenly distributed than whites or blacks, with 5.5% of treatment group Hispanics needing to move compared to 2.9 and 1.3 percent for whites and blacks, respectively. In particular, treatment-group Hispanics would need to move from MSAs into PMSAs (or Hispanic controls out of PMSAs and into MSAs).

Two variables that are particularly correlated with city type are quarter-16 average weekly earnings and the labor market experience accumulated during the study. Regarding earnings, Hispanics in the control group earn more in PMSAs and MSAs but only in PMSA is the difference statistically significant. In addition, there is a *positive* difference in mean earnings between treatment and controls groups for Hispanics residing in other areas, although not statistically significant, and the overall difference in mean earnings of -\$18.94 is significant at the 10% level. Table 3 also identifies one particular manner in which Hispanics differ from whites and blacks: like whites and (mostly) blacks, the within-assignment-group earnings is greater in PMSAs than MSAs and other areas; but unlike whites and blacks, the earnings difference between assignment groups in PMSAs is negative and statistically significant for Hispanics.

The pattern of differences in the accumulation of labor market experience during the study also differs by metropolitan area type. This measure of post-treatment experience is a measure available in the NJCS data:[11] during each of the baseline, 12, 30, and 48-month interviews study participants were asked how many hours they had worked each week since the last interview or since randomization in the case of the baseline interview. Using this information, the post-treatment experience variable available in the NJCS data is defined as the sum of all hours worked during the entire study divided by 208 weeks. This measure does not exclude periods "away" from the labor market such as Job Corps or other training programs, and each week's potential hours worked are top coded at 84 hours per week.

Table 3 shows that the greatest difference in experience acquired during the time of the study is found between Hispanic treatment and control groups: *within* cities, treatment-group Hispanics have 1.8 to 3.8 fewer hours of experience, all statistically significant at least at the 10% level. The corresponding difference for blacks and whites is only greater than 1 hour for whites in PMSA but never is any difference statistically significant. There is evidence, then, that the misalignment of some covariates might explain the lack of estimated effects of Job Corps on Hispanics.

III. Evidence from Alternative Estimators

In this section we present evidence on the effect of Job Corps on Hispanics using alternative estimators that adjust for systematic differences in pre-treatment observable and time-invariant unobservable characteristics. The main conclusion is that the lack of effect of Job Corps on Hispanics seems to be robust to the method used to construct a counterfactual, suggesting that there are no discernible effects of Job Corps on Hispanics 48 months after randomization. For reference, we also report results for whites and blacks using these alternative estimators.

A. The Restricted Sample

[11] We refer to this measure of experience "post-treatment" experience, although it is more accurately "post-randomization" experience.

The alternative estimators separate the sample into those who enroll in Job Corps training ("treated" individuals) and those that do not ("non-treated" individuals).[12] In implementing these estimators we employ standard covariates that have been found to be important in evaluating training programs. The pre-treatment covariates included in our baseline specification are: age, gender, whether the individual had a high school diploma or GED, speaks English as native language, is married, is household head, has children, has a vocational degree, has been convicted, his/her pre-treatment weekly earnings, employment status, and dummy variables for residence in PMSA or MSA. [13] Controlling for area of residence at the time of the baseline interview potentially accounts for the observed pre-treatment differences for Hispanics that may be correlated with unobserved labor market conditions in a particular type of metropolitan area.

In order to employ the alternative methods that control for pre-treatment observable variables to estimate the effect of Job Corps on Hispanics 48 months after randomization, we need to restrict the original 48-month NJCS sample of 11,313 individuals by dropping 219 individuals that do not complete the baseline interview. Furthermore, we exclude 1,295 individuals for which any of the variables we use in obtaining alternative estimators are not available, arriving to a "restricted sample" of 9,105 individuals.[14] It is worth noting that the individuals excluded are proportionately distributed across race/ethnic groups.

Table A.1 in the appendix features means of selected variables of interest in our restricted sample for treated and non-treated individuals by race/ethnicity. Importantly, our restricted sample is consistent with the overall profile of the total Job Corps population: the average Job Corps youth at the time of application is around 18.8 years old, non-white (72% are non-white),

[12] This is in contrast to the experimental estimator used in the NJCS, which separates the sample into those randomly assigned to the treatment research group and those randomly assigned to the control group.
[13] Measures of labor market experience *before* randomization are unavailable in the NJCS data.
[14] This last figure includes an additional 694 observations that are lost since their race/ethnicity is not white, black or Hispanic.

male (about 43% are female), and with 10 years of schooling. In all, Hispanics comprise over 18% of our restricted sample.

To gauge the extent to which our restricted sample is consistent with the NJCS results, Table 1 presents and compares different estimates of DM_{comp} in (1) with the original NJCS estimator (Schochet et al., 2001). Before proceeding with our restricted sample, the second row successfully replicates the NJCS results employing a sample of individuals who complete the 48-month follow-up survey. These estimates can be used to gauge the effect of employing weekly earnings in quarter 16 as our outcome measure as opposed to weekly earnings in year 4 used by the NJCS in the results by race/ethnicity. Using weekly earnings in quarter 16, the DM_{comp} estimate for the overall sample is higher by 14%, but for whites it is larger by about 25%, 8% for blacks, and for Hispanics it is larger in absolute value by about 56%. Therefore, we obtain larger (in absolute value) estimates when we employ weekly earnings in quarter 16, which results in conservative estimates for Hispanics of the effect of Job Corps, but not for the other two racial groups.

When we obtain DM_{comp} using our restricted sample, the estimates diverge somewhat from those in the NJCS report in quantitative terms, but not qualitatively. Looking at the third row of Table 1 and using weekly earnings in year 4, the overall gain of \$18.7 to enrolling in Job Corps masks the fact that whites average nearly twice as much (\$37.8), blacks gain \$24.1 more per week, and Hispanics do not experience any programmatic gain (a statistically insignificant \$16.8 loss). Compared to the original NJCS estimator, these returns to Job Corps are lower for whites, while for Hispanics and blacks the difference is small (less than \$2). However, by switching to weekly earnings in quarter 16, our restricted sample DM_{comp} is virtually the same as the NJCS estimator for whites, 20% higher for blacks, while it yields significantly lower returns for Hispanics (78% lower returns and marginally statistically insignificant). Thus, it is likely that the differences that arise from using our restricted sample and weekly earnings in quarter 16 result in conservative estimates of the impact of Job Corps for Hispanics, but not necessarily for whites and blacks. Given that our main interest lies in explaining the lack of effects on Hispanics, we feel comfortable using the present restricted sample that yields conservative estimates for this group.

B. Econometric Framework and Alternative Estimators

We employ the potential outcomes framework to describe the alternative estimators we use in evaluating the effect of Job Corps. Let Y_i be the outcome of interest for individual *i*, while $Y_i(1)$ and $Y_i(0)$ denote the *potential* outcome if the individual receives training in Job Corps or not, respectively. For individual *i*, the effect of receiving Job Corps training on the outcome of interest is $Y_i(1) - Y_i(0)$. However, we only observe one outcome depending on whether the individual receives Job Corps training or not. This is a missing data problem, and the alternative estimators we employ to estimate the effects of Job Corps will make different assumptions to construct the appropriate counterfactual.

The alternative estimators we consider are the bias-corrected simple matching estimator (*BCSME*), the propensity score (*PSCORE*) estimator, and the differences-in-differences (*DID*) estimator. Both *BCSME* and *PSCORE* are matching estimators, and for them we also consider a differences-in-differences matching strategy (Heckman et al., 1998).

The recently proposed *BCSME* of Abadie and Imbens (forthcoming) is easy to implement, and has desirable large-sample ($\sqrt{N}$ -consistent) and good finite sample properties compared to other matching estimators available.[15] The crucial assumption behind matching estimators, including the *BCSME*, is that conditional on the observed variables upon which the match is undertaken, reception of training is independent of the outcome variable. In other words, conditional on *X*, D is independent of the potential outcomes, $(Y(0), Y(1))$.[16] Under these assumptions the *ATT* can be identified. Intuitively, the *BCSME* takes each individual that enrolled in Job Corps and finds matched individuals that did not enrolled in Job Corps that are closest in terms of the set of observable characteristics considered. In this way, the effect of Job Corps for each individual that enrolled is estimated using the matched individuals' weekly earnings as the counterfactual. Finally, to obtain the estimated *ATT*, the average of all the individually estimated effects of Job Corps is computed.

Formally, for D_i defined as before, we observe one outcome for each treated individual in the sample, $Y_i(1)$, and the set of pre-training covariates *X*. Also, denote the conditional regression function for each $d \in \{0,1\}$ by:[17]

$$\mu_d(x) \equiv E\left[Y(d) \mid X = x\right] \tag{3}$$

and the set of indices of the *M* observations found to match a given individual *i* by $\Upsilon_M(i)$. For each treated observation *i*, the counterfactual to estimate *ATT* is obtained by averaging over its *M* matched observations, thus obtaining estimates of the potential outcome as follows:

$$\hat{Y}_i(0) = \frac{1}{M} \sum_{j \in \Upsilon_M(i)} Y_j \ . \tag{4}$$

[15] Abadie and Imbens (forthcoming) provide some Monte Carlo evidence about the finite-sample properties of the bias-corrected simple matching estimator.

[16] This assumption is known as "unconfoundedness" in the literature. In addition, it is also requires that the probability of receiving training conditional on *X* is bounded away form zero and one. Together, these two assumptions are known as "strong ignorability" (Rosenbaum and Rubin (1983)).

[17] The conditional regression function is assumed to be continuous in *x* for all *d*. Furthermore, Abadie and Imbens (forthcoming) assume that the conditional distribution of *Y* given *D* and *X* has finite fourth moments, and that the data (Y_i, D_i, X_i) are drawn independently from the population distribution.

Defining N_1 as the number of observations for which $D_i = 1$, the simple matching estimator for *ATT* is just: $\frac{1}{N_1}\sum_{i:D_i=1}^{N_1}(Y_i(1)-\hat{Y}_i(0))$. However, this simple matching estimator has been found to exhibit a bias term that does not vanish asymptotically (Abadie and Imbens, forthcoming). The *BCSME* removes that bias term by adjusting for the difference in covariate values within the matches, making the estimator $\sqrt{N}$ - consistent. In the *BCSME* the potential outcomes in (4) are estimated by:

$$\tilde{Y}_i(0) = \frac{1}{M}\sum_{l \in \Upsilon_M(i)}^{N}(Y_l + \hat{\mu}_0(X_i) - \hat{\mu}_0(X_l)), \tag{5}$$

where $\hat{\mu}_0(X)$ can be obtained using simple linear regression. The mean effect of Job Corps on participants (*ATT*) is:

$$BCSME_{ATT} = \frac{1}{N_1}\sum_{i:D_i=1}^{N}(Y_i(1)-\tilde{Y}_i(0)). \tag{6}$$

We implement this estimator using the 4 nearest matches to *i* in terms of the covariates *X*, where the norm employed is the Mahalanobis distance.[18]

One of the potential drawbacks of the *BCSME* estimator is that the quality of the matches worsens as the number of characteristics to match on grows, which could be a concern in our baseline specification that uses 14 pre-treatment variables to undertake the matching. The propensity score estimator (*PSCORE*), originally proposed by Rosenbaum and Rubin (1983) allows matching on a richer set of variables than the *BCSME* estimator. They show that it is possible to match on the estimated probability of participation (the propensity score), avoiding in this way the dimensionality problem of using a large vector of covariates. Similar to the *BCSME*,

[18] Abadie and Imbens (forthcoming) present some simulation evidence supporting the use of 4 matches. We obtain these estimators using the STATA codes described in Abadie, Drukker, Herr and Imbens (2002) obtaining standard errors that allow for heteroskedasticity.

the crucial assumption is that conditional on the propensity score, reception of training is independent of the potential outcomes, $(Y(0), Y(1))$ (see also footnote 16).

We estimate the effect of Job Corps 48 months after randomization using the *PSCORE* estimator based on a Gaussian kernel matching procedure.[19] The model specified for the propensity score is a probit model that includes the same variables used in *BCSME* plus their squares. While Abadie and Imbens (forthcoming) compare the performance of the *BCSME* estimator versus some *PSCORE* estimators in a prototypical dataset, we are unaware of any further comparisons in actual applications, and thus it is worthwhile to compare how the two estimators perform in our data.

Another estimator we consider is the difference-in-differences (*DID*) estimator, used to identify the *ATT* of Job Corps under the assumption that participation in training depends on a "fixed effect" that is invariant over time and can thus be differenced out. We obtain the *DID* estimator based on the following linear regression for both Job Corps participants ($D = 1$) and non-participants ($D = 0$):

$$(Y_{i16} - Y_{i0}) = \beta_0 + \beta_1^{'}(X_{i16} - X_{i0}) + \alpha D_i + (\varepsilon_{i16} - \varepsilon_{i0}) \tag{7}$$

where Y_{i0} and Y_{i16} are weekly earnings before randomization (quarter 0) and at quarter 16, respectively, $(X_{i16} - X_{i0})$ are the differenced covariates, $(\varepsilon_{i16} - \varepsilon_{i0})$ are the differenced error terms, and D_i is the binary variable indicating participation in Job Corps. The parameter α represents the *ATT* of the Job Corps program using the *DID* estimator.

Finally, if there are systematic differences in weekly earnings between treated and non-treated individuals after conditioning (matching) on observable characteristics that arise due to

[19] Other methods for matching based on the propensity score were tried (as well as using the Epanechnikov kernel), essentially obtaining the same results. These other matching methods are nearest neighbor, radius, and stratification matching, which are described in Becker and Ichino (2002). This lack of sensitivity to the method employed for matching has also been reported in Mueser et al. (2004) and Smith and Todd (2005). Dehejia

time-invariant factors, then employing a differences-in-differences matching strategy would be appropriate (Heckman et al., 1998). Operationally, this strategy implies estimating *BCSME* or *PSCORE* using the differences in pre- and post-treatment earnings between treated and non-treated individuals. Compared to the *DID* estimator employed above, differences-in-differences matching relaxes the linear functional form restriction implicit in (7).

C. Results from Alternative Estimators

The results of the alternative estimators are shown in Table 4. For comparison, column I in panel A presents the simple difference in average weekly earnings for treated and non-treated individuals. Consistent with the results of the experimental estimators presented in Table 1, whites ($19.1) and blacks ($15.1) that received Job Corps training have higher average weekly earnings that are statistically significant, and Hispanics (-$8.5) earn less but the effect is not statistically significant.

[TABLE 4 HERE]

The results from the *BCSME* are presented in column II of Table 4 (panel A). The estimated effect of Job Corps measured at the 48th month after randomization for Hispanics is small and positive ($4.5) although statistically insignificant. It is interesting to contrast this positive point estimate with the -$26.9 estimated using DM_{comp} on the experimental groups and the simple differences in means (-$8.5) in column I, implying that the covariate adjustment works in the expected direction, although of course all estimates are statistically insignificant. The estimated effects for whites and blacks are both positive and statistically significant ($21.7 and $17.1, respectively), and similar to the simple differences in means.

and Wahba (2002) find a similar robustness result of different *PSCORE* matching estimators, which they attribute to the validity of the support condition, which is also satisfied in our data (see Figure 1).

To gauge how good of a job the *BCSME* is doing in matching Hispanic treated and non-treated individuals with the same observable characteristics, Table 5 compares the difference in the observable covariates before and after the matching procedure is applied. From this exercise, we note that matching does a very good job aligning the observable characteristics of treated and non-treated individuals, with the exception of two variables that show a statistically significant difference after the matching procedure is implemented: age and number of children.

[TABLE 5 HERE]

The results for the *PSCORE* estimator are presented in column III of Table 4 (panel A). The estimated effect of Job Corps at the 48th month after randomization for Hispanics is -$5.1, which is statistically insignificant. The *PSCORE* estimate for whites and blacks are very similar to the *BCSME* estimates. Since the discrepancy in the estimated effects for Hispanics using both matching estimators is relatively small and statistically insignificant, the comparison between *BCSME* and *PSCORE* in our sample is consistent with the findings reported in Abadie and Imbens (forthcoming) about the relative performance of *BCSME* and *PSCORE*.

We undertake two exercises to check the specification of the *PSCORE* estimator. The first is to check the specification of the model for estimating the propensity score using the method proposed in Dehejia and Wahba (2002), which consists on stratifying the sample based on the estimated propensity score and testing, within strata, that the average propensity score and values of all the covariates are not statistically different between treated and non-treated individuals. Using Becker and Ichino's (2002) implementation of this method, the model used for the estimation of the propensity score satisfies this specification check at least at the 1% level in all instances.[20] The second exercise we conduct is to check that for all treated individuals there

[20] Table A.2 presents the probit-estimated coefficients of the propensity score model by race/ethnicity. All estimated coefficients have the expected signs.

are non-treated individuals in the sample with the same value of the estimated propensity score so that quality matches are possible; in other words, we check that the so-called "support condition" is satisfied. Figure 1 presents a histogram of the estimated propensity score for treated and non-treated Hispanics, where it is evident that there is almost a perfect overlap of the support for both groups.[21]

[FIGURE 1 HERE]

We present results for the effect of Job Corps at the 48th month after randomization on weekly earnings using the *DID* estimator in column I in panel B of Table 4. The estimated effects of Job Corps on Hispanics using *DID* shows a small loss of -\$3.0, which is statistically insignificant and similar to both matching estimators. The estimated effects for whites and blacks (\$20.6 and \$20.4, respectively) are also similar to those obtained using the matching estimators and are statistically significant. These results suggest that time invariant unobserved factors do not play a considerable role in the decision to participate in Job Corps, which is also confirmed by the differences-in-differences matching strategy employed next.

Lastly, in panel B in Table 4, columns II and III report the results of the differences-in-differences matching strategy using *BCSME* and *PSCORE*. The notable feature of these results is their similarity with the results based on the level of earnings in quarter 16, which again suggests that the role of unobserved time-invariant factors is negligible.

D. Summary of Alternative Estimators

The use of alternative methods to estimate the effect of Job Corps reveals that the same qualitative results as in the original NJCS report hold: the estimated effects of Job Corps at the 48th month after randomization are statistically insignificant for Hispanics, and positive and significant for whites and blacks. This implies that adjusting for systematic differences in pre-

[21] The histograms for the propensity score for whites and blacks are qualitatively similar to those for Hispanics.

treatment observable and time-invariant unobservable characteristics of the Hispanic subsample (including controlling for city type of residence) is not enough to reverse the lack of effects for Hispanics. [22] In the next section, we advance an explanation for the seemingly robust lack of effects of Job Corps on Hispanics 48-months after randomization.[23]

IV. Explaining Hispanic Outcomes

In this section we advance a plausible explanation for the observed lack of effects of Job Corps on Hispanics 48 months after randomization. An important conclusion is that low-income Hispanics, in the absence of Job Corps training, acquire labor market experience that puts them at an early earnings advantage that Job Corps trained Hispanics are not able to overcome during the 48-month period covered in the NJCS.

A. Labor Market Dynamics of Hispanics

Despite having nearly identical employment rates as treated Hispanics, non-treated Hispanics work a statistically significant 3.2 more hours *per week* than Job-Corps-trained Hispanics during the 208 weeks of the study (see the bottom panel of Table A.1 in the appendix). Furthermore, while the difference in length of workweek is also significant for blacks (but not for whites), the difference between treated and non-treated blacks is only 0.7 hours per week, a magnitude that would not be expected to result in a large accumulated differential between both black groups during the length of the study. In addition, although not statistically significant, only among Hispanics is the case that the non-treated group has *higher* employment rates than the treated group, a net difference of -1.0 percentage points between the treated and non-treated group, compared to +3.0 percentage points for treated whites and blacks. Clearly, the labor force

[22] We have previously considered other estimators such as instrumental variables and self-selection (heckit model). The conclusion reached here is unchanged using those estimators (Flores-Lagunes et al., 2004).

[23] A potential explanation for the lack of effect of Job Corps on Hispanics is that a proportion of Hispanic individuals that do not have English as their native language may not obtain valuable returns from Job Corps. We explored this issue by restricting the analysis to Hispanics that did not

dynamics affect low-income Hispanic youth differently than whites and blacks, and this is not explained by controlling for city type of residence. The different labor market dynamics for Hispanics is consistent with other findings in the literature documenting the large labor force participation and higher labor market attachment of Hispanics (Antecol and Bedard, 2004; Borjas, 1982; DeFreitas, 1991; Gonzalez, 2002; Trejo, 1997).

Table 3 showed that Hispanics differ from each other with regards to the type of city of residence and, in particular, by average hours of work per week during the 48-month study. Although we control for the type of city of residence in the alternative estimators of the previous section, this obviously does not account for the experience earned during the time of the study, as this variable is a post-treatment variable. Nevertheless, since the average labor market experience gained during the study differentiates Hispanic treated and non-treated groups so prominently relative to whites and blacks, this section examines if such experience accounts for the lack of estimated Job Corps-effect on earnings at the 48th month after randomization.

B. Can Post-Randomization Experience Explain the Lack of Effects for Hispanics?

To check whether this pervasive difference in the accumulation of labor market experience during the study between Hispanics treated and non-treated accounts for the lack of effects of Job Corps at the 48th month follow-up survey, we employ the matching estimators of the previous section controlling for the average hours of experience per week, in addition to the covariates contained in the baseline specification. This exercise is nonstandard given that the experience accumulated during the time of the study is a post-treatment variable and is most likely affected by the treatment. As a result, the set of estimates that control for post-treatment

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take ESL classes while in Job Corps. The negative and statistically insignificant effect of Job Corps still holds for this sample of non-ESL treated Hispanics.

experience are **not** interpreted as average treatment effects on the treated, but rather as the "net treatment difference" (*NTD*) (Rosenbaum, 1984).

Rosenbaum (1984) and, more recently, Imbens (2004) discuss controlling for post-treatment covariates when estimating treatment effects. In short, the *ATT* is no longer identified when controlling for post-treatment covariates that are influenced by the treatment (such as our measure of experience). Nevertheless, this approach can be used to gauge the extent to which controlling for experience during the study explains the previously estimated effects. In other words, we can use them to learn about the mechanism through which the treatment works, or in our case through which the treatment *fails* to work.

More formally, let $S_i(1)$ and $S_i(0)$ denote the *potential* observable values of a post-treatment variable, such as our measure of experience, if the individual receives training in Job Corps or not. Rosenbaum (1984) introduces a population parameter, $\tilde{v}$, called the net treatment difference (*NTD*). To derive the *NTD*, first define $v(x,s)$ as:

$$v(x,s) = E[Y(1) \mid S(1) = s, X = x] - E[Y(0) \mid S(0) = s, X = x]. \tag{8}$$

Then, the *NTD* is defined as the expectation over the distribution of $(X, S(D))$: $\tilde{v} = \mathrm{E}[v(X, S(D))]$. This parameter can be consistently estimated by controlling for post-treatment experience using any of the two matching estimators above under an expanded "strong ignorability" assumption: conditional on *X*, D_i is independent of *both* $(Y(0), Y(1))$ *and* $(S(0), S(1))$.[24]

To illustrate how the *NTD* sheds light on the mechanism or process by which a treatment produces its effects, we use the following example from Rosenbaum (1984).[25] Consider an

[24] Just as with the matching estimators (see footnote 16), strong ignorability also requires that the probability of receiving training conditional on *X* is bounded away form zero and one.
[25] This example discussed in Rosenbaum (1984) is taken from Cochran (1957, Section 2.3).

experiment in which soil fumigants are used to increase crop yields. The potential outcomes are the oat yields on a plot, $Y(D_i)$, that would have been observed in the presence or absence of the fumigant (the treatment). The post-treatment variable is the number of eelworms found on the plots, $S(D_i)$, which is affected by the fumigant. Assuming that the fumigant produces an increase in oat yield, by employing the *NTD* it is possible to learn whether the fumigant works through the control of the damage done by eelworms: the estimated effect of the fumigant should be zero after controlling for the number of eelworms if eelworms are entirely responsible for the increase in yield, or positive if they are not entirely responsible. In our framework, given the lack of effects of Job Corps on Hispanics, controlling for post-treatment experience makes it possible to ascertain if the post-treatment experience explains why Job Corps fails to work for Hispanics (at the 48^{th} month). In other words, if there is an estimated increase in weekly earnings for Hispanics net of post-treatment experience, the latter is a plausible process through which Job Corps fails to work for Hispanics.

Estimates of the *NTD* employing the *BCSME* and *PSCORE* estimators are reported in columns IV and V of Table 4 for the differences in earnings and for the difference-in-differences strategy. Both the *BCSME* and *PSCORE* estimates yield similar results regardless of whether simple differences or differences-in-differences are considered: the *NTD* of Job Corps training for Hispanics is positive and statistically significant, ranging from \$15.3 to \$18.5. Interestingly, the *NTD* estimates are of similar magnitude as the estimated *ATT* effects for whites and blacks in columns II and III. Furthermore, both *NTD* estimates for whites and blacks are within one standard deviation from the *ATT* estimates that do not control for post-treatment experience, which underscores the fact that such post-treatment variable is unimportant for these two groups.[26]

In summary, this set of estimates highlight the importance of labor market experience during the time of the study in explaining the previously estimated lack of effects of Job Corps on Hispanics. These results point to an important difference between Hispanics and non-Hispanics with regards to the interaction between certain labor market dynamics and low-skilled youth, at least in our data.

C. Could Job Corps Have Longer-Run Effects on Hispanics?

An important policy question is whether Job Corps training has long-term benefits for Hispanics. If the next-best alternative to Job Corps training is what the labor market has to offer, and this outcome does not leave them worse off than Job Corps training, what are the justifications for encouraging Job Corps training for them? The results in section III cast doubt on the value of job training programs for young Hispanics. Yet these findings do not necessarily imply that Job Corps training fails to increase the earnings of trained Hispanics in the long term. The NJCS covered the earnings of young people up to 48 months after randomization; however, Job Corps is an intensive program with an average time of participation of 8 months, and its effects are measured only after an average of 37.5 months of receiving it. Combining this with the fact that trained Hispanics enter the labor market on average slightly later than trained whites or blacks because they spent around 1.5 more months in training, it is natural to speculate if there would be any noticeable effects of Job Corps training on Hispanics beyond the 48th month after randomization.

[26] Of course, post-treatment labor market experience could be just one of other potential post-treatment variables through which Job Corps fails to work for Hispanics. Since there are other post-treatment variables available in the NJCS data, we estimated *NTD* with a number of them. However, it is only with post-treatment experience that the *NTD* is positive and statistically significant. These other post-treatment variables are: percent of weeks in training or education, average hours per week in training or education, number of jobs held, number of weeks in recent job, and average hours per week in either training or work.

Few papers are able to explicitly estimate long-run effects from active labor market programs, mainly due to the lack of appropriate data. A notable exception is Hotz, Imbens and Klerman (2001) who estimate long-run effects (9 years) using follow-up data of a randomized training program. Importantly, they find that human capital-intensive programs (such as Job Corps) have longer-lasting effects than programs that emphasize working as soon as possible over human capital accumulation ("work-first" program). In our framework, we show that non-treated Hispanics successfully enter the labor market and accumulate valuable labor market experience, suggesting an analogy with a "work-first" program whose effect may be important in the short-run but perhaps not as much in the long term. In order to test this conjecture, we would ideally like to have earnings information for individuals in the NJCS beyond the 48th month after randomization, but this is not currently available. Instead, we analyze earnings growth trends of Hispanics and non-Hispanics.

Table 6 presents earnings growth rates for treated and non-treated individuals by race/ethnicity and by city type for different periods during the 48 months of the NJCS. We compute these figures for the last 36, 30, and 24 months of the NJCS study. The varying lengths are used to gauge the different effects of including individuals who have not finished Job Corps yet and computing the earnings growth rates over a shorter period of time.[27] Additionally, to account for some of the potential self-selection in the treated and non-treated groups, the earnings growth rates are first computed within propensity score intervals using the baseline

[27] Employing the last 36 months includes about 33% of individuals still in Job Corps, the last 30 months includes around 18% while using the last 24 months includes about 8% of them. Thus, there is a clear tradeoff between a longer span of time to compute the earnings growth figures and the amount of individuals who are still undergoing training.

specification of Table 4, obtaining then a weighted average of the within-interval earnings growth.[28]

The third column (in italics) for each ethnic and racial group in Table 6 shows the difference in earnings growth between treated and non-treated individuals. The first and second panels show that during the last 36 and 30 months of the study and regardless of the type of city of residence, Hispanics have a considerably higher (more than twice) earnings growth difference relative to whites and blacks. For instance, during the last 36 months of the NJCS, the earnings of Hispanics with Job Corps training grew 4.1% faster than Hispanics without Job Corps training, compared to only 1.6 and 1.3% for whites and blacks, respectively. This difference declines as we shorten the length of time considered, but more so for Hispanics. Looking at the last 24 months of the NJCS, trained Hispanics outpace non-trained Hispanics by a minuscule 0.05 percent, compared to -0.03 for whites and 0.26 for blacks. The large drop in the difference in earnings growth for Hispanics as we move from the last 36 months to the last 24 months of the NJCS might reflect the effect of the higher labor market experience attained by non-treated Hispanics during the NJCS. In this case, one interpretation is that the rate of growth in earnings during the last 24 months of the NJCS for treated Hispanics is on average of roughly the same magnitude as a similar non-treated individual with higher labor market experience.

A revealing finding in Table 6 is the fact that the difference in earnings growth rates for all groups varies by type of city of residence, and the pattern is different for each group. In particular, Hispanics have their smallest difference in earnings growth between treated and non-treated among those residing in PMSA, followed by MSA and then other. In fact, in PMSAs the difference is negative for the two most recent growth rates (-0.02 and –0.98 percent for the 30

[28] The range of values of the propensity score is 0.1-0.7 for all race/ethnicity groups. We divide this range in intervals of length 0.05 and compute the difference in earnings growth rates within each interval. Finally, an overall average is obtained, weighted by the number of individuals

and 24 month rates, respectively). For whites, the largest difference in earnings growth is in PMSA followed by other and MSA (except when considering the last 24 months); while for blacks is in other areas followed by PMSA and MSA. In no case, however, is the difference among city type's figures as substantial as for Hispanics.

These results for Hispanics are consistent with our previous finding that living in PMSAs is favorable for the outcomes of non-treated Hispanics at the 48th month after randomization. Specifically, the negative difference in earnings growth rates between treated and non-treated Hispanics in PMSAs is due to the non-treated Hispanics having growth rates ranging from 5.82 to 6.87 percent over the 24-, 30-, and 36-month period. These growth rates are greater than those of whites and blacks by 0.8 to 2.6 percentage points. The earnings growth rates for Job Corps trained Hispanics in PMSAs, however, are not much different than trained Hispanics in MSAs and other areas.

The evidence regarding the growth in earnings suggests that Job Corps does impart a higher rate of growth on the earnings of Hispanics that undertake training. These growth rates are always greater than those of trained whites and typically greater than those of trained blacks. In addition, these rates are typically higher than the growth rates of non-treated Hispanics even though the latter have more labor market experience. This evidence suggests that long-run positive effects of Job Corps on Hispanics cannot be ruled out.

V. Conclusions

In this paper we provide an explanation to the puzzling result in the National Job Corps Study (NJCS) that Job Corps, a federally funded residential job training program, has no earnings effect on Hispanic youth. Our results indicate that labor market experience gained

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contained in each interval. We note, however, that not employing within-propensity score figures yields a very similar story.

during the time of the study is an important factor in explaining the lack of estimated effects of Job Corps on Hispanics at the 48th month after randomization.

We start with the observation that randomization in the NJCS did not create comparable treatment and control groups for Hispanics possibly because they represent 20% of the entire randomized sample and are more concentrated in metropolitan areas. We then apply alternative estimators that adjust for systematic differences in pre-treatment observable and time-invariant unobservable characteristics of the Hispanic subsample, but still find statistically insignificant effects of Job Corps 48 months after randomization. After documenting a pervasive difference in labor market experience gained during the study between treated and non-treated Hispanics, we estimate the "net treatment difference" (Rosenbaum, 1984) to show that this factor explains the lack of effects of Job Corps on Hispanics. While it is not possible to estimate if Job Corps has a long-term positive impact on Hispanics, we show earnings growth figures that suggest that the program has been beneficial to Hispanics in this respect. A full analysis of why non-treated Hispanics exhibit positive labor market outcomes is left for future research, although we presume that the labor markets in large cities offer Hispanics unique opportunities and potentially provide networking effects that help their labor market prospects. It is important to emphasize that these positive early market outcomes of non-treated Hispanics mask the fact that Job Corps trained Hispanics have the largest earnings growth of all the treated groups.

We also illustrate how the "net treatment difference" (*NTD*) parameter can be used to learn about the process through which a treatment works (or fails to work). While this parameter has been used in other disciplines (see examples in Rosenbaum (1984)) to learn about the process or mechanism through which a particular treatment works, we are unaware of its application in the economics literature.

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Table 1. Comparisons of the Differences-in-Means Estimator of Average Weekly Earnings Using Different Samples and Earnings Measures

	Total		Hispanic		White		Black	
DM *comp*	Year 4[1]	Quarter 16	Year 4[1]	Quarter 16	Year 4[1]	Quarter 16	Year 4[1]	Quarter 16
NCJS Study Estimator[2]	22.1	25.2	-15.1	--	46.2	--	22.8	--
p-value	0.00	0.00	0.19	--	0.01	--	0.0	--
Entire 48-month Sample[3]	22.1	25.2	-15.1	-23.6	46.2	58.0	22.8	24.7
p-value	0.00	0.00	0.19	0.11	0.01	0.01	0.01	0.01
Restricted Sample[4]	18.7	20.6	-16.8	-26.9	37.8	44.8	24.1	27.4
p-value	0.00	0.00	0.17	0.08	0.00	0.00	0.00	0.00

[1] For consistency with the NJCS report, earnings is the average weekly earnings in year 4.

[2] Schochet, Burghardt, and Glazerman (2001, Tabless VI.1 and D.14). Adjusted using sampling weights. Quarter 16 earnings is not provided for subgroups in the original NJCS report.

[3] Adjusted using 48-month sampling weights.

[4] The restricted sample contains those who completed both a 48-month and baseline interview, as well as all those with non-missing information on the covariates used in the alternative estimators (see Table 4). Estimates are unweighted.

Table 2. Summary Statistics for Control and Treatment Groups in Original NCJS Samples[1]

	Hispanic					White					Black				
	Control		Treatment			Control		Treatment			Control		Treatment		
Characteristics	Mean	S.E.	Mean	S.E.	z-stat	Mean	S.E.	Mean	S.E.	z-stat	Mean	S.E.	Mean	S.E.	z-stat
At Baseline															
Age	19.0	0.07	18.9	0.05	-0.60	18.8	0.06	18.8	0.04	-0.04	18.7	0.04	18.8	0.03	1.69
Percent Female	0.41	0.02	0.45	0.01	1.80	0.33	0.01	0.33	0.01	0.03	0.43	0.01	0.44	0.01	0.57
Number of children	0.24	0.02	0.28	0.02	1.88	0.15	0.01	0.13	0.01	-1.73	0.32	0.01	0.33	0.01	0.75
Percent who are married or cohabitating	0.09	0.01	0.10	0.01	0.37	0.08	0.01	0.08	0.01	0.18	0.04	0.00	0.04	0.00	-1.09
Percent who are Household Heads	0.12	0.01	0.12	0.01	-0.36	0.11	0.01	0.11	0.01	-0.43	0.13	0.01	0.13	0.01	-0.31
Percent living in a MSA	0.40	0.02	0.46	0.01	2.85	0.49	0.01	0.47	0.01	-1.29	0.47	0.01	0.47	0.01	-0.32
Percent living in a PMSA	0.48	0.02	0.42	0.01	-2.72	0.15	0.01	0.16	0.01	1.30	0.37	0.01	0.37	0.01	0.27
Percent who speak English as a Native Language	0.46	0.02	0.48	0.01	0.94	0.99	0.00	0.98	0.00	-1.26	0.98	0.00	0.97	0.00	-1.85
Percent that have ever been convicted	0.15	0.01	0.14	0.01	-0.61	0.23	0.01	0.24	0.01	0.56	0.14	0.01	0.14	0.01	-0.30
Highest Grade Completed	10.0	0.1	10.0	0.04	-0.09	10.1	0.04	10.1	0.03	-0.51	10.1	0.03	10.1	0.02	-0.51
Percent with High School Diploma or GED	0.23	0.01	0.22	0.01	-0.70	0.28	0.01	0.27	0.01	-0.78	0.21	0.01	0.20	0.01	-1.22
Percent unemployed at randomization	0.54	0.02	0.60	0.01	2.79	0.62	0.01	0.61	0.01	-0.47	0.57	0.01	0.56	0.01	-0.62
Percent never employed	0.23	0.01	0.22	0.01	-1.11	0.13	0.01	0.11	0.01	-1.32	0.25	0.01	0.25	0.01	0.00
Percent employed at randomization	0.22	0.01	0.19	0.01	-2.30	0.25	0.01	0.28	0.01	1.48	0.18	0.01	0.19	0.01	0.81
Average weekly Pre-treatment Earnings[2]	$112	$4.30	$103	$3.50	-1.35	$128	$3.60	$137	$3.09	1.69	$98	$2.44	$98	$1.92	0.20
At 48 Month Interview															
Percent took Job Corps training	0.73	0.00	0.74	0.01	--	0.02	0.00	0.72	0.01	--	0.01	0.00	0.75	0.01	--
Percent took any type of training/education program	0.73	0.02	0.93	0.01	11.78	0.67	0.01	0.91	0.01	16.33	0.73	0.01	0.93	0.00	20.11
Highest grade completed	10.73	0.06	10.63	0.05	-1.30	10.65	0.05	10.70	0.04	0.96	10.83	0.03	10.82	0.03	-0.20
Percent completed a High School Diploma	0.09	0.01	0.05	0.01	-3.12	0.08	0.01	0.05	0.01	-2.37	0.07	0.01	0.05	0.00	-2.60
Percent completed a GED	0.25	0.02	0.42	0.02	6.66	0.31	0.02	0.50	0.01	8.69	0.25	0.01	0.38	0.01	8.88
Percent completed a Vocational Diploma	0.19	0.01	0.39	0.01	9.11	0.13	0.01	0.38	0.01	14.73	0.14	0.01	0.36	0.01	17.66
Percent who worked in Quarter 16	0.72	0.02	0.70	0.01	-1.04	0.76	0.01	0.80	0.01	2.22	0.63	0.01	0.67	0.01	2.58
Average weekly earnings in quarter 16	$227	$8.58	$210	$6.79	-1.63	$232	$6.44	$272	$6.11	4.80	$171	$4.38	$189	$3.8	3.32
Average hours worked per week during study	21.35	0.52	19.57	0.39	-2.89	25.53	0.46	25.39	0.35	-0.26	18.58	1.63	18.39	0.23	-0.51
N	787		1,161			1,193		1,760			2,179		3,338		

[1] Estimates are weighted using NCJS weights for baseline and 48 month interviews, respectively. Results are based on all available responses for each question.

[2] Zero if not employed in previous year.

Table 3. Means of Selected Variables for Treatment and Control Groups by City and Race/Ethnicity

	Hispanic			White			Black		
	Control	Treatment	z-stat	Control	Treatment	z-stat	Control	Treatment	z-stat
MSA									
Distribution within city[1]	0.37	0.63	0.77	0.41	0.59	-0.97	0.40	0.60	-0.39
Earnings, quarter 16	215.36	198.03	-1.17	237.29	261.57	2.02	169.81	195.01	3.24
Average hours worked per week during study	21.57	19.79	-2.65	26.15	25.60	-0.66	18.78	19.03	0.47
N	280	471		499	725		924	1,415	
	-1.78			-0.55			0.25		
PMSA									
Distribution within city[1]	0.43	0.57	-1.70	0.35	0.65	1.30	0.38	0.62	0.47
Earnings, quarter 16	238.84	210.98	-1.74	266.81	299.69	1.37	182.00	189.25	0.75
Average hours worked per week during study	20.95	17.77	-5.10	25.86	24.82	-0.73	18.40	17.73	-1.77
N	323	427		141	260		678	1,092	
	-3.18			-1.04			-0.67		
Other									
Distribution within city[1]		0.59	-0.46	0.40	0.60	-0.47	0.41	0.59	-0.77
Earnings, quarter 16	212.78	231.49	0.58	227.79	256.38	2.06	149.17	178.49	2.47
Average hours worked per week during study	23.79	20.04	-5.07	26.04	25.49	-0.99	17.82	17.71	-0.26
N	79	113		363	545		314	456	
	-3.75			-0.55			-0.10		
Total									
Duncan Dissimilarity Index	0.055			0.029			0.013		
Distribution within city[1]	0.40	0.60	-0.83	0.40	0.60	-0.48	0.39	0.61	-0.30
Earnings, quarter 16	226.18	207.24	-1.85	238.00	266.20	3.30	170.74	190.34	3.56
Average hours worked per week during study	21.53	18.96	-3.90	26.07	25.43	-1.13	18.49	18.35	-0.37
N	682	1,011		1,003	1,530		1,916	2,963	

Notes : The z-statistic tests the difference between control and treatment group members of the same ethnicity.

[1] The z-statistic corresponds to the test that the mean for the treatment group equals 0.61. All estimates unweighted.

Table 4. Non-experimental Estimators of the Effect of Job Corps on Quarter 16 Earnings on Treated and Non-Treated Individuals

	I	II	III	IV	V
		Baseline Specification[1]		Baseline Specification + Post-treatment Experience (*NTD*)[4]	
	Linear Differences	BCSME[2]	PSCORE[3]	BCSME[2]	PSCORE[3]
Panel A: Earnings as Levels					
Hispanic	-8.5	4.5	-5.1	18.5 *	15.3 *
	(10.10)	(10.74)	(3.32)	(9.62)	(8.61)
White	19.5 **	21.7 **	20.7 **	23.7 ***	26.4 ***
	(8.57)	(8.96)	(8.75)	(8.35)	(6.15)
Black	15.1 ***	17.1 ***	16.7 ***	17.8 ***	21.6 ***
	(5.48)	(5.83)	(5.20)	(5.07)	(4.20)
Panel B: Earnings as Difference in Differences[5]					
Hispanic	3.0	4.5	-2.6	18.5 *	18.0 **
	(10.53)	(10.92)	(10.49)	(9.76)	(7.54)
White	20.6 **	21.7 **	20.0 **	23.7 ***	24.7 ***
	(8.98)	(9.00)	(9.67)	(8.47)	(7.20)
Black	20.3 ***	17.1 ***	19.2 ***	17.8 ***	23.3 ***
	(5.73)	(5.89)	(5.49)	(5.18)	(4.70)

Notes: Number of observations: White: 2,533 Hispanic: 1,693 Black: 4,879.

Standar errors in parentheses; *, **, *** significant at the 10%, 5%, and 1% level, respectively.

[1] Baseline specification uses the following variables at baseline interview: Had high school diploma or GED, age, speaks English, married, household head, has child, gender, has vocational degree, been convicted, pre-treatment weekly earnings, employment status, dummy variables for PMSA and MSA. In addition, the PSCORE specification adds the square of these variables, where applicable.

[2] Uses 4 matches for each treated individual based on Mahalanobis distance. Standard errors allow for heteroskedasticity.

[3] Computed using the kernel matching method with a Gaussian kernel and bandwidth=0.06. Bootstrapped standard errors with 50 replications.

[4] The post-treatment experience variable is defined as average hours worked per week during the study period (see text for details).

[5] Difference in the difference in average weekly earnings between most recent job at baseline and in quarter 16.

Table 5. Difference in Observed Characteristics Before and After Matching[1]

	Before					After				
	Non-Treated		Treated			Non-Treated		Treated		
Characteristics	Mean	S.E.	Mean	S.E.	z-stat	Mean	S.E.	Mean	S.E.	z-stat
At Baseline										
Age	18.9	0.07	18.8	0.08	-0.72	18.6	0.02	18.8	0.08	2.33
Percent Female	0.45	0.02	0.49	0.02	1.94	0.47	0.02	0.49	0.02	0.95
Number of children	0.28	0.02	0.27	0.02	-0.22	0.19	0.00	0.27	0.02	3.04
Percent who are married or cohabitating	0.11	0.01	0.08	0.01	-1.97	0.08	0.01	0.08	0.01	0.40
Percent who are Household Heads	0.13	0.01	0.10	0.01	-1.84	0.09	0.01	0.10	0.01	0.93
Percent living in a MSA	0.43	0.02	0.47	0.02	1.62	0.46	0.02	0.47	0.02	0.28
Percent living in a PMSA	0.45	0.02	0.43	0.02	-1.00	0.45	0.02	0.43	0.02	-0.63
Percent who speak English as a Native Language	0.46	0.02	0.47	0.02	0.35	0.46	0.02	0.46	0.02	0.09
Percent that have ever been convicted	0.12	0.01	0.10	0.01	-0.95	0.10	0.01	0.10	0.01	0.34
Has Vocational Degree	0.02	0.00	0.03	0.01	0.25	0.03	0.00	0.03	0.01	0.00
Percent with a High School Diploma or GED	0.23	0.01	0.22	0.02	-0.18	0.20	0.01	0.22	0.02	1.09
Percent unemployed at randomization	0.57	0.02	0.60	0.02	1.22	0.60	0.02	0.60	0.02	-0.19
Percent employed at randomization	0.21	0.01	0.17	0.01	-1.85	0.16	0.01	0.17	0.01	0.34
Average Weekly Pre-Treatment Earnings[2]	$112	$3.71	$100	$3.95	-2.12	$97	$1.75	$100	$3.95	0.61

[1] The working sample contains those who completed both a 48-month interview and a Baseline interview, as well as all those with non-missing information on the covariates used in the non-experimental estimators (see Table4). Estimates are unweighted.

[2] Zero if not employed in previous year.

Figure 1. Histogram of Estimated Propensity Score: Hispanic Sample

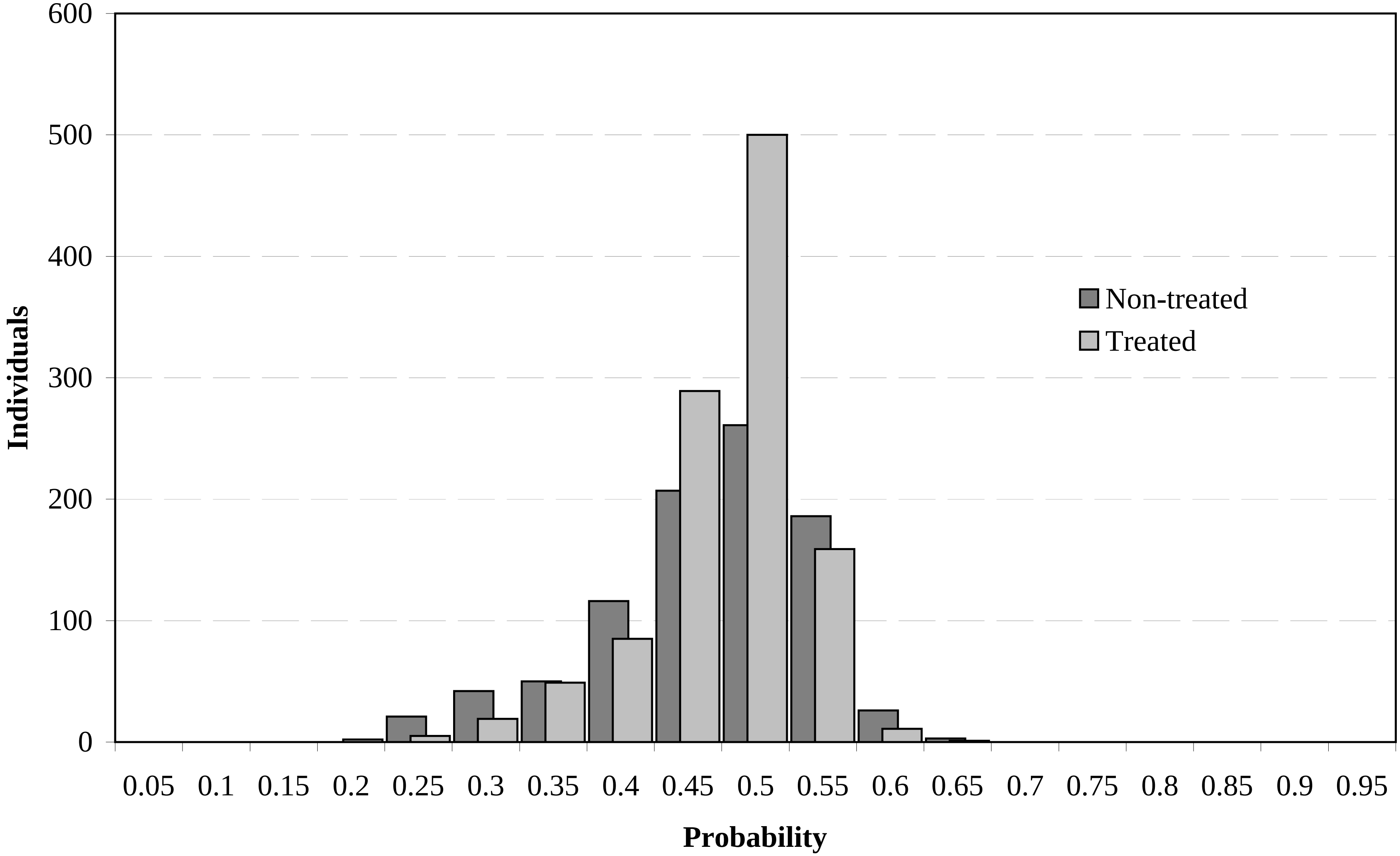

Table 6. Earnings Growth Rates at Various Lengths by City Type

	Hispanics			Whites			Blacks		
	Non-Treated	Treated	*Difference*	Non-Treated	Treated	*Difference*	Non-Treated	Treated	*Difference*
Last 36 months									
All Areas	5.37	9.48	*4.11*	4.50	6.06	*1.56*	5.83	7.14	*1.31*
PMSA	6.87	9.46	*2.59*	4.21	6.87	*2.66*	6.04	7.61	*1.57*
MSA	4.77	9.63	*4.86*	4.37	5.47	*1.10*	5.71	6.60	*0.89*
Other	2.76	11.03	*8.27*	4.87	6.74	*1.87*	5.42	8.26	*2.84*
Last 30 months									
All Areas	4.86	6.12	*1.27*	3.53	4.09	*0.56*	5.02	5.54	*0.52*
PMSA	6.12	6.10	*-0.02*	3.73	5.67	*1.94*	5.21	5.82	*0.61*
MSA	4.11	5.75	*1.64*	3.52	3.72	*0.20*	5.20	5.45	*0.26*
Other	3.41	7.78	*4.36*	3.58	3.85	*0.27*	4.06	5.42	*1.36*
Last 24 months									
All Areas	4.31	4.36	*0.05*	3.57	3.54	*-0.03*	4.33	4.59	*0.26*
PMSA	5.82	4.84	*-0.98*	4.65	5.42	*0.77*	4.53	4.85	*0.32*
MSA	2.86	3.67	*0.81*	2.87	3.49	*0.62*	4.54	4.27	*-0.27*
Other	4.30	5.12	*0.82*	4.09	2.87	*-1.22*	3.13	4.50	*1.37*

Notes: The figures are computed within propensity score intervals, and then averaged. See text for details.

Table A.1. Summary Statistics for Job Corps Treated and Non-Treated in Restricted Sample [1]

	Hispanic					White					Black				
	Non-Treated		Treated			Non-Treated		Treated			Non-Treated		Treated		
Characteristics	Mean	S.E.	Mean	S.E.	z-stat	Mean	S.E.	Mean	S.E.	z-stat	Mean	S.E.	Mean	S.E.	z-stat
At Baseline															
Age	18.89	0.07	18.81	0.08	-0.72	18.91	0.06	18.71	0.06	-2.42	18.86	0.04	18.71	0.05	-2.51
Percent Female	0.45	0.02	0.49	0.02	1.94	0.35	0.01	0.34	0.01	-0.73	0.47	0.01	0.47	0.01	0.41
Number of kids	0.28	0.02	0.27	0.02	-0.22	0.17	0.01	0.11	0.01	-3.12	0.40	0.02	0.33	0.01	-3.25
Percent who are married or cohabitating	0.11	0.01	0.08	0.01	-1.97	0.09	0.01	0.06	0.01	-3.51	0.04	0.00	0.03	0.00	-2.20
Percent who are Household Heads	0.13	0.01	0.10	0.01	-1.84	0.12	0.01	0.08	0.01	-3.58	0.14	0.01	0.13	0.01	-1.04
Percent living in a MSA	0.43	0.02	0.47	0.02	1.62	0.49	0.01	0.47	0.01	-1.30	0.48	0.01	0.48	0.01	-0.01
Percent living in a PMSA	0.45	0.02	0.43	0.02	-1.00	0.16	0.01	0.16	0.01	0.40	0.36	0.01	0.36	0.01	0.04
Percent who speak English as a Native Language	0.46	0.02	0.47	0.02	0.35	0.99	0.00	0.99	0.00	-0.50	0.98	0.00	0.97	0.00	-2.19
Percent that have ever been convicted	0.12	0.01	0.10	0.01	-0.95	0.20	0.01	0.19	0.01	-0.42	0.12	0.01	0.12	0.01	-0.14
Highest Grade Completed	10.05	0.05	9.95	0.06	-1.33	10.15	0.04	10.02	0.05	-2.24	10.18	0.03	10.05	0.03	-3.06
Percent with a High School Diploma or GED	0.23	0.01	0.22	0.02	-0.18	0.29	0.01	0.28	0.01	-0.55	0.23	0.01	0.20	0.01	-3.02
Percent unemployed at randomization	0.57	0.02	0.60	0.02	1.22	0.62	0.01	0.60	0.01	-1.19	0.58	0.01	0.57	0.01	-1.30
Percent never employed	0.22	0.01	0.23	0.02	0.31	0.12	0.01	0.13	0.01	0.14	0.24	0.01	0.26	0.01	1.48
Percent employed at randomization	0.21	0.01	0.17	0.01	-1.85	0.26	0.01	0.28	0.01	1.21	0.18	0.01	0.18	0.01	0.01
Average Weekly Pre-treatment Earnings[2]	$112	$3.71	$100	$3.95	-2.12	$134	$3.13	$133	$3.57	-0.22	$100	$2.09	$95	$2.19	-1.71
At 48 Month Interview															
Percent took any type of training/education program	0.72	0.01	1.00	0.00	--	0.68	0.01	1.00	0.00	--	0.74	0.01	1.00	0.00	--
Highest Grade Completed	10.74	0.05	10.56	0.06	-2.27	10.72	0.04	10.65	0.05	-1.12	10.92	0.03	10.77	0.03	-3.56
Percent completed a High School Diploma	0.09	0.01	0.04	0.01	-3.54	0.08	0.01	0.05	0.01	-2.60	0.07	0.01	0.05	0.00	-3.01
Percent completed a GED	0.25	0.01	0.47	0.02	8.10	0.34	0.01	0.60	0.01	10.96	0.26	0.01	0.43	0.01	11.48
Percent completed a Vocational Diploma	0.17	0.01	0.48	0.02	13.96	0.14	0.01	0.48	0.01	18.85	0.16	0.01	0.44	0.01	21.30
Average Hours Worked per week during study	21.40	0.45	18.25	0.43	-5.08	26.06	0.38	25.20	0.40	-1.56	18.74	0.26	18.00	0.26	-2.01
Percent who worked in quarter 16	0.72	0.01	0.71	0.02	-0.40	0.78	0.01	0.80	0.01	1.69	0.65	0.01	0.68	0.01	2.30
Average weekly earnings during quarter 16	$219	$6.75	$210	$7.50	-0.85	$246	$5.61	$266	$6.52	2.30	$176	$3.65	$191	$4.10	2.76

[1] The working sample contains those who completed both a 48-month interview and a baseline interview, as well as all those with non-missing information on the covariates used in the non-experimental estimators (see Table4). Estimates are unweighted.

[2] Zero if not employed in previous year.

Table A.2. Propensity Score Coefficient Estimates

	Hispanic	White	Black
Age	-0.315	-0.022	-0.240
	(0.240)	(0.205)	(0.148)
Age Squared	0.008	0.000	0.006
	(0.006)	(0.005)	(0.004)
Female	0.120	0.006	0.069
	(0.065)	(0.056)	(0.039)
Highest Grade Completed	-0.029	-0.044	-0.016
	(0.027)	(0.023)	(0.018)
Has High School Diploma or GED	0.065	0.118	-0.097
	(0.098)	(0.075)	(0.058)
Has Vocational Degree	0.079	-0.036	-0.094
	(0.203)	(0.174)	(0.142)
Lives in a PMSA	0.025	-0.003	-0.008
	(0.104)	(0.076)	(0.055)
Lives in a MSA	0.111	-0.057	0.009
	(0.104)	(0.056)	(0.053)
Speaks English as Native Language	0.026	-0.110	-0.285
	(0.062)	(0.222)	(0.125)
Married or Cohabitating	-0.218	-0.255	-0.157
	(0.109)	(0.102)	(0.099)
House Hold head	-0.174	-0.249	0.040
	(0.104)	(0.091)	(0.060)
Number of Kids	0.019	-0.076	-0.093
	(0.058)	(0.064)	(0.030)
Been Convicted	-0.075	-0.032	-0.007
	(0.101)	(0.064)	(0.057)
Unemployed at Randomization	0.137	0.020	-0.013
	(0.089)	(0.087)	(0.051)
Employed at Randomization	0.012	0.106	0.013
	(0.113)	(0.096)	(0.066)
Average Weekly Earnings from most recent job	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Constant	3.076	0.679	2.646
	(2.307)	(1.974)	(1.421)

Notes : Estimated standard errors in parentheses.