

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

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Abstract

The National Job Corps Study (NJCS) was a four-year longitudinal study 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 participants 48 months after randomization, but not for Hispanics, a puzzling outcome that eluded explanation using experimental methods. 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 failed to create comparable treatment and control groups for Hispanics, possibly because randomization did not consider explicitly race and ethnicity. We then apply non-experimental estimators to the Hispanic subsample but still find statistically insignificant effects of Job Corps 48 months after randomization. Finally, we 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. This higher 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.

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 (Gonzalez, 2002; Trejo, 1997). 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 have limited options, especially if they have no interest in enrolling in the formal school system. 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 youths were raised in. The Job Corps program, established in 1964, differs from other job training programs because it targets youth trying to overcome these issues.

Indeed, the National Job Corps Study (NJCS)² 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, Schochet, McConnell, Johnson, Gritz, Glazerman, Homrighausen and Jackson, 2001). For the Job Corps evaluation, eligible applicants were randomly assigned to a treatment or control group, and the former were permitted to enroll in Job Corps while the latter were denied enrollment for three years. Overall, the NJCS report found that treatment-group members earned a statistically significant 12 percent more than control-group members during the 48-month

¹ Authors' calculations from the 2003 March Current Population Survey show that 56.4% of Hispanics ages 16 and older and not in school 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.

follow-up survey (Burghardt et al., 2001). However, despite multiple positive achievements in Job Corps, treatment-group Hispanics earned 10 percent *less* than control-group Hispanics 48 months after randomization, although the figure was not statistically significant.³ This was perhaps the most prominent “failure” of Job Corps and it could not be explained by individual and institutional variables. In contrast, black and white treatment-group members experienced a statistically significant earnings increase of 14 percent and 24 percent with respect to their control group members. It is puzzling why Hispanics and non-Hispanics seem to obtain very different benefits from Job Corps.

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 main objective of this paper is to explore two possible explanations for the lack of earnings gain for Hispanics in Job Corps. The first considers 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 estimator for whites and blacks but not for Hispanics, as randomization generated Hispanic treated and non-treated groups that are not statistically comparable. However, using non-experimental estimators does not reverse the lack of effects of Job Corps for Hispanics. Given the apparent robustness of the lack of effects of Job Corps on Hispanics 48 months after randomization, we consider which labor market dynamics affect Hispanic youth differently. Specifically, we document that non-treated Hispanics accumulate a large amount of labor market experience during the time of the study. Then, estimating the “net treatment difference”

³ This is in contrast to Mallar, Kerachsky and Thornton (1980) who report that in 1977 Hispanics that completed Job Corps had “significantly larger than average impacts” with regards to employment and earnings (pg. 348).

(Rosenbaum, 1984), our analysis suggests that this labor market experience explains the lack of positive earnings 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 the different early labor market dynamics across racial and 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, particularly for Hispanics. This dynamic can be 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 can 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 will be noticed only in the medium- to long-term. We provide some 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 and National Job Corps study (NJCS), provides evidence of the failure of randomization for the Hispanic subgroup and presents some of its consequences. Section III describes our working sample and presents results using non-experimental estimators of the average treatment effect on the treated (*ATT*) that do not rely on a successful 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, discusses some implications of our findings, and points to directions for future research.

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

A. The Job Corps Program

The Job Corps program 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 1998 Workforce Investment Act and administered by the Department of Labor's Employment and Training Administration.⁴ The purpose of Job Corps is to provide low-skilled and low-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 to approximately 70,000 new students every year at a cost of about \$1 billion. The program's educational, job training, and post-completion services are provided at Job Corps centers, which are either operated by Civilian Conservation Centers, or by private and nonprofit organizations under contract to the Job Corps (Department of Labor, 1999; Schochet, Burghardt and Glazerman, 2001). Job Corps is unique from other job training programs because most of the students reside in on-site dormitories during training.

In addition to the education and vocational training, Job Corps also provides health services and a stipend during program enrollment. 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 being a citizen or permanent resident. The typical Job Corps student is a minority (70 percent of all students), 18 years of age, who has dropped out of high school (80 percent) and reads at a seventh grade level (Department of Labor, 1999).

B. The National Job Corps Study (NJCS)

The data collected and used for this paper come from a randomized experiment. From a national pool of over 80,000 Job Corps eligible young persons, 15,386 were selected for the National Job Corps Study in the mid-1990s. The experimental study assigned 61 percent of the selected Job Corps eligible young persons to the treatment group and 39 percent were assigned to the control group. Only the treatment group was permitted to enroll in Job Corps and approximately 73 percent eventually did. The control group could not enroll in Job Corps for three years after randomization, yet they were not prevented from enrolling in other training programs. In order to assess the effects of Job Corps both 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 original NJCS program evaluation is mostly based on a differences-in-means (or cross-section) estimator modified to account for some features of the data (Schochet, 2001). The randomization involved in the NJCS is used to justify the required assumption that on average Job Corps non-participants would have the same treatment outcomes as the participants if permitted to enroll, thus identifying the average treatment effect on the treated (*ATT*).⁵ More specifically, let R be a binary variable indicating whether an eligible Job Corps applicant is

⁴ From 1982 to mid-2000, Job Corps operated under the Job Training Partnership Act.

⁵ The estimators we employ in this paper identify the average treatment effect on the treated (under different assumptions): $ATT = E[Y_i(1) - Y_i(0) | D_i = 1]$, where D_i is a binary variable that indicates whether the individual

randomly permitted to enroll in the program ($R = 1$) or prevented from enrolling ($R = 0$). Yet assignment to the treatment group ($R = 1$) does not rule out non-participation in Job Corps ($D_i = 0$) and vice-versa.⁶ Therefore, the differences-in-means estimator 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 who cross-over from one assignment group to the other (P_{cross}). Using this estimator, the effect of Job Corps on the “compliers” is

$$DM_{comp} = \frac{\bar{Y}(1)_{16} - \bar{Y}(0)_{16}}{P_{part} - P_{cross}}, \quad (1)$$

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 16.⁷

The first row of Table 1 reports the original NJCS estimator. 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 will employ earnings in quarter 16 as our measure since it is the most recent labor market outcome, but for comparison with the NJCS estimates by race, we will also present in this table experimental estimators that use the average weekly earnings in year 4.⁸ The NJCS estimates imply an overall gain of \$22.1, although it is not uniform across demographic groups: whites show a \$46.2 gain and blacks a \$22.8 gain, both of them statistically significant, while

receives Job Corps. For details about the different estimators and their assumptions, see Heckman, LaLonde and Smith (1999).

⁶ Approximately 26 percent of individuals in the treatment group ($R = 1$) never enrolled in Job Corps; while over 1 percent of control group members ($R = 0$) receive training from Job Corps and another 72 percent enrolled in alternative training programs during the entire 48-month period of interest.

⁷ This estimator assumes that the mean effect of training is the same on those taking it in both the treatment and control groups. Formally, $E[Y_i(1) | R = 1, D_i = 1] = E[Y_i(1) | R = 0, D_i = 1]$.

Hispanics show a statistically insignificant loss of \$15.1. In the next section, we compare the NJCS estimates with similar estimates obtained using our working sample.

[TABLE 1 HERE]

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 (e.g. Smith, 2000).⁹ Burghardt et al. (1999) describe the randomization design employed in the (NJCS), which was undertaken using the entire sample of Job Corps eligible applicants without consideration for race or ethnicity.

1. Valid Control and Treatment Groups?

Given that the original randomization was applied to the whole 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 and experimental group status (control or treatment), and compares their observable average characteristics. An important characteristic of the sampling design employed in the NJCS is that the sampling rate for females in the control group 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, McConnell, Meckstroth, Schochet, Johnson and Homrighausen (1999)). For this reason, the figures in Table 2 and the original NJCS experimental estimator in Table 1 use sampling weights.

⁸ 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.

⁹ Some other important assumptions are needed, such as the absence of an effect of randomization on the impact of participation. See also Heckman, LaLonde and Smith (1999).

[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 being the smallest group of the three (less than 20 percent of the sample). They exhibit differences in the percentage of females (even though sampling weights are used), 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 1.96): 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 non-experimental methods to try disentangling the reasons why Hispanics show no effects from Job Corps. We do this in section III.

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 academic or vocational achievement is not the reason for the lack of experimentally estimated labor market effects of Job Corps on Hispanics at the 48-month after randomization. This panel also illustrates, in the first row, the pervasiveness of non-compliance and substitution present in the study.

Rows 2 through 5 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 tends 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 6 through 8 show some variables for which Hispanics considerably 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 Failure of Randomization.

Probably the most labor-market 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. Recall that the original sample design randomly assigned 61 percent of the sample in the treatment group and 39 percent 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 at 57 percent, which is statistically different than the expected 61 percent at the 10 percent 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 percent 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

$$D_r = 0.5 \sum_{i=1}^3 |c_{ri} - t_{ri}|, \quad (2)$$

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 percent of treatment group Hispanics needing to move compared to 2.9 and 1.3 percent for whites and blacks, respectively. In particular, treated 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 than Hispanics in the treatment group, but only in PMSA is the difference statistically significant. In addition, there is

a *positive* difference in mean earnings 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 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. We define experience as the average weekly hours worked during the time of the study (208 weeks after randomization).¹⁰ Table 3 shows that the greatest difference in experience acquired during the time of the study is found within Hispanic treatment and control groups: *within* cities, treatment-group Hispanics have 370 to 780 fewer hours of experience, all statistically significant at least at the 10 % level. If the typical full-time, year-round employee works 2000 hours, Hispanics in the control group average over one-quarter year more experience than Hispanics in the treatment group, and this difference most likely translates into higher earnings for the former. The corresponding difference for blacks and whites is only greater than 200 hours for whites in PMSA but never is any difference statistically significant. These issues of city type and accumulated labor market experience during the 48 months of the study are considered in greater detail below.

III. Evidence from Non-Experimental Estimators

In this section we present evidence on the effect of Job Corps on Hispanics using non-experimental estimators. 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, lending support to the NJCS

¹⁰ Measures of labor market experience *before* randomization are unavailable in the NJCS data.

result that there are no effects of Job Corps on Hispanics 48 months after randomization. For reference, we also report non-experimental estimates for whites and blacks.

We employ the potential outcomes framework to describe the 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 non-experimental estimators we employ to estimate the effects of Job Corps will make different assumptions in trying to construct the appropriate counterfactual.

A. The Working Sample

In order to employ non-experimental 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 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 non-experimental estimators are not available, arriving to a working sample of 9,105 individuals.¹¹ Table A.1 in the appendix features means of selected variables of interest in our working sample for *treated* and *non-treated* individuals by race/ethnicity. Importantly, our working 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 percent are non-white), male (about 43 percent are female), and with 10 years of schooling. In all, Hispanics comprise over 18 percent of our working sample.

To gauge the extent to which our working 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 employing our working 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 the measure of weekly earnings in quarter 16 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 percent, but for whites it is larger by about 25 percent, 8 percent for blacks, and for Hispanics it is larger in absolute value by about 56 percent. Therefore, we obtain larger (in absolute value) estimates when we employ weekly earnings in quarter 16, which would result 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 working 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), implying lower returns to Job Corps than the original NJCS estimator for whites, while for Hispanics and blacks the difference is small (less than \$2). However, by switching to weekly earnings in quarter 16, our working sample DM_{comp} is virtually the same as the NJCS estimator for whites, 20 percent higher for blacks, while it yields significantly lower returns for Hispanics (78 percent lower returns but still statistically insignificant). Thus, it is likely that the differences

¹¹ This last figure includes an additional 694 observations that are lost since their race/ethnicity is not white, black or Hispanic. It is also worth noting that the individuals excluded are proportionately distributed across race/ethnic

that arise from using our working 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 working sample.

B. Non-Experimental Estimators Employed

Non-experimental methods are needed to analyze the effects of Job Corps on Hispanics given the likely failure of randomization documented in the previous section. The non-experimental 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, Ichimura, Smith and Todd (1998)).¹² In all of these estimators we employ standard covariates available to us that have been found to be important in evaluating training programs, which are listed at the bottom of Table 4. We note that our baseline specification also controls for metropolitan area of residence at the time of the baseline interview to account for the observed pre-treatment differences for Hispanics. For comparison, column I in panel A of Table 4 presents the simple difference in average weekly earnings for treated and non-treated individuals. Consistent with previous results, 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]

groups.

¹² For a review of all these estimators, see Heckman, LaLonde and Smith (1999).

The intuition behind both of the matching estimators we employ (*BCSME* and *PSCORE*) is straightforward: in order to obtain a valid comparison group, program participants are “matched” with observable similar persons that did not participate in the program. By matching on observable characteristics, these estimators align the distribution of observable characteristics of the comparison group to that of the treated group. The crucial assumption behind matching estimators is that, conditional on the observed variables upon which the match is undertaken, participation in Job Corps is independent of weekly earnings, i.e., that conditional on X , D_i is independent of the potential outcomes ($Y(0)$, $Y(1)$).¹³

The *BCSME* (Abadie and Imbens (2002)) is intuitive, has desirable large-sample properties, and good finite sample properties compared to other matching estimators available.¹⁴ Intuitively, the *BCSME* takes each individual that enrolled in Job Corps and finds matched individuals that did not enroll 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. 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 compare 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, although of course all estimates are statistically insignificant. The estimated effects for whites and blacks are both positive and

¹³ This assumption is known as “unconfoundedness” in the literature. In addition, it also requires that the probability of receiving training conditional on X is bounded away from zero and one. Together, these two assumptions are known as “strong ignorability” (Rosenbaum and Rubin, 1983).

statistically significant (\$21.7 and \$17.1, respectively), and larger than the simple differences in means.

One of the 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. In short, the *PSCORE* estimator avoids the dimensionality problem by matching only on the estimated probability that an individual undertakes Job Corps, known as the propensity score.¹⁵

For this reason we also estimate the effect of Job Corps 48 months after randomization using the *PSCORE* estimator based on (Gaussian) kernel matching.¹⁶ The model specified for the propensity score is a probit model that includes the same variables as *BCSME* plus their squares. While Abadie and Imbens (2002) 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 valuable to compare how the two estimators perform in our data.

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 a small - \$5.1 that 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 is

¹⁴ Abadie and Imbens (2002) provide some Monte Carlo evidence about the finite-sample properties of the bias-corrected estimator.

¹⁵ This is possible due to the important theoretical result by Rosenbaum and Rubin (1983).

¹⁶ Other methods for matching based on the propensity score were tried, 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

relatively small and statistically insignificant, the comparison between *BCSME* and *PSCORE* in our sample is consistent with the findings reported in Abadie and Imbens (2002) about the relative performance of *BCSME* and *PSCORE*.

The next estimator considered 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 ($D = 0$):

$$(Y_{i0} - Y_{i16}) = \beta_0 + \beta_1'(X_{i0} - X_{i16}) + \alpha D_i + (\varepsilon_{i1} - \varepsilon_{i16}), \quad (3)$$

where Y_{i0} and Y_{i16} are weekly earnings before randomization (quarter 0) and at quarter 16, respectively, $(X_{i0} - X_{i16})$ are the differenced covariates, $(\varepsilon_{i1} - \varepsilon_{i16})$ are the differenced error terms, and D_i is the binary variable indicating participation in Job Corps. The parameter α represents the treatment effect of the Job Corps program using the *DID* estimator.

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 documented above. 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 in our working sample, which is also confirmed by the differences-in-differences matching strategy employed next.

Mueser et al. (2004) and Smith and Todd (2004). Dehejia and Wahba (2002) find a similar result, which they

If there are systematic differences in weekly earnings between treated and non-treated individuals after conditioning (matching) on observable characteristics that arise due to time-invariant factors, then employing a differences-in-differences matching strategy would be appropriate. 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 (3). 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 time-invariant factors is very small.

C. Specification Checks

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]

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

attribute to the validity of the support condition, which is also satisfied in our data (see Figure 1).

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.¹⁷ The second exercise we conduct is to check that for all treated individuals there are non-treated individuals in the sample with similar 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.¹⁸

[FIGURE 1 HERE]

D. Summary of Non-Experimental Estimators

The use of non-experimental 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 taking into account the likely failure of randomization alone, including controlling for city type, is not enough to reverse the lack of effects for Hispanics. In the next section, we advance an explanation for the seemingly robust lack of effects of Job Corps on Hispanics 48-months after randomization.

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

¹⁷ Table A.2 presents the probit-estimated coefficients of the propensity score model by race/ethnicity. All estimated coefficients have the expected signs.

¹⁸ The histograms for the propensity score for whites and blacks are qualitatively similar to those for Hispanics.

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 at the end of the study, 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 percent between the treated and non-treated group, compared to +2.0 and +3.0 percent, respectively for treated whites and blacks. Clearly, the labor force dynamics affecting low-income youth earnings affect Hispanics differently than whites and blacks, and this cannot be 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 supply by 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 labor market experience accumulated during the 48-month study. Although we control for the type of city of residence in the non-experimental 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 accumulated labor market

experience during the study differentiates Hispanic treatment and control groups so prominently relative to whites and blacks, this section examines if such experience accounts for the lack of Job Corps-effect on earnings at the 48th month after randomization.

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

To check whether this pervasive difference in accumulated 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 experience in addition to the covariates included in the baseline specification. This exercise is nonstandard given that actual 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 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 this approach to learn about the mechanism through which the treatment works (Rosenbaum, 1984), or in our case through which Job Corps *fails* to work for Hispanics.

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:

$$v(x, s) = E[Y(1)|S(1) = s, X = x] - E[Y(0)|S(0) = s, X = x]. \quad (4)$$

Then, the *NTD* is defined as the expectation over the distribution of $(X, S(D))$:

$\tilde{v} = E[v(X, S(D))]$. This parameter can be consistently estimated 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 $(S(0), S(1))$.¹⁹

To illustrate how the *NTD* sheds light on the mechanism or process by which the treatment produces its effects, we use the following example from Rosenbaum (1984).²⁰ Consider an 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 48th month). In other words, if there is an estimated increase

¹⁹ Just as with the matching estimators (see footnote 13), strong ignorability also requires that the probability of receiving training conditional on X is bounded away from zero and one.

²⁰ This example discussed in Rosenbaum (1984) is taken from Cochran (1957, Section 2.3).

in weekly earnings for Hispanics net of post-treatment experience, the latter is the 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 panel A (for differences in earnings) and panel B (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 for Hispanics. 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 only slightly different than 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.

In summary, this set of estimates highlight the importance of accumulation 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 Hispanics? The results in section III cast doubt on the value of job training programs for young Hispanics 48 months after randomization.

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.

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” programs). In our framework, non-treated Hispanics that successfully enter the labor market and accumulate valuable labor market experience resemble 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 in an attempt to control 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.²¹ Additionally, to account for some of the potential self-selection in the treated and non-treated groups, the earnings growth rates are computed within propensity score intervals using the baseline specification of Table 4, obtaining then a weighted average of the within-interval earnings growth.²²

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, Hispanics in all areas 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 percent faster than Hispanics without it, compared to only 1.6 and 1.3 percent 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 documented higher labor market experience accumulated by non-treated Hispanics during the NJCS. In this case, one interpretation is that the rate of growth in earnings during the last 24

²¹ Employing the last 36 months includes about 33 percent of individuals still in Job Corps, the last 30 months includes around 18 percent while using the last 24 months includes only about 8 percent 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.

²² 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 contained in each interval. We note, however, that not employing within-propensity score figures yields a very similar story. These results are available from the authors.

months of the NJCS for treated Hispanics is on average of roughly the same magnitude as a similar 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 PMSA the difference is negative for the two most recent periods (-0.02 and -0.98 percent for the 24 and 30 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 largest and smallest city type 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 that 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 and Implications

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 suggest that actual experience gained 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 failed to create comparable treatment and control groups for Hispanics because it did not incorporate race and ethnicity explicitly into the sample design. We then apply non-experimental estimators to the Hispanic subsample but still find statistically insignificant effects of Job Corps 48 months after randomization. After documenting a pervasive difference in accumulated labor market experience 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 directly 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. 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

DM_{comp}	Total		Hispanic		White		Black	
	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
Working 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 estimates by race/ethnicity in the NJCS report, earnings is the average weekly earnings in year 4.

² Schochet, Burghardt, and Glazerman (2001, Tables VI.1 and D.14). Adjusted using weights. Quarter 16 earnings is not provided for subgroups.

³ Adjusted using 48-month sample, survey, and nonresponse weights.

⁴ The working 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 non-experimental estimators (see Table 4). Estimates are unweighted.

Table 2. Summary Statistics for Control and Treatment Groups in Original NCJS Samples¹

<i>Characteristics</i>	Hispanic					White					Black				
	Control		Treatment		z-stat	Control		Treatment		z-stat	Control		Treatment		z-stat
	Mean	S.E.	Mean	S.E.		Mean	S.E.	Mean	S.E.		Mean	S.E.	Mean	S.E.	
<i>At Baseline</i>															
Age	19.0	2.2	18.9	2.19	-0.60	18.8	2.2	18.8	2.12	-0.04	18.7	2.1	18.8	2.19	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	0.56	0.28	0.64	1.88	0.15	0.47	0.13	0.43	-1.73	0.32	0.70	0.33	0.71	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	1.6	10.0	1.6	-0.09	10.1	1.5	10.1	1.5	-0.51	10.1	1.5	10.1	1.5	-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	\$181	\$104	\$203	\$810	0.82	\$179	\$108	\$188	\$124	1.95	\$174	\$101	\$171	\$102	-0.89
<i>At 48 Month Interview</i>															
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	1.65	10.63	1.62	-1.30	10.65	1.64	10.70	1.66	0.96	10.83	1.44	10.82	1.44	-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	\$224	\$210	\$216	-1.63	\$232	\$204	\$272	\$239	4.80	\$171	\$192	\$189	\$205	3.32
Average Hours Worked per week during study	21.35	13.64	19.57	12.48	-2.89	25.53	14.16	25.39	13.63	-0.26	18.58	13.19	18.39	12.56	-0.51

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 Conditional on having a job during the 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
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
Total average hours worked per week	4358.18	3696.25	-3.43	5378.79	5163.20	-0.73	3827.18	3688.15	-1.06
<i>N</i>	323	427		141	260		678	1,092	
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
Total average hours worked per week	4485.58	4116.34	-1.74	5439.87	5324.57	-0.66	3905.78	3957.96	0.47
<i>N</i>	280	471		499	725		924	1,415	
Other									
Distribution within city ¹	0.41	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
Total average hours worked per week	4947.64	4167.86	-1.72	5417.11	5301.76	-0.59	3705.66	3684.07	-0.11
<i>N</i>	79	113		363	545		314	456	
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	-0.54
Earnings, quarter 16	226.18	207.24	-1.85	238.00	266.20	3.30	170.74	190.34	3.55
Total average hours worked per week	4478.77	3944.67	-3.90	5423.05	5289.02	-1.13	3845.17	3816.37	-0.37
<i>N</i>	682	1,011		1,003	1,530		1,916	2,936	

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

	I	II	III	IV	V
	Linear	Baseline Specification ¹		Baseline Specification + Experience (NTD) ⁴	
	Differences	BCSME ²	PSCORE ³	BCSME ²	PSCORE ³
<i>Panel A: Earnings as Levels</i>					
Hispanic	-8.5	4.5	-5.1	18.5	15.3
p-value	0.19	0.43	0.65	0.05	0.06
White	19.1	21.7	20.7	23.7	26.4
p-value	0.03	0.02	0.01	0.00	0.00
Black	15.1	17.1	16.7	17.8	21.6
p-value	0.01	0.00	0.01	0.00	0.00
<i>Panel B: Earnings as Difference in Differences⁵</i>					
Hispanic	-3.0	4.5	-2.6	18.5	18.0
p-value	0.61	0.67	0.70	0.06	0.04
White	20.6	21.7	20.0	23.7	24.7
p-value	0.03	0.02	0.02	0.01	0.00
Black	20.4	17.1	19.2	17.8	23.3
p-value	0.00	0.00	0.01	0.00	0.00

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

¹ Baseline specification uses the following variables at baseline interview: Had HSD or GED, Age, Speaks English, Married, Household Head, Has Child, Gender, Has Vocational degree, Been convicted, Pre-treatment weekly earnings, employment status, PMSA, MSA and other. In addition, the PSCORE specification adds the square of these variables.

² Uses 4 matches for each treated individual based on Mahalanobis distance. Bootstrapped standard errors employed.

³ Computed using the Gaussian kernel matching method. Bootstrapped standard errors employed.

⁴ NTD: Net Treatment Difference. Experience controlled for using average total hours worked during the study.

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 for Hispanic Sample Before and After Matching

<i>Characteristics</i>	Before					After				
	Non-Treated		Treated		z-stat	Non-Treated		Treated		z-stat
	Mean	STD	Mean	STD		Mean	STD	Mean	STD	
<i>At Baseline</i>										
Age	18.9	2.1	18.8	2.21	-0.72	18.6	1.9	18.8	2.21	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	0.63	0.27	0.62	-0.22	0.19	0.48	0.27	0.62	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	0.16	0.03	0.16	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	\$112	\$114	\$100	\$108	-2.12	\$97	\$96	\$100	\$108	0.61

Figure 1. Histogram of Estimated Propensity Score: Hispanic Sample

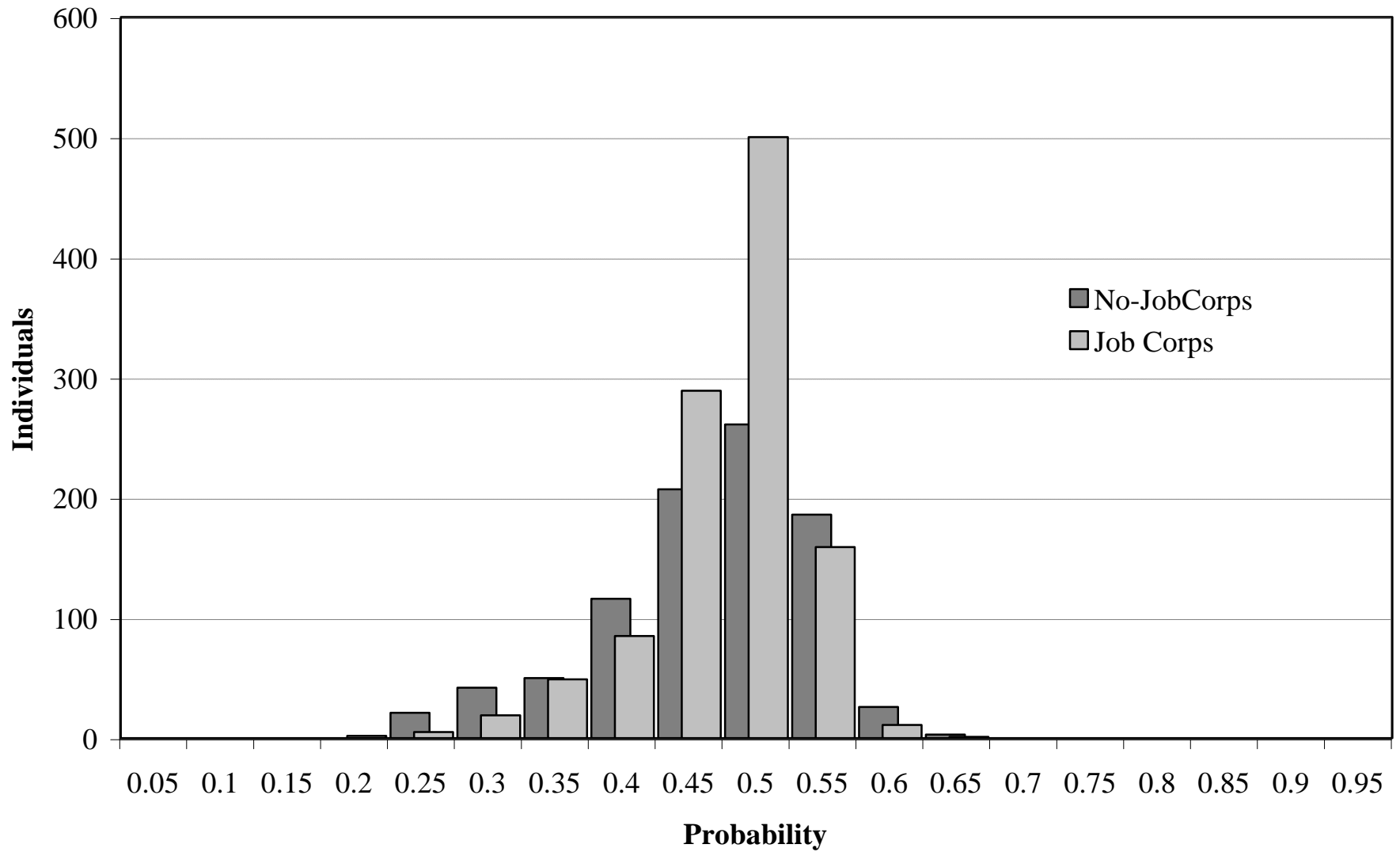


Table 6. Earnings Growth Rates at Various Lengths by City Type

	Hispanics			Whites			Blacks		
	Non-Treated	Treated	<i>Difference</i>	Non-Treated	Treated	<i>Difference</i>	Non-Treated	Treated	<i>Difference</i>
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 Working Sample¹

<i>Characteristics</i>	Hispanic					White					Black				
	Non-Treated		Treated		z-stat	Non-Treated		Treated		z-stat	Non-Treated		Treated		z-stat
	Mean	STD	Mean	STD		Mean	STD	Mean	STD		Mean	STD	Mean	STD	
<i>At Baseline</i>															
Age	18.89	2.14	18.81	2.21	-0.72	18.91	2.15	18.71	2.09	-2.42	18.86	2.16	18.71	2.17	-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	0.63	0.27	0.62	-0.22	0.17	0.50	0.11	0.41	-3.12	0.40	0.80	0.33	0.70	-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	1.57	9.95	1.62	-1.33	10.15	1.50	10.02	1.54	-2.24	10.18	1.47	10.05	1.51	-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	\$114	\$100	\$108	-2.12	\$137	\$134	\$134	\$124	-0.48	\$100	\$110	\$96	\$111	-1.43
<i>At 48 Month Interview</i>															
Percent took any type of training/education program	0.72	0.01	1.00	0.00	15.70	0.68	0.01	1.00	0.00	20.88	0.74	0.01	1.00	0.00	25.95
Highest Grade Completed	10.74	1.67	10.56	1.60	-2.27	10.72	1.68	10.65	1.66	-1.12	10.92	1.41	10.77	1.48	-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
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	\$221	\$222	\$213	\$221	-0.76	\$248	\$220	\$269	\$233	2.30	\$177	\$199	\$193	\$206	2.67
Average Hours Worked per week during study	21.40	13.84	18.25	11.72	-5.08	26.06	14.39	25.20	13.42	-1.56	18.74	13.19	18.00	12.34	-2.01

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 Table 4). Estimates are unweighted.

² Zero if not employed in previous year.

Table A.2. Propensity Score Coefficient Estimates

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

Notes: Estimates from a probit model for the probability of undertaking Job Corps.
Estimated standard errors in parentheses.