Learning But Not Earning? The Impact of Job Corps Training on Hispanic Youth

Alfonso Flores-Lagunes

Arturo Gonzalez

and

Todd Neumann*

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Abstract

Why Hispanics who participated in Job Corps (JC) training did not experience earnings gains like whites and blacks, despite achieving similar human capital gains? We find that the differential labor market outcomes of each group are related to the different levels of local labor market unemployment rates (LUR) they face. Furthermore, the groups exhibit differential impacts on their earnings from the LUR they face, which also vary by randomization status. We find that (i) blacks and Hispanics face higher LUR that mitigate their potential gains from JC; and (ii) JC "shields" whites from adverse LUR, but not blacks and Hispanics.

Alfonso Flores-Lagunes
University of Florida and IZA, Bonn, Germany
Food and Resource Economics Department
P.O. Box 110240, Gainesville, FL 32611-0240
Phone: (352) 392-1826 ext. 307
E-mail: alfonsofl@ufl.edu

Arturo Gonzalez
Ernst & Young and IZA, Bonn, Germany
560 Mission St, Suite 1600, San Francisco, CA 94105
Phone: (415) 894-8654
E-mail: arturo.gonzalez@ey.com

Todd Neumann
University of California at Merced
School of Social Sciences, Humanities and Arts
P.O. Box 2039, Merced, CA 95344
Phone: (209) 228-4020
E-mail: tneumann@ucmerced.edu

I. INTRODUCTION

Job Corps (JC) is a federally funded training program for disadvantaged youth ages 16 to 24 that provides educational and vocational training, life skills, and job placement services. With few exceptions, JC participants reside at the corresponding JC facility. Through this comprehensive approach, the goal of JC is to tackle the adverse effects of being raised under low socio-economic conditions to enable disadvantaged youths obtain and keep well-paying jobs. From the mid- to late-1990s, the National Job Corps Study (NJCS) employed a randomized social experiment to evaluate the effectiveness of JC across various dimensions including employment and earnings, educational achievement, criminal behavior, and health outcomes.[1] The main finding of the NJCS 48 months after randomization reported in Schochet, Burghardt and Glazerman (2001) was that the treatment group earned, on average, a statistically significant 12% more per week than the control group. However, this effect was not homogeneous, varying from $46 per week for whites, $23 for blacks, to -$15 for Hispanics (this last estimate was not statistically significant; see their Table D.14).[2] These differential effects are especially puzzling given similar gains in measurable human capital across the three groups.[3]

Since Hispanics represent a significant and growing proportion of the U.S. population and disproportionately exhibit disadvantaged characteristics, it is important to understand the heterogeneous labor market impacts of JC. We trace these heterogeneous treatment effects to different local labor market conditions. First, we note that a higher and statistically significant level of labor market experience is gained by Hispanics in the control group relative to the treatment group during the time of the NJCS (which does not happen among whites or blacks). After adjusting for this labor market experience the estimated negative effects of JC for

Hispanics decrease considerably. Subsequently, we show that the differential labor market outcomes of the three groups is in part due to the distinct local labor market conditions faced on average by each group, and, importantly, to the differential impact that those labor market conditions have on each group's earnings by randomization status. Specifically, we find that (i) blacks and Hispanics face higher local labor market unemployment rates (LUR) that mitigate the potential gains from JC for them; and (ii) JC is able to "shield" white participants from adverse LUR, but not blacks and Hispanics.

This study contributes to different strands of literature. Most labor market program evaluations in the literature generally focus on individuals that have been in the labor market for some time, frequently avoiding the inclusion of youths such as in e.g., Heckman, Ichimura, Smith and Todd (1998) and Mueser, Troske and Gorislavsky (2007); or doing separate analyses for adults and youths as in e.g., Heckman et al. (2000) and Heckman and Smith (1999). We add to this literature by focusing on young persons, and stressing the importance of "lock-in" effects as defined by van Ours (2004), local labor market conditions, and different early labor market dynamics across racial and ethnic groups.

Recent work in Lechner and Wunsch (2006), Lechner, Miquel and Wunsch (2007), Johansson (2001), Kluve (2007), and others emphasize that the effects of active labor market programs in Europe depend on the state of the economy, typically finding a direct relationship between their effectiveness and the unemployment rate. For the U.S., Hotz, Imbens and Klerman (2006) present evidence for California pointing to an inverse relationship between labor market programs and the unemployment rate. While most of the previous papers rely heavily on aggregate time series variation of economic conditions (e.g. the unemployment rate), we present new evidence for the U.S. exploiting large cross sectional variation of county-specific local unemployment rates. Moreover, by analyzing the differential effect that the state of the local labor market has across different groups defined by race, ethnicity and randomization status, we document that there exist important differences in JC program effectiveness across the demographic groups by randomization status.

Finally, in analyzing different disadvantaged groups, we contribute to the literature on how changing economic conditions impact different low-skilled race and ethnic groups in the U.S.[4] Some distinctive contributions of the present study are the availability of a well-defined set of disadvantaged (low-skilled) youth that allows abstracting from defining such a group from other covariates (e.g., education); having a relatively sizeable group of Hispanics—whom many of the previous literature do not analyze—and being able to determine if a training program aimed at disadvantaged youth can help (or not) alleviate their vulnerability to changing economic conditions.

Our paper is organized as follows. Section II describes the JC program and the NJCS, and presents some findings from the analysis of the original NJCS data to contextualize and motivate the rest of the paper. Section III analyzes in depth JC participants' earnings and the role of post-treatment labor market experience in explaining the lack of a treatment effect of JC on Hispanics' average weekly earnings. Section IV presents our analysis of the role of local labor market conditions and documents their differential impact on subgroups defined by race, ethnicity, and randomization status. Section V concludes and discusses implications of our results.

II. JOB CORPS, THE NATIONAL JOB CORPS STUDY, AND ITS FINDINGS

The Job Corps Program

The purpose of JC—created in 1964—is to provide low-skilled and less-educated young people with marketable skills to enhance their labor market outcomes. According to Department of Labor (1999) and Schochet, Burghardt and Glazerman (2001), it does this by offering academic, vocational, and life and social skills training at over 115 centers throughout the country where nearly all students reside during the program. In addition to education and vocational training, JC 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 citizen or permanent resident. Approximately 70,000 new students participate every year for an average of 8 to 9 months, at a cost of about $1 billion. According to Department of Labor (1999), the typical JC student is an 18-year old, minority (70% of all students), who has dropped out of high school (80%) and reads at a seventh grade level. The residential centers distinguish JC from other job training and education programs and are run by private and not-for-profit groups or by the U.S. Department of Agriculture under contract with the Department of Labor. Currently, the Congressional mandate for JC is derived from the Workforce Investment Act (WIA) of 1998 and administered by the Department of Labor's Employment and Training Administration.[5]

The National Job Corps Study & Randomization

The data collected and used for this paper come from the NJCS, a randomized experiment carried out during the mid- to late-1990s. The sampling frame for the NJCS consisted of first-time JC applicants from nearly all outreach and admissions (OA) agencies—which are responsible for the recruitment and screening of JC applicants—in the 48 contiguous states and the District of Columbia.[6] All pre-screened applications from November 1994 through December 1995 were eligible 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 1996. Approximately 7% of the eligible applicants were assigned to the control group (N = 5,977) while 12% were assigned to the program research group (N = 9,409). The remaining 65,497 eligible applicants were assigned to a group permitted to enroll in JC but were not part of the research sample.

Randomization took place *before* assignment to a JC center. As a result, not all of those randomized into the research treatment group enrolled in JC (73% of the treatment group enrolled). Meanwhile control group members were barred from enrolling in JC 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 JC 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 JC.[7]

The process analysis reported in Johnson et al. (1999) indicated that randomization was for the most part successfully implemented. 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 JC before the three-year embargo period had elapsed. The process analysis also concluded that the study had at most a modest effect on the program itself.

One of the main reasons why social experiments are employed is the notion that due to randomization the treatment and control group have the same distribution of observed and unobserved characteristics, allowing the direct comparison between both groups. Burghardt et al. (1999) describe the randomization design employed in the NJCS, which was undertaken on the entire sample of JC eligible applicants but without particular consideration for race or ethnicity. As long as the original randomization is valid, partitioning the sample into racial and ethnic groups should yield treatment effects estimates that are unbiased for the corresponding parameters. Nevertheless, adjusting for pre-treatment covariates should improve the precision of the estimates, and therefore we control for such covariates throughout the paper. We document the extent to which randomization aligns the pre-treatment covariates of treatment and control groups by race and ethnicity in the Internet Appendix to the paper, concluding that there are just a few imbalanced covariates, although Hispanics show a higher proportion of them.

The NJCS Experimental Estimator and Findings

The original NJCS program evaluation reported in Schochet (2001) 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 JC, and individuals in the control group that enroll in JC before the three-year embargo. More specifically, let R_i be a binary variable indicating whether an eligible JC 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 JC ($D_i = 0$) and vice-versa.[8] 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 JC, $P_{T(1)}$, minus the proportion of those individuals in the control group that enroll in JC before the end of the three-year embargo, $P_{C(1)}$. Using this estimator, the effect of JC on the "compliers" is

(1) $$DM_{comp} = [\bar{Y}(1)_{16} - \bar{Y}(0)_{16}] / [P_{T(1)} - P_{C(1)}],$$

where $\overline{Y}(1)_{16}$ is the sample average of weekly earnings for individuals in the treatment group (R = 1) in quarter 16 and $\overline{Y}(0)_{16}$ is the sample average of weekly earnings for individuals in the control group (R = 0) in quarter 16.[9]

The first row of Table 1 reports the original NJCS estimates, which are based on average weekly earnings in quarter 16 for the entire sample but employ average weekly earnings in year 4 for the estimates by race and ethnic group.[10] We employ average weekly earnings in quarter 16 as the outcome since it is the most recent measure available, but present in this table estimates that use average weekly earnings in year 4 for comparison.[11] 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.[12] In the second row of Table 1 we are able to replicate the NJCS estimates using the entire 48-month sample. These estimates can be used to gauge the effect of using 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 and ethnicity. Compared to year 4 estimates, the quarter 16 estimate for the overall sample is higher by 14%, but for whites it is larger by about 26%, 8% for blacks, and for Hispanics it is larger in absolute value by 57%. Therefore, we believe that this measure results in conservative estimates of the effect of JC for Hispanics, our group of interest, but not necessarily for the other two subgroups.

[TABLE 1 HERE]

Another informative parameter typically estimated in social experiments is the "intention-to-treat" (ITT), which simply compares earnings of individuals by random assignment, R_i, instead of actual receipt of JC training, D_i, thereby ignoring non-compliance. The main advantage of ITT is that no assumptions are needed for estimation in addition to the validity of randomization, while being a policy relevant parameter since it reflects how the availability of JC affects participant's outcomes. See, for example, Heckman, LaLonde and Smith (1999). The third row of Table 2 reports ITT estimates using the entire 48-month sample. As expected, the ITT estimates are about 70% of the corresponding previous estimates, since that is the factor of adjustment for "compliers" in equation (1). In the subsequent sections, we will focus attention on estimation of ITT parameters given their straightforward interpretation.[13]

In order to analyze potential explanations for the estimated lack of effects of JC on Hispanics found in the NJCS, we need to restrict the original NJCS sample to those individuals without missing values in relevant covariates available in the NJCS data.[14] The bottom panel of Table 1 shows the LATE and ITT estimates (without controlling for covariates) using the "restricted" sample to be used in the rest of the paper. The main feature of these estimates is that they are remarkably similar to those that use the entire sample.

III. ANALYZING EARNINGS IN QUARTER 16 AND LABOR MARKET EXPERIENCE

Adjusting for Pre-treatment Variables

Using the restricted sample, we first present estimates that adjust for the rich set of (pre-treatment) covariates available in the NJCS data. Panel I of Table 2 reports estimates of ITT using our "baseline specification" for Hispanics, whites, and blacks employing average weekly earnings in quarter 16 as the outcome of interest.[15] In each case, the estimates are obtained with linear regression using the following specification:[16]

$$Y_i = \alpha R_i + \beta' X_i + \varepsilon_i \qquad (2)$$

where Y_i is average weekly earnings in quarter 16, X_i is the set of pre-treatment variables including a constant, ε_i is a stochastic disturbance with mean zero, and R_i is a randomization indicator used to capture the ITT effect. The set of variables in X_i included in the baseline specification are those listed in Table A.1 in the Data Appendix, which include interactions of selected variables with gender.[17] All models pool all individuals and treatment effects are identified with interactions of randomization status with indicators for race/ethnicity.

Comparing the differences-in-means estimates of Table 1 to those that adjust for covariates in Panel I of Table 2, the two sets of estimates are qualitatively similar in that Hispanics experience negative effects from JC training while whites and blacks experience positive effects, although there are some quantitative differences. While the ITT estimates are smaller in absolute value for the three groups, the difference is largest for Hispanics, for whom the estimates drop from -21.3 to -13.5 (neither statistically significant). For whites and blacks, the difference is negligible. The relatively large difference in point estimates resulting from covariate adjustment on Hispanics is likely due to the fact that they exhibit the largest amount of differences in pre-treatment variables between treatment and control groups (as discussed in the Data Appendix). This misalignment of pre-treatment covariates accounts for a portion though not all of Hispanics' estimated lack of effects of JC.

[TABLE 2 HERE]

<u>Role of Post-treatment Labor Market Experience</u>

We now analyze the potential role that the higher accumulation of labor market experience by control-group members, referred to as the "lock-in effect" by van Ours (2004), plays in the estimated JC treatment effects. Post-randomization labor market experience (hereafter referred to as "experience") is measured as the total average hours worked per week over the 48-month duration of the NJCS. Comparing treatment and control group averages of this variable shows that control-group Hispanics gained a statistically significant 1.85 hours of additional experience per week relative to the treatment-group (21.1 vs. 19.3). Since neither whites (25.4 vs. 25.3) nor blacks (18.6 vs. 18.6) show a similar pattern of experience accumulation, it is justified to explore its role in the estimated lack of effects of JC on Hispanics.[18]

Panels II and III in Table 2 present linear regression estimates of ITT (similar to those of Panel I) that directly control for post-treatment labor market experience in different ways. Importantly, we point out that these estimates are not to be interpreted as having causal interpretation since the experience accumulated during the time of the study is a post-treatment variable that is affected by the treatment. See Imbens (2004), Lechner (2008), and Rosenbaum (1984).[19] An intuitive (albeit informal) way to interpret these estimates is to note that, given the documented lack of effects of JC on Hispanics, if there is an estimated increase in ITT for Hispanics after controlling for experience then this is a "plausible" factor through which JC fails to work for Hispanics.

The ITT estimates in Panel II of Table 2 control for the level of experience as well as an interaction between experience and the randomization indicator (R_i).[20] They show that experience has a positive and highly significant effect (7.9) on the earnings of control-group individuals. For individuals in the treatment group, the effect of experience (in weeks) grows by 0.6. In other words, the return to experience increases nearly 8 percent for those in the treatment group from \$7.9 per hour of weekly experience to \$8.5. The p-value on this increase is 0.12. Note that the difference in the estimated ITT effects between Panels I and II conforms to the expectation: all three groups show a smaller treatment effect (in absolute value) after controlling for experience. The change is not large for Hispanics, however. Surprisingly, the estimated ITT for whites falls by approximately 25 percent and that of blacks is almost halved and loses statistical significance.

Panel III enriches the specification of Panel II by allowing the coefficients on experience and on the interaction between experience and the randomization indicator to differ by race/ethnicity. Here, the relationship between experience and the level of earnings for control group members varies by race/ethnicity: 8.5 for Hispanics, 8.3 for whites, and 7.0 for blacks. Even though the estimates are fairly similar, their equality can be rejected at the 5 percent significance level. The coefficients on the interaction between experience and the randomization indicator also differ by race/ethnicity (0.3 for Hispanics, 1.3 for whites, and 0.3 for blacks), although they are not statistically different from each other and only that of blacks' is statistically significant. Thus, the returns to experience appear not to differ across race/ethnicity for control and treatment group members, as the p-value of a test that they are jointly statistically insignificant is 0.12

The most notable result in panel III is in the estimated ITT parameters. Accounting for experience in a flexible way results in non-significant treatment effects for all groups (marginally for blacks) and substantially reduces the point estimates for Hispanics and whites (in absolute value). These estimates suggest that a "lock-in" effect is present for all racial and ethnic groups. The estimates do not fully account for the estimated lack of effect of JC on Hispanics since the point estimate of their treatment effect is still negative and statistically insignificant. Nevertheless, the point estimate suggests that experience does play a role since it drops to -6.1 compared to the -13.5 in Panel I and the NJCS estimate of -21.3.[21]

A plausible way in which a differential accumulation of labor market experience might arise—both across randomization status and racial/ethnic groups—is differential local labor market conditions. To analyze this possibility directly, the next section employs local labor market unemployment rates to explore their role in the magnitude of the estimated ITT effects.

IV. LOCAL LABOR MARKET CONDITIONS AND THE EFFECTIVENESS OF JC

This section employs variation in the LUR at the county level to analyze differences in labor market outcomes across racial and ethnic groups, broken down by JC randomization. This allows us to gain additional insight of how disadvantaged youth groups fare in the labor market under different economic conditions.

The program evaluation literature has recently been concerned with the important question of how the effectiveness of active labor market programs is influenced by changing economic conditions. For example, Lechner and Wunsch (2006) follow participation in German active labor market programs for 10 years and using matching methods conclude that a direct relationship exists between the effectiveness of these programs and variations in the (aggregate) German unemployment rate over time. This direct relationship has been reported as well by Johansson (2001) and Kluve (2007) using different data and approaches.[22]

In a recent study re-analyzing data from California's influential Greater Avenues to Independence (GAIN) program, Hotz, Imbens and Klerman (2006) estimate average treatment effects of alternative programs, controlling for post-treatment local labor market conditions in four counties under study.[23] They find that these controls are statistically significant in the estimation of treatment effects on employment but not earnings, and that including them reduces the effects in absolute value to the point of mostly losing statistical significance. We add to this literature by exploiting large cross sectional variability in the LUR in U.S. counties—presumably tightly related to the economic reality of the individuals—as opposed to aggregate time variation.

In addition, we pay special attention to the differential effect of the LUR on different subgroups of participants defined by race/ethnicity and randomization status.

By focusing on subgroups defined by race, ethnicity, and randomization status, we also add to the literature on how economic conditions differentially impact low-skilled minorities, who are considered particularly vulnerable to adverse economic conditions. In one of the most comprehensive studies on this subject, Hoynes (2000) finds that, in general, low-skilled minorities (females, blacks, and Hispanics) are disproportionately impacted by the business cycle relative to whites. Her study, which is one of the few that analyze Hispanics, also documents that Hispanic males are more like whites in terms of their response to business cycle variations. DeFreitas (1991) documents that the unemployment rate for young Hispanics improves relative to whites during expansionary periods, but falls more than proportionally at the start of recessionary periods. Both of these studies use CPS data.[24]

While the literature on the labor market dynamics of Hispanics is not as abundant as that of blacks, some studies document their distinctive dynamics. For instance, it is known that Hispanics—who are disproportionally disadvantaged—have relatively large labor force participation and higher labor market attachment.[25] In addition, in the context of the immigrant status of Hispanics, studies such as DeFreitas (1991) and Fry and Lowell (2002) find that young second- and third-generation Hispanics earn more than white and black youth, although their unemployment rate is still 50% higher than the rate for white youths. Our results below complement previous studies by focusing on disadvantaged youth eligible for an active labor market program—precisely those individuals that policymakers have identified as source of concern due to their vulnerability—examining any differential impacts of economic conditions across groups, and analyzing if JC changes these impacts in any noticeable way. Furthermore, the fact that only legal permanent residents are eligible for the JC program ensures that undocumented immigrants, the majority of whom are Hispanic, have no bearing on our results.

The LURs to be used are constructed for each individual in our sample based on his/her county of residence at the time of the 48-month survey. Constructing these variables implies matching restricted-use NJCS data on zip-code of residence (made available to us by Mathematica Policy Research, Inc.) to county-level unemployment rates from the Local Area Unemployment Statistics (LAUS) for different years. Furthermore, we construct race/ethnicity-specific LURs employing individuals ages 16-35 in the 2000 5% IPUMS that belong to each race/ethnicity. For the results presented below, however, we employ the former LAUS rates for 2000. A detailed discussion regarding the construction of these variables is in the Data Appendix, while alternative results employing alternative unemployment rates (to explore the robustness of our results) can be found in the Internet Appendix.

It is important to note that the LURs are not affected by the treatment (as was the case with labor market experience) as long as there are no "general equilibrium" effects of JC training in the local economies, which is unlikely given the small number of participants relative to the local populations. In fact, the exercise we perform below is similar to that in Hotz, Imbens and Klerman (2006) described above. Given the randomization into JC in our sample, the interpretation of the estimates below is of ITT estimates that adjust for differences in LURs faced by individuals.

Before turning to the LUR-adjusted ITT estimates, it is interesting to look at a simple linear relationship between earnings and LUR. Figure 1 shows the linear-regression fitted relationship between average weekly earnings in quarter 16 and the LUR. This is done by race/ethnicity and separately for those in either treatment (solid line) or control (dashed line) group. Not surprisingly, given that the difference between the dashed and solid lines can be thought of the (unadjusted) ITT parameter for a given LUR, the solid line is generally above the dashed line for blacks and whites, but below the dashed line for Hispanics. Of special interest are the slopes of the lines in the figure: the slope of the solid line for blacks and Hispanics is steeper than the dashed line, suggesting that as the LUR increases the weekly earnings for those in the treatment group decreases more than those in the control group. In fact, the figure for Hispanics shows that the earnings of those in the treatment group are higher than those of control-group individuals only at a low enough LUR. Whites display the exact opposite pattern: the difference between the solid and dashed lines increases with the level of the LUR. We now move on to a formal regression analysis.

[FIGURE 1]

The set of estimates that control for the LUR are presented in Table 3 (Panels II and III). Similar to Table 2, they include the same set of control variables as in the baseline specification (reproduced here for reference), and allow different degrees of flexibility. Panel II includes the LUR and an interaction between the LUR and randomization status. The coefficient on LUR is -9.6 and highly statistically significant, implying that for every one percentage point increase in the unemployment rate control group members see their average quarter-16 weekly earnings fall by $9.6. The coefficient on the interaction between the LUR and the randomization indicator is -1.4, although it is not statistically significant. In terms of the ITT estimates, including the LUR and its interaction with randomization status results in an increase of approximately $6.00 for each race/ethnic group. The ITT estimates are also less precisely estimated after adding the LUR controls.

[TABLE 3 HERE]

We allow the effect of the LUR and its interaction with randomization status to differ across racial/ethnic groups in panel III, which reveals important heterogeneities. In this specification, the estimated ITT effects differ considerably from the previous panels: the effect for Hispanics becomes large and positive at 18.6—now of similar magnitude to that of blacks' at 22.9. Perhaps surprisingly, whites' estimated ITT effect is now smallest at 9.4. Interestingly, none of these ITT effects is statistically significant (not just that of Hispanics), which is similar to some of the results obtained by Hotz, Imbens and Klerman (2006) when they control for local labor market conditions. These results represent new evidence on how local economic conditions can dramatically impact the effectiveness of active labor market programs.

Looking at the interactions of LUR with randomization status and race/ethnicity in the specification in panel III allows analyzing whether JC mitigates or exacerbate the consequences of poor labor market conditions for the different groups. The coefficient on the LUR for Hispanics, whites, and blacks are respectively (p-values in parenthesis) -5.4 (0.14), -18.2 (0.00), and -8.5 (0.02). An F-test of the hypothesis that these coefficients are equal to each other is rejected at the 5 percent significance level. These estimates indicate that the LUR affects control group members from each group differently. For example, a one percentage-point increase in the LUR lowers the average weekly earnings of Hispanics and blacks by $5.40 and $8.50, respectively; while control-group whites experience a decrease of over $18 in their average weekly earnings.

The way in which the LUR affects treatment group members is also heterogeneous, although the estimated coefficients are not statistically significant. While the imprecision of the estimates may be the result of the intrinsic heterogeneity of the effects or the relatively small sample sizes used to estimate these interactions, we believe that the sign of the point estimates are nevertheless revealing. For Hispanics and blacks the coefficients on the interaction between the LUR and the treatment indicator are negative (-6.9 and -1.2, respectively), suggesting that treatment-group members are more adversely impacted by unfavorable labor market conditions than those assigned to the control group. Specifically, a one percentage-point increase in the LUR reduces the average weekly earnings of Hispanics assigned to the treatment group by $12.3 compared to a decrease of $5.4 for those assigned to the control group. Similarly, black treatment-group members experience a decrease of $9.7 in average weekly earnings relative to $8.5 for those in the control group. In contrast, for whites, the estimated coefficient of the interaction of LUR and randomization status is +6.4, implying that treatment group members are less harmed by adverse LUR relative to control-group members.

These point estimates have strong implications. They suggests that JC training helps whites overcome adverse economic conditions, while Hispanics and blacks do not benefit from the program in this way. In addition, as a whole, these estimates constitute new evidence on the way changing economic conditions (e.g., variation in local unemployment rates) differentially impact disadvantaged and unskilled youth, both by race and ethnicity and by eligibility to a government-sponsored active labor market program. In summary, the effects estimated within this section suggest that the estimated ITT effects of JC are highly dependent on the state of the individual's local labor market conditions, and that the effect of those conditions vary dramatically by race/ethnicity.

To increase the confidence in the results presented herein, we conducted a number of robustness checks that are reported in the Internet Appendix to the paper while briefly mentioned here. First, we explore whether using different definitions of the local unemployment rate from the one employed thus far (year 2000) changes our conclusions in any way. To this end, we produced results using an earlier local unemployment rate available (for 1997) obtaining essentially the same results. Similarly, employing the race/ethnicity-specific LUR constructed using individuals aged 16-35 that belong to each group from the 2000 5% IPUMS results in qualitatively similar results, albeit stronger.[26] Second, we explore if mobility of individuals in our sample across counties from the time of the random assignment to that of the 48-month interview—the one actually used—changes our results. To this end, we obtain estimates for a sample of individuals that moved less than 50 miles from their baseline survey location (including the non-movers) and obtained essentially the same results. Thirdly, similar conclusions are obtained considering (i) LATE estimates, and (ii) estimates that specify the outcome (weekly earnings) in differences between quarter 16 and quarter 0.

Finally, we perform a counterfactual exercise to learn how the model of Panel III (Table 3) predicts the ITT effects would change under counterfactual LURs. The first panel of Table 4 present predicted ITT effects using the average LUR faced by each of the three groups. Perhaps surprisingly, the average LUR faced by each group is not dramatically different: 4.77, 4.05, and 4.35 percent for Hispanics, whites and blacks, respectively. As a result, the predicted ITT effects are very similar across rows, indicating a negative and insignificant effect for Hispanics and positive and highly significant effects for whites and blacks. A test of equality of the effects across race and ethnicity (last column) strongly rejects that hypothesis. The main message to take from these rows is that differences in the average LUR faced by each group are not the main source of the disparate estimated ITT effects of JC.

[TABLE 4 HERE]

Next, we consider counterfactual values of LUR between 1 percent and 7 percent and compute the predicted ITT effects of JC, presented in the middle panel of Table 4. Using a common LUR allows us to learn the kind of LUR needed to hypothetically equate the ITT effects of JC across racial and ethnic groups. Considering an extremely low LUR of 1 percent yields predicted ITT effects of 11.8 (Hispanics), 15.8 (whites) and 21.7 (blacks), which are statistically indistinguishable according to the p-value of the test of their equality in the last column. Nevertheless, all of these estimates are individually statistically insignificant. Similar results are obtained under a 2 percent LUR. While unrealistic, a 1 or 2 percent LUR is indicative of the kind of rate that would result in a similar benefit of JC across the three groups, underscoring the importance of the substantially different impacts of the stance of economic conditions on these groups. At the other extreme, for a LUR of 6 or 7 percent the predicted ITT effects are negative and statistically significant for Hispanics, large and significant for whites, and insignificant for blacks.

The last panel in Table 4 presents predicted ITT effects of JC employing the average race/ethnicity-specific LUR as counterfactual, for illustrative purposes.[27] To be consistent, the predicted ITT effects are computed with the estimated coefficients of a similar specification to Panel III of Table 3 that uses the race/ethnicity-specific LUR (these estimates are reported in the Internet Appendix). These estimates show that according to this model the predicted ITT effects are positive, statistically significant, and of similar magnitude across the three groups when the average white-specific LUR is used. However, when either the Hispanic-specific or the black-specific LUR are used, the predicted ITT effects become negative for Hispanics and decrease for blacks. Taken at face value, these counterfactual results reinforce the notion that the three groups face quite different LURs and are differentially impacted by them.

V. CONCLUSION

We provide an explanation to the puzzling result in the NJCS that JC, a federally funded residential job training program, has no earnings effect on Hispanic youth 48 months after randomization. Starting with the observation that Hispanics, whites, and blacks differ in their accumulation of labor market experience during the time of the study, we find that the differential labor market outcomes of each group are related to the different levels of LUR they face. Furthermore, we document that these three groups exhibit differential impacts on their earnings from the LUR they face that also appear to vary by whether they have been assigned to the treatment or control group. In particular, using our preferred (and most flexible) specification, we find that (i) blacks and Hispanics face higher average LURs that mitigate the potential benefits from JC for them; and (ii) JC is able to “shield” white participants from adverse LUR, but not black and Hispanic participants.

In obtaining these results, we also provide new cross-sectional evidence indicating that the efficacy of active labor market programs is highly dependent on the economic conditions present, as previously documented elsewhere using mainly time series variability. Contrary to some of those studies, we document an inverse relationship between the LUR and the effectiveness of the training program we analyze—which may be due to our use of cross sectional variability in LURs. In addition, we provide new evidence of the differential impact of economic conditions on policy-relevant demographic groups of the U.S.: disadvantage and unskilled youth of three different race and ethnicities. Our evidence suggests noticeably different impacts of the LUR on whites relative to Hispanics and blacks (who share similar but not identical impacts), that also vary by whether they undergo JC training. In this way, we complement existing work on how economic conditions impact low-skilled minorities.

Why does JC not work for Hispanics? According to our analysis, important factors are the higher local unemployment rates faced by Hispanics and the differential effects LURs have on them, especially relative to whites. Our counterfactual exercise suggests that the latter is more relevant than the former, as the predicted effects change little for Hispanics when the average (baseline) LUR faced by any of the other groups is assumed to apply to them. Therefore, while Hispanics achieve program milestones in similar rates as whites and blacks (e.g., they "learn" during the program), they do not seem to reap earnings benefits 48-months after the NJCS randomization.

An important implication is that, for Hispanics, job training alone is not enough to help youth succeed in the labor markets they face, standing in stark contrast to what we find for whites. Our results also suggest that the effects of JC for blacks are sensitive and adversely affected by local labor markets with high unemployment rates. Given the robust economy of the late1990s—when the NJCS was performed—it is particularly important to think about the effectiveness of Job Corps during downturns, when the economy provides less favorable local labor market conditions.

A question left unanswered in the present paper pertains to the reasons behind the "structural" differences in labor market outcomes between Hispanics and whites—and to a lesser extent blacks. One obvious possibility is discrimination in the labor market, but others are plausible as well, such as the industry composition of the locations the different groups tend to live in, or their ability to find jobs—such as informal employment networks—and keep them.

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TABLE 1

Differences-in-Means Estimators for the Impact of Job Corps Using Different Samples and Earnings Measures

	Total		Hispanics		Whites		Blacks	
	Year 4	Quarter 16	Year 4	Quarter 16	Year 4	Quarter 16	Year 4	Quarter 16
	Entire 48th Month Sample							
1. NCJS Study Estimator (LATE)[a]	22.1	25.2	-15.1	--	46.2	--	22.8	--
p-value	(0.00)	(0.00)	(0.19)	--	(0.01)	--	(0.01)	--
2. LATE Estimate	22.1	25.2	-15.1	-23.6	46.2	58.0	22.8	24.7
p-value	(0.00)	(0.00)	(0.25)	(0.10)	(0.00)	(0.00)	(0.00)	(0.00)
3. ITT Estimate	15.9	18.1	-10.9	-17.0	32.2	40.1	16.61	18.3
p-value	(0.00)	(0.00)	(0.24)	(0.10)	(0.00)	(0.00)	(0.00)	(0.00)
N	10896	10872	1883	1878	2865	2863	5357	5343
	Restricted Sample [b]							
4. LATE Estimate	21.8	23.4	-20.4	-29.1	44.8	56.3	23.4	25.3
p-value	(0.00)	(0.00)	(0.16)	(0.07)	(0.00)	(0.00)	(0.00)	(0.00)
5. ITT Estimate	15.9	17.0	-14.9	-21.3	31.6	39.7	17.2	18.6
p-value	(0.00)	(0.00)	(0.16)	(0.07)	(0.00)	(0.00)	(0.00)	(0.00)
N	9108		1570		2398		4647	

Note: The earnings measures are average weekly earnings in year 4 and average weekly earnings in quarter 16. The NJCS only reports estimates for the different racial and ethnic groups using average weekly earnings in year 4, which we present here for comparison. All estimates are adjusted using 48-month sample survey and nonresponse weights. The difference in sample size between Total and the 3 race/ethnicity groups are for a relatively small asian group of participants.

[a] Schochet, Burghardt, and Glazerman (2001, Tables VI.4 and D.14). Results using quarter 16 earnings are not provided for racial and ethnic groups.

[b] See Data Appendix for a description of the Restricted Sample.

TABLE 2

Estimates of the Intention-to-Treat (ITT) Effect of Job Corps on Weekly Earnings in Quarter 16 Adjusting for Experience

	Panel I (Baseline Specification)			Panel II			Panel III		
	Hispanics	Whites	Blacks	Hispanics	Whites	Blacks	Hispanics	Whites	Blacks
ITT	-13.5	36.7	17.3	-12.3	27.4	9.8	-6.1	8.9	15.3
	(0.22)	(0.00)	(0.00)	(0.28)	(0.01)	(0.14)	(0.67)	(0.53)	(0.09)
Experience					7.9		8.5	8.3	7.0
					0.0		(0.00)	(0.00)	(0.00)
							P-value for Test of Null that Experience Does not Differ by Race/Ethnicity:		
								(0.03)	
R_i * Experience					0.6		0.3	1.3	0.3
					(0.12)		(0.68)	(0.64)	(0.02)
							P-value for Test of Null that R_i * Experience Does not Differ by Race/Ethnicity:		
								(0.37)	
							P-value for Test of Null that Experience Does not Belong in the Model:		
								(0.00)	

Notes: P-values in parentheses. Number of observations: Hispanics: 1,570; Whites: 2,398; Blacks: 4,647. All estimates adjusted using 48-month sample survey and nonresponse weights. The experience variable is defined as average hours worked per week during the NJCS. The estimates adjusting for experience do not have acusal interpretation (see text for details). The p-value of tests reported in the table are F-tests for the null hypothesis indicated.

TABLE 3

Estimates of the Intention-to-Treat (ITT) Effect of Job Corps on Weekly Earnings in Quarter 16 Adjusting for the Local Unemployment Rate (LUR)

	Panel I (Baseline Specification)			Panel II			Panel III		
	Hispanics	Whites	Blacks	Hispanics	Whites	Blacks	Hispanics	Whites	Blacks
ITT	-13.5	36.7	17.3	-8.0	41.4	23.6	18.6	9.4	22.9
	(0.22)	(0.00)	(0.00)	(0.66)	(0.01)	(0.10)	(0.50)	(0.73)	(0.25)
LUR					-9.6		-5.4	-18.2	-8.5
					(0.00)		(0.14)	(0.00)	(0.02)
								P-value for Test of Null that LUR Does not Differ by Race/Ethnicity:	
								(0.04)	
R_i * LUR					-1.4		-6.9	6.4	-1.2
					(0.65)		(0.18)	(0.30)	(0.78)
								P-value for Test of Null that R_i * LUR Does not Differ by Race/Ethnicity:	
								(0.25)	
								P-value for Test of Null that LUR Does not Belong in the Model:	
								(0.00)	

Notes: P-values in parentheses. Number of observations: Hispanics: 1,570; Whites: 2,398; Blacks: 4,647. All estimates adjusted using 48-month sample survey and nonresponse weights. The LUR variable is defined as the local unemployment rate for each individual at the county of residence level. See Data Appendix for details on the construction of this variable. The p-value of tests reported in the table are F-tests for the null hypothesis indicated.

TABLE 4

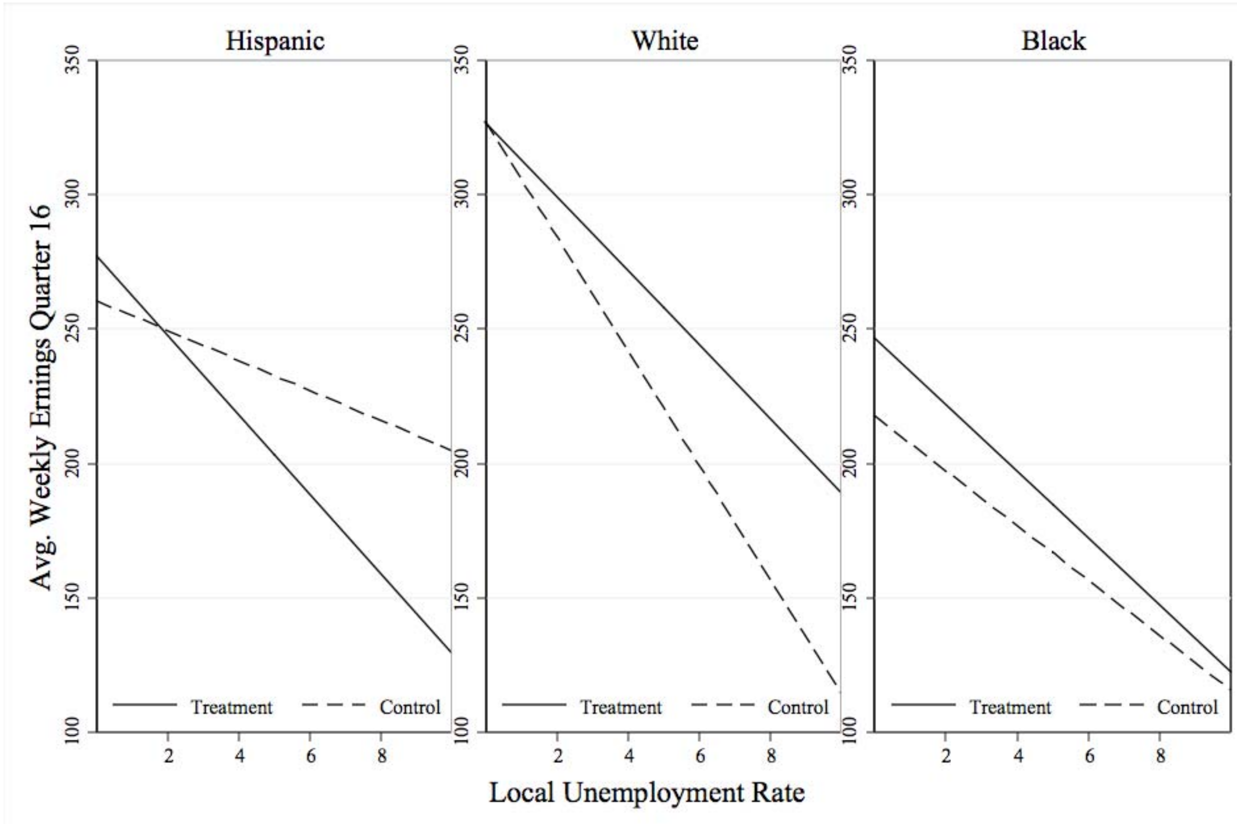
Estimates of the Intention-to-Treat (ITT) Effect of Job Corps using Counterfactual Local Unemployment Rates.

Counterfactual LUR	Hispanic		White		Black		Test of Equality of ITT Estimates across columns (p-value)
Average LUR							
Hispanics' Avg. LUR (4.77%)	-14.14		39.96	***	17.22	***	0.01
Whites' Avg. LUR (4.05%)	-9.19		35.34	***	18.08	***	0.00
Blacks' Avg. LUR (4.35%)	-11.25		37.26	***	17.72	***	0.00
Alternative LUR values							
1 percent	11.76		15.79		21.74		0.93
2 percent	4.89		22.2		20.54	*	0.74
3 percent	-1.98		28.61	***	19.34	**	0.25
4 percent	-8.85		35.02	***	18.14	***	0.01
5 percent	-15.72		41.43	***	16.94	***	0.00
6 percent	-22.59	*	47.84	***	15.74	*	0.00
7 percent	-29.46	**	54.25	***	14.54		0.00
Average Race/Ethnicity-Specific IPUMS LUR							
Hispanic-Specific Avg. LUR (14.75%)	-43.03	*	145.14	**	16.21	**	0.01
White-Specific Avg. LUR (5.3%)	21.89		38.83	***	29.92		0.82
Black-Specific Avg. LUR (10.16%)	-11.50		93.50	***	22.87		0.01

Notes: *, **, *** significant at the 10%, 5%, and 1% level, respectively. The ITT estimates using the counterfactual LUR are computed using the estimated coefficients reported on Panel III of Table 3. The race/ethnicity-specific IPUMS LUR employed in the last panel is constructed using individuals aged 16-35 that belong to the race or ethnic group from the Census' 5% IPUMS (see Data Appendix for details). The corresponding counterfactual ITT effect is computed using estimates of a model similar to that of Panel III Table 3 but using the race/ethnicity-specific LUR (estimates reported in the Internet Appendix to the paper).

FIGURE 1.

Relationship Between Average Weekly Earnings in Quarter 16 and Local Unemployment Rates by Race/Ethnicity and Randomization Status



DATA APPENDIX

Local Labor Market Unemployment Rates (LUR)

This appendix describes the construction of the LUR variables and some of the issues arising in matching these variables to the restricted-use NJCS data. Matching the various data to the same county posed some challenges, and the steps taken to arrive at our variables are described here.

The geographic unit of analysis is the county or county group, and all county-level variables are obtained from data widely available from the Census Bureau. The restricted-use NJCS data contains the respondent's ZIP code of residence at the time of the 48-month follow up interview, between December 1998 and May 2000. See Schochet (2001). The Census data is organized at the PUMA level for the PUMS (public use micro sample), and the county-level information from the SF-3 data is based on quasi-ZIP code information gathered in 1999. Each data has unique characteristics. The 5% PUMS data is organized around the PUMA, a geographic construct that may span several counties or be within a county so as to be of the size (approximately 100,000 persons). Thus, it is possible that three counties each the size of 35,000 will be combined into one PUMA, while a large city will be broken up into multiple PUMAs. For this reason, the common geographic measure is the county or county-group. That is, observations taken from PUMAs contained within one county were defined as belonging to the same county, while observations in PUMAs that spanned multiple counties are grouped into a "county group." A crosswalk between the PUMA, county, MSA, PMSA, and a quasi-ZIP code variable was obtained from the MABLE '98/Geocorr Geographic Correspondence Engine, available at http://mcdc2.missouri.edu/websas/geocorr2k.html. This engine provides a crosswalk between the ZIP Census Tabulation Area variable (ZCTA) and the other Census geographic units. The ZCTA “in most instances” is the same as the ZIP code for an area, but differ in the sense that ZCTA are defined to have a geographic meaning, which is not the case for ZIP code, and is based on ZIP codes that existed in 2000. See http://www.census.gov/geo/ZCTA/zcta.html for further details regarding ZCTAs.

Therefore all of the 2000 Census data was organized into a county or county group and the corresponding ZCTA was merged. The ZTCA was then merged with the ZIP code reported at the 48-month interview in the NJCS data. Since the NJCS ZIP code is self-reported, we assume that the most recent ZIP code is used, and are therefore confident that the ZIP code is merged with the correct county. Because the NJCS data was merged “up” to the county level, this minimized geographic mismatch between the Census and NJCS data sets. In certain cases (about 30%), a single ZTCA is found in more than one county or county group. In this case the labor market statistics assigned to this zip code are an average of the county or county groups that contain that zip code weighted by the number of people in that zip code that live in a certain county or county group. Finally, in a small number of cases (2.6%), there was no match between the ZIP code reported by the JC participant and the ZTCA. In this case the closest ZTCA was used. The results are not materially different if these observations are excluded.

Data pertaining to the race/ethnic group-specific LUR used in the results reported in the Internet Appendix and the counterfactual exercises in Table 4 are derived from the 2000 5% PUMS file and the Summary File-3. The Summary File-3 from the 2000 Census is a 100% count of the population. The 5% PUMS makes it possible to obtain unemployment estimates for whites, blacks, and Hispanics ages 16-35, with the potential drawback that these estimates are based on a 5% sample from the population, rather than a count of all individuals. To the extent that young persons and minorities are less likely to be counted in the Census, these estimates will be less than ideal. We exclude persons living in group quarters, in the military, or with missing employment status. Additionally, for precision, all county groups with fewer than 25 observations are excluded from these calculations.

The "Restricted" Sample

The original NJCS sample consists of all individuals who completed a 48-month survey since the compliance-adjusted difference-in-means estimator reported in the NJCS only requires information at the 48th month. In order to adjust for pre-treatment covariates and employ our variables of interest, we need to restrict the entire 48-month sample. 10,896 or 10,872 (year 4 or quarter 16 earnings) individuals have measures required to estimate the Full Sample ITT parameters in row 1 of Table 1. After dropping individuals whose race/ethnicity is not white, black or Hispanic and restricting the sample to those individuals that have both year 4 and quarter 16 earnings we are left with a sample of 9,975. Next, we drop 156 individuals who did not have any information from the baseline interview, 245 that lack information on post-treatment labor market experience, and 959 for whom it was not possible to match them to a local unemployment rate. These restrictions leave a sample size of 8,615. Of these, 1,413 individuals are missing at least one response from the list of pre-treatment covariates that we use (enumerated in Table A.1 below). To avoid losing any more observations, we use dummy variables for non-response in any of the control variables employed. To check the robustness of this choice, the Internet Appendix contains estimates using the sample that drops those 1,413 individuals that have a non-response to any of our control variables, finding similar results to those reported here. It is worth noting that individuals excluded are proportionately distributed across race and ethnic groups and that our restricted sample is consistent with the overall profile of the total JC population. Rows 2 and 3 of Table 2 present estimates of LATE and ITT using the restricted sample, which are remarkably similar to those using the entire 48-month sample (first row).

Baseline Specification

Our baseline specification controls for the pre-treatment covariates listed in Table A.1. This table also contains summary statistics by race/ethnicity and random assignment, and p-values of a test of equality of means between control and treatment groups. The Hispanic control and treatment groups show more statistically significant differences in mean characteristics of these variables than any of the other two groups, which can be a result of representing a smaller percent of the entire randomized population. They exhibit significant differences at the 10% level in the percentage of them living in a PMSA, living in a MSA, unemployed at randomization, employed at randomization, ever smoked, knew what JC center wanted to attend, expected to improve ability to get along, and knew a person who attended JC.

TABLE A.1.

Summary Statistics of Variables Used in the Baseline Specification by Control and Treatment Groups (Restricted Sample)[a]

	Higher Order Used	***with Female Indicator***	Hispanic			White			Black		
			Control Mean	Treatment Mean	p-value	Control Mean	Treatment Mean	p-value	Control Mean	Treatment Mean	p-value
Age	Square	x	18.9	18.9	0.53	18.8	18.9	0.95	18.7	18.8	0.03
Percent Female		x	0.46	0.50	0.20	0.38	0.36	0.44	0.48	0.48	0.92
Has Child		x	0.21	0.20	0.85	0.13	0.10	0.04	0.23	0.24	0.73
Percent who are married or cohabitating		x	0.10	0.10	0.64	0.08	0.08	0.97	0.04	0.04	0.75
Percent who are Household Heads		x	0.11	0.11	0.91	0.12	0.10	0.13	0.13	0.13	0.77
Percent living in a MSA		x	0.40	0.47	0.00	0.48	0.48	0.12	0.48	0.47	0.18
Percent living in a PMSA		x	0.49	0.41	0.01	0.14	0.17	0.80	0.35	0.37	0.53
Percent who speak English as a Native Language		x	0.46	0.48	0.47	0.99	0.99	0.03	0.98	0.97	0.02
Percent that have ever been convicted		x	0.13	0.14	0.68	0.22	0.23	0.62	0.13	0.13	0.77
Highest Grade Completed		x	10.1	10.1	0.92	10.1	10.1	0.99	10.1	10.1	0.38
Percent with High School Diploma or GED		x	0.22	0.24	0.32	0.30	0.29	0.59	0.21	0.22	0.63
Percent enrolled in Education or Training in Previous Year		x	0.61	0.64	0.18	0.64	0.63	0.66	0.72	0.70	0.14
Percent unemployed at randomization		x	0.56	0.61	0.03	0.62	0.60	0.53	0.57	0.57	0.82
Percent ever employed		x	0.78	0.80	0.38	0.88	0.89	0.49	0.76	0.76	0.72
Percent employed at randomization		x	0.23	0.19	0.07	0.26	0.28	0.24	0.18	0.19	0.55
Average weekly Pre-treatment Earnings[b]	Cube	x	$114	$111	0.60	$126	$138	0.01	$99	$101	0.60
Percent Report in Good Health			0.43	0.41	0.68	0.45	0.45	0.85	0.38	0.38	0.98
Percent Report in Fair Health			0.13	0.13	0.76	0.13	0.12	0.41	0.12	0.12	0.98
Percent Report in Poor Health			0.01	0.01	0.94	0.01	0.00	0.09	0.01	0.01	0.72
Percent Ever Smoked a Cigarette			0.47	0.52	0.07	0.79	0.77	0.42	0.40	0.41	0.49
Percent Ever Drank Alcohol			0.60	0.62	0.52	0.75	0.76	0.68	0.48	0.50	0.18
Percent Ever Smoked Pot			0.35	0.38	0.35	0.47	0.47	0.90	0.31	0.32	0.58
Percent Had Worries about Job Corps			0.36	0.37	0.73	0.36	0.39	0.17	0.31	0.35	0.00
Percent Knew Type of Job Training Desired			0.85	0.83	0.20	0.88	0.86	0.36	0.85	0.84	0.32
Percent Knew what Center wished to Attend			0.5	0.4	0.02	0.5	0.5	0.06	0.5	0.5	0.52
Percent Expect to Improve Math Skills			0.78	0.75	0.16	0.59	0.59	0.98	0.70	0.73	0.05
Percent Expect to Improve Reading Skills			0.62	0.62	0.99	0.42	0.44	0.43	0.55	0.56	0.56
Percent Expect to Improve Ability to Get Along			0.59	0.66	0.00	0.54	0.56	0.63	0.61	0.61	0.94
Percent Expect to Self Control			0.61	0.60	0.82	0.56	0.59	0.07	0.59	0.56	0.01
Percent Expect to Improve Self Esteem			0.61	0.60	0.80	0.56	0.58	0.23	0.56	0.56	0.94
Percent Expect to Improve Specific Job Skills			1.0	1.0	0.66	1.0	1.0	0.50	1.0	0.9	0.47
Percent Expect to meet Friends			0.70	0.73	0.26	0.70	0.73	0.19	0.68	0.68	0.64
Percent Knew a person who Attened Job Corps			0.63	0.57	0.02	0.50	0.54	0.03	0.79	0.77	0.27
Percent Joined to Reach a Career Goal			1.00	1.00	0.70	0.99	0.99	0.76	0.99	0.99	0.45
Percent Joined to get Training			0.99	0.99	0.66	0.97	0.98	0.10	0.98	0.98	0.93
Percent Joined to get a GED			0.74	0.73	0.65	0.66	0.66	0.88	0.76	0.75	0.68
Percent Joined to be able to find work			0.93	0.92	0.27	0.89	0.89	0.89	0.92	0.91	0.69
Percent Joined to get away from community			0.64	0.60	0.16	0.42	0.49	0.00	0.72	0.71	0.37
Percent Joined to leave home			0.57	0.58	0.66	0.54	0.51	0.18	0.64	0.62	0.28
Percent Joined for other reason			0.7	0.8	0.69	0.7	0.7	0.70	0.7	0.7	0.98
Percent Predicted to Attend nonresidential center			0.15	0.16	0.67	0.08	0.08	0.90	0.20	0.18	0.11
N			635	935		955	1,443		1,816	2,831	

[a] Estimates are weighted using NCJS sampling weights for baseline interview.

[b] Zero if not employed in previous year. In baseline specification a dummy variable is used to indicate zero earnings.

ABBREVIATIONS

GAIN: Greater Avenues to Independence

IPUMS: Integrated Public Use Microdata Series

ITT: Intention to Treat

JC: Job Corps

LATE: Local Average Treatment Effect

LAUS: Local Area Unemployment Statistics

LUR: Local Labor Market Unemployment Rate

NJCS: National Job Corps Study

OA: Outreach and Admissions

WIA: Workforce Investment Act

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Flores-Lagunes: Food and Resource Economics Department, University of Florida, and IZA, Bonn, Germany, P.O. Box 110240, Gainesville, FL 32611-0240, Phone (352) 392-1826 ext. 307, Email: alfonsofl@ufl.edu.

Gonzalez: Ernst & Young and IZA, Bonn, Germany, 560 Mission St, Suite 1600, San Francisco, CA 94105, Phone: (415) 894-8654, E-mail: arturo.gonzalez@ey.com

Neumann: University of California at Merced, School of Social Sciences, Humanities and Arts, P.O. Box 2039, Merced, CA 95344, Phone: (209) 228-4020, E-mail: tneumann@ucmerced.edu

[1] The NJCS was sponsored by the Department of Labor following a mandate from Congress to assess the effectiveness and social value of the Job Corps program.

[2] The lack of impact on Hispanics is perhaps the most prominent "failure" of Job Corps and it can not be explained by individual and institutional variables, including the potential differences across Job Corps centers, according to Schochet, Burghardt and Glazerman (2001).

[3] Based on authors' calculations, the estimated effects of JC on the probability of earning a high school or GED degree is very similar across racial/ethnic groups: 0.17, 0.23, and 0.15 for

Hispanics, whites, and blacks, respectively. Similar effects of JC across groups are obtained on the probability of earning a vocational degree and highest grade completed.

[4] See, for example, Bound and Holzer (1993), Cain and Finnie (1990), Hoynes (2000), Gladden and Taber (2000), among others.

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

[6] The OA centers may include JC centers, but they are also state employment agencies, profit and non-profit firms. See Burghardt et al. (1999).

[7] As is the case with any other survey-based data, an important issue is the potential survey non-response bias. The original NJCS estimates reported below are based on the 48-month interview, for which the effective response rate was 79.9%. While Schochet (2001) reports that the pre-treatment characteristics of treatment and control groups are similar for this sample, our summary statistics presented in the Internet Appendix show some imbalances, especially for Hispanics. Furthermore, using administrative data on individuals included in the survey, Schochet, McConnell and Burghardt (2003) conclude that survey non-respondents have smaller impacts, suggesting that the original NJCS results may be biased upwards.

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

[9] Note that this estimator identifies the local average treatment effect (LATE) parameter of Imbens and Angrist (1994) since it is a Wald estimator where random assignment (Ri) is used as an instrumental variable for the actual receipt of treatment (Di).

[10] 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. We note that using average weekly earnings during the 4th year after randomization in the analysis below yields results that are qualitatively similar to those presented here.

[11] All our estimates throughout the paper employ NJCS sampling weights. An important reason to use them is that the NJCS design set a lower sampling rate for females in the control group since Job Corps officials were concerned that the study would cause slots for residential females to go unfilled given that they are difficult to recruit, as explained in Burghardt et al. (1999).

[12] We point out that Schochet, McConnell and Burghardt (2003) re-examined the survey-based NJCS results using administrative earnings records data, finding smaller and less statistically significant effects of JC on participants. In addition, they find that the impacts disappear in years 5 to 7 after the random assignment. The reasons for the differences in estimates employing survey and administrative records data are not clear and are beyond the scope of this paper.

[13] We point out that the same conclusions are reached when adjusting for non-compliance. These results are available in the Internet Appendix of the paper.

[14] See the Data Appendix for a detailed description of this “restricted” sample and its creation.

[15] In the results presented here, we use as outcome variable the average weekly earnings in quarter 16 *in levels*. In addition, we have also estimated models in which the outcome variable is specified in differences between quarters 16 and 0 (baseline interview). This specification, which accounts for the potential effect of an unobservable fixed effect, yields essentially the same results as those reported here (this may not be surprising given the randomized nature of the data). These alternative results are available in the Internet Appendix of the paper.

[16] We have also estimated more flexible specifications (e.g. using propensity score methods) with almost identical results as those presented here. Estimates from other non-experimental

estimators (using a similar sample) are presented in Flores-Lagunes, Gonzalez and Neumann (2004).

[17]Most of the pre-treatment variables are interacted with the gender indicator, with the exception of the health variables, the cigarette, alcohol and pot use variables, the indicators of individual expectations and motivations to enroll in JC, and the prediction by the JC official about whether the individual would be a residential or non-residential student (see Data Appendix). A fully interacted model for females is rejected against the model that only includes the interactions in our baseline specification.

[18] While 1.85 hours of experience per week might appear small, it translates to 385 hours over the duration of the NJCS (208 weeks), or about a one-fifth of a full-time work year.

[19] There are no straightforward methods to deal with estimation of causal effects controlling for post-treatment variables. See Frangakis and Rubin (2002) and more recently Flores and Flores-Lagunes (2007) for some alternative useful methods.

[20] A similar specification that does not interact post-treatment experience with randomization status yields essentially the same results and thus is omitted for brevity. This and other specifications are available in the Internet Appendix of the paper.

[21] There is the possibility that labor market experience is just one of many other potential post-treatment variables through which JC fails to work for Hispanics. Given that there are other post-treatment variables available in the NJCS data, we address this concern by estimating similar effects to those in Table 2 with a number of such other variables. We find that experience is the only variable for which ITT estimates become significantly smaller (in absolute value). These other post-treatment variables are: duration of Job Corps enrollment, 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.

[22] Johansson (2001) analyzes Swedish active labor market programs using individual data and time-series variation in economic conditions of Swedish regions. Kluve (2007) takes a meta-analysis approach in which he analyzes over 100 studies that evaluate active labor market programs in Europe and controls for (among other things) the economic conditions in the corresponding place and time of each study.

[23] Their measures of local labor market conditions are total labor-to-population ratios and average real earnings per worker for the retail trade sector. Our measures, explained below, are arguably more targeted measures of the labor conditions disadvantaged youth face.

[24] A related important question is how wage *growth* differs for different groups of disadvantaged youth over the business cycle. While Gladden and Taber (2000) examine how wage growth in general varies over different low-skilled groups and documents only small differences across them (and relative to medium-skilled workers), there is no work to our knowledge that addresses how the wage growth of these groups varies over the business cycle.

[25] See Antecol and Bedard (2004), Borjas (1982), DeFreitas (1991), Gonzalez (2002), and Trejo (1997).

[26] We choose to present here the results employing the 2000 LAUS over the race/ethnicity-specific LUR—which yield stronger results—since the latter is constructed using a 5% IPUMS sample and is not necessarily comparable with those of LAUS.

[27] Recall that the race/ethnicity-specific LUR is constructed using individuals aged 16-35 belonging to the corresponding race/ethnicity from the Census' 5% IPUMS.