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Does Job Corps Work? Impact Findings from the National Job Corps Study

By PETER Z. SCHOCHET, JOHN BURGHARDT, AND SHEENA MCCONNELL*

This paper presents findings from an experimental evaluation of Job Corps, the nation's largest training program for disadvantaged youths. The study uses survey data collected over four years and tax data over nine years on a nationwide sample of 15,400 treatments and controls. The Job Corps model has promise; program participation increases educational attainment, reduces criminal activity, and increases earnings for several postprogram years. Based on tax data, however, the earnings gains were not sustained except for the oldest participants. Nonetheless, Job Corps is the only federal training program that has been shown to increase earnings for this population. (JEL I28, I38, J13, J24)

Job Corps is the nation's largest vocationally focused education and training program for disadvantaged youths. It serves youths between the ages of 16 and 24, primarily in a residential setting. The program's goal is to help youths become more responsible, employable, and productive citizens. Each year, it serves more than 60,000 new participants at a cost of about \$1.5 billion, which is more than 60 percent of all funds spent by the US Department of Labor (DOL) on youth training and employment services (the remaining 40 percent of funds is spent on a wide range of state-administered services). To examine the effectiveness of the program, DOL sponsored the National Job Corps Study in 1993.

The National Job Corps Study is the first *nationally representative* experimental evaluation of a federal employment and training program for disadvantaged youths, unlike previous evaluations of similar programs that were conducted in purposively selected sites only (Robert J. LaLonde 1995, 2003). Thus, study results can be generalized to the full Job Corps program as it operated at the time of the study.

From late 1994 to early 1996, nearly 81,000 eligible applicants nationwide were randomly assigned to either a program group, who were allowed to enroll in Job Corps, or to a control group, whose 6,000 members were not. Study findings are based on the comparisons of the outcomes of program and control group members using survey data collected during the four years after random assignment, and administrative earnings data covering the nine years after random assignment.

This paper presents impact findings from this evaluation. The effectiveness of Job Corps is of particular interest because other federal employment and training programs for low-income youth have not been found to be effective (LaLonde 2003). Based on experimental designs,

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the Supported Work Demonstration (Robinson Hollister, Peter Kemper, and Rebecca Maynard 1984), National Job Training Partnership Act (JTPA) Study (Larry Orr et al. 1996), and JobStart Demonstration (George Cave et al. 1993) all found insignificant impacts on youths' postprogram earnings. In contrast, an earlier study of Job Corps in the 1970s found Job Corps increased annual earnings by \$2,000 for males and \$1,000 for females and reduced criminal behavior (Charles Mallar et al. 1982). That evaluation was based, however, on a nonexperimental design where Job Corps participants were matched to a comparison group of high school dropouts and applicants to the Employment Service in areas with low Job Corps enrollments.

The rest of the paper is in eight sections. Section I provides a brief overview of Job Corps. Section II discusses the study design. Sections III and IV discuss the data and analytical methods. Sections V and VI present impact findings for the full sample and for key population subgroups. Reasons for differences in earning measures based on survey and administrative data are examined in Section VII. Section VIII presents our conclusions.

I. Job Corps

The Job Corps program, established by the Economic Opportunity Act of 1964, currently operates under the provisions of the Workforce Investment Act (WIA) of 1998. DOL administers Job Corps through a national office and six regional offices. Private and public agencies under contract to DOL provide Job Corps services.

Applicants must meet 11 criteria to be eligible for Job Corps: (1) be age 16 to 24; (2) be a legal US resident; (3) be economically disadvantaged (receiving welfare or food stamps or having income less than 70 percent of DOL's "lower living standards income level"); (4) live in an environment characterized by a disruptive home life, high crime rates, or limited job opportunities; (5) need additional education, training, or job skills; (6) be free of serious behavioral problems; (7) have a clean health history; (8) have an adequate child care plan (for those with children); (9) have registered with the Selective Service Board (if applicable); (10) have parental consent (for minors); and (11) be judged to have the capability and aspirations to participate in Job Corps.

Job Corps services are delivered in three stages: outreach and admissions, center operations, and placement. The outreach and admissions functions are performed by agencies typically located in disadvantaged communities. These agencies recruit for Job Corps mainly by providing program information to community organizations working with youth, such as schools, courts, employment services, and welfare agencies. When youth apply to Job Corps, these agencies ensure that applicants are informed about the program and meet eligibility criteria. During the study, it took about three weeks on average between program application and eligibility determination. Once found eligible, the agencies assigned the youths to a center, typically about one month later. The 73 percent of youths in the program group who enrolled in centers typically did so within one to four weeks after their center assignments.

The heart of Job Corps is the services provided at centers. At the time of the study, there were 110 Job Corps centers nationwide. These centers ranged in size from about 200 to 2,600 slots; about half of participants were served in 49 medium-sized centers with 226 to 495 slots, and about one-third were served in 16 large centers with 496 slots or more. Most centers are operated by private contractors, although about one-quarter are operated by the US Departments of Agriculture and Interior. Centers are located in both rural and urban areas. About two-thirds of participants in our sample enrolled in centers in their home state, and half enrolled in a center that was the closest or second closest center to their home.

While at centers, participants receive intensive vocational training, academic education, and a wide range of other services, including counseling, social skills training, and health education. At the time of the study, Job Corps offered vocational training in more than 75 trades, and

a typical center offered 10 or 11 trades. The vocational curricula were developed with input from business and labor organizations, and emphasize the achievement of specific competencies necessary to work in a trade. Academic education aims to alleviate deficits in reading, math, and writing skills, and to provide a General Educational Development (GED) certificate. Job Corps has a uniform, computer-based curriculum for major academic courses. Most academic and vocational instruction is individualized and self-paced. The length of time a youth spends at Job Corps varies widely, but at the time of the study, the average participant was enrolled for eight months.

A unique feature of Job Corps is that most participants reside at a center while training. Even the 13 percent of participants who are nonresidential and reside at home spend most of each weekday at the center. Both residents and nonresidents receive meals, health and dental care, and can participate in student government and recreation activities. Centers with many nonresidents are typically located in the low-income neighborhoods from which Job Corps participants come, unlike centers that primarily serve residents.

Job Corps provides placement services to help participants find jobs or pursue additional training. These services are provided by center staff while youth are enrolled in centers and for six months afterward by placement agencies in the communities in which the youth reside. Contracts to provide placement, outreach and admissions, and center services are separate, but some agencies hold multiple types of contracts.

Using data from week-long visits to 23 randomly selected centers and from surveys of outreach and admissions agencies and centers, Terry Johnson et al. (1999) concluded that Job Corps uses a well-developed program model and that the program is well implemented. One exception is that placement services provided after participants left the centers were found to be limited in scope and substance, and relatively few participants reported receiving these services (Schochet, Burghardt, and Steven Glazerman 2001).

As Congress intended when it formed the program, Job Corps serves disadvantaged youths (Table 1). Only 23 percent of youths in our sample had a high school credential at program application, and about 70 percent were members of racial or ethnic minority groups. About one-fourth of applicants (and nearly one-third of male applicants) had been arrested before application. Nearly half lived in families that received food stamp benefits in the previous year, and mean annual earnings were less than \$3,000. Using 1995 March Current Population Survey (CPS) data, we find that, compared to a nationwide population of low-income youths between the ages of 16 and 24, an eligible Job Corps applicant is more likely to be male, African American, younger, without a high school credential, from a large urban area, and to have been employed in the previous year (Table 1).

Other education and training programs besides Job Corps are available to disadvantaged youths, including high school, community colleges, and other programs funded by WIA and its precursors (LaLonde 2003). Job Corps differs from these other programs, however, in three main ways. First, Job Corps offers more comprehensive services than other programs. While other programs may offer basic education, vocational training, or counseling, only Job Corps offers all these services along with a wide range of support services associated with residential living (such as health care and recreation). Second, Job Corps is more intensive and, hence, expensive. In 1995, Job Corps cost about \$16,500 per participant, compared to \$6,000 to \$7,000 for a year of high school or community college and less than \$3,000 per participant for a typical JTPA program (US Department of Education National Center for Education Statistics 1999; LaLonde 2003). Third, while other education or training programs are administered by state or local agencies, Job Corps is administered by DOL. This difference explains the greater nationwide uniformity in program form and content in Job Corps than in other programs.

TABLE 1—CHARACTERISTICS OF ELIGIBLE JOB CORPS APPLICANTS

Characteristic	Percentage of eligible applicants	Low-income youths in the 1995 March CPS
Gender		
Male	59	39
Females without children	29	30
Females with children	12	31
Age at application		
16 to 17	41	24
18 to 19	32	33
20 to 24	27	44
Race and ethnicity		
White, non-Hispanic	27	54
Black, non-Hispanic	48	29
Hispanic	18	14
Other	8	3
Had a high school credential	23	47
High school diploma	18	na
GED certificate	5	na
Lived in a metropolitan statistical area	78	70
Arrest history (self-reported)		
Ever arrested	27	na
Arrested for serious crimes ^a	5	na
Received food stamps in the past year	44	50
Had a job in the past year	65	49
Average earnings in the past year (dollars) ^b	\$2,975	\$1,686

Source: Baseline survey data for 14,327 eligible applicants, and 1995 March CPS data for 2,677 16- to 24-year-old youths who lived in families with incomes below the poverty line. All figures were calculated using weights to adjust for the sample and survey designs.

^a Serious crimes include aggravated assault, murder, robbery, and burglary.

^b Figures include zero values for nonworkers.

na = Not available.

Job Corps has not changed substantially since the mid-1990s when the study took place. The changes that have taken place include more emphasis on the attainment of a high school diploma, putting more resources into English as a Second Language (ESL) programs, providing intensive job search training and career guidance in the first two months a student is on center, placing more Job Corps graduates in postsecondary education, and improving placement services (US Department of Labor 2006).

II. Study Design

The cornerstone of the study was the random assignment of eligible Job Corps applicants in nearly all locations to either a program or control group. Each Job Corps applicant was randomly assigned soon after the youth was found eligible for the program. We considered randomly assigning youth when they first applied for Job Corps, but rejected this option as the sample would have included youth ineligible for the program. We also rejected randomly assigning youth once they had been assigned to, or enrolled at, a center because this design would have placed an unacceptable burden on youth assigned to the control group and on program operations.

To initiate random assignment, outreach and admissions counselors sent program intake forms to Mathematica Policy Research, Inc. (MPR) soon after applicants were determined to be eligible for the program. MPR then conducted random assignment and notified outreach and admissions

counselors of the random assignment results within 48 hours of receiving completed program intake forms. Outreach and admissions counselors notified youths of the results. Over 1,300 Job Corps outreach and admissions counselors nationwide were directly involved in random assignment.

The National Job Corps Study is based on a national sample of eligible program applicants. With a few exceptions, *all* youths who applied to Job Corps in the 48 contiguous states between November 1994 and December 1995 and were found eligible by the end of February 1996 were randomly assigned to either a program or control group.¹ Program group members were allowed to enroll in Job Corps; control group members were not for three years after random assignment, although they could enroll in other training or education programs. Thus, the counterfactual for the evaluation is *other* available programs that the study population would enroll in if Job Corps were not an option.

The nonclustered design was adopted because the national sample produced more precise impact estimates than a clustered design of the same size. Furthermore, a relatively low control group sampling rate could be set to achieve target sample sizes. This feature minimized study-induced distortions to normal program operations and the types of participants typically served by the program, because only a small number of controls needed to be recruited in each location. This approach also spread the burden of random assignment across all outreach and admissions agencies and Job Corps centers, which helped make the random assignment process acceptable to Job Corps staff.

Unlike previous evaluations of federal education and training programs, we could adopt a non-clustered design because of the strong federal presence in Job Corps operations, and the strong commitment of senior program managers to conducting a study that would produce impact estimates that could be generalized to the full program. At the time of the study, Job Corps was under intense scrutiny in Congress, and senior Job Corps staff, who believed the program was effective, understood that the future of the program hinged on credible study findings.

The evaluation is based on large samples. Nearly 81,000 eligible applicants were randomly assigned. During the sample intake period, 5,977 youths (about 7 percent of the total) were randomly assigned to the control group, 9,409 youths were randomly assigned to the program group as part of the research sample—which we refer to hereafter as the *treatment* group—and the remaining youths were randomly assigned to a program nonresearch group (Schochet 2001).

As expected, random assignment produced treatment and control groups whose distributions of characteristics prior to random assignment were similar. Of the 94 statistical tests conducted to assess differences in the baseline characteristics of the two groups, 5 were statistically significant at the 5 percent level, which is what would be expected by chance (Schochet 1998). In addition, Job Corps staff implemented random assignment procedures well (Burghardt et al. 1999). Using weekly extracts from the Job Corps management information system on all new center enrollees, fewer than 0.6 percent of enrollees arrived at a center without having been previously randomly assigned. Furthermore, only 1.4 percent of controls enrolled in Job Corps before the end of their three-year embargo period.² Thus, we believe that the research sample is representative of the youths in the study population, and contamination of the control group is very small.

¹ The following groups of youths were excluded from the study: (1) youths who previously participated in Job Corps; (2) those who applied to one of seven small, special Job Corps programs whose eligibility criteria or services differed from those in the regular Job Corps program (these special programs contain less than 0.5 percent of all center slots nationwide); and (3) for cost reasons, applicants from four outreach and admissions agencies in Alaska, Hawaii, Puerto Rico, and the Virgin Islands that recruit about 3 percent of Job Corps participants.

² Crossing over occurred due to staff errors; about 30 percent of crossovers enrolled before random assignment, and 70 percent enrolled after random assignment. For most of the study period, DOL instituted a policy whereby crossovers

The study also did not appear to alter program operations substantially, which suggests that the study evaluated Job Corps as it would have normally operated in the absence of the study. Johnson et al. (1999) found that the effects of the random assignment process on outreach and admissions counselors' activities and on the composition of the participants enrolling in the program appear to have been modest, as would be expected due to the small percentage of applicants assigned to the control group in each location. Furthermore, there is no evidence that the study had an adverse effect on the behavior and outcomes of controls; as discussed below, many controls attended other education and training programs or worked soon after being rejected from Job Corps.

III. Data

Outcome measures for the study were obtained from two sources: (1) survey data covering the four years after random assignment; and (2) administrative earnings (tax) records covering the 9 years after random assignment (in year 9, sample members were between the ages of 25 and 33).

A. Survey Data

Surveys were conducted at baseline (shortly after random assignment) and at 12, 30, and 48 months after random assignment. *Baseline* interviews were conducted by telephone and in-person for those not reachable by telephone. To conserve data collection costs, in-person *baseline* interviews were conducted in randomly selected "intensive baseline" areas only, resulting in a survey sample that is slightly clustered. *Follow-up* interviews were attempted by telephone and, if necessary, in person (in all areas) with (1) those who completed baseline interviews, and (2) those in the "intensive baseline" areas who did not complete baseline interviews.³ The survey analysis sample includes 11,313 youths (6,828 treatments and 4,485 controls) who completed a 48-month interview. The response rate to the 48-month interview in the "intensive baseline" areas was 81 percent for treatments and 78 percent for controls.

This paper presents impact findings for three categories of outcome measures from the surveys: (1) education and training; (2) employment and earnings (the primary outcomes); and (3) crime. Schochet, Burghardt, and Glazerman (2001) present impact findings for other nonlabor market outcomes.

B. Administrative Earnings Records

The evaluation also relied on two forms of tax data: (1) 1993 to 2003 annual summary earnings records (SER) data reported by employers to the Internal Revenue Service (IRS) and maintained by the Social Security Administration (SSA) to determine workers' eligibility for social security, and (2) 1999 to 2001 quarterly wage records reported by employers to state unemployment insurance (UI) agencies in 22 randomly selected states.

The primary source for the SER data is the W-2 form. To protect confidentiality, SSA does not release earnings data for individuals. Accordingly, SSA ran computer programs that we provided to estimate impacts for the full sample and for a small number of key population subgroups. The

could remain at centers, and held recruitment and center staff accountable for random assignment errors. An additional 3.2 percent of control group members enrolled in Job Corps after their three-year restriction period ended.

³ To reduce data collection costs, we randomly selected for 48-month interviewing about 93 percent of treatment group members who were eligible for these interviews.

analysis sample included 15,138 of the 15,301 youths in the full study sample whose social security numbers (SSNs) were validated by SSA's Enumeration Verification System.

UI wage records consist of total quarterly earnings reported by employers to state UI agencies. By law, most employers are subject to a state UI tax and must report what is paid to each employee, including regular earnings, overtime, and tips and bonuses. Each state maintains UI wage records separately. Thus, to minimize costs for UI data collection, we randomly selected 25 states with probabilities proportional to state Job Corps enrollments; 22 provided UI data.⁴ The analysis sample includes 79 percent of the research sample who signed a consent form for records release when they applied to Job Corps (9,369 treatments and 5,940 controls). The UI data available at the time of our two data requests cover 1999 to 2001, which is largely a post-survey period.⁵

The SER and UI tax data cover most workers in formal jobs. The major difference between coverage in the two data sources is that UI data do not cover the following workers covered by the SER data: federal workers, military staff, self-employed persons, and independent contractors. Another difference is that the SER data do not cover some state and local government employees who are covered by UI data. Agricultural labor (except workers on large farms), railroad workers, and some domestic service workers are not covered in either data source.

IV. Analytic Methods for the Impact Analysis

Average impact estimates *per eligible applicant* were obtained by computing differences in mean outcomes between all treatments and controls. Weights were used in all calculations to adjust for the sample and survey designs. Similar estimates were found using regression models to control for baseline factors correlated with the outcome measures (Schochet 2001). Average impact estimates *per participant* (that is, for those "treated") were obtained using an instrumental variable approach where the estimated impacts per eligible applicant were divided by the difference between the treatment group Job Corps enrollment rate (73 percent) and the control group crossover rate (Howard Bloom 1984; Joshua Angrist, Guido Imbens, and Donald Rubin 1996; James Heckman, Jeffrey Smith, and Chris Taber 1998). We did not estimate other policy-relevant treatment effect parameters discussed by Heckman and Edward Vytlacil (2005) such as marginal and local average treatment effects.

UI earnings data are not available for those who worked outside the 22 UI states. Because the 22 states were selected randomly, however, we found that similar numbers of sample members moved into and out of the states during the follow-up period. Thus, "mean" treatment group earnings in state s were obtained by dividing total UI earnings for the treatment group in state s by the number of sample members who lived in state s at baseline, and similarly for the control group. Estimated impacts for the study population were then calculated as a weighted average of the 22 state impacts (Schochet, McConnell, and Burghardt 2003).

This paper presents impact estimates for subgroups defined by youth characteristics—age at random assignment, gender, and race and ethnicity—and for residents and nonresidents. The

⁴ The following 25 states were selected for UI data collection: Arkansas, Arizona, California, Florida, Georgia, Idaho, Illinois, Kansas, Louisiana, Maryland, Maine, Michigan, Missouri, Mississippi, North Carolina, Nebraska, Nevada, New Jersey, Ohio, Oklahoma, Pennsylvania, South Carolina, Texas, Virginia, and Washington. Only Georgia, Nevada, and Pennsylvania refused to release their data.

⁵ The UI data cover this period for three reasons. First, data needed to determine UI eligibility pertain to a base period that is usually defined as the first four of the last five completed calendar quarters. Thus, wage records are typically maintained online for, at most, six or seven quarters, and archived data for earlier quarters are not generally available. Second, UI data for a given calendar quarter are not complete for about six months. Finally, for cost reasons, DOL did not fund the collection of more recent UI data.

impacts by residential status are of considerable policy interest because the two components serve participants with different characteristics and needs; nonresidents are more likely to be female, to have children, and to be older. Furthermore, the previous studies discussed earlier found that disadvantaged youths do not benefit significantly from participation in nonresidential employment and training programs. Finally, the program cost per participant is about 25 percent higher for residential participants (McConnell and Glazerman 2001).

Impacts for subgroups defined by youth characteristics were estimated by comparing the average outcomes of treatments and controls in the subgroup of interest. To estimate impacts by residential status, we used data on the *predictions* of outreach and admissions counselors (collected at intake on a special study form) as to whether sample members would be assigned to a residential or a nonresidential slot. These predictions (collected prior to random assignment) were very accurate; about 98 percent of program group enrollees designated for residential slots actually enrolled in them, and the corresponding figure is 89 percent for nonresidential designees. Thus, impacts for residents were estimated by comparing the average outcomes of residential designees in the treatment and control groups, and similarly for nonresidents.

V. Impact Results for the Full Sample

Treatments received a substantial dose of Job Corps services. About 73 percent enrolled in centers. The average length of stay per participant was about eight months, although duration varied considerably; nearly one-quarter stayed for over a year, and 28 percent stayed for less than three months. About 85 percent of participants reported receiving academic and vocational instruction in Job Corps, and the average participant received nearly 1,200 hours of instruction. Participants also took part in the many other Job Corps activities, such as parenting education, health education, social skills training, cultural awareness classes, and recreation.

A. Impacts on Education and Training

About 70 percent of controls enrolled in education and training programs other than Job Corps during the four years after random assignment (Table 2). Participation rates were highest in programs that substitute for Job Corps: GED programs, high school, and vocational, technical, or trade schools (about 33 percent each).

Despite this activity, Job Corps substantially increased the education and training that program participants received (Table 2). About 93 percent of treatments enrolled in education and training programs (63 percent enrolled in programs other than Job Corps). Program participants spent about 1,000 hours in total more in education or training in Job Corps and elsewhere than they would have if they had not enrolled in the program. This impact per participant corresponds to *roughly one high school year*. The impact on time spent in vocational training was more than triple the impact on time spent in academic classes (774 hours, compared to 215 hours).

Job Corps had large impacts on the receipt of credentials it emphasizes most: GED certificates (21 percentage points) and vocational certificates (31 percentage points) (Table 2). The expected effect of the GED impact on earnings, however, is unclear, due to the debate about the value of a GED in the labor market (Steve Cameron and Heckman 1993; David Boesel, Nabeel Alsalam, and Thomas M. Smith 1998). Slightly more controls than treatments earned a high school diploma (7.5 percent versus 5.3 percent), although most of those who returned to high school did not graduate. Job Corps had no effect on college attendance or completion.

Next, we examine the extent to which beneficial impacts on education outcomes led to post-program earnings gains as human capital theory would suggest (Robert Willis 1986; David Card 1999).

TABLE 2—IMPAIRS ON KEY EDUCATIONAL OUTCOMES

	Treatment group	Control group	Estimated impact per eligible applicant ^a	Estimated impact per participant ^b
Percentage ever enrolled in an education or training program during the 48 months after random assignment	92.5	71.7	20.8* (0.7)	28.9* (1.0)
Average hours ever in education or training	1,559.8	848.2	711.6* (23.5)	989.0* (31.8)
Degrees, diplomas, and certificates received (percentage)				
GED certificate ^c	41.6	26.6	15.0* (1.0)	20.9* (1.4)
High school diploma ^c	5.3	7.5	-2.2* (0.5)	-3.1* (0.7)
Vocational, technical, or trade certificate	37.5	15.2	22.3* (0.9)	30.9* (1.2)
College degree (two- or four-year)	1.3	1.5	-0.2 (0.2)	-0.3 (0.3)
Sample size	6,828	4,485	11,313	

Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews. Note: Standard errors are in parentheses.

^a Estimated impacts per eligible applicant are measured as the difference between the weighted means for treatment and control group members.

^b Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of treatments who enrolled in Job Corps and the proportion of controls who enrolled in Job Corps during their three-year restriction period.

^c Figures pertain to sample members who did not have a high school credential at random assignment.

* Significantly different from zero at the 0.05 level, two-tailed test.

B. Earnings Impacts Based on the Survey and SER Data in Years 1 to 4

The *survey* data indicate that earnings impact estimates were negative in the first two years after random assignment when many treatments were enrolled in Job Corps, but the estimated impacts became positive in year 3 (Figure 1 and Table A1). In year 4, average weekly earnings for treatments were \$211, compared to \$195 for controls (these figures include the zero earnings for nonworkers). This translates into an estimated impact per Job Corps participant of \$22 per week (or \$1,150 annually), which is a 12 percent earnings gain. Impacts on employment rates followed a similar pattern (Table A1).

Employed treatments earned an average of \$0.22 more per hour than employed controls in their most recent job in quarter 16 (\$7.55, compared to \$7.33), and were somewhat more likely to receive fringe benefits (Table 3). Job occupations, however, were similar for the two research groups, with more than 40 percent working in service and construction jobs.⁶

Table 4 displays impacts on *calendar* year earnings using the survey, SER, and UI data. The survey data cover 1996 to 1998, the SER data cover 1993 to 2003, and the UI data cover 1999 to 2001. Impacts measured in *calendar* time differ from those measured in *random assignment* time, because random assignment took place between late 1994 and early 1996. However, it is fairly accurate, based on when treatments were enrolled in Job Corps, to consider 1995 and 1996 as an in-program period (roughly years 1 and 2 after random assignment) and 1997 and beyond as a post-program period (roughly years 3 to 9).

Similar to the pattern discussed above, the estimated annual earnings impacts using the *SER* data were negative in 1995 and 1996 and positive and statistically significant in 1997 and 1998 (Table 4). The *SER*-based impact estimates, however, are *smaller* than the corresponding *calendar*-year survey-based estimates, due to considerably higher mean earnings *levels* in the survey

⁶ These results are conditional on being employed and, thus, may not be impact estimates. This is because Job Corps may have had a compositional effect on those who were working.

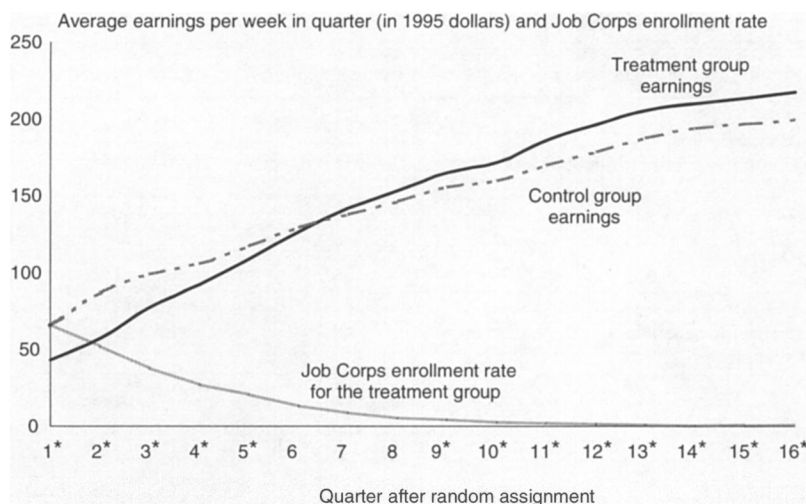


FIGURE 1. AVERAGE EARNINGS PER WEEK BASED ON SURVEY DATA, BY QUARTER
(*Job Corps enrollment rate for the treatment group is also displayed*)

Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

Note: Figures include zero earnings for nonworkers. Table A1 in the Appendix displays the full set of earnings estimates used to construct this figure.

*Difference between the mean outcome for treatment and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

data (Table 4). For instance, in 1998, the impact estimate per eligible applicant was \$972 using the survey data, compared to \$218 using the SER data. Furthermore, the impacts grew more between 1997 and 1998 according to the survey data. We examine possible explanations for these differences in Section VII.

C. Earnings Impacts Based on the SER and UI Data after 1998

Based on the SER and UI data, no statistically significant beneficial impacts of Job Corps on annual *earnings* were found *after* 1998 (Table 4). No earnings impacts were found during the period of strong economic growth in 1999 and 2000 (which may have benefited the earnings of the lower-skilled control group more, as suggested by Hillary Hoynes (1999) and Larry Katz and Alan Krueger (1999)), or as economic conditions worsened between 2001 and 2003 (as the employment rate decreased for both research groups).⁷ The reduction in the SER earnings impact between 1998 and 1999 is statistically significant. Similarly, estimated impacts on annual *employment rates* were insignificant after 1998 according to the SER data, and were significantly negative in 2000 and 2001 according to the UI data (Table 4). It is impossible to say whether estimated impacts based on survey data would have also disappeared after 1998, especially given the nontrivial survey-tax earnings differences.

⁷ The earnings impacts are not likely to have been affected by differences across the research groups in school enrollment rates or control group participation rates in Job Corps. Only about 13 percent of both treatments and controls were enrolled in school at 48 months after random assignment, and only about 1 percent of controls enrolled in Job Corps after year 4.

TABLE 3—HOURLY WAGES, FRINGE BENEFITS, AND OCCUPATIONS IN THE MOST RECENT JOB IN QUARTER 16

Outcome measure from survey data	Treatment group	Control group	Difference ^a
Percent employed in quarter 16	71.1	68.7	2.4* (0.9)
Hourly wage (in 1995 dollars) ^{a, b}			0.001*
\$5.15 (federal minimum wage in 1999) or less	13.3	14.3	-1.1
\$5.16 to \$7.50	43.0	46.6	-3.6
More than \$7.50	43.7	39.0	4.6
(Average wage)	7.55	7.33	0.22* (0.08)
Benefits available (percentage) ^a			
Health insurance	57.4	54.3	3.0* (1.2)
Paid vacation	62.9	60.7	2.2* (1.1)
Retirement or pension benefits	48.3	43.7	4.6* (1.2)
Occupation (percentage) ^{a, b}			0.030*
Service	21.3	20.8	0.4
Sales	9.7	12.1	-2.3
Construction	20.9	20.3	0.5
Private household	6.9	7.2	-0.2
Clerical	11.8	12.8	-1.0
Mechanics/machinists	13.9	13.1	0.7
Agriculture/forestry	2.6	2.6	0.0
Other	12.9	11.1	1.9
Sample size	6,828	4,485	11,313

Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews. All figures were calculated using weights to adjust for the sample and survey designs.

Note: Standard errors are in parentheses.

^a Because these estimates are conditional on being employed in quarter 16, they may not be impact estimates.

^b The value in the header row displays the *p*-value for a significance test of differences in the hourly wage and occupational distributions across the research groups.

* Significantly different from zero at the 0.05 level, two-tailed test.

Average earnings levels were about 17 percent higher in the SER than in the UI data (Table 4). This probably reflects the lower accuracy of the UI data, because (1) employers have financial incentives to underreport earnings to state UI programs to avoid paying UI taxes, whereas earnings reported to the IRS can be counted as a business expense that will lower its income tax; (2) SSA verifies SSNs before inputting reported earnings data or matching people to their earnings records, whereas UI agencies do not (12 percent of the sample reported multiple SSNs during the study); and (3) UI data do not cover self-employed workers. The SER-to-UI earnings ratios that we find are similar to those reported in Robert Kornfeld and Bloom (1999) for youths in the National JTPA Study.

D. Impacts on Crime

Job Corps significantly reduced arrest and conviction rates, as well as time spent incarcerated (Schochet, Burghardt, and Glazerman 2001). About 33 percent of controls were arrested during the 48-month follow-up period, compared to 29 percent of treatments (a statistically significant reduction). Arrest rate reductions were largest during the first year after random assignment, although Job Corps also led to small arrest reductions afterward. Although treatments were less likely to have arrest charges for all categories of crimes, Job Corps had a larger impact on reducing arrests for less serious crimes (such as disorderly conduct and trespassing) than for more serious crimes (such as murder and aggravated assault). Job Corps also reduced conviction rates by 3 percentage points per eligible applicant (from 25 to 22 percent), and incarceration rates for convictions by 2 percentage points (from 18 to 16 percent).

TABLE 4—IMPAIRS PER ELIGIBLE APPLICANT ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES, BY DATA SOURCE

Outcome measure	Data source								
	Survey data			Annual social security earnings records			Quarterly UI earnings records from 22 states		
	Treatment group	Control group	Estimated impact ^a	Treatment group	Control group	Estimated impact ^a	Treatment group	Control group	Estimated impact ^a
<i>Average calendar year earnings (in 1995 dollars)</i>									
1993 ^b				1,010	1,016	−7 (37)			
1994 ^b				1,590	1,543	47 (44)			
1995				1,761	2,030	−270* (43)			
1996	5,145	5,729	−584* (127)	3,101	3,279	−179* (64)			
1997	8,111	7,819	292 (163)	4,559	4,387	173* (85)			
1998	10,296	9,324	972* (186)	5,830	5,612	218* (103)			
1999				6,700	6,667	33 (118)	5,686	5,660	26 (189)
2000				7,603	7,627	−24 (133)	6,312	6,506	−194 (216)
2001				7,865	7,823	42 (145)	7,260	7,395	−135 (257)
2002				7,815	7,744	71 (150)			
2003				7,822	7,796	27 (157)			
<i>Percentage employed in calendar year</i>									
1993 ^b				43.0	43.1	−0.1 (0.8)			
1994 ^b				59.5	58.8	0.7 (0.8)			
1995 ^c				89.2	73.3	15.9* (0.6)			
1996 ^c	70.4	74.5	−4.2* (0.9)	88.8	78.4	10.3* (0.6)			
1997	77.7	76.9	0.8 (0.8)	83.6	81.5	2.1* (0.6)			
1998	81.4	78.9	2.4* (0.8)	84.6	83.3	1.3* (0.6)			
1999				84.5	83.0	1.5* (0.6)	78.3	77.3	1.0 (1.3)
2000				83.6	83.0	0.6 (0.6)	75.0	77.2	−2.1* (1.0)
2001				80.6	80.3	0.2 (0.6)	79.6	82.3	−2.7* (1.4)
2002				76.7	76.6	0.1 (0.7)			
2003				73.8	73.1	0.7 (0.7)			
Sample size	6,828	4,485	11,313	9,264	5,874	15,138	4,613	2,855	7,468

Sources: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews, annual social security earnings SER records, and quarterly UI earnings records from 22 randomly selected states.
 Notes: Earnings include zero earnings for nonworkers. Blank entries signify that figures are not applicable due to unavailable data. Standard errors are in parentheses.

^a Estimated impacts pertain to eligible applicants and are measured as the difference between the weighted means for treatment and control group members.

^b Preprogram.

^c Employment rates in the SER data are high for the treatment group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

* Significantly different from zero at the 0.05 level, two-tailed test.

Karen Needels and Burghardt (2000) present impact results on arrests and convictions covering the 30-month follow-up period using official crime records from North Carolina and Texas. These estimated impacts are consistent with the impacts based on the survey data.

VI. Impact Findings for Subgroups

The *survey* data indicate that positive earnings impacts in 1998 were found across most subgroups (Tables 5 and A2). Earnings gains were similar for males and females, the youngest and oldest participants, whites and African Americans, and for residential and nonresidential designees (the difference between the impacts by residential designation status is not statistically significant). The key exceptions are that no earnings gains were found for Hispanics or those

TABLE 5—IMPACTS PER PARTICIPANT ON EARNINGS AND ARREST RATES, FOR KEY SUBGROUPS

Subgroup (percentage of study population in parentheses)	Calendar year earnings (in 1995 dollars)					Percentage ever arrested during the 48-month period (survey)
	Survey	SER data				
	1998	1998	1999	2002	2003	
Full sample	1,350*	297*	46	97	36	-5.2*
Age at application ^a	+			+		
16 to 17 (41)	1,307*	195	2	-36	-47	-4.3*
18 to 19 (32)	297	76	-399	-545	-588	-6.7*
20 to 24 (27)	2,663*	704*	626	1,094*	917	-4.5*
Gender ^a		+				+
Male (59)	1,530*	512*	175	252	69	-6.8*
Female (41)	1,134*	-28	-142	-136	2	-2.2
Race and ethnicity ^a						
White, non-Hispanic (27)	2,459*	609*	23	503	406	-5.9*
Black, non-Hispanic (47)	1,178*	353*	131	118	215	-5.4*
Hispanic (18)	187	-269	-402	-511	-1,069	-2.5
Other ^b (8)	1,227	164	619	65	287	-9.0*
Residential/nonresidential status ^a						
Residential designees (87)	1,378*	308*	69	119	44	-5.6*
Nonresidential designees (13)	1,149	223	-116	-39	-4	-2.7

Source: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; and (2) annual social security earnings SER records for the full sample. All figures were calculated using weights to adjust for the sample design and survey design (for the survey-based estimates).

Notes: Estimated impacts per Job Corps participant are measured as the difference between the weighted means for treatment and control group members divided by the difference between the proportion of treatments who enrolled in Job Corps and the proportion of controls who enrolled in Job Corps during their three-year restriction period. Standard errors of estimates are shown in Table A2.

^a The “+” signs in the header rows signify that differences in impacts across subgroup levels are statistically significant at the .05 level, two-tailed test. The *p*-values for these tests are shown in Table A2 in the Appendix.

^b This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

* Impact is significantly different from zero at the 0.05 level, two-tailed test.

18 to 19 at program application (Schochet, Burghardt, and Glazerman (2001) discuss possible explanations).

As with the full sample, the estimated subgroup impacts on 1998 earnings were smaller using the *SER* than survey data (Table 5). In particular, the 1998 *SER* impacts for the 16- and 17-year-olds and females are not statistically significant. However, for the 16- and 17-year-olds, the earlier *SER* impacts in 1996 and 1997 (\$212 and \$391, respectively) are statistically significant, but the same pattern does not hold for females (not shown).

In the postsurvey 1999 to 2003 period, no statistically significant, positive *SER* earnings impacts were found for any subgroup, except for those ages 20 to 24 at program application (Table 5). The earnings impacts for the oldest participants grew from about \$700 in 1998 to \$900 in 2003. The \$4,500 impact on *total* postprogram earnings from 1998 to 2003 for the older participants is statistically significant, and differs significantly from the corresponding impacts for the younger participants (not shown).

It is possible that the findings for older participants are spurious due to the multiple testing of earning impacts across a large number of subgroups. The impact estimates for this group become statistically insignificant after adjusting *p*-values for multiple comparisons using the Yoav Benjamini and Yosef Hochberg (1995) or Zbynek Sidak (1967) correction procedures (although the study has low power for detecting subgroup impacts using these corrections). There is corroborating study evidence, however, that the earnings impacts for the oldest participants may be real. First, older participants in our sample remained in Job Corps for an average of 1.3 months longer than younger ones. Second, the older participants were more highly motivated and well

behaved as reported by a wide range of program staff at 23 randomly selected centers (Johnson et al. 1999). Finally, the estimated impacts on total hours spent in education and training were larger for the older participants, because they stayed longer in Job Corps, and because nearly half of the younger control group attended high school after being rejected for Job Corps.

Finally, impacts on crime were very similar across subgroups (Table 5). Although the level of criminal activity differed substantially across youth subgroups, the impacts on crime outcomes in percentage terms were similar.

VII. Understanding Differences in Earnings Measures Using the Survey and Tax Data

Postprogram earnings impacts were larger in the survey than tax data. These differences affect the benefit-cost analysis for the National Job Corps Study, which relies heavily on the estimated earnings impacts to measure program benefits (see Section VIII). More generally, differences between the survey and tax data are of policy concern, because these data sources are typically used to measure the performance of programs that serve disadvantaged youths and to measure youth poverty rates.

This section examines several possible explanations for the survey-tax differences, although our analysis is limited somewhat by data constraints. For instance, the SER data are available only at the aggregate level and do not contain earnings for individual jobs.⁸ The UI data contain individual- and job-level earnings information, and thus, more detailed analyses could be conducted using these data. However, the UI data are not available for all states, do not contain information on job characteristics (such as wages or hours worked), and overlap with the survey data only in quarter 16 after random assignment. Finally, although the survey data contain some information on jobs that sample members held (such as start and end dates, usual hours worked, occupation and industry, type of employer, wage rates, and available fringe benefits), the survey was not structured to gather sufficiently detailed job information to accurately determine earnings from jobs that were and were not likely to have been reported to the government. Nonetheless, our analysis provides insights into potential reasons for the survey-tax earnings differences that can help guide future research.

Earnings impacts were larger according to the survey than tax data for two potential reasons: (1) survey nonresponse bias and (2) reporting differences between the data sources for the same sample. To disentangle these two effects, we estimated impacts using the SER data for *48-month survey respondents only* (Table 6). In 1998, this estimated SER impact was \$393, compared to \$218 for the full research sample. Thus, accounting for survey nonresponse bias reduces the difference between the survey and SER impacts from \$754 (\$972–\$218) to \$579 (\$972–\$393), or by nearly one-quarter. Hence, holding the sample constant, reporting differences account for about three-quarters of the impact differences between the two data sources. Reported 1998 earnings levels for survey respondents were 65 to 70 percent larger according to the survey than SER data (Tables 4 and 6) and survey-based earnings were larger for 75 percent of the sample (not shown).

A. Survey Nonresponse Bias

The survey nonresponse bias arose because, in the treatment group, survey respondents had higher SER earnings than survey nonrespondents (and increasingly so over time), whereas in the

⁸ Our contract with SSA specified that they would estimate earnings impacts for the full sample and for a few relatively large key subgroups. Thus, it was not feasible to request that SSA run computer programs for specific analyses to examine survey-SER differences.

TABLE 6—IMPAIRMENTS ON SER EARNINGS FOR SURVEY RESPONDENTS AND NONRESPONDENTS

Selected years ^b	48-month survey respondents			48-month survey nonrespondents			Full sample
	Treatment group	Control group	Estimated impact ^a	Treatment group	Control group	Estimated impact ^a	Estimated impact ^a
<i>Average calendar year earnings (in 1995 dollars)</i>							
1994 (Preprogram)	1,571	1,525	46 (50)	1,660	1,610	50 (111)	47 (44)
1996	3,161	3,283	-123 (74)	2,876	3,266	-390* (153)	-179* (64)
1998*	6,016	5,622	393* (119)	5,130	5,573	-443 (252)	218* (103)
2000*	7,858	7,657	201 (154)	6,644	7,515	-871* (326)	-24 (133)
2003*	8,125	7,845	280 (183)	6,681	7,609	-928* (369)	27 (157)
Sample size	6,828	4,485	11,313	1,495	1,187	2,682	15,138^c

Source: Annual social security earnings SER records. All figures were calculated using weights to adjust for the sample and survey designs.

Notes: The earnings figures include zero earnings for nonworkers. Standard errors are in parentheses.

^a Impacts are measured as the difference between the weighted means for treatment and control group members.

^b Stars next to calendar years represent significance levels for tests of differences between estimated impacts for survey respondents and nonrespondents.

^c The sample size for the full sample is larger than the combined sample size for respondents and nonrespondents because of the random subsampling of youths for 48-month interviews to conserve project resources.

* Significantly different from zero at the 0.05 level, two-tailed test.

control group, the SER earnings of respondents and nonrespondents were more similar (Table 6). This also explains why impacts measured as a *percentage* of control group mean earnings are larger using the SER than survey data (10 percent versus 4 percent in 1998; Table 4).

The nonresponse bias occurred even though survey response rates were similar for treatments and controls, and standard testing and correction procedures (such as adjusting sampling weights and multiple imputation procedures) failed to detect the bias even with a large number of baseline covariates available for the analysis (Schochet, McConnell, and Burghardt 2003, 2006). John Baj, Sean Fahey, and Charles Trott (1992) and Smith (1997) also found evidence of upward survey bias using samples from JTPA training centers.

B. Reporting Differences

To identify sources of reporting differences between the tax and survey data for the same sample, we compared individual-level earnings measures based on the UI and survey data in quarter 16 after random assignment, the only overlapping period (Schochet, McConnell, and Burghardt (2003) provide more details). To ensure that the UI data would be as complete as possible, the analysis sample included 4,424 youths who (1) completed the 48-month interview, (2) lived in the 22 UI states for the entire follow-up period, and (3) did not work outside of the 22 UI states. The earnings measures include zero earnings for nonworkers.

Impacts on quarter 16 earnings were similar for this restricted analysis sample and the full sample. For the UI data, the quarter 16 impact was \$28 for the restricted sample (Table 7) and \$33 for the full sample (not shown), and both are statistically insignificant. The corresponding figures for the survey data are \$188 (Table 7) and \$235 (Table A1), and both are statistically significant.

For the restricted sample, mean quarter 16 earnings levels are 91 percent greater in the survey data than in the UI data for treatments and 81 percent greater for controls (Table 7). The survey-UI differences are large for all subgroups, but are somewhat larger for younger participants and males (Table 7). Survey measures are also substantially greater for each component of mean

TABLE 7—QUARTER 16 EARNINGS COMPONENTS ACCORDING TO THE UI AND SURVEY DATA

Quarter 16 measure	Treatment group	Control group	Estimated impact
<i>Mean earnings per sample member (in 1995 dollars)</i>			
Survey	2,696	2,508	188* (80)
UI	1,409	1,382	28 (63)
Survey-to-UI ratio	1.91	1.81	
Gender: males, females	2.1, 1.7	2.0, 1.5	
Age at program application: 16–17, 18–19, 20–24	2.2, 1.9, 1.7	2.0, 1.8, 1.6	
Components of earnings			
<i>Percentage employed according to:</i>			
Survey and UI	49	47	
Survey only	22	22	
UI only	9	9	
Neither data source	20	22	
<i>Average number of jobs per worker for those employed according to both data sources</i>			
Survey	1.2	1.2	
UI	1.0	1.0	
Survey-to-UI ratio	1.2	1.2	
<i>Mean earnings per job for those with the same number of reported jobs in both data sources (in 1995 dollars)</i>			
Survey	3,752	3,604	
UI	2,633	2,686	
Survey-to-UI ratio	1.42	1.34	
Sample size	2,645	1,779	

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records from 22 randomly selected states for 48-month survey respondents who lived in the 22 UI states for the entire 48-month period, did not work elsewhere, and signed the consent form for records release. All figures were calculated using weights to adjust for the sample and survey designs.

Note: Standard errors are in parentheses in the “estimated impact” column.

* Significantly different from zero at the 0.05 level, two-tailed test.

earnings—the employment rate, the mean number of jobs per worker, and mean earnings per job (Table 7). The correlation between survey and UI earnings levels is only 0.41.

The higher reported earnings levels in the survey than UI data could be due to a number of factors. First, the UI data may have missed earnings from sample members with SSNs that were incorrectly reported by employers or sample members. Second, job coverage could be less complete in the UI than survey data; some sectors are exempt from UI reporting requirements (such as federal jobs), some nonexempt employers may fail to report UI wages, and casual or cash-only jobs are not covered in the UI data but are likely to have been reported in the survey (survey respondents were asked to provide information on “paid full-time or part-time jobs that they may have had, including odd jobs, paid baby-sitting jobs, military service, work in their own businesses, or other types of jobs they may have had on a regular basis”). Third, some survey respondents may have overreported their earnings and employment levels due to recall error or other reasons.

To assess the undercoverage of jobs in the UI data, we calculated the percentage of youth who were employed according to the survey data who were also employed according to the UI data—that is, *agreement rates* for survey-based workers—by gender, occupation, and type of employer (Table 8). We hypothesized that agreement rates would be lower in sectors with low expected UI coverage rates than in other sectors.

This hypothesis is weakly supported by the data. Agreement rates are somewhat lower for the small percentage of workers in sectors exempt from UI reporting requirements (shown in

TABLE 8—AGREEMENT RATES BETWEEN THE SURVEY AND UI DATA, BY JOB TYPE AND GENDER

Job characteristic according to survey data	Males employed using survey data ^a		Females employed using survey data ^a	
	Percentage with job characteristic	Percentage also employed according to the UI data	Percentage with job characteristic	Percentage also employed according to the UI data
Total	100	66	100	73
Occupation				
Services	20	73	24	75
Sales	5	68	21	72
Construction	30	65	6	76
Private household ^b	4	53	11	57
Clerical	6	69	21	76
Mechanics/repairers/machinists	18	63	8	75
Agriculture ^b	3	60	1	73
Other	13	64	9	78
Type of employer				
Private company	81	69	80	75
Military ^b	2	8	0	na
Federal government ^b	2	58	3	70
State or local government	8	66	9	78
Self-employed ^b	5	45	5	27
Other	2	40	3	69
Sample size	1,471	1,471	1,175	1,175

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records from 22 randomly selected states for 48-month survey respondents who lived in the 22 UI states for the entire 48-month period, did not work elsewhere, and signed the consent form for records release. All figures were calculated using weights to adjust for the sample and survey designs.

^a Figures were calculated using the pooled sample of treatments and controls, because the figures are similar by research status.

^b Job category is exempt from UI reporting requirements.

na = Not applicable because no females reported being in the military.

italics in Table 8) than for workers in other sectors, but the differences are smaller than expected. Similarly, agreement rates are only slightly lower in occupations (such as construction and mechanical-related trades) in which we might expect cash-only or casual jobs to be more common. These weak relationships could reflect inaccuracies in classifying jobs due to limited job description information obtained from brief responses to a single open-ended question asking for these data. The survey also did not collect sufficient information to identify independent contractors, a potentially pertinent group for the sample. Paul Burgess, Arthur Blakemore, and Stuart Low (1998) found that the earnings of independent contractors accounted for half of all unreported UI wages based on audits of 875 Illinois firms in 1987.

Results from several additional analyses suggest that earnings from cash-only or casual jobs account for some of the survey-UI earnings differences. First, *annual* employment rates are similar in the survey and tax data (Table 4), but *quarterly* employment rates are considerably higher in the survey data, suggesting that the tax data are not capturing short-term casual jobs held by sample members. Second, we compared the characteristics of jobs reported in *both* the survey and UI data with the characteristics of jobs reported in the survey data *only*. The expectation was that survey-only workers were more likely to have held jobs with shorter job tenure, fewer hours worked per week, lower wages, and fewer benefits available on the job, and to have lower skills.

These hypotheses are somewhat supported by the data (Table 9). Job tenure and usual hours worked per week were similar for those employed according to both data sources and those

employed according to the survey data only. However, average hourly wages were somewhat lower for the survey-only group, because a larger percentage of these workers reported receiving very low wages. Furthermore, the likelihood of having available fringe benefits was somewhat lower for the survey-only group. Thus, the UI data are particularly likely to have missed earnings from low-wage, low-benefit jobs, and to have recorded zero earnings for these jobs instead of the true positive amount. This undercount may have affected controls more than treatments, because controls tended to receive slightly lower wages. Thus, treatment-control differences in mean earnings levels based on the UI data could be biased upward slightly.

The omission of very-low-wage jobs in the UI data, however, explains only a small portion of the UI-survey gap, because only about 13 percent of survey respondents reported receiving wages below the minimum wage (Table 3). Furthermore, it is puzzling that the demographic characteristics (and, in particular, the education levels) of survey-only and survey-and-UI workers are similar. We estimated multivariate logistic regression models to examine the associations between worker and job characteristics on the probability that a worker was in the survey-only group (Schochet, McConnell, and Burghardt 2003). Apart from the factors discussed above, very few explanatory variables included in the models are statistically significant; probabilities did not differ by education level, marital status, parenting status, health status, welfare receipt status, crime and drug use experiences, or employment experiences during the 48-month follow-up period. Thus, there are substantial unobserved factors that account for the survey-UI employment rate differences.

The UI-survey earnings differences could also be due to survey reporting error. For most jobs, earnings in the survey data were calculated by multiplying reported hourly wages, hours worked per week, and weeks worked on the job (Schochet 2001). The survey first asked workers about their hourly rate of pay before taxes and deductions (including tips, commissions, and regular overtime pay). Workers reported this mode of payment for about 85 percent of jobs. For those not paid by the hour (15 percent of jobs), the survey asked about earnings for a specific pay period (such as per week, per day, per month, twice a month, or per year). Workers were asked about their most recent wages on each job, which could have led to upwardly biased earnings measures if wages on a job increased over time (or if workers projected their current wages onto prior jobs). Similar measurement issues pertain to the hours per week variable, which was constructed by multiplying responses to the following two questions: (1) "How many days per week do/did you usually work?" and (2) "How many hours per day do/did you usually work, including overtime hours?" Finally, the weeks worked variable was constructed using reported job start and end dates, which could have been affected by recall error.⁹

For these earnings components, there is most evidence that hours worked was overreported in the survey. The average worker in the sample reported working about 42 hours per week, and about 75 percent reported working at least 40 hours (Table 9). These figures are *higher* than the corresponding 1999 CPS figures for all US workers (39.6 hours per week and 69 percent, respectively; US Census Bureau 2000). Furthermore, for those with the same number of reported jobs in the survey and UI data, the survey-to-UI mean earnings ratio is 1.70 for those who reported working more than 40 hours per week, compared to 1.20 for those who reported working 40 hours and 1.10 for those who reported working less than 40 hours (not shown). The hourly wages reported by our sample (\$7.40 in 1995 dollars) are more consistent with 1999 national figures (\$11.43 in 1995 dollars). Smith (1997) also found evidence of upward-biased survey measures of usual and overtime hours for a sample of JTPA-eligible nonparticipants.

⁹ Recall error and other measurement issues for quarter 16 earnings, however, may have been mitigated somewhat because quarter 16 was close to the 48-month follow-up interview date for most survey respondents.

TABLE 9—JOB CHARACTERISTICS IN QUARTER 16, BY SURVEY AND UI EMPLOYMENT STATUS

Job characteristic according to survey data	Treatment group (percentages)		Control group (percentages)	
	Employed in both survey and UI data ^a	Employed in survey data only	Employed in both survey and UI data ^a	Employed in survey data only
Number of months on job				
Fewer than 6	60	62	61	60
(Average months)	13	13	12	13
Usual hours worked per week				
Fewer than 30	9	12	11	12
30 to 39	13	14	12	14
40	42	32	41	30
More than 40	37	41	37	45
(Average hours)	42	43	42	43
Hourly wage (in 1995 dollars)				
Less than \$3.50	2	8	2	6
\$3.51 to \$5.15 ^b	9	12	9	15
\$5.16 to \$7.50	47	39	53	42
More than \$7.50	43	41	36	37
(Average hourly wage)	7.50	7.20	7.30	7.20
Benefits available on job				
Health insurance	64	47	60	46
Paid vacation	68	56	64	61
Retirement benefits	54	40	45	38
Sample size	860^a	475	524^a	332

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records from 22 randomly selected states for 48-month survey respondents who lived in the 22 UI states for the entire 48-month period, did not work elsewhere, and signed the consent form for records release. All figures were calculated using weights to adjust for the sample and survey designs.

^a Includes only those with the same number of reported jobs in both data sources.

^b The federal minimum wage was \$5.15 in 1999.

We simulated the effects of reducing hours worked in the survey data on the survey-to-UI mean earnings ratios for those with the same number of reported jobs in both data sources. Reducing hours matters. If hours are reduced by 10 percent for all workers (so that mean hours worked decreases from 42 to 38 hours), the survey-to-UI ratio decreases from 1.42 to 1.27 for treatments and 1.34 to 1.21 for controls. The earnings per job ratios reduce to 1.0 if hours for all workers were reduced by 25 percent (in which case mean hours worked become about 32 hours).

Previous studies also report higher earnings in survey than UI data for low-income populations (V. Joseph Hotz and John Scholz (2001) provide a comprehensive review). However, reporting differences for our sample are somewhat larger. For example, Kornfeld and Bloom (1999) found that mean quarterly earnings were about 35 to 70 percent higher according to the survey than UI data for youths in the National JTPA Study. One possible explanation for the larger differences in our study is that it was conducted during a period of strong economic growth, so that in the tight labor market, our sample may have been more likely than the JTPA sample to have earnings from casual or cash-only jobs.

Our results indicate that the survey and tax data provide complementary earnings information for low-income youths. It is difficult to assess which data source provides more accurate information. Reported earnings levels for the Job Corps sample are nearly double in the survey data, suggesting that considerable amounts of earnings are not captured in the tax data. This pattern emerges across broad groups of youths defined by their demographic and job characteristics, and the undercount appears to be especially large for those in short-term casual jobs that offer low

wages and few fringe benefits. On the other hand, survey-based earnings measures appear to be biased upward, because of overreporting of hours worked and survey nonresponse bias (which was more pronounced for treatments than controls).

Additional research in this area is needed to provide more complete explanations for the survey-tax earnings differences for low-income youth. Such research would entail collecting UI and survey (and, perhaps, focus group) data for a subsample of low-income youth (such as former Job Corps participants), where the survey data could be used to distinguish between earnings that are likely to be covered and those not covered in the UI data. The survey would collect detailed job information about employers, work responsibilities, modes of payment, regular and overtime hours worked, and independent contracting. Interviews with the youths' employers would also shed light on the earnings that employers reported to the government, as well as on potential discrepancies between hourly wages and hours worked from the perspective of workers and employers.

VIII. Conclusions

The National Job Corps Study found that Job Corps improves outcomes for disadvantaged youth. Job Corps provides broad groups of participants—most of whom enroll in the program without a high school credential—with the instructional equivalent of one additional year in school and has large effects on the receipt of credentials it emphasizes most: GED and vocational certificates. These impacts must be viewed in terms of the counterfactual for the evaluation: active participation of the control group in education and training programs. The 12 percent survey-based earnings gain observed in year 4 is commensurate with what would be expected from an additional year of school (Card 1999). The statistically significant short-term earnings gains experienced by program participants makes Job Corps the only large-scale education and training program that has been shown to increase the earnings of disadvantaged youth.

These program benefits appear to be small, however, compared to the program's cost of \$16,500 per participant (McConnell and Glazerman 2001; Schochet, McConnell, and Burghardt 2003, 2006). A benefit-cost analysis found that all measured benefits during the *four-year* survey period—including the benefits of increased earnings, reduced use of other services (education and training programs and public assistance), and reduced crime—were less than \$4,000. Earnings impacts based on the tax data disappeared between 1999 and 2003 (roughly years 5 to 9), suggesting that few additional program benefits accrued after year 4. Although it is impossible to say what the earnings impacts according to the *survey* data would have been after year 4, costs would exceed benefits as long as the survey-based earnings impacts observed in year 4 decreased by more than 8 percent per year for the rest of the youths' working lifetime. Thus, under most scenarios, program costs exceed program benefits for the full sample, unless Job Corps has a large effect on earnings that are not reported in tax data, or leads to increased earnings impacts later in the youths' lives.

The benefits of Job Corps, however, appear to offset costs for the oldest youth. Furthermore, benefits exceed costs for the participants themselves. While enrolled in Job Corps, participants receive a weekly cash payment as well as free meals and a cash allotment for clothing. The value of these items, plus participants' earnings gains soon after program exit, exceed the earnings forgone while they are enrolled in Job Corps. Hence, Job Corps does effectively redistribute resources toward low-income youth.

The positive initial postprogram earnings gains and the finding that the earnings gains appear to persist for older youth suggest that there is promise for the Job Corps model (which is largely unchanged since the study took place). The challenge is to improve program services to sustain

APPENDIX

TABLE A1—IMPAIRS ON EARNINGS AND EMPLOYMENT RATES BASED ON SURVEY DATA,
BY PERIOD AFTER RANDOM ASSIGNMENT

Period after random assignment	Treatment group	Control group	Estimated impact per eligible applicant ^a	Estimated impact per participant ^b
<i>Average earnings per week, by quarter</i>				
1	44.5	65.5	−22.0* (2.0)	−30.6* (2.8)
2	57.9	87.4	−29.5* (2.5)	−41.0* (3.4)
3	77.6	99.2	−21.6* (2.7)	−30.1* (3.7)
4	92.4	106.0	−13.6* (2.7)	−19.0* (3.8)
5	108.8	117.7	−8.9* (2.9)	−12.3* (4.1)
6	126.8	129.3	−2.5 (3.3)	−3.4 (4.6)
7	142.3	138.2	4.1 (3.5)	5.8 (4.9)
8	153.3	146.9	6.4 (3.6)	8.9 (5.0)
9	164.8	155.8	9.0* (3.6)	12.5* (5.1)
10	171.6	160.0	11.6* (3.6)	16.2* (5.0)
11	186.1	170.2	15.9* (3.7)	22.1* (5.1)
12	196.2	178.6	17.6* (4.0)	24.5* (5.5)
13	205.3	188.0	17.3* (4.1)	24.1* (5.7)
14	209.8	194.2	15.7* (4.2)	21.8* (5.8)
15	213.7	197.2	16.5* (4.1)	22.9* (5.7)
16	217.5	199.4	18.1* (4.1)	25.2* (5.8)
<i>Average total earnings, by year</i>				
1	3,513	4,661	−1,148* (109)	−1,595* (150)
2	6,875	6,931	−56 (150)	−78 (208)
3	9,286	8,590	696* (165)	968* (230)
4	10,990	10,163	828* (196)	1,150* (272)
<i>Percentage employed, by quarter</i>				
1	33.2	42.1	−8.9* (1.0)	−12.4* (1.3)
2	32.8	47.5	−14.7* (0.9)	−20.4* (1.3)
3	41.8	53.0	−11.1* (1.0)	−15.4* (1.3)
4	49.8	57.7	−7.9* (1.0)	−10.9* (1.3)
5	52.6	56.7	−4.1* (1.0)	−5.7* (1.3)
6	52.1	54.3	−2.2* (1.0)	−3.0* (1.3)
7	55.2	55.8	−0.6 (1.0)	−0.8 (1.3)
8	59.0	57.9	1.2 (1.0)	1.6 (1.3)
9	62.7	61.4	1.2 (0.9)	1.7 (1.3)
10	65.6	63.7	1.9* (0.9)	2.7* (1.3)
11	67.1	64.3	2.9* (0.9)	4.0* (1.3)
12	66.2	63.0	3.2* (0.9)	4.4* (1.3)
13	66.8	63.4	3.4* (0.9)	4.8* (1.3)
14	67.5	65.1	2.4* (0.9)	3.3* (1.3)
15	69.2	65.6	3.6* (0.9)	5.0* (1.2)
16	71.1	68.7	2.4* (0.9)	3.3* (1.2)
Sample size	6,828	4,485	11,313	

Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews. All figures were calculated using weights to adjust for the sample and survey designs.

Note: Standard errors are in parentheses.

^a Estimated impacts per eligible applicant are measured as the difference between the weighted means for treatment and control group members.

^b Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of treatment group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period.

* Significantly different from zero at the 0.05 level, two-tailed test.

the earnings gains for younger participants and make the program cost-effective for a population that has been extremely hard to serve. In particular, Job Corps needs to fully address differences by age in program structure and student program readiness, and to improve job placement services, which were found to be limited in scope and substance at the time of the study.

TABLE A2—STANDARD ERRORS OF IMPACTS PER PARTICIPANT ON EARNINGS AND ARREST RATES, FOR KEY SUBGROUPS

Subgroup (percentage of study population in parentheses)	Calendar year earnings (in 1995 dollars)					Percentage ever arrested during the 48-month period (survey)
	Survey		SER			
	1998	1998	1999	2002	2003	
Full sample	259	103	118	150	157	1.2
Age at application ^a	0.007	0.347	0.106	0.026	0.063	0.714
16 to 17 (41)	350	161	195	256	268	1.8
18 to 19 (32)	486	265	295	383	400	2.2
20 to 24 (27)	558	342	384	475	503	2.2
Gender ^a	0.308	0.046	0.320	0.337	0.870	0.027
Male (59)	345	180	205	261	274	1.6
Female (41)	366	220	251	322	335	1.6
Race and ethnicity ^a	0.066	0.317	0.584	0.543	0.206	0.604
White, non-Hispanic (27)	564	284	312	406	426	2.5
Black, non-Hispanic (47)	330	179	204	260	270	1.7
Hispanic (18)	623	384	454	567	600	2.7
Other ^b (8)	998	553	641	798	857	4.4
Residential/nonresidential status ^a	0.553	0.749	0.695	0.787	0.935	0.229
Residential designees (87)	280	149	171	215	226	1.3
Nonresidential designees (13)	670	423	466	624	653	2.7

Source: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; and (2) annual social security earnings SER records for the full sample. All figures were calculated using weights to adjust for the sample and survey designs.

Note: Estimated impacts per Job Corps participant are measured as the difference between the weighted means for treatment and control group members divided by the difference between the proportion of treatments who enrolled in Job Corps and the proportion of controls who enrolled in Job Corps.

^a Values in header rows display *p*-values for tests of significance of differences in impacts across subgroup levels.

^b This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

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