Contract No.: AF1254000000230
MPR Reference No.: 6095-300

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Policy Research, Inc.

National Job Corps Study and Longer-Term Follow-Up Study:

Impact and Benefit-Cost Findings Using Survey and Summary Earnings Records Data

Final Report

August 2006

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ACKNOWLEDGMENTS

We would like to thank those whose efforts have made this report possible. Michael Risha at the Social Security Administration obtained the earnings data for our sample, and patiently ran our computer programs several times. Leah Hackleman provided valuable editorial assistance. Finally, Bryan Gustus did an excellent job of producing this report.

CONTENTS

Section		Page
A	OVERVIEW OF JOB CORPS	4
B	OVERVIEW OF THE STUDY DESIGN	7
C	DATA, SAMPLES, AND ANALYTIC METHODS	9
	1. Survey Data	9
	2. Administrative Earnings Records	10
	3. Analytic Methods	11
D	IMPACT RESULTS FOR THE FULL SAMPLE	14
	1. Education and Training	15
	2. Literacy	18
	3. Employment and Earnings Impacts Based on the Survey Data	20
	4. Employment and Earnings Impacts Based on the SER Data in Years 1 to 4	22
	5. Employment and Earnings Impacts Based on the SER Data After Year 4	26
	6. Other Outcomes	28
E	IMPACT RESULTS FOR SUBGROUPS	31
	1. Youth Characteristics	31
	2. Residents and Nonresidents	34
	3. Center Subgroups	35
F	ASSESSING SURVEY NONRESPONSE BIAS	40
	1. Impact Findings for Survey Respondents and Nonrespondents	41
	2. Explanations for Survey Nonresponse Bias	43
G	COMPARING BENEFITS AND COSTS	44
	1. Overview of the Benefit-Cost Methodology	44
	2. Results for the Full Sample	49
	3. Results for the Older Youth	51

CONTENTS ***(continued)***

Section		Page
H	SUMMARY AND CONCLUSIONS	52
	REFERENCES	55
	APPENDIX A: ADDITIONAL TABLES PRESENTING EARNINGS IMPACTS FOR KEY YOUTH SUBGROUPS	A.1
	APPENDIX B: ADDITIONAL TABLES PRESENTING FINDINGS FROM THE BENEFIT-COST ANALYSIS	B.1

TABLES

Tables		Page
1	CHARACTERISTICS OF ELIGIBLE JOB CORPS APPLICANTS	7
2	IMPACTS ON KEY EDUCATIONAL OUTCOMES DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT	16
3	IMPACTS ON LITERACY SKILLS AT 30 MONTHS	19
4	HOURLY WAGES, FRINGE BENEFITS, AND OCCUPATIONS IN THE MOST RECENT JOB IN QUARTER 16	22
5	IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES, BY DATA SOURCE	24
6	IMPACTS ON KEY PUBLIC ASSISTANCE, CRIME, AND OTHER OUTCOMES DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT	29
7	IMPACTS PER PARTICIPANT ON EARNINGS AND ARREST RATES, FOR YOUTH SUBGROUPS AND FOR RESIDENTS AND NONRESIDENTS	32
8	IMPACTS PER PARTICIPANT ON EARNINGS AND ARREST RATES, FOR CENTER SUBGROUPS	36
9	IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR 48-MONTH SURVEY RESPONDENTS AND NONRESPONDENTS	42
10	BENEFITS AND COSTS OF JOB CORPS FROM SOCIETY'S PERSPECTIVE	50
11	BENEFITS AND COSTS UNDER DIFFERENT ASSUMPTIONS ABOUT THE SIZE OF THE EARNINGS IMPACTS AND THEIR DECAY, FOR ALL JOB CORPS PARTICIPANTS, BY DATA SOURCE	51

FIGURES

Figures		Page
1	AVERAGE EARNINGS PER WEEK BASED ON SURVEY DATA, BY QUARTER	20
2	IMPACTS ON CALENDAR YEAR EARNINGS, BY DATA SOURCE	23
3	TOTAL 1998 TO 2004 SER EARNINGS, BY CENTER PERFORMANCE LEVEL	40

Job Corps stands out as the nation's largest, most comprehensive education and job training program for disadvantaged youths. It serves disadvantaged 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 U.S. Department of Labor (DOL) on youth training and employment services. Because Job Corps is one of the most expensive education and training programs currently available to youths, DOL sponsored the National Job Corps Study (conducted from 1993 to mid-2004) to examine the effectiveness of the program. Mathematica Policy Research, Inc. (MPR), was the prime contractor for the study, with subcontractors Battelle Human Affairs Research Centers and Decision Information Resources, Inc. (DIR). DOL subsequently contracted with MPR to examine longer-term earnings impacts and benefit-cost comparisons using the same Job Corps sample and earnings data from administrative records.

The Job Corps evaluation was designed to address the following research questions:

- ***How effective is Job Corps overall at improving the outcomes of its participants?*** Does the program increase educational attainment and literacy? Does it reduce criminal behavior and the receipt of welfare benefits? And, most importantly, does it improve postprogram employment and earnings?
- ***Do Job Corps impacts differ across groups defined by youth and center characteristics and for residents and nonresidents?*** Do impacts differ by age, gender, race and ethnicity, arrest history, or educational level? Are impacts associated with center performance level, type of center operator, or center size?
- ***Do program benefits exceed program costs?*** Is Job Corps a good investment of society's resources?

The Job Corps study is based on an experimental design where, from late 1994 to early 1996, nearly 81,000 eligible applicants nationwide were randomly assigned to either a program group, whose members were allowed to enroll in Job Corps, or to a control group, whose 6,000

members were not. The study research questions have been addressed by comparing the outcomes of program and control group members using survey data collected during the four years after random assignment and using administrative earnings records covering the ten years after random assignment (at which point sample members were between the ages of 26 and 34).

This report is the final in a series of project reports presenting impact and benefit-cost findings from this large-scale random assignment evaluation of Job Corps.[1] The report serves two main purposes. First, it presents an additional year of earnings impacts to those presented in the previous project report (Schochet and Burghardt 2005) and updates findings from the benefit-cost analysis. Second, it places the earnings impact findings in perspective, by providing a comprehensive summary of key study findings across all project reports. Thus, this self-contained report pulls together and interprets the main evaluation results from the past twelve years.

A synopsis of study findings is as follows:

- ***Job Corps substantially increases the education and training services that youths receive. It also improves their educational attainment.*** During the four-year survey period, Job Corps increased the education and job training that participants in our sample received both inside and outside the program by about 1,000 hours. This is about equal to the hours of instruction received in a regular 10-month school year. Job Corps also substantially increased the receipt of GED and vocational certificates by more than 20 percentage points each.
- ***Job Corps improves literacy.*** Job Corps produced measurable improvements in literacy skills needed to function successfully in daily life.
- ***Job Corps generates earnings gains during the first two years after program exit.*** Statistically significant earnings gains were found using both survey and administrative records data in years 3 and 4 after random assignment. In year 4, the gain in earnings per participant was about $1,150, or 12 percent, according to the survey data. The gains were smaller according to the administrative records data,

[1] The key project reports include Johnson et al. (1999; process analysis); Burghardt et al. (1999; implementation of random assignment); Schochet et al. (2001; impacts based on survey data); McConnell and Glazerman (2001; initial benefit-cost analysis based on survey data); Schochet et al. (2003) and Schochet and Burghardt (2005; earnings impacts and updated benefit-cost analysis based on tax data).

primarily because reported earnings levels are about 75 percent higher according to the survey data and also due to survey nonresponse bias. Earnings gains were found across broad groups of students, and in particular, for both residential and nonresidential students.

- ***Overall, there are no long-term program impacts on earnings.*** According to the administrative records data, the estimated impacts in years 5 to 10 for the full sample are all near zero and none are statistically significant. Earnings impacts are zero for all youth subgroups, except for the oldest students.

- ***Earnings gains persist for those 20- to 24 years old at program application.*** Participation in Job Corps produced earnings gains for 20- to 24-year-olds (about one-quarter of Job Corps students) in years 5 to 10. These findings are consistent with other study findings that the older students remain in Job Corps longer than younger ones and are more highly motivated and disciplined.

- ***Earnings impacts are not associated with center performance level, type of center operator, or center size.*** Similar earnings gains were found for centers rated as high-, medium-, and low-performing based on the Job Corps performance measurement system. Thus, the performance measurement system does not seem to be achieving the goal of distinguishing between more and less effective centers. In addition, similar impacts were found for centers that are operated by government agencies and those that are operated by private contractors. Finally, similar impacts were found for students attending large, medium, and small centers.

- ***Job Corps significantly reduces involvement with crime.*** According to the survey data, the arrest rate was reduced by 16 percent (about 5 percentage points), and similar reductions were found also for conviction and incarceration rates. Reductions in criminal activity were found across all youth subgroups.

- ***Job Corps has modest or no impacts on a range of other outcomes.*** The survey data indicate that the program had small beneficial impacts on receipt of public assistance and on self-assessed health status, but no impacts on illegal drug use or fertility. Job Corps also had small positive impacts on the percentage married or living with a partner and on the percentage living on their own.

- ***Because overall earnings gains do not persist, the benefits to society of Job Corps are smaller than the substantial program costs.*** The finding that costs exceed benefits for the full sample holds under a wide range of reasonable assumptions. However, Job Corps appears to be cost-effective for the 20- to 24-year-olds, whose earnings impacts persisted in years 5 to 10. In addition, benefits exceed costs from the perspective of program participants.

The study findings suggest that the Job Corps model has promise; program participation increases educational attainment and literacy and reduces criminal activity and welfare receipt. It is also the only federal training program that has been shown to increase earnings for this

population for several years after program exit. These positive findings, however, need to be balanced against the lack of long-term earnings impacts for the overall sample based on the administrative records data, and the finding that social benefits do not appear to offset program costs except for the oldest students. Thus, the policy challenge is to improve program services to sustain the earnings gains for the younger students and make the program cost-effective for a population that has been extremely hard to serve.

The remainder of this report provides evidence for these conclusions and is divided into eight sections. In the first two sections, we provide a brief overview of Job Corps and then discuss the study design. In the next section, we discuss the data, samples, and statistical methods for the analysis. In the fourth and fifth sections, we present impact findings for the full sample and for population subgroups. In the sixth section, we assess the extent to which survey nonresponse influenced the estimated earnings impacts. In the seventh section, we present key findings from the benefit-cost analysis, where a dollar value is placed on the individual impact estimates and program benefits are compared to program costs. Finally, we present a synopsis of the study's findings.

A. OVERVIEW OF JOB CORPS

The Job Corps program, established by the Economic Opportunity Act of 1964, operates under the provisions of the Workforce Investment Act of 1998. At the time of the study, DOL administered Job Corps through a national office and nine regional offices.[2] Applicants must meet eleven criteria to be eligible for Job Corps. Some of the key criteria are that the applicant must be age 16 to 24; be a legal U.S. resident; be economically disadvantaged; need additional

[2] There are currently (2006) six regional offices.

education, training, or job skills; be free of serious behavioral problems; have a clean health history; and have an adequate child care plan (for those with children).

Job Corps services are delivered in three stages: outreach and admissions, center operations, and placement. Outreach and admissions (OA) counselors recruit students, inform them about the program, and ensure they meet eligibility criteria. Center operations, which are the heart of the program, involve vocational training, academic education, residential living, health care, and a wide range of other services, including counseling, social skills training, health education, and recreation. At the time of the study, these comprehensive services were delivered at 110 Job Corps centers nationwide. Most centers are operated by private contractors, although about one-quarter are operated by the U.S. Department of Agriculture and the U.S. Department of the Interior. For six months after the youths leave the program, placement agencies help participants find jobs or pursue additional training.

Most Job Corps students reside at the Job Corps center while training, although about 13 percent are nonresidential students who reside at home. Enrollment in Job Corps does not have a fixed duration (duration is eight months on average but varies widely). The program has a distinctive open-entry, open-exit educational philosophy, where instruction is individualized and self-paced. 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 GED certificate. Job Corps has a uniform, computer-based curriculum for major academic courses.

Job Corps has a number of distinctive characteristics: a high degree of uniformity in program form and content; a high degree of federal direction; and continuity, especially in center

operations (where in many cases the same contractor has been operating the center for decades). Job Corps also uses a comprehensive performance measurement system, which drives program operations; heavy emphasis is placed on a contractor's performance in awarding competitive contracts.

Using data from week-long visits to 23 randomly-selected centers and from surveys of outreach and admissions agencies and centers, 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 were found to be limited in scope and substance—relatively few students got help from placement agencies in securing a job or further training.

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 are members of racial or ethnic minority groups (nearly 50 percent are African American and 20 percent are Hispanic). About one-fourth of applicants (and nearly one-third of male applicants) had been in trouble with the law before applying to Job Corps. More than half lived in families that received public assistance while they were growing up. About 59 percent of youths are male. About 30 percent of females had children at program application. More than 40 percent of applicants were 16 or 17 years old, and about three-quarters were younger than 20.

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 (59 percent versus 39 percent), African American (47 percent versus 29 percent), 16 or 17 years old (41 percent versus 24 percent), without a high school credential (77 percent versus 53 percent), and from a large urban area (78 percent versus 70 percent) (Schochet 1998a).

TABLE 1

CHARACTERISTICS OF ELIGIBLE JOB CORPS APPLICANTS

Characteristic	Percentage of Eligible Applicants
Gender	
Male	59.4
Females without children	28.9
Females with children	11.7
Age at Application	
16 to 17	41.5
18 to 19	31.8
20 to 24	26.7
Race and Ethnicity	
White, non-Hispanic	27.1
Black, non-Hispanic	47.7
Hispanic	17.7
Other	7.6
Had a High School Credential	23.0
Arrest History (Self-Reported)	
Ever arrested	26.5
Arrested for serious crimes[a]	5.4
Family Received Welfare When Growing Up	53.4

Source: 14,327 eligible applicants who completed baseline interviews.

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

B. OVERVIEW OF THE STUDY DESIGN

The Job Corps evaluation is based on an experimental design in which, with a few exceptions, *all* youths nationwide who applied to Job Corps in the 48 contiguous states between November 1994 and December 1996 and were found to be eligible by the end of February 1996 were randomly assigned to either a program group or a control group.[3] 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

[3] Burghardt et al. (1999) provide more details on the sample design and the implementation of random assignment.

comparisons of program and control group outcomes represent the effects of Job Corps relative to *other* available programs that the study population would enroll in if Job Corps were not an option. Due to the nationally representative sample, study results can be generalized to the full Job Corps program as it operated at the time of the study.

The nonclustered design was adopted because it produced more precise impact estimates than a clustered design with the same sample size. Furthermore, this approach spread the burden of random assignment across all OA agencies and Job Corps centers, which helped sell random assignment to Job Corps staff. Over 1,300 Job Corps OA counselors nationwide were directly involved in random assignment.

The evaluation is based on large samples. Nearly 81,000 Job Corps-eligible applicants were randomly assigned to a research status. 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 program group), and the remaining youths were randomly assigned to a program nonresearch group (whose members could enroll in the program but were not followed for the study).

As expected, random assignment produced program 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 program and control groups, 5 were statistically significant at the 5 percent level, which is what would be expected by chance (Schochet 1998a). A joint test of differences across all baseline variables considered together yields a p-value of more than .70.

Job Corps staff implemented random assignment procedures well (Burghardt et al. 1999). In examining weekly extracts from the Job Corps Student Pay, Allotment, and Management Information System (SPAMIS) on all new center enrollees, we found that less than 0.6 percent of

enrollees arrived at a center without having been previously randomly assigned; thus, nearly all those in the study population were subject to random assignment. Furthermore, only 1.4 percent of control group members enrolled in Job Corps before the end of the three-year embargo period during which they were not supposed to enroll.[4] Hence, we believe that the research sample is representative of the youths in the intended study population, and that any potential bias in the impact estimates due to contamination of the control group is very small.

The study 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 OA counselors' activities and on the composition of the students coming to the program appear to have been modest.[5]

C. DATA, SAMPLES, AND ANALYTIC METHODS

The outcome measures for the analysis were obtained from two sources: (1) survey data covering the four years after random assignment and (2) administrative earnings (tax) records covering the ten years after random assignment. In this section, we discuss these data sources and analysis samples for each. In addition, we discuss statistical methods for the impact analysis.

1. Survey Data

Surveys were conducted at baseline (shortly after random assignment) and at 12, 30, and 48 months after random assignment. Interviews were conducted by telephone and in person for

[4] An additional 3.2 percent of control group members enrolled in Job Corps after their three-year restriction period ended.

[5] The presence of the control group without a substantial increase in recruitment led to a decrease in the percentage of center slots that were filled in the first half of 1995. However, Johnson et al. (1999) found no evidence that the reduction in the number of students affected center services.

those not reachable by telephone. During the baseline interview, in-person interviews were conducted in randomly selected areas only in order to conserve data collection costs, resulting in a survey sample that is slightly clustered (Schochet 2001). For the follow-up interviews, however, in-person interviews were conducted in all areas.

The research sample includes 11,313 youths (6,828 program group and 4,485 control group members) who completed a 48-month interview. The response rate to the 48-month interview was 79.9 percent (81.5 percent for the program group, and 77.4 percent for the control group). The distributions of baseline characteristics of program and control group members in the 48-month sample are very similar (Schochet 2001). The distribution of baseline characteristics, however, differs slightly for those in the 48-month sample and the full sample of respondents and nonrespondents. Thus, we adjusted the sample weights using propensity scoring procedures so that the observable baseline characteristics of the 48-month sample matched those of the full sample.

The interviews obtained information on outcome measures in the following areas that we hypothesized could by influenced by participation in Job Corps: (1) education and training; (2) employment and earnings; and (3) nonlabor market outcomes, including welfare receipt, crime, alcohol and illegal drug use, health, family formation, and child care.

2. Administrative Earnings Records

The evaluation relied also on 1993 to 2004 annual summary earnings records (SER) data reported by employers to the Internal Revenue Service (IRS), which are maintained by the Social Security Administration (SSA) to determine workers' eligibility for social security. The primary

source for these data is the W-2 form. The SER data cover about 96 percent of all workers in formal employment or self-employment (Social Security Administration 2001).[6]

The SER (tax) data were used for two purposes. First, they were used to verify the survey-based earnings impact estimates during the four-year period covered by the survey (roughly calendar years 1995 to 1998). Impact estimates using the two data sources could differ because of interview nonresponse or reporting or coverage differences. For instance, earnings from casual jobs or the underground economy are not covered in the SER data, although they may be reported in surveys. Second, the SER data were used to cost effectively provide longer-term earnings impacts through 2004 (when sample members were between the ages of 26 and 34).

To protect confidentiality, SSA does not release earnings data for individuals. Accordingly, SSA ran computer programs that we provided to estimate impacts and their associated levels of statistical significance for the full sample and for key subgroups of the Job Corps population. The sample for the analysis included 15,138 of the 15,301 youths in the full study sample who provided social security numbers and whose social security numbers were validated by SSA's Enumeration Verification System.

3. Analytic Methods

As discussed next, we estimated impacts for the full sample as well as for key subgroups defined by youth and center characteristics. All figures were calculated using sample weights to account for the sample design (and the survey design for estimates based on the survey data).

[6] The evaluation also collected 1999 to 2001 quarterly wage records reported by employers to state unemployment insurance (UI) agencies in 22 randomly selected states. Earnings impacts using these data are very similar to those using the SER data (Schochet et al. 2003) and are not presented in this report.

a. Estimating Impacts for the Full Sample

Random assignment was conducted at the point that program applicants were determined to be eligible for Job Corps, *not* at the point that they enrolled in Job Corps. Thus, differences between the average earnings of all program and control group members are impacts *per eligible applicant*. Consequently, these estimates include the 73 percent of program group members who enrolled in a Job Corps center at some point after random assignment as well as the 27 percent of program group members who did not enroll ("no-shows").

To obtain impact estimates *per program participant*, we divided the impact estimates per eligible applicant by the program participation rate (Bloom 1984; Angrist et al. 1996).[7] This approach is commonly used in the evaluation literature, and yields unbiased impact estimates for participants under the assumption that Job Corps has no effect on program no-shows. This is likely to be a tenable assumption, because the offer of a Job Corps slot without active participation is unlikely to appreciably affect a youth's long-term earnings.[8,9]

The impact estimates per participant should be interpreted as the difference between the mean outcomes of participants and what these outcomes would otherwise have been in the absence of Job Corps. Because the program participation rate was high, the findings for eligible applicants and program participants are very similar.

[7] We obtained Job Corps participation data from SPAMIS.

[8] There is no evidence that no-shows received additional counseling from Job Corps staff on options other than Job Corps, because their enrollment rates in other education and training programs were not unusual (Schochet et al. 2001).

[9] The impacts reported in this report are based on a variant of this statistical procedure where we account also for the 1.2 percent of the control group who enrolled in Job Corps during its three-year restriction period (Schochet 2001). This variant divides the estimated impacts per eligible applicant by the *difference* between the program group participation rate and the control group crossover rate. Standard errors of the impact estimates were inflated to account for the estimation error in the participation and crossover rates.

We also estimated regression-adjusted impacts using multivariate models to control for other factors measured at baseline that affect the outcome measures. The impact estimates and their standard errors using the regression approach are very similar to the simple weighted differences-in-means estimates that are presented in this report (Schochet 2001).

b. Estimating Impacts for Subgroups

An important part of the impact analysis was to assess whether impacts differed for subgroups defined by youth and center characteristics and for residential and nonresidential students. This information can be used by policymakers to improve program services and to target them appropriately.

Subgroup definitions and sample sizes are shown in Appendix Table A.1. The youth subgroups include age at application to Job Corps; gender, race and ethnicity; arrest history at random assignment; and educational level at random assignment. The center characteristics include type of operator (CCC or contract centers), size, and performance ranking. (For further description of center characteristics, see Burghardt and Schochet 2001.)

The impacts by residential status are of considerable policy interest for several reasons. First, the residential and nonresidential components serve students with different characteristics and needs; relative to residents in our sample, nonresidents are more likely to be female (70 percent versus 36 percent), to have had children at program application (49 percent versus 13 percent), and to be older (74 percent versus 56 percent were at least 18 years old at program application). Second, previous studies have found that disadvantaged youths do not benefit significantly from participation in nonresidential training programs (Orr et al. 1996 and Cave et al. 1993). Finally, the cost per participant is about 25 percent more for residential students than for nonresidential students (McConnell and Glazerman 2001).

Impacts for subgroups defined by youth characteristics were estimated by comparing the average outcomes of program and control group members in the group of interest. Impacts for females, for example, were computed by comparing the outcomes of females in the program and control groups. We conducted t-tests to determine the statistical significance of impact estimates for each subgroup, and F-tests to jointly determine whether impacts differed across levels of a subgroup— for example, across the three age groups or across males and females.

The estimation of impacts for the residential and center-related subgroups is complicated by missing data on residential and center assignments for control group members and program group no-shows. To account for this problem, we estimated impacts for these subgroups using *predictions* by Job Corps OA staff as to whether each applicant was likely to be assigned to a residential or nonresidential slot and the likely center assignment. These predictions were collected before random assignment (and thus, are available for both program and control group members), are rarely missing, and proved to be very accurate (Schochet 1998b). Thus, impacts for residential designees were estimated by comparing the mean outcomes of residential designees in the program and control groups and similarly for nonresidential designees and those in each center-level subgroup.

Finally, we used the SER data to estimate impacts separately for respondents and nonrespondents to the 48-month interview. We conducted this analysis to assess the extent to which interview nonresponse may have affected the survey-based earnings impact estimates.

D. IMPACT RESULTS FOR THE FULL SAMPLE

Program group members received extensive and meaningful Job Corps services (Schochet et al. 2001). Most enrolled in centers (about 73 percent), and participants typically enrolled soon after being found eligible (about six weeks on average). 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. Program "completers"—the 49 percent of participants who completed a vocational trade or GED in Job Corps—enrolled for about eleven months on average, compared to less than three months for program noncompleters.

Participants used center activities extensively. More than 82 percent reported receiving academic instruction, and nearly 89 percent received vocational training. The average participant received 1,140 hours of academic and vocational instruction, equivalent to about one year of high school classroom instruction. Participants also took part in the many socialization activities in Job Corps, such as parenting education, health education, social skills training, and cultural awareness classes.

1. Education and Training

Impacts on education and training outcomes depend critically on what education and training the control group received, because these youths were allowed to enroll in education and training programs other than Job Corps. We find, using survey data, that many control group members received substantial amounts of education and training (Table 2). More than 70 percent participated in an education or training program during the 48 months after random assignment. On average, they received about 850 hours of education and training, roughly equivalent to three-quarters of a year of high school. Participation rates were highest in programs that substitute for Job Corps: GED programs (37 percent); high school (32 percent); and vocational, technical, or trade schools (29 percent) (not shown).

Because control group members demonstrated the motivation to go to Job Corps, it is not surprising that many had the motivation to find other programs. These results highlight that the counterfactual for the evaluation is active participation in education and training programs other than Job Corps, rather than the absence of such participation. Furthermore, they highlight that study findings pertain to a specialized group of motivated youths interested in Job Corps rather

TABLE 2

IMPACTS ON KEY EDUCATIONAL OUTCOMES DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT

	Program 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***	28.9***
Average Percentage of Weeks Ever in Education or Training	24.4	18.2	6.3***	8.7***
Average Hours Ever in Education or Training	1,559.8	848.2	711.6***	989.0***
Vocational Training	788.2	231.5	556.8***	773.8***
Academic Classes	771.6	616.8	154.8***	215.2***
Degrees, Diplomas, and Certificates Received (Percentage)				
GED certificate[c]	41.6	26.6	15.0***	20.9***
High school diploma[c]	5.3	7.5	–2.2***	–3.1***
Vocational, technical, or trade certificate	37.5	15.2	22.3***	30.9***
College degree (two- or four-year)	1.3	1.5	–0.2	–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.

[a]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program 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 program 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.

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

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

than to average disadvantaged youths nationwide (Job Corps serves only a small fraction of disadvantaged youths).

Despite the activity of the control group, Job Corps substantially increased the education and training that program participants received (Table 2). Over the four year follow-up period, ninety-three percent of the program group engaged in some education or training (either from Job Corps or elsewhere), compared to 72 percent of the control group. Job Corps participants spent about 1,000 hours in total—about 5 hours per week—more in education or training than they would have if they had not enrolled in the program.[10] This impact per participant corresponds to *roughly one high school year*. The impact per participant 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 effects on the receipt of credentials it emphasizes most: GED and vocational certificates (Table 2). Among those program group members without a high school credential at random assignment, about 42 percent obtained a GED during the 48-month period, compared to only 27 percent of control group members. However, slightly more members of the control group earned a high school diploma. This is probably because only about one-quarter of Job Corps centers were accredited to grant high school diplomas, and some control group members returned to high school. (Very few control group members graduated, however, because on average they had only completed the tenth grade when applying for Job Corps and returned to high school for an average of only about nine months.) About 38 percent of program

[10] About 63 percent of the program group enrolled in programs *other* than Job Corps (Schochet et al. [2001] display enrollment rates for program group Job Corps participants and no-shows). The impact per participant on hours per week spent in programs other than Job Corps was about –2 hours per week.

group members reported receiving a vocational certificate, compared to about 15 percent of the control group.[11] Job Corps, however, had no effect on college attendance or completion.

2. Literacy

The study also measured impacts on participants' functional literacy—the ability to perform a wide variety of information processing tasks that adults encounter in everyday life (Glazerman et al. 2000). The assessment developed by the Educational Testing Service for the National Adult Literacy Study (Kirsch et al. 1993) was administered in person to a random subsample of about 2,300 program and control group members in conjunction with the 30-month interview. The response rate to the literacy study was about 60 percent.

The test developers identified five broad levels for each dimension of literacy (225 or less, 225 to 275, 275 to 325, 325 to 375, and 375 to 500). Not surprisingly, eligible Job Corps applicants, as measured by the performance of the study's control group, scored relatively low on these tests compared to young adults nationally. In the national sample of young adults, approximately 15 percent scored in the lowest level of each dimension. Among the control group, 27 percent scored in the lowest level of prose, 20 percent scored in the lowest level of document literacy, and 44 percent scored in the lowest level of quantitative literacy (Table 3).

Job Corps improved participants' functional literacy; the program group had higher average scores on the assessment measures than the control group (Table 3).[12] The impacts per participant were about 4 points on the prose scale, 2 points on the document scale, and 5 points

[11] Because of the emphasis given to documenting progress and certifying vocational completion in Job Corps, participants might have been influenced by this emphasis and over-reported their certificate completion. However, the impacts on vocational certification are in line with impacts on the receipt of vocational training, which lends credence to the findings.

[12] Due to relatively small sample sizes and relatively low response rates for the literacy study, we present regression-adjusted estimates in Table 3 rather than the simple differences-in-means estimates that are presented elsewhere in this report.

TABLE 3

IMPACTS ON LITERACY SKILLS AT 30 MONTHS

	Program Group	Control Group	Estimated Impact per Eligible Applicant[a]	Estimated Impact per Participant[b]
Prose (Percentages)[c]			*0.077**	
Level I	24.3	26.8	–2.5	–3.2
Level II	48.2	48.2	0.0	0.0
Level III	24.2	22.2	2.1	2.8
Level IV	3.2	2.8	0.4	0.6
Level V	0.0	0.0	0.0	0.0
Average Prose Proficiency	251.0	248.3	2.7*	3.7*
Document (Percentages)[c]			*0.339*	
Level I	18.9	20.0	–1.1	–1.5
Level II	47.4	47.8	–0.4	–0.5
Level III	29.3	28.1	1.2	1.7
Level IV	4.2	3.9	0.3	0.4
Level V	0.0	0.1	0.0	0.0
Average Document Proficiency	257.6	256.4	1.1	1.6
Quantitative (Percentages)[c]			*0.039***	
Level I	40.7	44.4	–3.7	–5.0
Level II	38.6	37.3	1.3	1.8
Level III	18.5	16.5	2.1	2.8
Level IV	1.9	1.6	0.3	0.4
Level V	0.2	0.2	0.0	0.1
Average Quantitative Proficiency	234.8	231.2	3.6*	4.9**
Sample Size	**1,117**	**1,156**	**2,273**	

Source: Data from Job Corps literacy assessments conducted in person with a random subset of sample members in conjunction with the 30-month follow-up interview.

Notes: All estimates are regression adjusted. Level I scores are 225 or less, Level II scores are between 225 and 275, Level III scores are between 275 and 325, Level IV scores are between 325 and 375, and Level V scores are between 375 and 500.

[a]Estimated impacts per eligible applicant are measured as the difference between the regression-adjusted means for program 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 program 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.

[c]Figures in header rows are p-values for statistical tests for differences in the distribution of test scores across the program and control groups.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

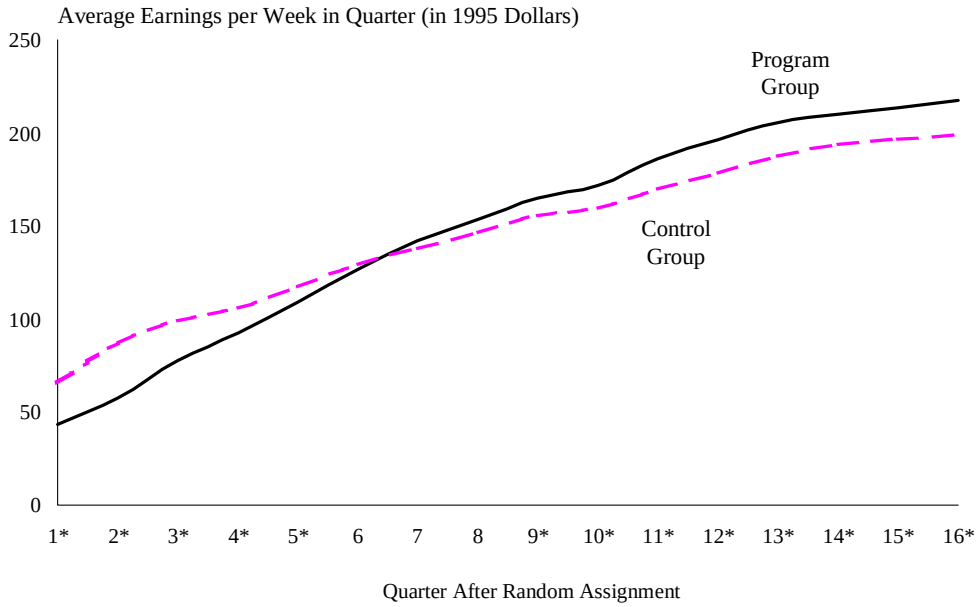
on the quantitative scale, with most of the gains due to movements out of Level I. The impacts on the average prose and quantitative scale are statistically significant at the 10 percent level. The impacts in effect size units are about .10 standard deviations.

3. Employment and Earnings Impacts Based on the Survey Data

The survey data indicate that Job Corps generated positive earnings impacts beginning in the third year after random assignment, and the impacts persisted through the end of the four-year follow-up period (Figure 1 and Table A.2). The control group's earnings were larger than those of the program group early in the follow-up period, because many program group members were

FIGURE 1

AVERAGE EARNINGS PER WEEK BASED ON SURVEY DATA, BY QUARTER



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

Note: Appendix Table A.2 displays the full set of estimates used to construct this figure.

*Difference between the mean outcome for program and control group members is statistically significant at the .05 level, two-tailed test. This difference is the estimated impact per eligible applicant.

enrolled in Job Corps then. It took about two years after random assignment for the program group's earnings to overtake those of the control group. The impacts grew in the third year of the follow-up period and persisted in the fourth year. In the fourth year, average weekly earnings for program group members were $211, compared to $195 for control group members. The estimated impact per Job Corps participant was $22 per week (or $1,150 in total) in the fourth follow-up year, which translates into a 12 percent earnings gain. These impacts are statistically significant at the 1 percent level. Impacts on employment rates (Table A.2) and hours of work followed similar patterns. Our decomposition analysis suggests that two-thirds of the earnings impact was due to the impact on hours worked and that one-third was due to the impact on earnings per hour (Schochet et al. 2001).

The survey data also indicate also that program group members found slightly higher-paying jobs with slightly more benefits (Table 4). Employed program group members earned an average of $0.22 more per hour than employed control group members in their most recent job in the last quarter of the fourth follow-up year ($7.55 compared to $7.33). In addition, employed program group members were slightly more likely than employed control group members to hold jobs that offered fringe benefits. For example, in the most recent job in the fourth year, about 57 percent of the employed program group received health insurance, compared to 54 percent of the employed control group, and about 48 percent of employed program group members were offered retirement or pension benefits, compared to 44 percent of employed control group members. The percentages of program and control group members who worked in each of several broad occupational areas, however, were similar, with more than 40 percent working in low-wage service and construction occupations.

TABLE 4

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

Outcome Measure	Program Group	Control Group	Difference[a]
Percent Employed in Quarter 16	71.1	68.7	2.4***
Average Hourly Wage (in 1995 Dollars)[a]	7.55	7.33	0.22
Benefits Available (Percentage)[a]			
Health insurance	57.4	54.3	3.0**
Paid vacation	62.9	60.7	2.2*
Retirement or pension benefits	48.3	43.7	4.6***
Occupation (Percentage)[a,b]			*.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.

[a]Because these estimates are conditional on being employed in quarter 16, they are not impact estimates.

[b]The value in the header row displays the p-value for a significance test of differences in the occupational distributions across the research groups.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

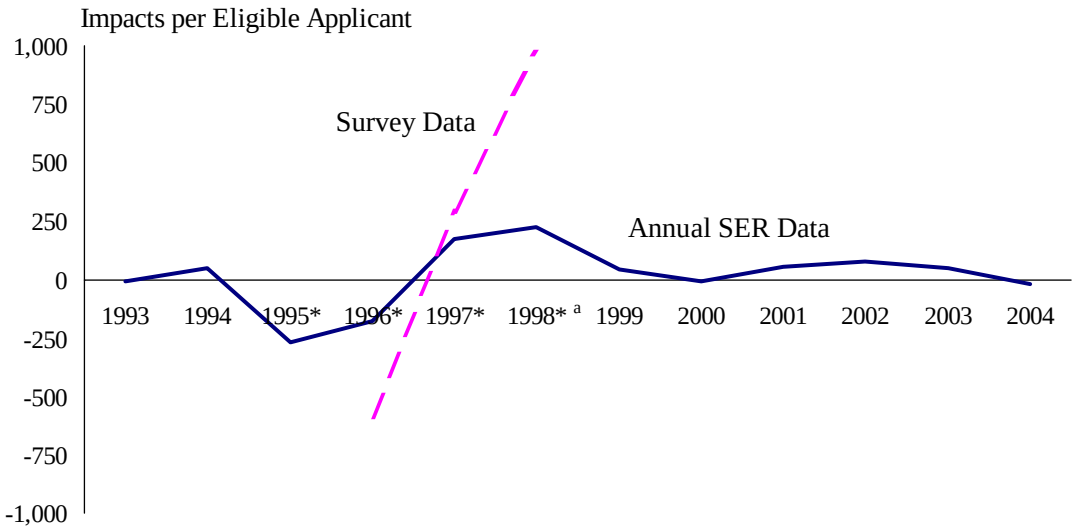
4. Employment and Earnings Impacts Based on the SER Data in Years 1 to 4

The *pattern* of the estimated impacts on annual earnings using the survey and SER data is similar for the calendar years covered by both data sources (Figure 2 and Table 5).[13] However, the survey-based impact estimates are larger. For instance, in 1998, the earnings impacts were

[13] The survey-based impact estimates differ slightly in Table 5 and Appendix Table A.2, because earnings in Table 5 are measured in *calendar* time, whereas earnings in Appendix Table A.2 are measured in the time since *random assignment*.

FIGURE 2

IMPACTS ON CALENDAR YEAR EARNINGS, BY DATA SOURCE



Source: Annual social security earnings SER records.

Note: Table 5 displays the full set of estimates used to construct this figure.

[a]Impact is statistically significant at the .05 level, two-tailed test using the SER data but not the survey data.

*Difference between the mean outcome for program and control group members is statistically significant at the .05 level, two-tailed test. This difference is the estimated impact per eligible applicant.

positive and statistically significant using both data sources, but the impact estimate per eligible applicant using the survey data is $972 (or a 10.4 percent earnings gain relative to the control group mean), compared to $222 (or a 4.0 percent earnings gain) using the SER data. Similarly, the estimated impact per eligible applicant on the annual employment rate in 1998 is larger using the survey data (2.4 percentage points, compared to 1.3 percentage points).

The survey-based impact estimates are larger for two main reasons (Schochet et al. 2003). First, reported earnings *levels* are about 75 percent higher according to the survey than SER data (Table 5). Second, according to the SER data, impacts for 48-month interview respondents are larger than those for 48-month interview nonrespondents, suggesting that the survey-based impact estimates are slightly biased upwards (see below).

TABLE 5

IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES, BY DATA SOURCE

Outcome Measure	Survey Data				Annual Social Security Earnings SER Records			
	Program Group	Control Group	Estimated Impact per Eligible Applicant[a]	Estimated Impact per Participant[b]	Program Group	Control Group	Estimated Impact per Eligible Applicant[a]	Estimated Impact per Participant[b]
Average Calendar Year Earnings (in 1995 Dollars)								
1993					1,010	1,016	–7	
1994					1,590	1,543	47	
1995					1,761	2,030	–270***	–368***
1996	5,145	5,729	–584***	–812***	3,101	3,279	–178***	–243***
1997	8,111	7,819	292*	406*	4,559	4,385	175**	238**
1998	10,296	9,324	972***	1,350***	5,831	5,610	222**	302**
1999					6,701	6,658	43	59
2000					7,601	7,611	–11	–14
2001					7,850	7,795	56	76
2002					7,820	7,741	79	107
2003					7,914	7,865	49	67
2004					8,384	8,401	-18	-25
1998 to 2004					52,101	51,682	419	571
Percentage Employed in Calendar Year								
1993					43.0	43.1	–0.1	
1994					59.5	58.8	0.7	
1995[c]					89.2	73.3	15.9***	21.6***
1996[c]	70.4	74.5	–4.2***	–5.8***	88.8	78.4	10.3***	14.0***
1997	77.7	76.9	0.8	1.1	83.6	81.5	2.1***	2.8***
1998	81.4	78.9	2.4***	3.4***	84.6	83.3	1.3**	1.7**
1999					84.5	83.0	1.5**	2.0**
2000					83.6	83.0	0.6	0.8
2001					80.6	80.4	0.3	0.4
2002					76.8	76.7	0.1	0.1
2003					74.5	73.7	0.7	0.9
2004					74.2	73.6	0.6	0.8
Sample Size	**6,828**	**4,485**	**11,313**		**9,264**	**5,874**	**15,138**	

TABLE 5 *(continued)*

Sources: (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 research sample.

Note: Blank entries signify that figures are not applicable.

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

[b]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[c]Employment rates in the SER data are high for the program 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 .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

Schochet et al. (2003) tried to unravel reporting differences in earnings levels according to the survey and administrative records data by comparing survey data to individual-level Unemployment Insurance (UI) wage records from 22 randomly-selected states. (The UI data produced very similar impact estimates to those based on the SER data.) The survey-UI differences are partly explained by under-the-table earnings not reported in the UI data, the noncoverage of some formal jobs in the UI program, the overreporting of hours worked in the survey data, mismatched records, and incomplete reporting of taxable earnings by employers. However, substantial residual factors remain.

The differences between the reporting levels based on the survey and administrative records data are somewhat larger than those found in previous studies using similar populations. For example, Kornfeld and Bloom (1999) found that mean quarterly earnings were about 35 to 70 percent higher according to the survey data than the UI data for youths in the National Job Training Partnership Act (JTPA) Study. Similarly, Cave (1995) found that the survey-to-UI ratios ranged from about 1.05 to 1.80 from several studies that examined the earnings of welfare recipients in welfare-to-work demonstration programs. One possible explanation for these findings is that the National Job Corps Study was conducted during a period of stronger economic growth. Thus, in the tight labor market, the Job Corps sample may have been more likely to collect under-the-table earnings from casual or cash-only jobs that are reported in the survey but not in the administrative data.

5. Employment and Earnings Impacts Based on the SER Data After Year 4

Based on the SER data, we find no impacts of Job Corps on employment or earnings *after* the four-year period covered by the survey. The estimated impacts on calendar year earnings in 1999 to 2004 are all near zero and not statistically significant (Figure 2 and Table 5). No 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 Hoynes 1999 and Katz and Krueger 1999), or as economic conditions worsened between 2001 and 2004 (as the employment rate decreased for both research groups).

Thus, the modest impacts on labor market activities that occurred during the initial postprogram period—that were likely caused by positive impacts on time spent in education and training, increases in the attainment of GED and vocational certificates, modest gains in functional literacy, and perhaps by the job placement services offered by Job Corps—did not put the program group on a different earnings trajectory than the control group. Instead, the earnings differences faded as both groups gained job-specific skills through increased work experience. The earnings of both groups increased over time, but youths in both groups continued to have low-paying, intermittent jobs, as demonstrated by the very low earnings levels reported in the tax data (in 2004, the average worker in both groups earned about $11,350, and only 10 percent of the sample earned more than $22,500).[14]

It is impossible to say whether estimated impacts based on survey data would have also disappeared after 1998. However, it is plausible that the decline in the survey impacts would have mirrored the decline in the SER impacts, because the pattern of impacts estimated using the SER and survey data were similar in overlapping periods, and we have no reason to expect this pattern to have diverged. Furthermore, a decline in the SER impacts without a decline in the survey impacts would suggest a growing impact on earnings from informal jobs. It would be difficult to imagine a scenario where Job Corps had a growing impact on earnings from informal jobs at the same time as it had a declining impact on earnings from formal jobs.

[14] The earnings impacts are not likely to have been materially affected by differences across the research groups in school enrollment rates or control group enrollment rates in Job Corps. Only about 13 percent of both the program and control groups were enrolled in school at 48 months after random assignment, and only about 1 percent of control group members were still enrolled in Job Corps after the four-year survey period.

Finally, the 1999 to 2004 earnings impacts for Job Corps completers—the 49 percent of program group participants who completed a vocational trade or GED in Job Corps—are also likely to be zero (Schochet et al. 2005). This is because the weighted average of the impacts for Job Corps no-shows, noncompleters, and completers must equal the zero impacts for eligible applicants (with weights .27, .36, and .37, respectively). Thus, impacts on completers could be large and positive only if impacts on noncompleters are equally large and negative. We believe, however, that this is unlikely, because it is difficult to construct a reasonable scenario in which a relatively brief stint in Job Corps for noncompleters (about 2.7 months on average) would actually cause their long-term earnings to be lower than they would have been without Job Corps (although their short-term earnings could have been negatively affected). Thus, our best estimate is that earnings impacts in 1999 to 2004 were zero for both the program completers and noncompleters.

6. Other Outcomes

Using survey data, the study examined the impacts of Job Corps on several additional outcomes to help assess whether the program achieves its goals of helping students become more responsible and productive citizens. A summary of key findings is displayed in Table 6.

Job Corps participation reduced the receipt of public assistance benefits. The program reduced the receipt of cash welfare plus food stamps by about $640 per participant—a statistically significant impact at the one percent level. The estimated program impacts on the receipt of individual types of assistance were small and in many cases not statistically significant. Impacts on welfare receipt were larger during the early part of the follow-up period but persisted through the end of the follow-up period.

Job Corps significantly reduced arrest and conviction rates, as well as time spent incarcerated. About 33 percent of control group members were arrested during the 48-month

TABLE 6

IMPACTS ON KEY PUBLIC ASSISTANCE, CRIME, AND OTHER OUTCOMES DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant[a]	Estimated Impact per Participant[b]
Average Amount of Benefits Received (in Dollars)[c]	3,696.0	4,155.7	–459.8***	–638.9***
Percentage Arrested or Charged with a Delinquency or Criminal Complaint	28.8	32.6	–3.7***	–5.2***
Percentage Convicted, Pled Guilty, or Adjudged Delinquent	22.1	25.2	–3.1***	–4.3***
Percentage Served Time in Jail for Convictions	15.8	17.9	–2.1***	–2.9***
Average Weeks in Jail for Convictions	6.0	6.6	–0.6	–0.8
Percentage Ever a Victim of a Crime at 12 Months	21.9	24.2	–2.3***	–3.1***
Percentage Reported Using Illegal Drugs in the Past 30 Days[d]				
At 12 months	9.9	9.5	0.4	0.6
At 30 months	8.7	8.8	–0.1	–0.1
At 48 months	7.4	7.7	–0.3	–0.4
Percentage in Fair or Poor Health at 48 Months	16.5	17.8	–1.3*	–1.8*
Percentage Married or Living Together at 48 Months	31.0	29.4	1.6*	2.2*
Percentage Not Living with Parents at 48 Months	68.2	65.3	2.9***	4.0***
Percentage Had New Children	39.0	37.8	1.2	1.7
Average Hours per Week Females Used Child Care	7.7	6.5	1.2***	1.8***
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.

[a]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program 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 program 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.

[c]Benefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance.

[d]Illegal drugs include marijuana, hashish, and hard drugs (cocaine powder; crack cocaine; speed, uppers, or methamphetamines; hallucinogenic drugs; and heroin, opium, methadone, or downers).

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

follow-up period, compared to 29 percent of program group members (a statistically significant reduction). Arrest rate reductions were largest during the first year after random assignment, (when most program enrollees were in Job Corps and heavily supervised). Interestingly, however, Job Corps also led to small arrest reductions during the later months of the follow-up period, after most youths had left Job Corps. Although program group members 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). The reductions in convictions and incarcerations for convictions follow a similar pattern.[15]

Job Corps participation also led to reductions in crimes committed against program participants, suggesting that Job Corps students were safer at the centers than they would have been in their home communities. On average, Job Corps reduced the number of times youths were victims of crimes by about 20 percent during the first 12 months after random assignment (when many program group members were enrolled in Job Corps).

We find small impacts on other outcomes. A slightly higher percentage of the control than the program group said their health was "poor" or "fair" (18 percent versus 16 percent). In addition, Job Corps participants were slightly more likely to be living independently at the 48-month interview point and to be married or living with a partner. We find no impacts on self-reported use of illegal drugs or on bearing children (about 38 percent of both groups had a child during the follow-up period; 49 percent for females and 31 percent for males). Job Corps

[15] Needels et al. (2000) present impact results on arrests and convictions covering the 30-month period after random assignment using official crime records from North Carolina and Texas. These estimated impacts are consistent with the impacts based on the survey data, suggesting that the reliance on self-reports for the impact analysis is unlikely to have created serious bias in the survey-based estimates of crime impacts.

participation, however, led to increases in the use of child care by females while they were in Job Corps and afterwards, due to positive impacts on their employment rates in years 3 and 4.

E. IMPACT RESULTS FOR SUBGROUPS

The impact findings for most subgroups are very similar to those for the full population. However, a few important exceptions to this generalization exist for the earnings impacts: (1) the earnings gains for the oldest students persisted beyond the survey period, and (2) no earnings gains were ever found for Hispanics or those 18 and 19 years old at program application.

1. Youth Characteristics

All subgroups of program group participants that we examined received extensive Job Corps services, and although many groups had different program experiences, the differences were small (Schochet et al. 2001). Consequently, impacts on total time spent in education and training programs and on the attainment of a GED or vocational certificate were large and statistically significant for all key subgroups.

The *survey* data indicate that positive earnings impacts in 1998 were found broadly across key subgroups (Tables 7 and A.3 to A.10).[16] Earnings gains were similar for males and females. Positive survey-based impacts were found for groups of students at special risk of poor outcomes (such as very young students, youths who had been arrested for nonserious offenses, and those who did not possess a high school credential at baseline). They were also found for groups at lower risk, such as older participants with a high school credential at baseline. Earnings gains were found for whites and African Americans.

[16] To keep the presentation manageable, we present the full set of earnings impact estimates in Appendix A only for the most important subgroups defined by age, gender, race and ethnicity, and residential status.

TABLE 7

IMPACTS PER PARTICIPANT ON EARNINGS AND ARREST RATES, FOR YOUTH SUBGROUPS AND FOR RESIDENTS AND NONRESIDENTS

	Impacts on Calendar Year Earnings (in 1995 Dollars)					Percentage Ever Arrested During the 48-Month Period
	Survey	SER Data				
Subgroup	1998	1998	1999	2002	2004	(Survey)
Full Sample	1,350***	302**	59	107	–25	–5.2***
Age at Application[a]	***			**		
16 to 17	1,307***	196	5	–28	–194	–4.3**
18 to 19	297	89	–362	–495	–463	–6.7***
20 to 24	2,663***	711**	628	1,060**	782	–4.5**
Gender[a]		**				**
Male	1,530***	518***	195	246	9	–6.8***
Female	1,134***	–24	–140	–97	–57	–2.2
Race and Ethnicity[a]	*					
White, non-Hispanic	2,459***	616**	24	439	260	–5.9**
Black, non-Hispanic	1,178***	348*	131	117	–151	–5.4***
Hispanic	187	–234	–330	–341	–179	–2.5
Other[b]	1,227	164	624	47	264	–9.0**
Educational Attainment at Baseline[a]						
High school credential	1,285**	169	489	669	131	–6.0***
No high school credential	1,399***	404***	34	131	50	–5.1***
Arrest History at Baseline[a]			***	**		
Never arrested	1,212***	401**	235	205	134	–5.3***
Arrested for nonserious crimes only[c]	1,753***	421	112	553	–248	–1.3
Arrested for serious crimes[c]	1,021	–857	–1,694**	–2076**	–915	–4.7
Residential/Nonresidential Status[a]						
Residents	1,378***	312**	85	130	–36	–5.6***
Nonresidents	1,149*	245	–118	–33	89	–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.

Note: Estimated impacts per Job Corps participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of program 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.

[a]Stars in the header rows signify that differences in impacts across subgroup levels are statistically significant.

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

[c]Serious crimes include aggravated assault, murder, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

The key exceptions are that no earnings gains were found for Hispanics or for those 18 to 19 at program application. We are not able to provide a satisfactory explanation for these exceptions, although we have been able to rule out several possible explanations (Schochet et al. 2001). In particular, the lack of impacts is not due to differences in the length of time in Job Corps—Hispanic students participated for nearly a month longer on average than non-Hispanic students, and Job Corps participation measures did not differ by age. Nor do the lack of impacts appear to have been due to the fact that Hispanic students exhibited other characteristics associated with low impacts; overall, the characteristics of Hispanic students and African American students are very similar (apart from primary language and region of residence), and the characteristics of those 18 and 19 are not unusual. Finally, the findings are not due to language barriers for Hispanics or the characteristics of centers or regions in which Hispanic or 18- and 19-year-old students are concentrated; we found no impacts for all subgroups of Hispanics and 18- and 19-year olds.

In general, the pattern of the estimated subgroup impacts on earnings using the *survey* and *SER* data are similar in periods covered by both data sources (Tables 7 and A.3 to A.10). However, as with the full sample, the survey-based impact estimates are larger and more often statistically significant, which results in several notable differences between the subgroup findings using the two data sources. First, for the 16- and 17-year-olds, the impact estimate in 1997 is statistically significant according to both the survey and tax data ($791 and $391, respectively), but the positive impact disappeared in 1998 according to the tax data. Second, for females, the impact estimates in 1997 and 1998 are statistically significant according to the survey data but not according to the tax data. Thus, these groups may have been more likely than other groups to have held informal (cash-only) jobs with earnings that were not covered in the SER data (Schochet et al. 2003).

In the 1999 to 2004 period, no statistically significant, positive *SER-based* earnings impacts were found for any subgroup, except for those ages 20 to 24 at program application. The earnings impacts for the oldest students were about $750 per year during this period, and were statistically significant in 2002 and 2003. Furthermore, the impact on *total* postprogram earnings for this group is statistically significant at the 10 percent level, and differs significantly from the corresponding impacts for the younger students. The impacts for the oldest students are larger for males than females and for whites than other racial and ethnic groups (Table A.13).

The findings for 20- to 24-year-olds are consistent with other project findings that older students in our sample remained in Job Corps for an average of 1.3 months longer than younger ones and were more highly motivated and well-behaved (as reported by program staff at 23 randomly-selected centers). In addition, the estimated impacts on total hours spent in education and training during the four-year survey period were larger for the older students than the younger ones, because older students stayed longer in Job Corps and because a large percentage (nearly half) of the younger control group members attended high school after being rejected for Job Corps (Schochet et al. 2001).

Finally, impacts on crime are very similar across the youth subgroups (Table 7). Although the level of criminal activity differed substantially across youth subgroups, the impacts on crime outcomes in percentage terms are similar. For instance, although the control group arrest rate was about 39 percent for males and 15 percent for females, Job Corps participation reduced the arrest rate by about 15 percent for both groups.

2. Residents and Nonresidents

Both residents and nonresidents received substantial Job Corps services. Our survey data indicate that nonresidential students had somewhat lower enrollment rates than residential ones (66 percent, compared to 75 percent). Once in Job Corps, however, the residents and

nonresidents got similar amounts, types, and intensity of education and training. Thus, it is not surprising that we find similar impacts on education and training outcomes for the two groups of participants.

The survey data indicate that both the residential and nonresidential program components improved earnings for the students they served soon after the youths left Job Corps (Tables 7, A.11, and A.12). The survey-based 1998 earnings impact estimate is statistically significant at the 5 percent level for residents and at the 10 percent level for the smaller sample of nonresidents, and the difference between the two impacts is not statistically significant. Similar to previous findings, however, the 1998 earnings impact estimates are smaller according to the SER data than the survey data for both residential groups, although the SER-based impact remains statistically significant for the residents.

After 1998, the earnings impacts according to the tax data are zero for both residential groups. Thus, Job Corps does not appear to improve the long-term earnings for either residential group.

Finally, participation in each component reduces arrest rates (Table 7). Arrest rates were much higher for residential control group members (31 percent) than nonresidential control group members (18 percent), who were more likely to be older and female. However, the arrest rate reductions as a percentage of the control group means are similar for the two residential groups (about 15 percent each), and the difference between the two arrest rate impacts is not statistically significant.

3. Center Subgroups

In this section, we discuss the impact findings for subgroups defined by type of center operator (CCCs and contract centers), center size, and center performance level. Table 8 presents impact estimates where the individual is treated as the unit of analysis (as is the case for all other

TABLE 8

IMPACTS PER PARTICIPANT ON EARNINGS AND ARREST RATES, FOR CENTER SUBGROUPS

	Impacts on Calendar Year Earnings (in 1995 Dollars)					Percentage Ever Arrested During the 48-Month Period
	Survey	SER Data				
Subgroup	1998	1998	1999	2002	2004	(Survey)
Full Sample	1,350***	302**	59	107	–25	–5.2***
Type of Center Operator[a]						
Contract centers	1,388***	275*	81	33	–42	–3.4***
CCC centers	1,730**	562	16	376	247	–8.8***
Center Size[a]						**
Small (< 226 slots)	1,103*	275	36	388	119	–7.7***
Medium (226 to 495 slots)	1,664***	396*	131	299	267	–6.1***
Large (> 495 slots)	1,355***	250	17	–353	–395	0.2
Performance Level[a,b]						
High	1,578**	754	252	–158	–47	–6.6**
Medium	1,304***	212	–87	–36	–194	–3.4**
Low	1,823***	347	471	696	737	–4.9*

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.

Note: Estimated impacts per Job Corps participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of program 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.

[a]Stars in the header rows signify that differences in impacts across subgroup levels are statistically significant.

[b]High-performing centers are defined as those that were in the top third of the performance ranking during program years 1994 and 1996. Similarly, low-performing centers are those that were in the bottom third of the performance ranking in each year; and the remaining centers are designated medium-performing centers.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

estimates presented in this report), and Table A.14 presents estimates where the *center* is treated as the unit of analysis, so that each center is given equal weight in the analysis.[17]

a. Type of Center Operator

Most Job Corps centers are operated by private organizations under competitively awarded contracts with DOL. At the time of the study, approximately 80 contract centers served about 88 percent of new students. Thirty CCCs were operated by agencies of the U.S. Department of Agriculture and the U.S. Department of the Interior. The two types of centers differ in important ways, including (1) type of staff (CCC employees are federal civil service employees, while contract center staff are employees of private for-profit and nonprofit organizations); (2) type of procurements (unlike CCCs, operators of contract centers must win competitive procurements to continue operating their centers); (3) size and location (most CCCs are small and located in isolated rural areas in the Pacific Northwest or Mountain states); (4) the trades offered (the CCC trades are heavily weighted toward construction); and (5) student characteristics (more students at CCCs are male, under age 18 at enrollment, white, without a high school credential at enrollment, and likely to have been arrested).

Despite the many differences between CCCs and contract centers, however, students at a typical CCC and contract center had similar large gains in attainment of the GED or vocational certificate (not shown). In addition, we found similar postprogram earnings gains and similar reductions in the percentage arrested over the follow-up period (Tables 8 and A.14). Thus, program impacts are not associated with the type of center operator.

[17] Estimates in Table 8 pertain to Job Corps impacts for the average student in a center with a particular attribute, whereas the estimates in Table A.13 pertain to impacts for a typical center with that attribute (see Burghardt and Schochet 2001).

b. Center Size

The capacity of Job Corps centers ranges from 200 to more than 2,000 students. The characteristics of students are similar at medium centers (226 to 495 slots) and large centers (496 or more slots). At small centers (225 or less), however, more students are under 18 years old, high school dropouts, white, and from a small town.

Capacity may affect students' experiences and, thus, impacts in several ways. Large centers might have the ability to provide students with more opportunities for vocational training and recreation and to reap the benefits of economies of scale and lower costs per student. However, a large-scale operation may make it more difficult to create the connections between staff members and students that are thought to be important for successful learning.

Johnson et al. (2000) found that students who attend small centers are more likely to complete their vocational trade and to stay on center for longer than students in other centers. However, we found similar impacts for large, medium, and small centers. Impacts on key education outcomes were similarly positive and statistically significant for all three center size groups. Furthermore, differences in earnings impacts across the groups are not statistically significant, and show no clear pattern over time (Tables 8 and A.14). Some evidence indicates, however, that arrest rate impacts were smaller in large centers than in centers with fewer students.

c. Center Performance

The Job Corps performance measurement system is intended to focus staff throughout Job Corps on ensuring that students achieve important milestones in Job Corps and positive outcomes after the program. Our process study concluded that this goal of the performance

measurement system is met: Job Corps is a performance-driven system (Johnson et al. 1999). Center staff, and especially managers, are aware of standards and care about their center's ranking. Center managers use the system for day-to-day management, and many receive financial incentives linked to center performance.

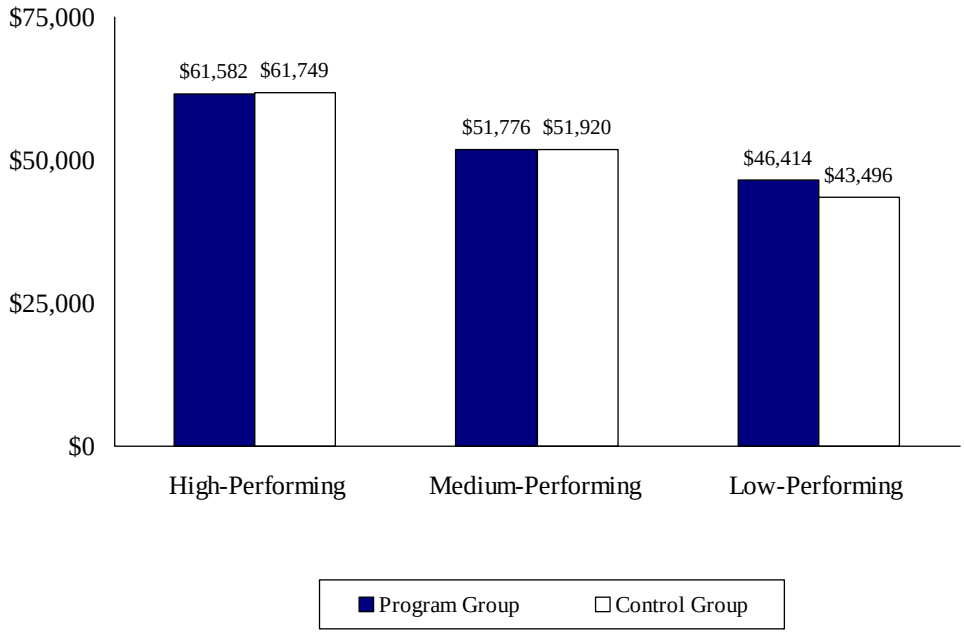
There are some differences, by performance level, in the characteristics of centers and the students they serve. Relative to low- and medium-performing centers, high-performing centers tend to be smaller, to have a higher fraction of nonresidential students, and to be operated by a government agency (instead of a private contractor). Furthermore, high-performing centers tend to serve less disadvantaged students; they serve a larger percentage of female students, a higher percentage with a high school credential at program entry, and a higher percentage who are white or Hispanic.

Despite Job Corps' emphasis on performance, however, the impacts of Job Corps are similar by performance level (Tables 8 and A.14). As one would expect, participants in higher-performing centers had better outcomes (Figure 3). However, the *same* pattern holds for comparable control group members. Thus, students served by higher-performing centers would tend to have better outcomes than those served by lower-performing centers even in the *absence* of Job Corps. These results are consistent with the finding that high-performing centers tend to serve less disadvantaged students.

Thus, the performance measurement system does not seem to be achieving the goal of distinguishing between more and less effective centers. Center performance appears to be related more to the types of students that a center serves than measured center quality. This finding is troubling because the lowest-ranking centers may be penalized financially or otherwise for not

FIGURE 3

TOTAL 1998 TO 2004 SER EARNINGS, BY CENTER PERFORMANCE LEVEL



Source: Annual social security earnings SER records.

showing satisfactory performance, even though they provide the same value added for their students as do high-performing centers.

F. ASSESSING SURVEY NONRESPONSE BIAS

We have documented that the earnings impacts are larger according to the survey data than the tax data. These differences reflect both the effects of (1) survey nonresponse bias due to sample differences, because the tax data contain earnings information on both survey nonrespondents and respondents, and (2) reporting differences between the two data sources for those with both types of data. To help disentangle these effects, we present earnings impacts using the SER data for the sample of 48-month survey respondents only, so that estimates using the survey and tax data can be compared using the same sample of interview respondents. In

addition, we present separate SER-based earnings impacts for the sample of survey nonrespondents.

1. Impact Findings for Survey Respondents and Nonrespondents

The pattern of SER-based impacts using the survey respondents only is similar to those based on the full sample; the impacts are negative in 1995 and 1996, positive and statistically significant in 1997 and 1998, and zero afterwards (Table 9). The similarity of the estimates, however, is due to the relatively high survey response rate and masks important differences between the impacts for survey respondents and those for nonrespondents.

We find the unexpected, and disturbing, result that impacts are substantially larger for survey respondents than nonrespondents during the postprogram period, which suggests that the survey-based earnings impact estimates are somewhat biased *upward* (Table 9). This pattern was not noticeable in 1993 or 1994 (the period before random assignment) or in 1995 and 1996 (the in-program period). However, starting in 1997, the earnings impacts for respondents were *positive*, while the earnings impacts for nonrespondents were *negative* (and statistically significant at the 10 percent level). This occurred because respondents earned more than nonrespondents (and increasingly so over time) for the program group, whereas the earnings of respondents and nonrespondents were more similar for the control group. In 2004, for instance, mean earnings were $8,673 for program group respondents, compared to only $7,223 for program group nonrespondents, whereas mean earnings were about $8,300 for both control group respondents and nonrespondents.

We calculate that survey nonresponse accounts for about one-quarter of the survey-SER earnings impact difference in 1998. The 1998 gap is $750 ($972-$222) when the full sample is used to obtain the SER impact, compared to $570 ($972-$402) when only survey respondents are

TABLE 9

IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES
FOR 48-MONTH SURVEY RESPONDENTS AND NONRESPONDENTS

	48-Month Survey Respondents			48-Month Survey Nonrespondents			Full Sample
Outcome Measure[b]	Program Group	Control Group	Estimated Impact[a]	Program Group	Control Group	Estimated Impact[a]	Estimated Impact[a]
Average Calendar Year Earnings (in 1995 Dollars)							
1993	999	999	0	1,050	1,085	–34	–7
1994	1,571	1,525	46	1,664	1,614	50	47
1995	1,747	2,010	–264***	1,815	2,109	–294***	–270***
1996	3,161	3,282	–122*	2,862	3,265	–404**	–178***
1997*	4,681	4,405	276***	4,074	4,307	–232	175**
1998**	6,018	5,616	401***	5,085	5,584	–499	222**
1999	6,927	6,732	195	5,790	6,362	–573	43
2000**	7,854	7,637	217	6,588	7,510	–922**	–11
2001**	8,164	7,877	287*	6,594	7,465	–871**	56
2002**	8,126	7,851	275	6,596	7,302	–706*	79
2003***	8,221	7,914	307*	6,686	7,669	–983**	49
2004**	8,674	8,470	204	7,223	8,129	-906*	-18
Percentage Employed in Calendar Year							
1993*	42.7	42.2	0.5	44.2	46.7	–2.5*	–0.1
1994	59.9	58.8	1.2	57.9	58.8	–0.9	0.7
1995***[c]	90.1	73.0	17.1***	85.6	74.5	11.1***	15.9***
1996[c]	89.6	78.9	10.7***	85.6	76.4	9.2***	10.3***
1997	84.9	82.6	2.3***	78.4	77.1	1.3	2.1***
1998	85.8	84.5	1.3*	79.8	78.5	1.3	1.3**
1999	86.1	84.7	1.4**	78.1	76.2	1.9	1.5**
2000	85.4	85.3	0.1	76.4	73.8	2.6	0.6
2001	82.3	82.4	–0.1	73.8	72.4	1.4	0.3
2002	78.5	78.5	0.0	70.0	69.5	0.5	0.1
2003	76.1	75.1	1.0	68.1	68.1	0.0	0.7
2004	76.3	75.5	0.8	65.8	66.0	-0.2	0.6
Sample Size	**6,828**	**4,485**	**11,313**	**1,495**	**1,187**	**2,682**	**15,138[d]**

Source: Annual social security earnings SER records.

[a]Impacts are measured as the difference between the weighted means for program and control group members.

[b]Stars next to a calendar year signifies that differences between estimated impacts for survey respondents and nonrespondents are statistically significant.

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

[d]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 .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

used to obtain the SER impact (Tables 5 and 9). This $180 reduction is a 24 percent narrowing of the survey-SER gap. Thus, reporting differences between the two data sources, holding the sample constant, account for about 76 percent of the survey-SER gap.

2. Explanations for Survey Nonresponse Bias

What accounts for the unexpected interview nonresponse bias? There are two possible explanations:

- ***There are differences in the baseline characteristics of respondents in the program and control groups that are correlated with earnings.*** If interview respondents in the program group were drawn from a somewhat more advantaged subpopulation of the full program group than was true for interview respondents in the control group, then the survey-based impact estimates would be biased upward.
- ***There are true differences in the earnings impacts for survey respondents and survey nonrespondents.*** If earnings impacts are truly larger for survey respondents than survey nonrespondents, then the survey-based earnings impacts would be biased upward even if the observable and unobservable characteristics of respondents in the program and control groups are similar. In this case, the impacts using interview respondents only are unbiased but are not generalizable to the full study population.

While it is difficult to disentangle these two effects, the first explanation is probably more plausible, because it is difficult to envision a scenario in which Job Corps participation would reduce the long-term earnings of survey nonrespondents. Among program group members, respondents had only slightly higher Job Corps participation levels than nonrespondents (77 percent compared to 70 percent), and respondents stayed in the program for only about one month longer on average than nonrespondents (7 months compared to 6 months). Furthermore, although there are some differences in the baseline characteristics of nonrespondents and respondents (for instance, nonrespondents are more likely to be male), these differences do not seem large enough to generate "true" impacts that are negative for nonrespondents and positive for respondents.

It is disturbing, however, that standard statistical testing and correction procedures failed to detect differences between program and control group respondents (Schochet et al. 2003). The response rates to the 48-month interview were similar for the program and control groups, and the two groups have similar baseline characteristics. In addition, the survey-based earnings impacts do not change when standard approaches are used to adjust for survey nonresponse bias, such as using regression models to control for observable baseline differences between the program and control group respondents, propensity scoring methods to adjust sample weights for interview nonresponse, and multiple imputation procedures to account for missing earnings data.

Our results clearly reinforce the importance of allocating sufficient resources to ensure high response rates in studies that conduct interviews with a population similar to that of Job Corps. If survey response rates had been lower in the National Job Corps Study, survey nonresponse bias could have yielded survey-based impact estimates that were seriously misleading.

G. COMPARING BENEFITS AND COSTS

Job Corps is an expensive program, costing the government nearly $16,500 per participant. To assess whether the impacts of Job Corps are large enough to justify this large outlay, we conducted a benefit-cost analysis in which a dollar value was placed on each impact of the program. This section presents final estimates from the benefit-cost analysis; we update estimates presented in McConnell and Glazerman (2001)—which provides a detailed discussion of the benefit-cost methodology—and Schochet et al. (2003).

1. Overview of the Benefit-Cost Methodology

Benefit-cost analysis involves identifying all the benefits and costs of the program and placing a dollar value on as many of them as possible. By placing a monetary value on the diverse impacts of the program, we can readily compare benefits with costs.

The measured benefits and costs of Job Corps fall into four categories:

- The benefits of the increased output resulting from the additional productivity of Job Corps participants, principally from increased earnings but also from output produced during vocational training on center
- The benefits from the reduced use of other programs and services, including other education and training programs, and public assistance
- The benefits from the reduced crime committed by and against participants
- Program operating and capital costs

Benefits and costs were measured from three perspectives: (1) society, (2) participants, and (3) the rest of society. Society's perspective is the most relevant for policymakers, because it indicates whether the aggregate benefits from the program are greater than the resources the program uses, abstracting from who enjoys the benefits of the program and who bears its costs. Hence, the analysis presented in this section focuses primarily on this perspective. Members of society fall into two groups: participants and everyone else (the rest of society). The perspective of participants indicates whether participating in Job Corps is a good investment for the youths themselves. The perspective of the rest of society indicates the magnitude of the investment that taxpayers and other citizens made in Job Corps. Appendix B presents detailed estimates of benefits and costs from all three perspectives.

We present comparisons of program benefits and costs (net benefits) for two groups of students. First, we present estimates for the full sample to examine the overall cost effectiveness of Job Corps. Second, we present net benefit estimates for those 20 to 24 years old at program application, because earnings gains persisted for these youths. We present estimates under our benchmark assumptions, and under alternative assumptions to assess the robustness of findings.

a. Measuring Program Benefits

In this section, we summarize our approach for measuring program benefits due to increased output, the reduced use of other programs and services, and reduced criminal activity. Program benefits were valued in 1995 dollars and discounted at a 4 percent real discount rate (approximately the real rate of return on 30-year Treasury bonds).

Increased Output of Job Corps Participants. The largest expected benefit from Job Corps is the increased productivity of its participants. The benefits from this increased productivity were obtained from the estimated impacts on earnings plus the estimated cost of fringe benefits (about 20 percent of earnings), net of the cost of any additional child care.

We used the *survey* earnings data to measure program benefits, because the survey data include informal earnings and other sources of income not reported on the SER data. However, we adjusted the survey impacts in two ways to reflect the likelihood that they overstate the true impacts (Schochet et al. 2003). First, we assumed that survey respondents overreported the hours they worked by 10 percent, to reflect the difference between the average hours reported by survey respondents and the national average. Second, we adjusted the survey-based earnings for interview nonresponse by multiplying these earnings by the ratio of average SER-based earnings for the full sample to average SER-based earnings for survey respondents only, separately for the program and control groups. These ratios were typically less than one.

Associated with the increase in participants' earnings is an increase in their tax payments. Although we did not observe directly how much our sample members paid in taxes, we estimated tax payments based on reported income and household composition. Tax payments, however, are not a program benefit from society's perspective, because they are a direct transfer from participants to the rest of society.

Job Corps is an intensive program and is expected to have a long-term impact on the lives of its participants—certainly longer than the four years observed with the survey data. To estimate the impact on lifetime earnings, we assumed that the observed survey earnings impacts in the fourth year of the observation period would have continued to follow the same pattern as the estimated SER impacts between 1998 and 2004 and that these patterns would continue for the rest of the participant's working life. Hence, for the full sample, we assumed that the survey-based earnings impacts in year 4 would decline by 80 percent per year, and for the 20- to 24-year-olds, we assumed that the impact in year 4 would remain constant.

Finally, we valued the output produced by participants as part of their vocational training projects in Job Corps. While the purpose of these projects is to provide hands-on training, the students often produce an output or a service whose value we included in the benefit-cost analysis. Estimates of the value of these projects were based on the market value of the output of 44 randomly selected projects at 22 randomly selected centers (McConnell 1999).

Reduced Use of Other Programs and Services. Participation in Job Corps reduces the use of a wide variety of other programs and services, including those involving education and training programs (such as high school, GED programs, vocational or trade schools, and two-year colleges) and public assistance (such as welfare, food stamps, Medicaid, and drug treatment programs). Because these programs and services are expensive, reducing their use benefits society. Further, because the government usually pays for them, their reduced use benefits the rest of society.[18]

Benefits from the reduced use of other programs and services were estimated by valuing their impacts at market (shadow) prices. These shadow prices were estimated using a wide

[18] Whether these savings are netted out of program costs or included as program benefits has no effect on the net benefit-cost estimates.

variety of published sources (McConnell and Glazerman 2001). To value reductions in the receipt of public assistance benefits from society's perspective, we included only the savings in administrative costs associated with these programs. This is because the value of public assistance benefits is a benefit to the participants and an equal cost to the taxpayers, and thus, on net, cancels from society's perspective.

Reduced Criminal Activity. Job Corps significantly reduces criminal activity; program group members were less likely than control group members to have been arrested, convicted, and incarcerated. The reduction in criminal activity is a benefit to society in the form of reduced victimization and criminal justice system costs, whose shadow prices were estimated from a variety of published sources (McConnell and Glazerman 2001).[19]

Job Corps participants were also less likely to have a crime committed against them. The benefit-cost analysis accounts for the benefits of this additional safety of Job Corps participants.

b. Measuring Program Costs

Program costs were measured as a weighted average of costs in program years 1994 to 1996, the period when program group participants were enrolled in Job Corps. As with program benefits, program costs were measured in 1995 dollars.

We measured three broad categories of costs: (1) reported program operating costs; (2) costs not reported on Job Corps' financial reports (such as the costs of administering the national and regional offices and donated goods and services); and (3) the economic costs of the capital—land, buildings, furniture, and equipment—used by Job Corps.

While in Job Corps, the average participant in our sample received about $1,400 in student pay (allowances and bonuses) and about $1,000 in food and clothing. As these items have

[19] Impacts on crime and the use of other programs and services declined during the 48-month observation period covered by the survey; therefore, we did not include the possible future benefits of these impacts after the 48-month follow-up period.

intrinsic value to the students irrespective of their contribution to the participants' future, we treat them as transfers from taxpayers to Job Corps students. Hence, in calculating the cost of Job Corps to society, these expenditures are subtracted from total program costs.

2. Results for the Full Sample

The benefit-cost analysis found that the benefits of Job Corps are less than the resources spent on it for all Job Corps participants. Our best estimate is that the costs to society of Job Corps exceed its benefits to society by about $10,300 per participant (Table 10). The benefits from increased lifetime earnings ($1,119), reduced use of other programs and services ($2,186), and reduced crime ($1,240) are small compared to its costs.

The rate of decay of the earnings impacts is key to this finding (Table 11). If we assumed, unrealistically, that the adjusted earnings impacts would not decline after the survey observation period, benefits exceed costs by over $4,000 per participant. Apart from the assumption of decay, the finding that costs exceed benefits for the full sample is not sensitive to other changes in assumptions. Even if we assumed there was no nonresponse bias or overreporting of hours, costs would exceed benefits. Costs would still exceed benefits if we had underestimated the benefits from reduced crime or reduced use of other programs and services by a factor of two. And costs exceed benefits as long as the impacts on earnings decay by more than only 2.5 percent per year.

Even with earnings impacts that decay over time, the benefits of Job Corps still exceed the costs from the perspective of its *participants*. Using the adjusted survey data and assuming a decay rate of 80 percent, the benefits of Job Corps exceed its costs to participants by $1,784 per participant (Table B.1). Even though our estimates of the impact on post-program earnings decreased, participants still benefit from Job Corps because the value of pay, food, and clothing they receive in the program offset the earnings forgone while they are enrolled in Job Corps. The cost of Job Corps to the rest of society is $12,084 per participant (Table B.1).

TABLE 10

BENEFITS AND COSTS OF JOB CORPS FROM SOCIETY'S PERSPECTIVE
(1995 DOLLARS)

Benefits or Costs	Present Discounted Value: All Job Corps Participants	Present Discounted Value: Those 20 to 24 Years of Age at Program Application
Total Benefits	**$3,544**	**$32,045**
Benefits from Increased Output	119	34,896
Years 1 to 4[a]	–281	338
After year 4[a]	179	34,308
Output produced during vocational training in Job Corps	220	250
Benefits from Reduced Use of Other Programs and Services	2,186	937
Benefits from Reduced Crime	1,240	–3,787
Total Costs	**–13,844**	**–15,193**
Program Costs	–16,205	–17,755
Program Costs Considered to Be Transfers	2,361	2,562
Net Benefits	**–$10,300**	**$16,853**

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security SER earnings records; and (3) McConnell and Glazerman (2001).

[a]The calculations are based on the survey-based earnings impacts, and assume a decline in the year 4 earnings impact equal to the rate of decline observed in the SER data after year 4. For the full sample, the calculations are based on an assumed decline in impacts of 80 percent per year. For those 20 to 24 years of age at program application, we assume no decline in the impact after year 4. Earnings reported in the surveys are adjusted also for survey nonresponse and the overreporting of hours worked by 10 percent. The length of time youth are in Job Corps is also adjusted for nonresponse. The value of increased output includes additional earnings and fringe benefits net of increased child care costs. Taxes are not included because they are a transfer from society's perspective.

TABLE 11

BENEFITS AND COSTS UNDER DIFFERENT ASSUMPTIONS ABOUT THE SIZE OF THE EARNINGS IMPACTS AND THEIR DECAY, FOR ALL JOB CORPS PARTICIPANTS, BY DATA SOURCE
(1995 Dollars)

	Adjusted Survey Data[a]		Annual Social Security Earnings Records		Unadjusted Survey Data	
	No Decay in Earnings Impacts	**Benchmark:** Decay Rate Observed in SER Data[b]	No Decay in Earnings Impacts	Decay Rate Observed in SER Data[b]	No Decay in Earnings Impacts	Decay Rate Observed in SER Data[b]
Total Benefits (Dollars)	$18,229	$3,544	$9,305	$3,528	$30,018	$4,490
Benefits from Increased Output	14,804	119	5,880	102	26,592	1,064
Years 1-4[c]	–60	–60	32	32	753	753
After Year 4	14,864	179	5,848	70	25,839	311
Other Benefits	3,426	3,426	3,426	3,426	3,426	3,426
Total Costs[d]	–13,844	–13,844	–14,128	–14,128	–14,128	–14,128
Net Benefits	**$4,385**	**–$10,300**	**–$4,823**	**–$10,600**	**$15,890**	**–$9,638**

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records; and (3) McConnell and Glazerman (2001).

[a]Earnings reported on the surveys are adjusted for survey nonresponse and overreporting of hours by 10 percent. The length of time youth are in Job Corps is also adjusted for nonresponse; this affects estimates of program costs and the output produced during vocational training in Job Corps.

[b]Assumes that impacts on earnings and child-care expenses decay at 80 percent per year.

[c]Figures include benefits from output produced during vocational training in Job Corps.

[d]Total costs are net of program costs that are considered to be transfers.

3. Results for the Older Youth

Although Job Corps is not cost effective for the full sample, the program appears to be cost effective from society's perspective for the subgroup of youth who were 20 to 24 years old at the time of program application (Table 10). Benefits exceed costs by about $17,000 for these youth. Total benefits are about twice the size of total costs. Net benefits are positive even though there

was an increase in serious crimes committed by this group, which is reflected in the negative "benefits from reduced crime." The calculations were conducted assuming that the survey earnings impacts would have remained approximately constant for the rest of the youth's working life.

H. SUMMARY AND CONCLUSIONS

Job Corps makes a difference; it produces beneficial impacts for disadvantaged youth, most of whom enroll in the program without a high school credential. Job Corps provides broad groups of participants with the instructional equivalent of one additional year in school, improves functional literacy, and has large effects on the receipt of credentials that 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 gains observed in the third and fourth years after random assignment are commensurate with what would be expected from an additional year of school (Card 1999). Job Corps also reduces criminal activity by about 16 percent per participant and the receipt of public assistance.

These positive findings, however, need to be balanced against the lack of long-term earnings impacts for the overall sample based on the administrative records data and the finding that social benefits do not appear to offset program costs, except for the oldest students. While it is possible that positive earnings impacts for the full sample will re-emerge as the sample matures into their late 20s and 30s, it seems unlikely that this will occur for several reasons. First, it has been more than six years since earnings gains were last observed. Second, zero earnings impacts were observed under both a strong and weak economy; thus, changes in economic conditions are not likely to affect future impacts.

Nonetheless, 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 youths. For instance, a study of the large youth training programs funded under Title II-A of the Job Training Partnership Act (JTPA) found no impacts on the earnings of low-income out-of-school youths who participated (Orr et al. 1996). However, the JTPA programs were nonresidential and provided less intensive services than Job Corps. Similarly, the Jobstart demonstration (which provided education, training, and job placement services in a nonresidential setting to disadvantaged dropouts ages 17 to 21) found statistically insignificant earnings impacts (Cave et al. 1993). In the end, Job Corps remains the only large-scale program to have produced statistically significant earnings gains for disadvantaged youths.

Several interesting impact findings related to program components emerge from the study. First, impacts for residents and nonresidents are very similar, which suggests that both components are equally effective for the (very different) types of students they serve. Second, impacts on key outcomes are similar for CCC centers (that are operated by government agencies) and contract centers (that are operated by private companies). Third, impacts appear to be similar in large and smaller centers. Finally, impacts are not associated with center performance level as measured by the Job Corps performance measurement system. High-performing centers provide the same value added for their students as lower-performing centers. Thus, the performance measurement system does not seem to be achieving the goal of distinguishing between more and less effective centers.

As is the case for any long-term study, there is a lag between the time study subjects receive the intervention and when study results are published. Thus, we emphasize that the findings presented in this report pertain to the Job Corps program as it operated in 1995 and 1996 (when

our program group members were enrolled in Job Corps) and not necessarily to the program as it operates today. There have been a number of changes that Job Corps has recently implemented in response to WIA provisions and other factors. For example, more Job Corps centers are now accredited to award high school diplomas, and Job Corps is more focused on providing longer-term support and placement services for their former students. These changes may have improved program effectiveness.

In conclusion, the positive study findings, including the important finding that the program is cost effective for the older students, suggest that there is promise in the Job Corps model. The challenge is to improve program services to sustain the earnings gains for the younger students and to 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.

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APPENDIX A

ADDITIONAL TABLES PRESENTING EARNINGS IMPACTS FOR KEY YOUTH SUBGROUPS

TABLE A.1

SUBGROUP SAMPLE SIZES, BY RESEARCH STATUS AND DATA SOURCE

Subgroup	Percentage of the Study Population	Survey Data		SER Data	
		Program Group	Control Group	Program Group	Control Group
Subgroups Defined by Youth Characteristics					
Age at Application					
16 to 17	41	2,742	1,907	3,709	2,439
18 to 19	32	2,175	1,402	2,948	1,857
20 to 24	27	1,911	1,176	2,607	1,578
Gender					
Male	59	3,741	2,787	5,314	3,854
Female	41	3,087	1,698	3,950	2,020
Race					
White, non-Hispanic	27	1,793	1,193	2,474	1,558
Black, non-Hispanic	47	3,366	2,179	4,462	2,814
Hispanic	18	1,175	787	1,623	1,051
Other[a]	8	494	326	699	444
Arrest History					
Never arrested	77	5,020	3,225	6,437	3,960
Ever arrested for nonserious crimes only[b]	19	1,158	795	1,529	984
Ever arrested for serious crimes[a]	5	294	203	392	262
Educational Level					
Had a high school diploma or GED	23	1,626	1,028	2,036	1,267
Had neither	77	5,161	3,436	6,777	4,272
Residents and Nonresidents					
Residential Designation Status					
Residents	86	5,484	3,753	7,499	4,982
Nonresidents	14	1,344	732	1,765	892
Subgroups Defined by Center Characteristics					
Type of Center Operator					
Contract centers (73 centers)	85	5447	3,540	7,411	4,595
CCC centers (30 centers)	15	914	617	1,240	816

TABLE A.1 *(continued)*

Subgroup	Percentage of the Study Population	Survey Data		SER Data	
		Program Group	Control Group	Program Group	Control Group
Center Size					
Small centers					
(< 226 slots; 38 centers)	20	1,272	837	1,716	1,087
Medium centers					
(226 to 495 slots; 49 centers)	45	2,909	1,891	3,952	2,462
Large centers					
(> 495 slots; 16 centers)	35	2,180	1,429	2,983	1,862
Performance Level[c]					
High (18 centers)	15	918	623	1,269	804
Medium (68 centers)	66	4,240	2,727	5,751	3,549
Low (17 centers)	20	1,203	807	1,631	1,058
Sample Size	**80,883**	**6,828**	**4,485**	**9,264**	**5,874**

Source: Annual social security earnings SER records.

Note: Subgroup sample sizes do not always sum to the full sample size because of missing values.

[a]This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

[b]Serious crimes include murder, assault, robbery, and burglary. Nonserious crimes include larceny, vehicle theft, other property crimes, drug law violations, other personal crimes, and other miscellaneous crimes.

[c]High-performing centers are defined as those that were in the top third of the performance ranking during program years 1994 and 1996. Similarly, low-performing centers are those that were in the bottom third of the performance ranking in each year; and the remaining centers are designated medium-performing centers.

TABLE A.2

IMPACTS ON EARNINGS AND EMPLOYMENT RATES BASED ON SURVEY DATA, BY QUARTER AFTER RANDOM ASSIGNMENT

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant[a]	Estimated Impact per Participant[b]
Average Earnings per Week, by Quarter (in 1995 Dollars)				
1				
2	44.5	65.5	–22.0***	–30.6***
3	57.9	87.4	–29.5***	–41.0***
4	77.6	99.2	–21.6***	–30.1***
5	92.4	106.0	–13.6***	–19.0***
6	108.8	117.7	–8.9***	–12.3***
7	126.8	129.3	–2.5	–3.4
8	142.3	138.2	4.1	5.8
9	153.3	146.9	6.4*	8.9*
10	164.8	155.8	9.0**	12.5**
11	171.6	160.0	11.6***	16.2***
12	186.1	170.2	15.9***	22.1***
13	196.2	178.6	17.6***	24.5***
14	205.3	188.0	17.3***	24.1***
15	209.8	194.2	15.7***	21.8***
16	213.7	197.2	16.5***	22.9***
	217.5	199.4	18.1***	25.2***
Total Earnings				
Year 3	9,286.2	8589.9	696.3***	967.6***
Year 4	10,990.4	10,162.8	827.5***	1,150.0***
Percentage Employed, by Quarter				
1	33.2	42.1	–8.9*	–12.4*
2	32.8	47.5	–14.7*	–20.4*
3	41.8	53.0	–11.1*	–15.4*
4	49.8	57.7	–7.9*	–10.9*
5	52.6	56.7	–4.1*	–5.7*
6	52.1	54.3	–2.2*	–3.0*
7	55.2	55.8	–0.6	–0.8
8	59.0	57.9	1.2	1.6
9	62.7	61.4	1.2	1.7
10	65.6	63.7	1.9*	2.7*
11	67.1	64.3	2.9*	4.0*
12	66.2	63.0	3.2*	4.4*
13	66.8	63.4	3.4*	4.8*
14	67.5	65.1	2.4*	3.3*
15	69.2	65.6	3.6*	5.0*
16	71.1	68.7	2.4*	3.3*
Sample Size	**6,828**	**4,485**	**11,313**	

TABLE A.2 *(continued)*

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 program 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 program 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 .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.3

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR 16- AND 17-YEAR-OLDS

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	119	127	–8	NA
1994[d]	417	402	15	NA
1995***	805	886	–81**	–102**
1996***	2,021	1,853	168**	212**
1997**	3,214	2,905	309***	391***
1998	4,253	4,098	155	196
1999	5,146	5,142	4	5
2000	5,937	5,999	–62	–78
2001	6,158	6,132	26	33
2002**	6,148	6,170	–22	–28
2003*	6,205	6,256	–51	–64
2004	6,637	6,791	–154	–194
1998 to 2004*	40,484	40,588	–105	–133
Survey Data				
1997	6,900	6,286	614***	791***
1998***	8,946	7,931	1,015***	1,308***
Percentage Employed in Calendar Year (SER Data)				
1993[d]	17.1	17.4	–0.4	NA
1994[d]	38.5	38.0	0.5	NA
1995***[e]	85.4	59.3	26.0***	32.8***
1996***[e]	86.5	70.7	15.9***	20.1***
1997	81.0	78.0	3.0***	3.7***
1998	82.6	81.8	0.8	1.0
1999	82.8	80.9	1.9*	2.4*
2000	81.8	80.7	1.1	1.3
2001	77.6	77.6	0.0	0.0
2002	72.9	73.0	–0.1	–0.1
2003	70.4	69.6	0.8	1.0
2004	70.3	69.7	0.6	0.7
Sample Size	**3,709**	**2,439**	**6,148**	

TABLE A.3 *(continued)*

Source: 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.

[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.4

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR 18- AND 19-YEAR-OLDS

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	748	793	–45	NA
1994[d]	1,599	1,561	37	NA
1995***	1,897	2,345	–448***	–631***
1996***	3,241	3,711	–470***	–662***
1997**	4,734	4,887	–153	–216
1998	6,187	6,124	63	89
1999	6,914	7,171	–257	–362
2000	7,849	8,158	–310	–436
2001	8,224	8,440	–217	–305
2002**	8,228	8,579	–351	–495
2003*	8,307	8,623	–316	–445
2004	8,890	9,218	–329	–463
1998 to 2004*	54,598	56,313	–1,715	–2,418
Survey Data				
1997	8,242	8,417	–175	–253
1998***	10,242	10,037	205	297
Percentage Employed in Calendar Year (SER Data)				
1993[d]	51.7	51.9	–0.2	NA
1994[d]	70.1	69.5	0.6	NA
1995***[e]	91.2	81.8	9.4***	13.2***
1996***[e]	89.3	83.4	5.9***	8.3***
1997	84.8	82.9	1.9*	2.6*
1998	84.5	83.1	1.4	1.9
1999	84.8	84.3	0.5	0.7
2000	83.7	84.6	–0.9	–1.2
2001	81.8	82.3	–0.5	–0.7
2002	78.4	79.3	–0.9	–1.2
2003	76.2	76.7	–0.5	–0.7
2004	76.5	77.1	–0.6	–0.8
Sample Size	**2,948**	**1,857**	**4,805**	

TABLE A.4 *(continued)*

Source: 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.

[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.5

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR 20- TO 24-YEAR-OLDS

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	2,658	2,653	5	NA
1994[d]	3,349	3,277	72	NA
1995***	3,043	3,415	–371***	–552***
1996***	4,567	4,956	–389**	–579**
1997**	6,387	6,062	325*	483*
1998	7,798	7,320	478**	711**
1999	8,801	8,379	422	628
2000	9,822	9,438	384	571
2001	9,969	9,581	388	578
2002**	9,867	9,154	713**	1,060**
2003*	10,035	9,435	600*	892*
2004	10,430	9,904	526	782
1998 to 2004*	66,721	63,210	3,511*	5,221*
Survey Data				
1997	9,800	9,478	322	482
1998***	12,393	10,616	1,778***	2,663***
Percentage Employed in Calendar Year (SER Data)				
1993[d]	71.8	71.9	–0.1	NA
1994[d]	78.9	78.2	0.7	NA
1995***[e]	92.8	84.6	8.2***	12.1***
1996***[e]	91.4	84.4	7.0***	10.4***
1997	86.1	85.2	0.9	1.3
1998	87.6	85.6	2.0*	2.9*
1999	86.9	84.8	2.1**	3.1**
2000	86.3	84.5	1.7	2.5
2001	83.8	82.3	1.5	2.2
2002	80.8	79.2	1.6	2.3
2003	78.5	76.5	2.0	2.9
2004	77.6	75.6	2.0	2.9
Sample Size	**2,607**	**1,578**	**4,185**	

Source: 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.

TABLE A.5 *(continued)*

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[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.6

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR MALES

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	1,067	1,052	15	NA
1994[d]	1,687	1,600	87	NA
1995	1,826	2,136	–310***	–407***
1996	3,278	3,507	–229***	–301***
1997	4,945	4,736	209*	275*
1998**	6,353	5,959	394***	518***
1999	7,185	7,037	148	195
2000	8,138	8,149	–11	–14
2001	8,339	8,239	99	131
2002	8,234	8,047	187	246
2003	8,361	8,255	106	139
2004	8,839	8,833	7	9
1998 to 2004	55,450	54,520	930	1,222
Survey Data				
1997	9,252	8,955	297	398
1998	11,721	10,581	1,140***	1,530***
Percentage Employed in Calendar Year (SER Data)				
1993[d]	43.5	44.1	–0.6	NA
1994[d]	60.1	59.1	1.0	NA
1995[e]	90.4	74.3	16.2***	21.2***
1996[e]	89.6	79.5	10.1***	13.2***
1997	84.7	82.7	2.0***	2.6***
1998	84.9	83.4	1.5**	1.9**
1999	84.1	83.1	0.9	1.1
2000	83.0	82.3	0.8	1.0
2001	79.4	80.0	–0.6	–0.7
2002	75.7	75.7	–0.1	–0.1
2003**	73.1	73.6	–0.5	–0.6
2004	73.3	72.7	0.6	0.7
Sample Size	**5,314**	**3,854**	**9,168**	

Source: 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.

TABLE A.6 *(continued)*

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[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.7

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS
AND EMPLOYMENT RATES FOR FEMALES

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993	926	963	–37	NA
1994	1,449	1,459	–10	NA
1995	1,665	1,873	–208***	–300***
1996	2,844	2,941	–97	–140
1997	4,000	3,864	136	196
1998**	5,075	5,091	–16	–24
1999	6,000	6,097	–97	–140
2000	6,821	6,814	8	11
2001	7,143	7,136	7	10
2002	7,220	7,287	–68	–97
2003	7,266	7,287	–21	–30
2004	7,723	7,763	–40	–57
1998 to 2004	47,247	47,474	–227	–327
Survey Data				
1997	6,472	6,152	320	468
1998	8,251	7,476	775***	1,134***
Percentage Employed in Calendar Year (SER Data)				
1993	42.2	41.6	0.7	NA
1994	58.6	58.4	0.2	NA
1995[d]	87.5	71.9	15.6***	22.4***
1996[d]	87.5	76.8	10.7***	15.4***
1997	81.9	79.7	2.3**	3.3**
1998	84.2	83.0	1.1	1.5
1999	85.2	82.9	2.3**	3.3**
2000	84.4	84.0	0.4	0.5
2001	82.4	80.9	1.5	2.1
2002	78.5	78.1	0.4	0.5
2003**	76.4	73.9	2.5**	3.6**
2004	75.5	75.0	0.5	0.7
Sample Size	**3,950**	**2,020**	**5,970**	

Source: 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.

TABLE A.7 *(continued)*

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[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.8

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR WHITE, NON-HISPANIC YOUTH

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	1,279	1,231	48	NA
1994[d]	1,938	1,876	62	NA
1995	2,086	2,512	–426***	–585***
1996	3,784	3,877	–93	–128
1997	5,509	5,345	164	226
1998	6,957	6,508	448**	616**
1999	7,717	7,700	18	24
2000	8,792	8,658	134	184
2001	8,992	8,738	254	349
2002	8,845	8,526	319	439
2003	8,764	8,437	327	449
2004	9,196	9,006	190	260
1998 to 2004	59,261	57,573	1,689	2,321
Survey Data				
1997	10,414	9,841	574*	823*
1998*	12,921	11,207	1,714***	2,459***
Percentage Employed in Calendar Year (SER Data)				
1993[d]	50.8	49.9	0.9	NA
1994[d]	69.6	68.1	1.5	NA
1995***[e]	93.3	82.4	10.9***	14.9***
1996[e]	92.8	85.0	7.8***	10.7***
1997*	89.9	88.8	1.0	1.3
1998	89.7	88.4	1.3	1.7
1999	89.0	88.7	0.3	0.4
2000	88.7	86.9	1.8*	2.4*
2001	85.7	84.0	1.7	2.3
2002	81.3	80.4	0.9	1.2
2003	77.8	76.2	1.6	2.1
2004	77.4	76.4	1.0	1.3
Sample Size	**2,474**	**1,558**	**4,032**	

Source: 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.

TABLE A.8 *(continued)*

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[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.9

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR BLACK, NON-HISPANIC YOUTH

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	851	864	–13	NA
1994[d]	1,357	1,319	38	NA
1995	1,542	1,748	–206***	–279***
1996	2,605	2,704	–99	–134
1997	3,788	3,566	222**	301**
1998	4,871	4,615	256*	348*
1999	5,669	5,573	96	131
2000	6,365	6,341	24	33
2001	6,534	6,320	214	291
2002	6,511	6,425	86	117
2003	6,620	6,505	114	155
2004	6,991	7,102	–112	–151
1998 to 2004	43,561	42,881	680	922
Survey Data				
1997	6,907	6,551	356*	489*
1998*	8,770	7,912	858***	1,178***
Percentage Employed in Calendar Year (SER Data)				
1993[d]	40.3	40.0	0.3	NA
1994[d]	56.1	56.1	0.0	NA
1995***[e]	87.8	69.9	17.9***	24.2***
1996[e]	86.4	75.3	11.1***	15.0***
1997*	80.7	78.2	2.5***	3.3***
1998	82.0	81.0	1.0	1.3
1999	82.7	80.5	2.1**	2.8**
2000	80.7	81.1	–0.4	–0.5
2001	77.2	78.4	–1.2	–1.6
2002	73.4	73.9	–0.5	–0.6
2003	71.1	71.0	0.1	0.1
2004	71.2	71.1	0.2	0.2
Sample Size	**4,462**	**2,814**	**7,276**	

Source: 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.

TABLE A.9 *(continued)*

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[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.10

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR HISPANIC YOUTH

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	1,105	1,112	–7	NA
1994[d]	1,740	1,650	90	NA
1995	1,782	2,053	–270**	–376**
1996	3,449	3,758	–309*	–430*
1997	5,167	5,007	160	222
1998	6,567	6,735	–168	–234
1999	7,740	7,977	–237	–330
2000	8,772	9,274	–502	–698
2001	9,348	9,980	–632	–879
2002	9,611	9,856	–245	–341
2003	9,756	10,326	–570	–793
2004	10,643	10,771	–129	–179
1998 to 2004	62,437	64,920	–2,484	–3,453
Survey Data				
1997	7,911	8,062	–151	–209
1998*	10,282	10,146	136	187
Percentage Employed in Calendar Year (SER Data)				
1993[d]	40.9	41.2	–0.3	NA
1994[d]	55.3	55.8	–0.4	NA
1995***[e]	87.9	71.5	16.4***	22.8***
1996[e]	88.8	77.3	11.5***	15.9***
1997*	83.9	79.6	4.4***	6.1***
1998	84.1	82.2	1.8	2.5
1999	83.0	82.7	0.2	0.2
2000	83.8	82.9	0.9	1.2
2001	82.2	80.7	1.5	2.0
2002	79.4	78.9	0.5	0.6
2003	78.7	76.8	1.9	2.6
2004	77.7	76.1	1.6	2.2
Sample Size	**1,623**	**1,051**	**2,674**	

Source: 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.

TABLE A.10 *(continued)*

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[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.11

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR RESIDENTIAL DESIGNEES

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	955	982	–27	NA
1994[d]	1,543	1,523	20	NA
1995**	1,711	2,018	–308***	–410***
1996*	3,038	3,259	–221***	–295***
1997	4,515	4,342	173*	231*
1998	5,759	5,525	234**	312**
1999	6,602	6,538	64	85
2000	7,445	7,477	–32	–43
2001	7,677	7,594	83	110
2002	7,642	7,544	98	130
2003	7,773	7,717	57	76
2004	8,211	8,238	–27	–36
1998 to 2004	51,108	50,632	476	634
Survey Data				
1997	8,171	7,903	267	365
1998	10,346	9,336	1,010***	1,378***
Percentage Employed in Calendar Year (SER Data)				
1993[d]	41.9	42.2	–0.2	NA
1994[d]	59.5	58.8	0.7	NA
1995[e]	89.7	73.8	15.8***	21.0***
1996[e]	88.9	78.6	10.3***	13.7***
1997	83.6	81.6	2.1***	2.7***
1998	84.5	83.0	1.5**	1.9**
1999	84.2	82.6	1.5**	1.9**
2000	83.3	82.5	0.8	1.0
2001	80.1	79.9	0.2	0.2
2002	76.2	75.8	0.4	0.5
2003	74.0	73.2	0.7	0.9
2004	73.6	73.3	0.3	0.3
Sample Size	**7,499**	**4,982**	**12,481**	

Source: 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.

TABLE A.11 *(continued)*

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[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.12

IMPACTS ON 1993 TO 2004 CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR NONRESIDENTIAL DESIGNEES

Outcome Measure[a]	Program Group	Control Group	Estimated Impact per Eligible Applicant[b]	Estimated Impact per Participant[c]
Average Calendar Year Earnings (in 1995 Dollars)				
SER Data				
1993[d]	1,354	1,230	124	NA
1994[d]	1,885	1,665	220*	NA
1995**	2,074	2,103	–28	–45
1996*	3,497	3,402	95	152
1997	4,840	4,653	187	300
1998	6,287	6,134	153	245
1999	7,328	7,402	–74	–118
2000	8,582	8,444	138	221
2001	8,943	9,038	–95	–152
2002	8,936	8,957	–21	–33
2003	8,797	8,787	10	16
2004	9,472	9,417	56	89
1998 to 2004	58,346	58,179	167	267
Survey Data				
1997	7,739	7,307	432	680
1998	9,984	9,254	730*	1,149*
Percentage Employed in Calendar Year (SER Data)				
1993[d]	49.3	48.5	0.9	NA
1994[d]	59.5	59.1	0.4	NA
1995[e]	86.4	70.0	16.4***	26.2***
1996[e]	87.9	77.3	10.6***	16.9***
1997	83.3	81.0	2.3	3.6
1998	84.9	84.8	0.1	0.1
1999	87.0	85.5	1.5	2.4
2000	85.7	85.8	–0.1	–0.1
2001	84.0	83.5	0.5	0.8
2002	80.3	81.9	–1.6	–2.5
2003	77.6	76.9	0.7	1.1
2004	78.1	75.9	2.3	3.6
Sample Size	**1,765**	**892**	**2,657**	

Source: 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.

TABLE A.12 *(continued)*

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[a]Stars next to a variable name represent statistical significance levels for joint tests of differences between estimated impacts per eligible applicant across levels of a subgroup.

[b]Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c]Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program 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.

[d]1993 and 1994 pertain to the period *before* random assignment.

[e]Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

NA = Not applicable

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.13

IMPACTS PER PARTICIPANT ON EARNINGS FOR 20- TO 24-YEAR-OLDS, BY GENDER AND RACE AND ETHNICITY

Subgroup	Impacts on Calendar Year Earnings (in 1995 Dollars)						
	1998	1999	2000	2001	2002	2003	2004
Full Sample of Those 20 to 24 at Program Application	711**	628	571	578	1,060**	892*	782
Gender[a]				**	**		
Male	890**	1,096**	858	1,365**	1,910***	1,542**	1,293*
Female	495	8	213	–489	–108	9	95
Race and Ethnicity[a]				**	**	**	**
White, non-Hispanic	1,429**	630	854	1,908**	3,124***	3,217***	3,047***
Black, non-Hispanic	888	1,056*	1,067	1,026	703	504	103
Hispanic	–542	–532	–1,255	–1,674	–118	–1,019	–618

Source: Annual social security earnings SER records for the full sample.

Note: Estimated impacts per Job Corps participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of program 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.

[a]Stars in the header rows signify that differences in impacts across subgroup levels are statistically significant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.14

IMPACTS PER PARTICIPANT ON EARNINGS AND ARREST RATES, FOR CENTER SUBGROUPS WHEN CENTERS ARE WEIGHTED EQUALLY IN THE CALCULATIONS

	Impacts on Calendar Year Earnings (in 1995 Dollars)					Percentage Ever Arrested During the 48-Month Period (Survey)
	Survey	SER Data				
Subgroup	1998	1998	1999	2002	2004	
Full Sample	1,581*	636***	355*	354	359	–7.1*
Type of Center Operator[a]						
Contract centers	1,456*	552***	371*	305	86	–6.2*
CCC centers	1,444	600	192	428	538	–8.9*
Center Size[a]				*		
Small (< 226 slots)	749	298	251	326	240	–10.2*
Medium (226 to 495 slots)	1,863*	856***	475	642*	468	–6.5*
Large (> 495 slots)	1,830*	341	10	–531	–592	–1.2
Performance Level[a,b]						
High	1,276	765	184	415	477	–9.2*
Medium	1,670	652***	261	252	-121	–5.7*
Low	766	106	622	576	1,150*	–10.0*

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.

Note: Estimated impacts per Job Corps participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of program 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. Centers are weighted equally in all analyses.

[a]Stars in the header rows signify that differences in impacts across subgroup levels are statistically significant.

[b]High-performing centers are defined as those that were in the top third of the performance ranking during program years 1994 and 1996. Similarly, low-performing centers are those that were in the bottom third of the performance ranking in each year; and the remaining centers are designated medium-performing centers.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

APPENDIX B

ADDITIONAL TABLES PRESENTING FINDINGS FROM THE BENEFIT-COST ANALYSIS

TABLE B.1

REVISED BENEFITS AND COSTS OF JOB CORPS, FULL SAMPLE, BASED ON ADJUSTED SURVEY DATA AND AN ASSUMPTION OF A DECAY IN EARNINGS IMPACTS[a]
(1995 Dollars)

Benefits or Costs	Perspective		
	Society	Participants	Rest of Society
Benefits from Increased Output (Dollars)	**119**	**–392**	**511**
Year 1			
Increased Earnings and Fringe Benefits	–1,715	–1,715	0
Increased Child Care Costs	–50	–47	–4
Increased Taxes	0	268	–268
Years 2 to 4			
Increased Earnings and Fringe Benefits	1,581	1,581	0
Increased Child Care Costs	–96	–77	–19
Increased Taxes	0	–514	514
After the Observation Period			
Increased Earnings and Fringe Benefits	196	196	0
Increased Child Care Costs	–17	–14	–4
Increased Taxes	0	–70	70
Output Produced During Vocational Training in Job Corps	220	0	220
Benefits from Reduced Use of Other Programs and Services	**2,186**	**–780**	**2,966**
Reduced Use of High School	1,189	0	1,189
Reduced Use of Other Education and Training Programs	874	0	874
Reduced Use of Public Assistance and Substance Abuse Treatment Programs	122	–780	902
Benefits from Reduced Crime	**1,240**	**643**	**597**
Reduced Crime by Participants	1,240	0	1,240
Reduced Crime Against Participants	0	643	–643
Program Costs	**–13,844**	**2,314**	**–16,158**
Reported Program Operating Costs (Net of Transfers)	–12,285	0	–12,285
Unreported Program Operating Costs (Net of Transfers)	–543	0	–543
Capital Costs	–1,016	0	–1,016
Student Pay, Food, and Clothing (Transfers)	0	2,314	–2,314
Net Benefits (Dollars)[b]	**–10,300**	**1,784**	**–12,084**

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records; and (3) McConnell and Glazerman (2001).

[a]Assumes that impacts on earnings and child-care expenses decay at 80 percent per year after the observation period.

[b]Because of rounding, net benefits may not equal the sum of the rows. Similarly, benefits to society may not precisely equal the sum of the benefits to participants and the benefits to the rest of society.

TABLE B.2

REVISED BENEFITS AND COSTS OF JOB CORPS, YOUTH AGES 20 TO 24 AT PROGRAM APPLICATION, BASED ON ADJUSTED SURVEY DATA AND AN ASSUMPTION OF NO DECAY IN EARNINGS IMPACTS[a]
(1995 Dollars)

	Perspective		
Benefits or Costs	Society	Participants	Rest of Society
Benefits from Increased Output (Dollars)	**34,896**	**31,077**	**3,819**
Year 1			
Increased Earnings and Fringe Benefits	–2,381	–2,381	0
Increased Child Care Costs	–83	–73	–9
Increased Taxes	0	513	–513
Years 2 to 4			
Increased Earnings and Fringe Benefits	3,006	3,006	0
Increased Child Care Costs	–204	–185	–19
Increased Taxes	0	–394	394
After the Observation Period			
Increased Earnings and Fringe Benefits	36,021	36,021	0
Increased Child Care Costs	–1,713	–1,424	–289
Increased Taxes	0	–4,006	4,006
Output Produced During Vocational Training in Job Corps	250	0	250
Benefits from Reduced Use of Other Programs and Services	**937**	**–1,358**	**2,295**
Reduced Use of High School	21	0	21
Reduced Use of Other Education and Training Programs	629	0	629
Reduced Use of Public Assistance and Substance Abuse Treatment Programs	287	–1,358	1,645
Benefits from Reduced Crime	**–3,787**	**643**	**–4,430**
Reduced Crime by Participants	–3,787	0	–3,787
Reduced Crime Against Participants	0	643	–643
Program Costs	**–15,193**	**2,562**	**–17,754**
Reported Program Operating Costs (Net of Transfers)	–13,487	0	–13,487
Unreported Program Operating Costs (Net of Transfers)	–554	0	–554
Capital Costs	–1,152	0	–1,152
Student Pay, Food, and Clothing (Transfers)	0	2,562	–2,562
Net Benefits (Dollars)[b]	**16,853**	**32,924**	**–16,071**

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records; and (3) McConnell and Glazerman (2001).

[a]Assumes that impacts on earnings do not decay after the observation period.

[b]Because of rounding, net benefits may not equal the sum of the rows. Similarly, benefits to society may not precisely equal the sum of the benefits to participants and the benefits to the rest of society.