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**National Job Corps
Study: The Impacts of
Job Corps on
Participants'
Employment and Related
Outcomes**

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ABSTRACT OF FINDINGS

The Job Corps program has long been a central part of federal efforts to provide training for disadvantaged youths. Because of the high costs of the program's intensive services, which are provided mainly in a residential setting, policymakers need to know just how effective Job Corps actually is. This report presents the findings of the National Job Corps Study on impacts of the program on participants' employment and related outcomes.

The cornerstone of the National Job Corps Study was the random assignment of all youths found eligible for Job Corps to either a program group or a control group. Program group members could enroll in Job Corps; control group members could not, but they could enroll in all other programs available to them in their communities. We estimated impacts by using data from periodic follow-up interviews to compare the experiences of the program and control groups. Findings on program impacts over the first four years after random assignment are summarized below.

Job Corps provided extensive education, training, and other services to the program group. Follow-up interviews show that 73 percent of the program group enrolled in Job Corps, with an average period of participation of eight months. Students received large amounts of academic classroom instruction and vocational skills training. They also participated extensively in the primary Job Corps activities outside the classroom.

Job Corps substantially increased the education and training services that eligible applicants received, and it improved their educational attainment. On average, Job Corps increased participants' time spent in education and training (both in and out of Job Corps) by about 1,000 hours, approximately the number in a regular 10-month school year. It also focused more on vocational instruction than did the training available elsewhere. Job Corps substantially increased the receipt of GED and vocational certificates, but it had no effect on college attendance.

Job Corps generated positive employment and earnings impacts by the beginning of the third year after random assignment, and the impacts persisted through the end of the 48-month follow-up period. During the last year of the 48-month follow-up period, the gain in average earnings per participant was about \$1,150, or 12 percent. Over the entire period, Job Corps participants earned about \$624 more than they would have if they had not enrolled in Job Corps.

Employment and earnings gains were found broadly across most subgroups of students. Employment-related impact estimates were similar for males and females. Earnings gains were found for groups of students at special risk of poor outcomes (such as very young students, females with children, and older students without a high school credential at enrollment), as well as for groups at lower risk (such as older students with a high school credential).

The residential and nonresidential programs were each effective for the youths they served. Postprogram earnings and employment impacts for those assigned to each component were positive overall, and for nearly all groups defined by gender and the presence of children. The beneficial impacts for nonresidential females with children are noteworthy, because they suggest that the nonresidential program allows Job Corps to serve effectively a group that, because of family responsibilities, would otherwise be unable to participate.

Job Corps significantly reduced youths' involvement with the criminal justice system. The arrest rate was reduced by 16 percent (about 5 percentage points). Arrest rate reductions were largest during the first year after random assignment (when most program enrollees were in Job Corps), although Job Corps also led to small reductions during the later months of the follow-up period. Reductions occurred for nearly all categories of crimes, although they were slightly larger for less serious ones. The impacts on arrest rates were very similar across subgroups. Job Corps participation also reduced convictions and incarcerations resulting from a conviction by about 17 percent. Finally, Job Corps led to reductions in crimes committed against program participants.

Job Corps had small beneficial impacts on the receipt of public assistance and on self-assessed health status, but it had no impacts on illegal drug use. Overall, program group members reported receiving about \$460 less in benefits (across several public assistance programs) than control group members. Program group members were slightly less likely than control group members to report their health as “poor” or “fair”—15.5 percent, compared to 17.5 percent at each interview point. There were no differences in the reported use of alcohol and illegal drugs or in the use of drug treatment services.

Job Corps had no impacts on fertility or custodial responsibility, but it slightly promoted independent living and mobility. Participation in Job Corps had no impacts on having a child or on the likelihood of living with or providing support for a child. However, a slightly smaller percentage of program group than control group members were living with their parents, and a slightly larger percentage (31 percent, compared to 29 percent) were living with a partner either married or unmarried. The average distance between the zip codes of residence at program application and at 48 months was slightly larger for the program group. However, because most students returned to their home communities, Job Corps had no effect on the characteristics of the places in which the youths lived.

In conclusion, we find that Job Corps produces beneficial impacts on the main outcomes that it intends to influence. Beneficial impacts on education-related, employment-related, and crime-related outcomes were found overall, as well as for broad subgroups of students in the program. The residential and nonresidential program components were each effective for the students they served. A companion report, which presents findings from the benefit-cost analysis, concludes that Job Corps is a worthwhile investment both for the students and for the broader society that supports their efforts.

EXECUTIVE SUMMARY

Since 1964, the Job Corps program has been a central part of federal efforts to provide employment assistance to disadvantaged youths between the ages of 16 and 24. It is an intensive, comprehensive program whose major service components include academic education, vocational training, residential living, health care and health education, counseling, and job placement assistance. These services are currently delivered at 119 Job Corps centers nationwide. Most Job Corps students reside at Job Corps centers while training, although about 12 percent are nonresidential students who live at home. Each year, Job Corps serves more than 60,000 new enrollees and costs more than \$1 billion.

The National Job Corps Study, funded by the U.S. Department of Labor (DOL), was designed to provide a thorough and rigorous assessment of the impacts of Job Corps on key participant outcomes. The cornerstone of the study was the random assignment of all youth found eligible for Job Corps to either a program group or a control group. Program group members were allowed to enroll in Job Corps; control group members were not (although they could enroll in other training or education programs).

This report presents estimates of the impacts of Job Corps on participants' employment and related outcomes during the 48 months after random assignment. The outcome measures for the analysis were obtained from interview data.

The report answers the following three research questions:

1. ***How effective is Job Corps overall at improving the employability of disadvantaged participants?*** Job Corps participation led to (1) increases of about 1,000 hours (or about one school year) in time spent in education and training; (2) substantial increases in the attainment of GED and vocational certificates; (3) earnings gains by the beginning of the third year after random assignment that persisted through the end of the follow-up period (resulting in a 12 percent gain in year 4); (4) reductions of about 16 percent in arrests, convictions, and incarcerations for convictions; (5) reductions in crimes committed against participants; (6) small beneficial impacts on the receipt of public assistance and self-assessed health status; (7) small increases in the likelihood of living with a partner and living independently; (8) no impacts on self-reported alcohol and illegal drug use, fertility, or custodial responsibility, but some increases in the use of child care.
2. ***Do Job Corps impacts differ for youths with different baseline characteristics?*** Job Corps is effective for broad groups of students. Program participation led to substantial improvements in education-related outcomes across diverse groups of students. Employment and earnings gains were similar for males and females, and were found for groups of students at special risk of poor outcomes (such as very young students, females with children, and older students without a high school credential at

enrollment), *as well as* for groups at lower risk (such as older students with a high school credential). Reductions in criminal activity were found for nearly all groups.

3. ***How effective are the residential and nonresidential components of Job Corps?*** Each component is effective for the groups it serves. Postprogram earnings and employment impacts for those assigned to each component were positive overall, and for nearly all groups defined by gender and the presence of children. Participation in each component led to reductions in criminal activity for most groups of students, except that no reductions were found for nonresidential males.

A separate report presents findings from the benefit-cost analysis (McConnell et al. 2001), where program benefits (calculated by placing a dollar value on the estimated program impacts) are compared to program costs. That report concludes that the benefits of Job Corps exceed the substantial public resources that are invested in it.

STUDY DESIGN

The results for the impact analysis are based on a comparison of eligible program applicants who were randomly assigned to a program group (who were offered the chance to enroll in Job Corps) or to a control group (who were not). The key features of this experimental design are as follows:

The impact evaluation is based on a fully national sample of eligible Job Corps applicants. With a few exceptions, the members of the program and control groups were randomly selected from *all* youths who applied to Job Corps in the contiguous 48 states and the District of Columbia and who were found eligible for the program.

Sample intake occurred between November 1994 and February 1996. All youths who applied to Job Corps for the first time between November 1994 and December 1995 and were found eligible for the program by the end of February 1996 were included in the study--a total of 80,883 eligible applicants.

During the sample intake period, 5,977 Job Corps-eligible applicants were randomly selected to the control group. Approximately 1 eligible applicant in 14 (7 percent of 80,883 eligible applicants) was assigned to the control group. For both programmatic and research reasons, the sampling rate to the control group differed somewhat across some youth subgroups. Thus, sample weights were used in all analyses, so that the impact estimates could be generalized to the intended study population.

Control group members were not permitted to enroll in Job Corps for a period of three years, although they were able to enroll in other programs available to them. Thus, the outcomes of the control group represent the outcomes that the program group would have experienced if they had not been given the opportunity to enroll in Job Corps. Because control group members were allowed to enroll in other education and training programs, the 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. The impact estimates do not represent the effect of the program relative to no education or training; instead, they represent the incremental effect of Job Corps.

During the sample intake period, 9,409 eligible applicants were randomly selected to the research sample as members of the program group.¹ Because random assignment occurred after youths were determined eligible for Job Corps (and *not* after they enrolled in Job Corps centers), the program group includes youths who enrolled in Job Corps (about 73 percent of eligible applicants), as well as those who did not enroll, the so-called “no-shows” (about 27 percent of eligible applicants). Although the study’s research interest focuses on enrollees, all youths who were randomly assigned, including those who did not enroll at a center, were included in the analysis to preserve the benefits of the random assignment design. However, as discussed below, statistical procedures were also used to estimate impacts for Job Corps participants only.

Job Corps staff implemented random assignment procedures well. Using program data on all new center enrollees, we estimate that less than 0.6 percent of youths in the study population were not randomly assigned. In addition, only 1.4 percent of control group members enrolled in Job Corps before the end of the three-year period during which they were not supposed to enroll.² Hence, we believe that the research sample is representative of the youths in the intended study population and that the bias in the impact estimates due to contamination of the control group is very small.

DATA SOURCES, OUTCOME MEASURES, AND ANALYTIC METHODS

The impact analysis used a variety of data sources, outcome measures, and analytic methods to address the main study questions, as outlined next.

The analysis relied primarily on interview data covering the 48-month period after random assignment. Follow-up interview data collected 12, 30, and 48 months after random assignment were used to construct outcome measures for the impact analysis. In addition, baseline interview data, collected soon after random assignment, were used to create subgroups defined by youth characteristics at random assignment, and to construct outcome measures that pertain to the period between the random assignment and baseline interview dates.

¹The remaining 65,497 eligible applicants were randomly assigned to a program nonresearch group. These youths were allowed to enroll in Job Corps but are not in the research sample.

²An additional 3.2 percent of control group members enrolled in Job Corps after their three-year restriction period ended and before four years after random assignment.

Response rates to the baseline, 12-month, 30-month, and 48-month interviews were fairly high and were similar for program and control group members. The response rate was 95 percent to the baseline interview, 90 percent to the 12-month follow-up interview, 79 percent to the 30-month interview, and 80 percent to the 48-month interview. Response rates were similar across key subgroups.

The primary sample used for the analysis includes those who completed 48-month interviews. This sample contains 11,313 youths (6,828 program group members and 4,485 control group members). About 88 percent of this sample also completed 30-month interviews, and 95 percent completed 12-month interviews. Furthermore, baseline interview data are available for everyone in this sample, because all youths completed either the full baseline interview or an abbreviated baseline interview in conjunction with the 12-month interview. Thus, complete data are available for most of the analysis sample.

The study estimated impacts on the following outcome measures that we hypothesized could be influenced by participation in Job Corps: (1) education and training, (2) employment and earnings, and (3) nonlabor market outcomes. The nonlabor market outcomes include welfare, crime, alcohol and illegal drug use, health, family formation, child care, and mobility. In general, outcome measures were defined over several periods after random assignment. We constructed measures by quarter (to examine changes in impact estimates over time), for year 1 (a period when many program group members were enrolled in Job Corps), for year 2 (a period of still significant but less intensive Job Corps participation), for years 3 and 4 each (a postprogram period for most program group members), and for the entire 48-month period.

We present estimates of Job Corps impacts per eligible applicant and per Job Corps participant. The estimates of Job Corps impacts *per eligible applicant* were obtained by computing differences in the distribution of outcomes between all program and control group members. This approach yields unbiased estimates of the effect of Job Corps for those offered the opportunity to enroll in the program. These impacts are pure experimental estimates, because random assignment was performed at the point that applicants were determined to be eligible for the program.

The comparison of the outcomes of all program and control group members yields *combined* impact estimates for the 73 percent of program group members who enrolled in Job Corps centers and the 27 percent who did not. Policymakers, however, are more concerned with the effect of Job Corps on those who enrolled in a center and received Job Corps services. This analysis is complicated by the fact that we do not know which control group members would have shown up at a center had they been in the program group. However, this complication can be overcome if we assume that Job Corps has no impact on eligible applicants who do not enroll in centers. In this case, the impact *per participant* can be obtained by dividing the impact *per eligible applicant* by the proportion of program group members who enrolled in Job Corps (73 percent).³ We present estimated impacts both per eligible applicant and per participant.

³The estimates per participant were further refined to adjust for the small number of control group members who enrolled in Job Corps during their three-year restriction period, by dividing the impacts per eligible applicant by the difference between the participation rate among the program group and the control group crossover rate.

Impact estimates were obtained for key subgroups defined by youth characteristics at baseline. The purpose of this subgroup analysis was to identify groups of Job Corps students who benefit from program participation and those who do not, so that policymakers can improve program services and target them appropriately. We estimated impacts of Job Corps on the following seven sets of subgroups: (1) gender, (2) age at application to Job Corps, (3) educational attainment, (4) presence of children for females, (5) arrest experience, (6) race and ethnicity, and (7) whether the youth applied to the program before or after new zero tolerance (ZT) policies took effect.⁴ Subgroup impact estimates were obtained by comparing the distribution of outcomes of program and control group members in that subgroup. For example, impacts for females were computed by comparing the outcomes of females in the program and control groups.

We estimated separate impacts for those assigned to the residential and nonresidential program components. These impacts were estimated using data on the predictions of outreach and admission (OA) counselors as to whether sample members would be assigned to a residential or a nonresidential slot. As part of the application process, OA counselors filled in this information on a special form developed for the study. The anticipated residential status information is available for both program *and* control group members, because it was collected prior to random assignment. Thus, the impacts of the residential component were estimated by comparing the distribution of outcomes of program group members designated for a residential slot with those of control group members designated for a residential slot. Similarly, the impacts of the nonresidential component were estimated by comparing the experiences of program and control group members designated for nonresidential slots. This analysis produced reliable estimates of program impacts for residential and nonresidential students, because the anticipated residential status information is available for all sample members, and because it matched actual residential status very closely for program group members who enrolled in Job Corps.

An important point about the interpretation of the impact findings for residents is that they tell us about the effectiveness of the residential component *for youths who are typically assigned to residential slots*. Similarly, the impact estimates for nonresidents tell us about the effectiveness of the nonresidential component *for youths who are typically assigned to nonresidential slots*. The characteristics of residential and nonresidential students differ (nonresidential students tend to be females with children and tend to be older). Consequently, our results cannot necessarily be used to measure the effectiveness of each component for the *average* Job Corps student. Nor can they be used to assess how a youth in one component would fare in the other one.

JOB CORPS EXPERIENCES

Job Corps staff have implemented a well-developed program model throughout the country (as described in a separate process analysis report by Johnson et al. [1999]). To understand the impacts that Job Corps had on the employment and related outcomes of participants, we must examine the

⁴In response to congressional concerns about the operation of the Job Corps program, and in particular, about safety on center, new ZT policies for violence and drugs were instituted in March 1995--during the sample intake period for the study. The new policies were instituted to ensure full and consistent implementation of existing policies for violence and drugs.

Job Corps experiences of the program group. Because we can expect meaningful Job Corps impacts on key outcomes only if program group members received substantial amounts of Job Corps services, we examined whether program group members received services, and then gauged the intensity and types of those services.

Our results, which indicate that program group members received extensive Job Corps services, can be summarized as follows:

Most program group members enrolled in Job Corps. Of those assigned to the program group, 73 percent reported enrolling in Job Corps within 48 months.

Participants typically enrolled very soon after random assignment. The average enrollee waited 1.4 months, or about six weeks, to be enrolled in a Job Corps center, although nearly three-quarters of those who enrolled did so in the first month, and only four percent enrolled more than six months after random assignment.

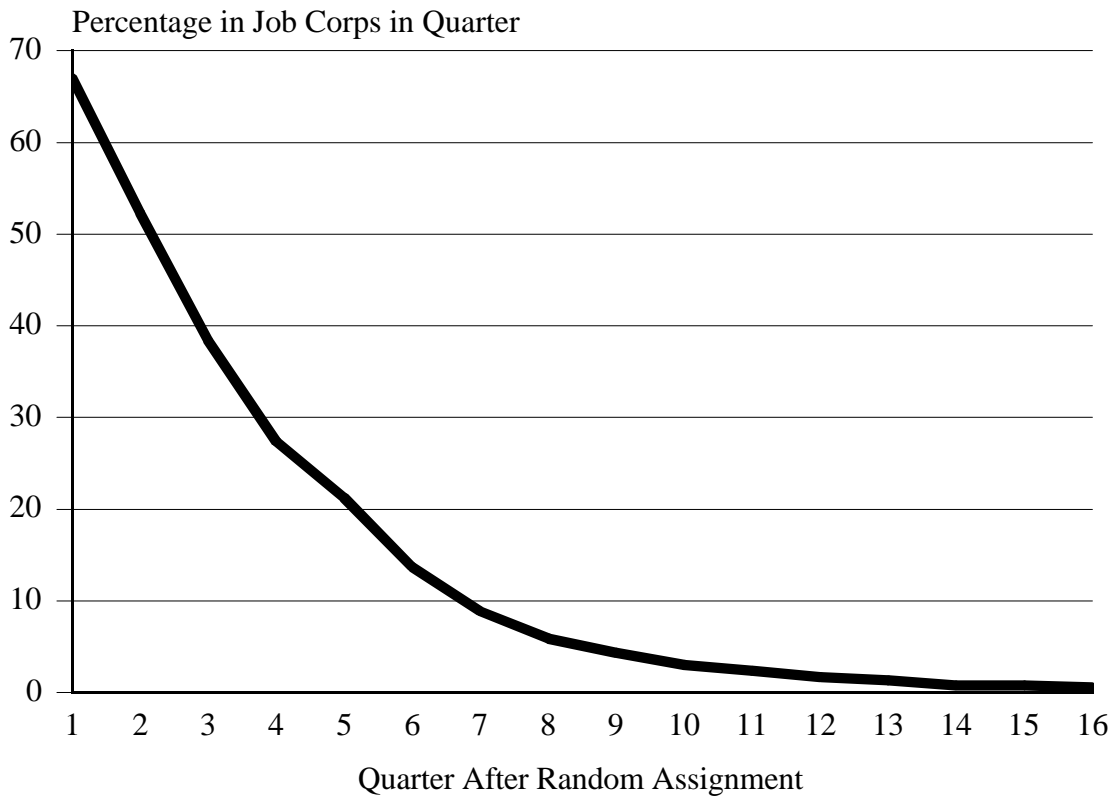
Most participants stayed in Job Corps for a substantial period of time, although the period of participation varied considerably. The average period of participation per enrollee was eight months. About 28 percent of all enrollees participated less than three months, and nearly a quarter participated for over a year. Because of this wide range in the duration of stay in Job Corps, participants left Job Corps at different points during the follow-up period.

The average postprogram period for participants was more than three years. Variations in the duration of participation in Job Corps resulted in variations in how much of the 48-month period was actually a postprogram period. However, most participants had been out of Job Corps for some time at the 48-month point: almost 67 percent of enrollees had been out for more than three years, and nearly 92 percent for more than two years. Less than 3 percent of enrollees had been out for less than one year.

Most participation occurred during the first 24 months after random assignment; the final two years of the 48-month period was a postprogram period for most participants (Figure 1). Figure 1 shows the fraction of program group members (including the no-shows) who participated in Job Corps during each quarter after random assignment. The participation rate declined from a peak of 67 percent in the first quarter after random assignment to 21 percent in the fifth quarter (beginning of the second year), and 3 percent in the tenth quarter. By the end of the 48-month period, almost all participants had left Job Corps. Only 0.3 percent of the program group (0.4 percent of enrollees) were in Job Corps in the final week of the 48-month follow-up period.

FIGURE 1

JOB CORPS PARTICIPATION RATES FOR THE FULL PROGRAM GROUP,
BY QUARTER



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

Based on these broad patterns of participation, we interpret the period from quarters 1 to 4 (year 1) as largely an “in-program” period. The period from quarters 5 to 8 (year 2) was a period of transition, in which smaller yet still substantial fractions of the program group were engaged in Job Corps training. The final eight quarters (years 3 and 4) were a postprogram period for most students. The use of these in-program, transition, and postprogram periods provides a framework to help explain the time profiles of employment and earnings and related impacts.

Program group enrollees participated extensively in the core Job Corps activities. As the program design intends, a large majority of Job Corps participants (77 percent) received both academic instruction and vocational training. More than 82 percent of enrollees reported receiving academic instruction, and nearly 89 percent received vocational training. The average enrollee reported receiving 1,140 hours of academic and vocational instruction (which is approximately equivalent to one year of classroom instruction in high school). Also, most enrollees participated in the many socialization activities in Job Corps, such as parenting education, health education, social skills training, and cultural awareness classes. Many enrollees, however, reported that they did not receive job placement assistance from the program.

While many subgroups had different experiences in Job Corps, the differences were small. The mix of academic and vocational training a student received depended on whether the youth had already received a high school credential (GED or diploma) before program entry. Students with no credential generally took both academic instruction and vocational training. High school graduates were more likely to focus on vocational training. Nonresidential students (especially females with children) had somewhat lower enrollment rates than residential students. Once in Job Corps, however, the residential and nonresidential students had similar amounts, types, and intensity of training, as well as similar exposure to the other program components. The many other subgroup differences were small, and overall each group’s experience was consistent with the conclusions drawn above for the program group as a whole.

EDUCATION AND TRAINING

Job Corps provides intensive academic classroom instruction and vocational skills training to increase the productivity and, hence, the future earnings, of program participants. The typical Job Corps student stays in the program for an extended period (about eight months on average), and Job Corps serves primarily students without a high school credential (about 80 percent of students do not have a GED or high school diploma at program entry). Thus, participation in Job Corps probably increases the amount of education and training participants receive and improves their educational levels relative to what they would have been otherwise.

Important elements of the impact analysis are to describe the education and training experiences of program and control group members and to provide estimates of the impact of Job Corps on key education and training outcomes during the 48 months after random assignment. We examine education and training experiences of the *program group*, both in Job Corps and elsewhere, to provide a complete picture of the services they received. The education and training experiences of the *control group* are the counterfactual for the study, showing what education and training the program group would have engaged in had Job Corps not been available. The net increase in

education and training due to Job Corps depends critically on what education and training the control group received and what education and training the program group received from other sources, as well as from Job Corps.

Our main findings can be summarized as follows:

Many control group members received substantial amounts of education and training. Nearly 72 percent participated in an education or training program during the 48 months after random assignment. On average, they received 853 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).⁵ These high participation rates are not surprising, because control group members demonstrated motivation to go to Job Corps, and thus had the motivation to find other programs.

It is noteworthy that although high school participation rates were high, those who returned to high school stayed there for an average of only about nine months. Because the typical sample member without a high school credential at random assignment had completed less than grade 10, very few control group members graduated from high school.

Job Corps substantially increased the education and training that program participants received, despite the activity of the control group (Tables 1 and 2). Nearly 93 percent of the program group engaged in some education or training (both in and out of Job Corps), compared to about 72 percent of the control group (an impact of 21 percentage points per eligible applicant). Job Corps participants spent about 4.8 hours per week--998 hours in total--more in programs than they would have if they had not enrolled in the program. This impact per participant corresponds to *roughly one school year*.

The program group also spent significantly more time in academic classes, and even more in vocational training (Table 2). Program group members spent an average of 3.1 hours per week in academic classes, as compared to 2.5 hours per week for the control group. The program group typically received about three times more vocational training than the control group (3.1 hours per week, compared to 0.9 hours per week).

The impacts on participation in education and training programs were concentrated in the first six quarters (that is, 18 months) after random assignment (Figure 2). Impacts were large during this period, because many program group members were enrolled in Job Corps then, but decreased as program group members started leaving Job Corps. About 76 percent of program group members were ever enrolled in an education or training program (including Job Corps and other programs) during the first quarter after random assignment, compared to 29 percent of control group members--an impact per eligible applicant of 47 percentage points. The impact on the participation rate decreased to 22 percentage points in quarter 3 and 10 percentage points in quarter

⁵The participation rates in GED programs and high school pertain to those who did not have a GED or high school diploma at random assignment.

TABLE 1

IMPACTS ON PARTICIPATION AND TIME SPENT IN EDUCATION
AND TRAINING PROGRAMS

	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 per Week Ever in Education or Training	7.6	4.1	3.5*	4.8*
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.

^aEstimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

^bEstimated 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 .05 level, two-tailed test.

TABLE 2

IMPACTS ON PARTICIPATION AND TIME SPENT IN ACADEMIC CLASSES AND VOCATIONAL TRAINING

	Program Group	Control Group	Estimated Impact per Eligible Applicant ^a	Estimated Impact per Participant ^b
Percentage Ever Took Academic Classes During the 48 Months After Random Assignment	80.8	57.2	23.7*	32.9*
Average Hours per Week Ever in Academic Classes	3.1	2.5	0.6*	0.8*
Percentage Ever Took Vocational Training	74.0	28.4	45.6*	63.4*
Average Hours per Week Ever Received Vocational Training	3.1	0.9	2.2*	3.1*
Sample Size^c	3,383	2,350	5,733	

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

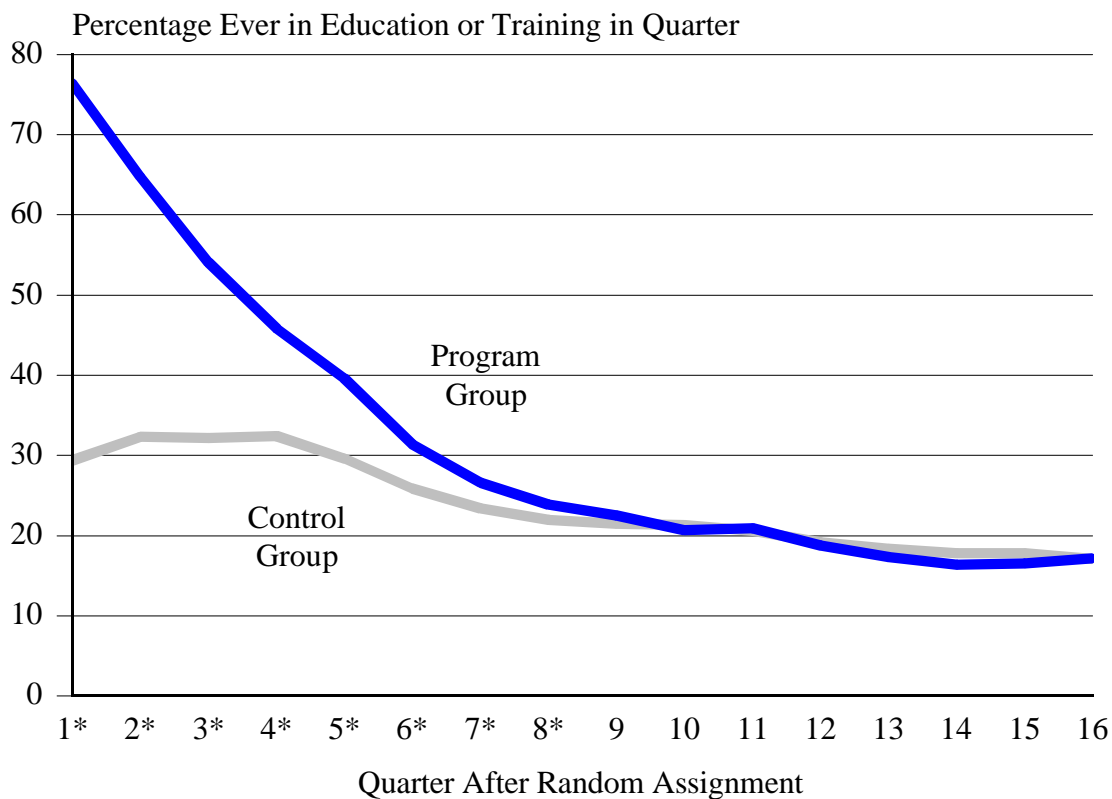
^aEstimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

^bEstimated 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.

^cThe sample consists of those in the 48-month sample (1) who completed a 30-month interview after April 1998, because of an error in the 30-month interview's skip logic before then; and (2) who did not complete a 30-month interview.

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

FIGURE 2
PARTICIPATION RATES IN EDUCATION AND TRAINING PROGRAMS,
BY QUARTER



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

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

5. The impact was about 3 percentage points in quarter 7 and near zero in each quarter in years 3 and 4.

Similar percentages of program and control group members were enrolled in education and training programs toward the end of the 48-month period. For example, about 13 percent of both research groups were enrolled in a program during the last week of the 48-month follow-up period. This finding is important, because it suggests that impacts on employment and earnings late in the 48-month period were not affected by differences in school enrollment rates by research status.

Control group members spent more time than program group members in programs other than Job Corps, although the differences were smaller than anticipated (Figure 3). About 71 percent of control group members enrolled in a program other than Job Corps during the 48-month period, compared to 63 percent of program group members. The differences in participation rates in programs that substitute for Job Corps (high school, GED programs, vocational schools, and ABE and ESL programs) are statistically significant. There were no differences in enrollment rates in two- or four-year colleges.⁶

While impacts on participation in alternative programs are statistically significant, they were smaller than expected. Program group members made considerable use of these same programs, which increased impacts on education and training and reduced the offset to Job Corps program costs.

Job Corps participation led to substantial increases in the receipt of GED and vocational certificates, but it led to slight reductions in the attainment of a high school diploma (Figure 4). Job Corps had large effects on the receipt of certificates that it emphasizes. Among those without a high school credential at random assignment, about 42 percent of program group members (and 46 percent of program group participants) obtained a GED during the 48-month period, compared to only 27 percent of control group members (an impact of 15 percentage points per eligible applicant). Similarly, more than 37 percent of program group members (and 45 percent of Job Corps participants) reported receiving a vocational certificate, compared to about 15 percent of control group members (an impact of 22 percentage points).

Among those without a credential at baseline, a slightly higher percentage of control group members than program group members obtained a high school diploma (7.5 percent, compared to 5.3 percent). As noted above, although many of the younger control group members attended high school, most of those in high school did not complete it, because they attended high school for an average of only about nine months.

⁶About 15 percent of Job Corps participants attended an education or training program during the follow-up period before they enrolled in Job Corps (that is, between their random assignment and Job Corps enrollment dates). Not surprisingly, most of this activity was high school. About one-half of Job Corps participants enrolled in an education or training program after leaving Job Corps. About 72 percent of the no-shows enrolled in a program during the 48-month period.

FIGURE 3

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS,
BY TYPE OF PROGRAM

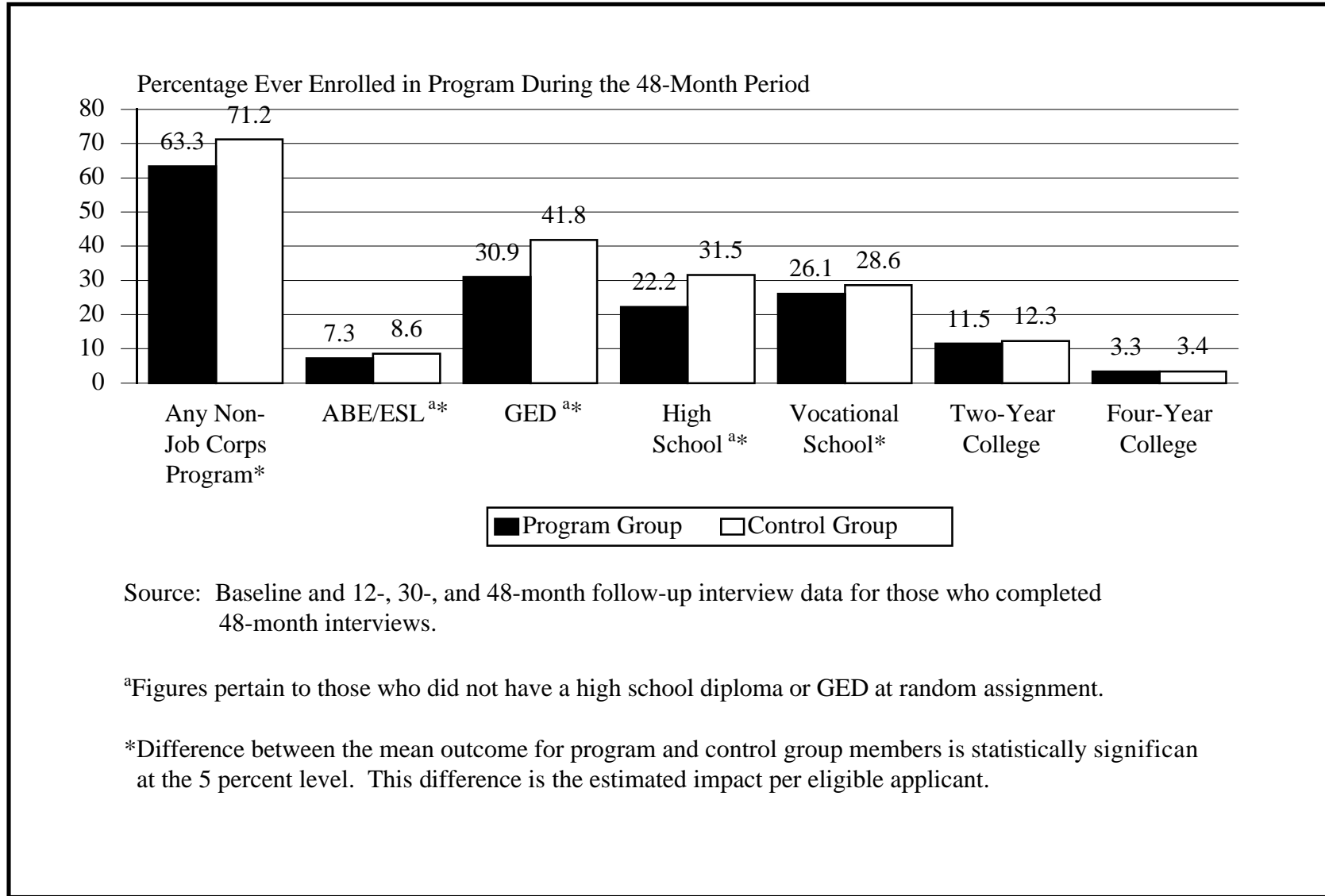
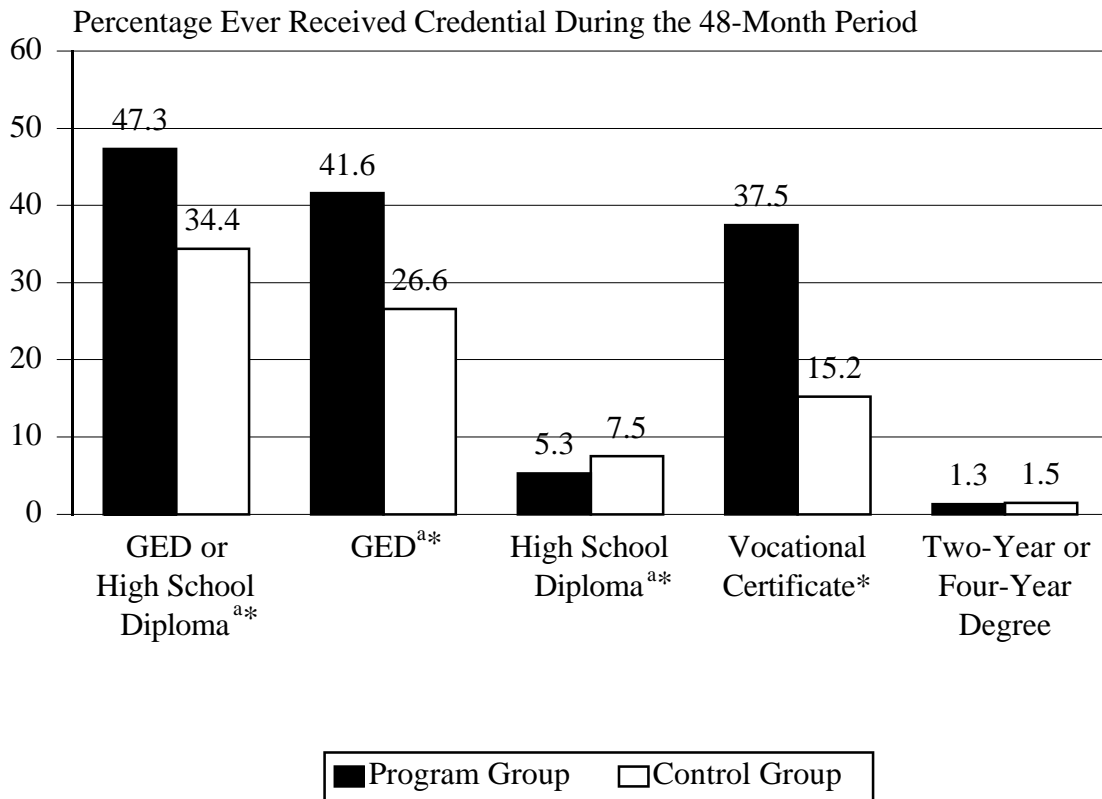


FIGURE 4

DEGREES, DIPLOMAS, AND CERTIFICATES RECEIVED



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

^aFigures pertain to those who did not have a high school credential at random assignment.

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

Job Corps had no effect on college attendance and completion (Figures 3 and 4). About 12 percent of each research group attended a two-year college, and about 3 percent attended a four-year college. Less than 2 percent obtained a two- or four-year college degree.

Impacts on education and training were large across all subgroups defined by youth characteristics. Impacts on total time spent in programs and on the attainment of a GED (among those without a high school credential at baseline) or a vocational certificate were very large and statistically significant for all key subgroups. However, the pattern of impacts across subgroups defined by age at application to Job Corps exhibited some differences. There were no impacts on hours in academic classes for those 16 and 17, because nearly half of all control group members who were 16 and 17 attended academic classes in high school. However, large impacts were found on hours spent in academic classes for the older youth, and on hours spent in vocational training for all age groups.

Of particular note, impacts were similar for those assigned to the residential and nonresidential components. This is consistent with findings from the process analysis (Johnson et al. 1999) that nonresidential students are fully integrated into the academic and vocational components of Job Corps.

EMPLOYMENT AND EARNINGS

We have seen that Job Corps participation leads to large impacts on time spent in academic classes and vocational training and on the attainment of GED and vocational certificates. These large impacts could increase participants' skill levels and, hence, their labor market productivity. This increased productivity may in turn enhance the time spent employed, earnings, wage rates, and fringe benefits of participants after they leave the program.

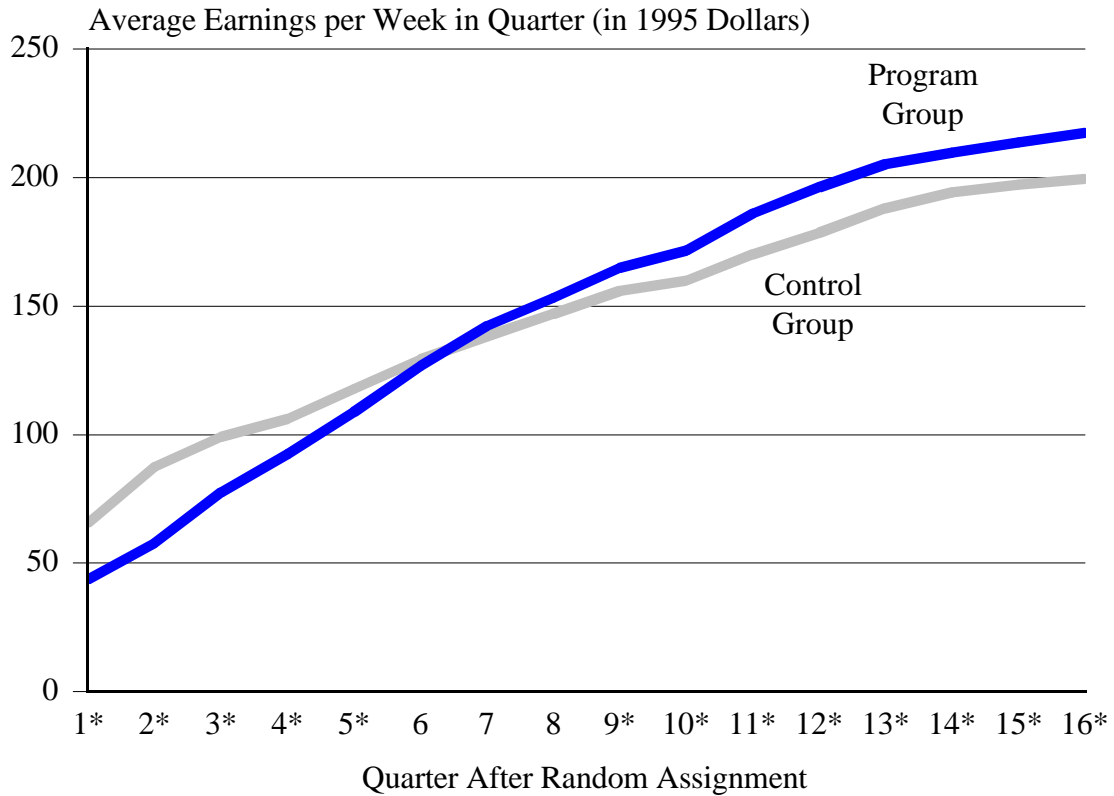
We expect negative impacts on participants' employment and earnings during the period of enrollment, because some would have held jobs if they had not gone to Job Corps. However, because of improvements in participants' skills, we expect positive impacts on employment and earnings after they leave the program and after a period of readjustment. In light of the variation in the duration of program participation and the period of readjustment, it is difficult to predict when positive impacts will emerge.

A summary of our findings is as follows:

Job Corps generated positive earnings impacts beginning in the third year after random assignment, and the impacts persisted through the end of the 48-month follow-up period (Figure 5 and Table 3). As expected, the earnings of the control group were larger than those of the program group early in the follow-up period, because many program group members were enrolled in Job Corps then. It took about two years from random assignment for the earnings of the program group to overtake those of the control group. The impacts grew between quarters 8 and 12 (that is, in year 3), and remained fairly constant from quarters 13 to 16 (that is, they *persisted* in year 4). In year 4, average weekly earnings for program group members were \$16 higher than for control group members (\$211, compared to \$195). The estimated year 4 impact per Job Corps *participant*

FIGURE 5

AVERAGE EARNINGS PER WEEK, BY QUARTER



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

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

TABLE 3

IMPACTS ON EARNINGS, EMPLOYMENT RATES, AND TIME EMPLOYED
IN QUARTERS 13 TO 16 (YEAR 4)

	Program Group	Control Group	Estimated Impact per Eligible Applicant ^a	Estimated Impact per Participant ^b
Average Earnings per Week, by Quarter After Random Assignment				
13	205.3	188.0	17.3*	24.1*
14	209.8	194.2	15.7*	21.8*
15	213.7	197.2	16.5*	22.9*
16	217.5	199.4	18.1*	25.2*
Percentage Employed, by Quarter				
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*
Average Percentage of Weeks Employed, by Quarter				
13	58.6	55.7	3.0*	4.1*
14	59.6	56.8	2.9*	4.0*
15	60.9	57.7	3.2*	4.4*
16	61.8	59.0	2.8*	3.9*
Average Hours Employed per Week, by Quarter				
13	26.8	25.4	1.5*	2.0*
14	27.3	25.9	1.4*	1.9*
15	27.7	26.3	1.5*	2.0*
16	27.9	26.4	1.5*	2.0*
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.

^aEstimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

^bEstimated 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 .05 level, two-tailed test.

was \$22 per week (or \$1,150 in total), which translates into a 12 percent earnings gain. These year 4 impacts are statistically significant at the 1 percent significance level.

Over the whole period, Job Corps participants earned about \$3 per week (or \$624 overall) more than they would have if they had not enrolled in Job Corps. This impact, however, is not statistically significant.

Job Corps also had statistically significant impacts on the employment rate and time spent employed beginning in year 3 (Figure 6 and Table 3). The impacts on the employment-related measures were negative during the in-program period. They became positive in quarter 8, increased sharply between quarters 8 and 12, and remained fairly constant afterwards. In year 4, the average quarterly impact on the employment rate was about 3 percentage points per eligible applicant (69 percent for the program group, compared to 66 percent for the control group). The year 4 impact on hours employed per week was 1.4 hours per eligible applicant (27.4 hours for the program group, compared to 26 hours for the control group).

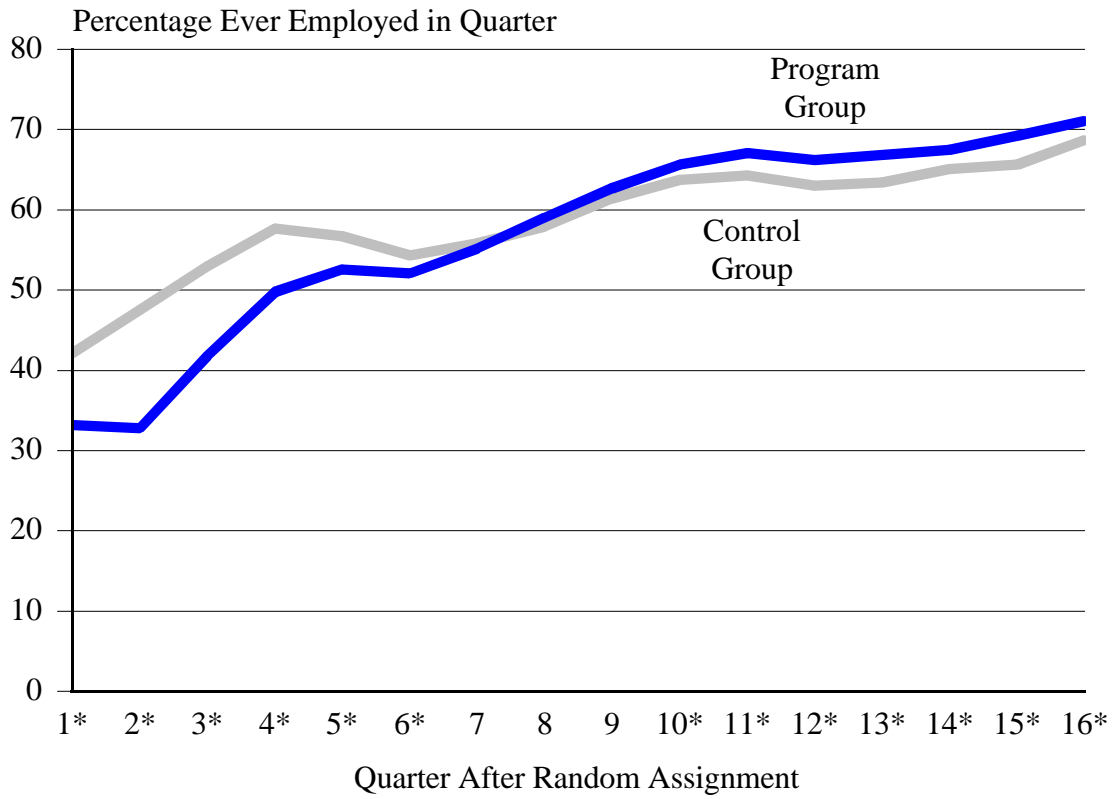
The earnings gains late in the period were due to a combination of greater hours of work and higher earnings per hour. Program group members earned about \$11 more per week in year 4 than control group members because they worked more hours, and they earned about \$5 more per week because they had higher earnings per hour. These gains sum to the \$16 impact on earnings per week in year 4.

Program group members secured higher-paying jobs with slightly more benefits in their most recent jobs in quarters 10 and 16. These findings are consistent with our findings from the literacy study (Glazerman et al. 2000) that Job Corps increases participants' skill levels and, hence, productivity. Employed program group members earned an average of \$0.24 more per hour than employed control group members in their most recent job in quarter 10 (\$6.77, compared to \$6.53), and an average of \$0.22 more per hour in their most recent job in quarter 16 (\$7.55, compared to \$7.33). Furthermore, the wage gains were similar across broad occupational categories, although similar percentages of program and control group members worked in each occupational area in both quarters.

Employed program group members were slightly more likely to hold jobs that offered fringe benefits in quarters 10 and 16. For example, in quarter 16, about 57 percent of the employed program group received health insurance, compared to 54 percent of the employed control group (a statistically significant increase of 3 percentage points, or nearly 6 percent). Similarly, about 48 percent of employed program group members were offered retirement or pension benefits, compared to 44 percent of employed control group members.

Earnings gains were found broadly across most key subgroups defined by youth characteristics at random assignment. Earnings gains during the postprogram period were very similar for males and females. Positive earnings impacts were found for groups of students at special risk of poor outcomes (such as very young students, females with children, youths who had been arrested for nonserious offenses, and older youths who did not possess a high school credential at baseline), *as well as* for groups at lower risk (such as older students with a high school credential at baseline). Impacts were similar for youth who applied to the program before or after the new ZT

FIGURE 6
EMPLOYMENT RATES, BY QUARTER



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

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

policies took effect, and for whites and African Americans.

Job Corps did not increase the employment and earnings of Hispanic youths and 18- and 19-year-olds. We are not able to provide a satisfactory explanation for these findings, although we have been able to rule out several possibilities. In particular, the lack of an impact is not due to differences in Job Corps enrollment rates or length of time in the program. Hispanics had similar enrollment rates as non-Hispanics, and Hispanic students participated for more than a month *longer*, on average than non-Hispanics. Job Corps participation measures did not differ by age.

The lack of impacts also does not appear to be related to other personal or family characteristics associated with low impacts. Overall, the characteristics of Hispanic students and African American participants are very similar (apart from primary language and region of residence), and the characteristics of those 18 and 19 are not unusual. We also found smaller impacts for Hispanic than non-Hispanic students and for those 18 and 19 compared with those in other age groups across nearly all subgroups defined by other key youth characteristics.

Language barriers do not explain the Hispanic findings, as we found similar impacts for Hispanic students whose primary language was English and for those whose primary language was Spanish. Finally, the findings are not due to characteristics of centers or regions in which Hispanic or 18- and 19-year-old students are concentrated. The patterns of impacts by race and ethnicity were similar for sample members designated for centers with a high concentration of Hispanic students and for those designated for centers with a lower concentration.⁷ Similarly, impacts were smaller for Hispanic than non-Hispanic students both in regions with a high concentration of Hispanics and in other regions. Centers attended by those 18 and 19 were similar to centers attended by older participants.

The residential program component was effective for broad groups of students it served. Earnings and employment impacts in years 3 and 4 for those assigned to the residential component were positive overall, and they were similar for residential males, females with children, and females without children.

The nonresidential component was also effective for the students it served. Participation in the nonresidential component improved postprogram earnings overall. It improved average earnings per week in year 4 by more than \$35 for females with children (an increase of 24 percent), and by more than \$55 for males (an increase of 26 percent). The nonresidential component had no effect, however, on females without children.

We emphasize again that the impact findings by residential status should be interpreted with caution. As discussed, our estimates provide information about the effectiveness of each component for the populations it serves. The estimates cannot be used to assess how a youth in one component

⁷These impacts were estimated using information provided by OA counselors on the center to which each eligible applicant in our study population was likely to be assigned. This information was collected prior to random assignment, and thus is available for both program and control group members.

would fare in the other one, or how effective each component would be for the average Job Corps student. This is because the characteristics of residents differ from those of nonresidents in ways that can affect outcomes.

WELFARE, CRIME, ILLEGAL DRUG USE, AND OTHER OUTCOMES

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. This section reports on impacts on welfare dependence; involvement with the criminal justice system; use of tobacco, alcohol, and illegal drugs; the overall health of participants; the likelihood of bearing or fathering children while unmarried; custodial responsibility; the likelihood of forming stable, long-term relationships; mobility; and the use of child care.

Our main results are as follows:

Job Corps participation reduced the receipt of public assistance benefits (Table 4). Overall, program group members reported receiving about \$460 less in benefits (across several public assistance programs) than control group members, and this impact is statistically significant at the 1 percent level. The estimated average reduction per participant was \$640. The estimated program impacts on the receipt of individual types of assistance were small and in many cases not statistically significant. The number of months receiving AFDC/TANF benefits differed by just 0.4 months (5.0 months for the program group and 5.4 months for the control group). Control group members received food stamps for slightly more months on average than program group members (7.0 months, compared to 6.5 months). Impacts on the receipt of GA, SSI, and WIC benefits and on the likelihood of being covered by public health insurance were small.

Contrary to our expectations that reductions in welfare benefits would be concentrated during the in-program period, when students' material needs were met by the program, the reductions in benefit receipt were fairly uniform across the 48-month follow-up period. To some extent, this reflects different time patterns of the impacts for different groups. The benefit reductions for males were uniform throughout the follow-up period. For females without children at baseline, benefit reductions were largest early in the follow-up period and then declined to nearly zero. In contrast, the benefit reductions for females with children at baseline, many of whom were nonresidential students, were negligible during the in-program period, when welfare helped support the participant and her child, but became larger during the postprogram period, when earnings also increased.

Job Corps participation significantly reduced arrest and conviction rates, as well as time spent in jail (Table 4). About 33 percent of control group members were arrested during the 48-month follow-up period, compared to 29 percent of program group members (a statistically significant impact of -4 percentage points per eligible applicant). The impact per participant was about -5 percentage points, which translates to a 16 percent reduction in the arrest rate. Arrest rate reductions were largest during the first year after random assignment (when most program enrollees were in Job Corps). 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.

TABLE 4

IMPACTS ON KEY PUBLIC ASSISTANCE AND CRIME OUTCOMES

	Program Group	Control Group	Estimated Impact per Eligible Applicant ^a	Estimated Impact per Participant ^b
Average Amount of Benefits Received, by Year (in Dollars)				
All years	3,696.0	4,155.7	-459.8*	-638.9*
1	1,109.8	1,225.9	-116.2*	-161.4*
2	978.7	1,101.6	-122.9*	-170.8*
3	893.3	1,001.4	-108.1*	-150.2*
4	745.5	825.6	-80.1*	-111.3*
Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year				
All years	28.8	32.6	-3.7*	-5.2*
1	11.1	14.1	-3.1*	-4.3*
2	10.5	11.3	-0.8	-1.2
3	11.1	11.4	-0.4	-0.5
4	9.6	10.3	-0.7	-0.9
Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment				
	22.1	25.2	-3.1*	-4.3*
Percentage Served Time in Jail for Convictions During the 48-Month Period				
	15.8	17.9	-2.1*	-2.9*
Average Weeks in Jail for Convictions During the 48-Month Period				
	6.0	6.6	-0.6	-0.8
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.

^aEstimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

^bEstimated 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.

^cBenefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance.

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

Program group members were less likely to have arrest charges for nearly all categories of crimes. However, reductions were slightly larger for less serious crimes (such as disorderly conduct and trespassing).

Job Corps participation also reduced convictions and incarcerations resulting from a conviction. More than 25 percent of control group members were ever convicted during the follow-up period, compared to 22 percent of program group members. Similarly, Job Corps reduced the percentage incarcerated for convictions by 2 percentage points (from 18 percent to 16 percent) and the average time spent in jail by about six days.

Although the level of criminal activity differed substantially across youth subgroups, the impacts on crime outcomes were very similar (in particular, by gender and age). We find some differences, however, in crime impacts by residential status. Job Corps reduced arrest rates for male residents, female residents, and female nonresidents. However, the program had no effect for male nonresidents.

Job Corps participation led to reductions in crimes committed against program participants. On average, Job Corps reduced the average number of victimizations by about 130 victimizations per thousand during the first 12 months after random assignment--a 20 percent reduction. As expected, the frequency of victimizations was reduced most during the in-program period, but the reductions persisted somewhat afterwards. Reductions were found for almost every crime type, and across most subgroups.

Job Corps had no impacts on the self-reported use of tobacco, alcohol, and illegal drugs. This finding applied for the full sample and for key subgroups. Job Corps also had little effect on time spent in drug treatment.

Job Corps improved participants' perceived health status. At each interview, about 17.5 percent of the control group and 15.5 percent of the program group said their health was "poor" or "fair."

Job Corps had no impacts on fertility or custodial responsibility, either for the full sample or by gender. About 38 percent of those in both the program and control groups had a child during the follow-up period (49 percent of females and 31 percent of males), and more than 80 percent of children were born out of wedlock. About two-thirds of all parents (and 42 percent of male parents) were living with all their children, and about 82 percent of male parents provided support for noncustodial children.

Job Corps participation slightly promoted independent living at the 48-month interview point. A slightly smaller percentage of program group members were living with their parents (32 percent, compared to 35 percent of control group members), and a slightly larger percentage were living with a partner either married or unmarried (31 percent, compared to 29 percent). Furthermore, program group members were more likely to report being the head of their household (52 percent, compared to 50 percent). This same pattern holds for males and females with and without children at baseline.

Job Corps slightly increased mobility, but had no impact on the types of areas in which participants lived at the 48-month interview point. Program group members were slightly less likely than control group members to have lived less than 10 miles from where they lived at application (73 percent, compared to 75 percent of the control group), and were slightly more likely to have lived more than 50 miles away (17 percent, compared to 16 percent). Thus, the average distance between the zip codes of residence at application to Job Corps and at the 48-month interview was slightly larger for the program group (94 miles, compared to 86 miles). The average characteristics of the counties of residence at 48 months, however, were similar for program and control group members. Furthermore, they were similar to the average county characteristics of residence at the time the youths applied to Job Corps (because most youths lived in the same areas at program application and at 48 months).

Job Corps participation led to increases in the use of child care. During the 48-month period, Job Corps participants used an average of about 146 more hours of child care than they would have if they had not enrolled in Job Corps.⁸ Impacts on child care use were positive during the first year after random assignment (when many program group members were enrolled in Job Corps) and during the fourth year (when employment impacts were the largest), but not in years 2 and 3. Impacts were found for females but not for males, because only a small percentage of fathers were living with their children and needed to find child care.

CONCLUDING OBSERVATIONS

Job Corps provided participants with the instructional equivalent of one additional year in school. Enrollees reported receiving extensive Job Corps services. Overall, they received an average of about 1,000 hours of education and training that they would not have received otherwise. This is approximately the hours of instruction delivered in a typical school year. These impacts on education and training could have led to the postprogram earnings gains we observed.

Of course, Job Corps also provides other services that could have contributed to the postprogram earnings gains. It provides a residential living program, health care, and a broad range of services designed to help youth who have not succeeded in school to become productive young adults. Many staff and observers of the program believe that the distinctive residential component of Job Corps is a key ingredient, both because the residential component is necessary for delivering effective academic and vocational instruction and because the experience of living in a community committed to learning has intrinsic benefits apart from the formal education and training that Job Corps provides. Because of the comprehensive nature of Job Corps, it is difficult to determine the relative contributions of the different parts of the program to the beneficial impacts that we find. However, viewing Job Corps as providing an additional year of schooling offers a way to place the earnings impacts into perspective.

Earnings gains observed beginning in the third year after random assignment are commensurate with what would be expected from an additional year of school. Economists

⁸Child care use pertains only to arrangements used by parents while they were working or attending education and training programs.

have long been concerned about the returns to schooling. They pose the question, How much difference does an additional year of schooling make in the lifetime earnings of an individual? The answers they have developed over the last two decades provide an important perspective on the study's findings.

Studies of the average returns to a year of schooling consistently find that a year of schooling increases earnings over a worker's lifetime by 8 to 12 percent. Measured in hours spent in academic classes and vocational training, Job Corps provided roughly the equivalent of a year of additional schooling per participant. In this context, the 12 percent earnings gains and the persistence of the earnings gains during the latter part of the 48-month period are in line with what one would expect from an intensive education and training program that serves primarily school-aged youth.

Most subgroups of students benefited from Job Corps. The finding that Job Corps improves key outcomes for broad groups of students rather than for only a subset provides further evidence that the program is effective. Participation led to substantial improvements in education-related outcomes for all subgroups of students that we investigated. Employment and earnings gains were similar for males and females. Postprogram earnings gains were found for groups of students at special risk of poor outcomes (such as very young students, females with children, those arrested for nonserious crimes, and older youths who did not possess a high school credential at baseline), *as well as* for groups at lower risk (such as older students with a high school credential at baseline). The program increased earnings for whites as well as for African Americans (although earnings gains were not found for Hispanics), and for those who applied before and after the ZT policies took effect. Reductions in criminal activity were found for nearly all groups of students. Thus, Job Corps effectively serves a broad group of students with differing abilities and needs.

While Job Corps is broadly effective, the impacts for several particularly vulnerable or difficult-to-serve groups are especially noteworthy.

Beneficial program impacts were found for 16- and 17-year-old youth. For this group: (1) average earnings gains per participant were nearly \$900 in year 4, (2) the percentage earning a high school diploma or GED was up by 66 percent, and (3) arrest rates were reduced by 11 percent and rates of incarceration for a conviction by 19 percent. While staff find this group difficult to deal with, and while more of them leave Job Corps before completing their education and training than do older students, the youngest age group does appear to benefit from their program experiences.

Females with children at the time of enrollment enjoyed significant earnings gains and modest reductions in welfare receipt. More than one-half of young women with children enrolled in Job Corps as nonresidential students, because child-rearing responsibilities required that they live at home. However, these young women received similar amounts of academic classroom instruction and vocational training as other students, despite living at home. Furthermore, in year 4, they enjoyed increases of more than 20 percent in their earnings and reductions of about 12 percent in their receipt of public assistance.

The residential and nonresidential programs serve different groups of students, and each is effective for the groups it serves. Earnings and employment impacts during the last two years of the follow-up period were positive overall for those assigned to each component. Furthermore, earnings gains were positive in each component for nearly all subgroups defined by gender and the presence of children at random assignment.

Importantly, it is *not* appropriate to conclude that the residential component could be abolished and everyone served just as well in the less expensive nonresidential component, for several reasons. First, the two components serve very different students. Nonresidential students tend to be females with children and older youths who would be unable to participate in the residential Job Corps program because of family responsibilities. On the other hand, residential students tend to be younger and less educated, and are deemed by Job Corps staff to require training in a residential setting to fully benefit from the program. Consequently, our results cannot be used to assess how students in the residential component (for example, 16- and 17-year-old residents) would fare in the nonresidential component.

Second, most centers with nonresidential slots also have residential slots, so nearly all nonresidential students train with residential students and may benefit from interacting with them. The program experiences of nonresidential students would probably be much different if the residential component were abolished.

Finally, nonresidential students receive services that are similar in many ways to those received by residential students, and the nonresidential component of Job Corps is more intensive and comprehensive than most other nonresidential training programs. In fact, the program cost per nonresidential student is only about 16 percent less than the program cost per residential student (McConnell et al. 2001). Thus, the cost of Job Corps would not be reduced significantly if all students were served in the nonresidential component.

In conclusion, we find that Job Corps produces beneficial impacts on the main outcomes that it intends to influence. Beneficial impacts on education-related, employment-related, and crime-related outcomes were found overall, as well as for broad subgroups of students. The residential and nonresidential program components were each effective for the students they served. A companion report, presenting findings from the benefit-cost analysis, concludes that Job Corps is a worthwhile investment both for the students and for the broader society that supports their efforts.

I. INTRODUCTION

Job Corps plays a central role in federal efforts to provide employment assistance to disadvantaged youths ages 16 to 24. The program’s goal is to help disadvantaged youths become “more responsible, employable, and productive citizens” by providing comprehensive services, including basic education, vocational skills training, counseling, and residential support. Each year, Job Corps serves more than 60,000 new enrollees and costs more than \$1 billion.

The National Job Corps Study, funded by the U.S. Department of Labor (DOL), was designed to provide information about the effectiveness of Job Corps in attaining its goal.¹ The cornerstone of the study was the random assignment of all youths found eligible for Job Corps to either a program group or a control group. Program group members were permitted to enroll in Job Corps, and control group members were not (although they could enroll in other training or education programs). The research sample for the study consists of approximately 9,400 program group members and 6,000 control group members randomly selected from among nearly 81,000 eligible applicants nationwide. Sample intake occurred between November 1994 and February 1996.

This report presents estimates of the impacts of Job Corps on participants’ employment and related outcomes during the 48 months after random assignment. The report addresses the following research questions:

- How effective is Job Corps overall at improving the employability of disadvantaged participants?

¹The study is being conducted by Mathematica Policy Research, Inc. (MPR) and its subcontractors, Battelle Human Affairs Research Centers and Decision Information Resources, Inc.

- Do Job Corps impacts differ for youths with different characteristics?
- How effective are the residential and nonresidential components of Job Corps?

To examine these questions, we estimated the impact of Job Corps on key outcome measures by comparing the distribution of outcomes of program and control group members, for the full sample and for key subgroups. The outcome measures for the analysis were constructed using follow-up survey data collected 12, 30, and 48 months after random assignment, and key subgroups were defined using baseline interview and program intake data. The findings presented here update those presented in our report on the short-term program impacts over the first two and a half years after random assignment (Schochet et al. 2000).

The rest of the report begins in Chapter II with an overview of the Job Corps program and the National Job Corps Study (with a focus on the design of the impact study). Chapter III describes data sources, outcome measures, and analytic methods used for the analysis. Chapter IV provides a brief summary of the Job Corps experiences of those in the program group. These three chapters provide important background and contextual information to aid in the interpretation of study findings. Chapters V, VI, and VII present impact estimates on the following categories of outcome measures that we hypothesized could be influenced by participation in Job Corps: (1) education and training; (2) employment, earnings, and job characteristics; and (3) nonlabor market outcomes, including the receipt of public assistance and other sources of income; criminal activities; tobacco, alcohol, and illegal drug use; and health, family formation, child care, and mobility.

II. OVERVIEW OF JOB CORPS AND THE NATIONAL JOB CORPS STUDY

Job Corps is an intensive and comprehensive program whose goal is to help disadvantaged youths become “more responsible, employable, and productive citizens.” The first part of this chapter summarizes the operational structure of Job Corps, key program elements, and the characteristics of youths who apply for the program and are determined to be eligible. The second part of the chapter provides an overview of the National Job Corps Study, including the primary research questions and the main study features that are being employed to assess the effectiveness of Job Corps. The focus of this section is to describe the study design for the impact analysis.

A. OVERVIEW OF JOB CORPS

The Job Corps program, established by the Economic Opportunity Act of 1964, operates under provisions of the Workforce Investment Act (WIA) of 1998.¹ The operational structure of Job Corps is complex, with multiple levels of administrative accountability, several distinct program components, and numerous contractors and subcontractors. DOL administers Job Corps through a national office and nine regional offices. The national office establishes policy and requirements, develops curricula, and oversees major program initiatives. The regional offices procure and administer contracts and perform oversight activities, such as reviews of center performance.

Through its regional offices, DOL uses a competitive bidding process to contract out center operations, recruiting and screening of new students, and placement of students into jobs and other educational opportunities after they leave the program. At the time of the study, 80 centers were operated under such contracts. In addition, the U.S. Departments of Agriculture and of the Interior operated 30 centers, called Civilian Conservation Centers (CCCs), under interagency agreements

¹For much of the study, Job Corps operated under provisions of the Job Training Partnership Act (JTPA) of 1982.

with DOL. Figure II.1 shows the location of the 105 Job Corps centers in the contiguous 48 states and the District of Columbia that were in operation at the time our program group members were enrolled, and displays the nine Job Corps regions.^{2,3}

Next, we briefly outline the roles of the three main program elements and then highlight key characteristics of youths served by the program. The section concludes with a discussion of major policy changes that occurred during the study period. The process analysis report for the evaluation provides more details on these topics (Johnson et al. 1999).

1. Outreach and Admissions

Outreach and admissions (OA) agencies conduct recruitment and screening for Job Corps. OA agencies include private nonprofit firms, private for-profit firms, state employment agencies, and the centers themselves. These agencies provide information to the public through outreach activities (for example, by placing advertisements and making presentations at schools), screen youths to ensure that they meet the eligibility criteria, assign youths to centers (when the regional office delegates this function), and arrange for transportation to centers.

2. Job Corps Center Services

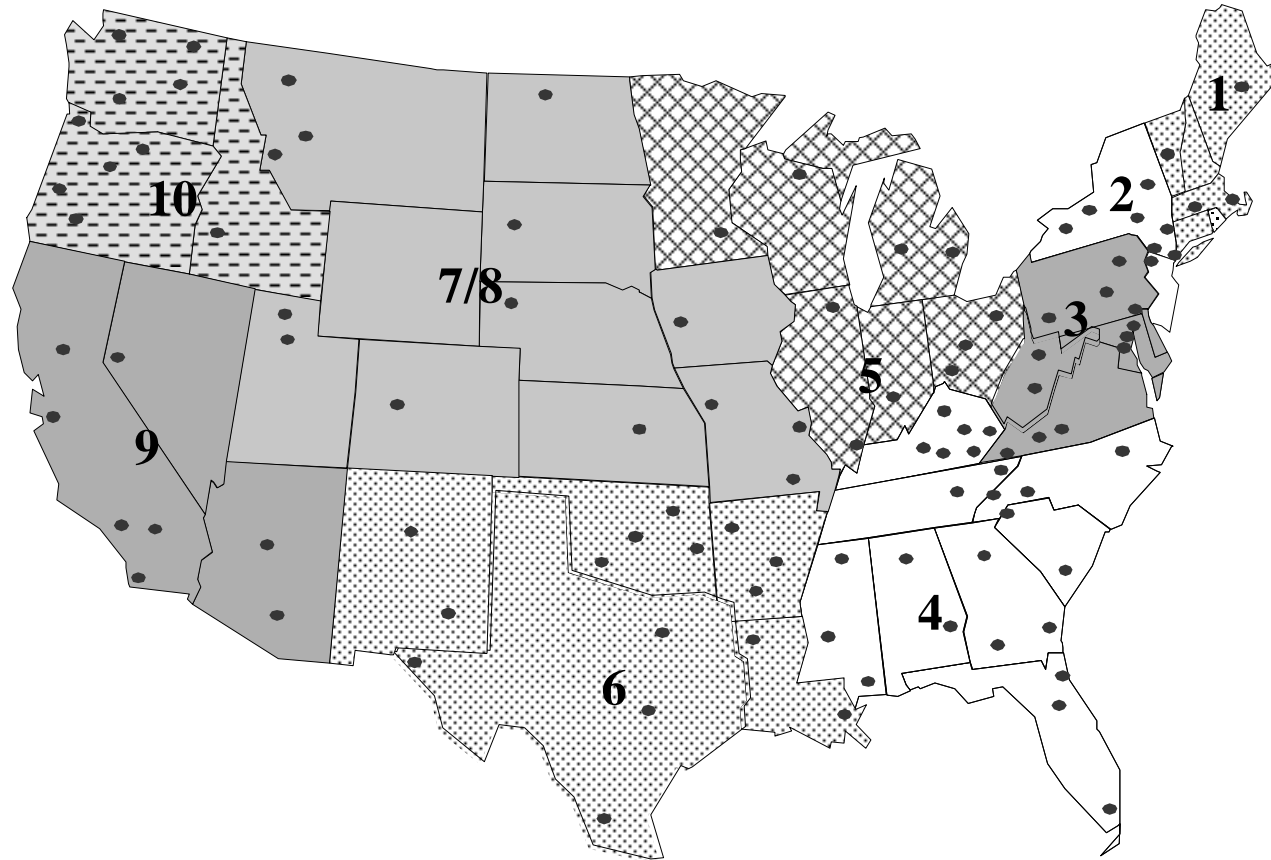
Job Corps is a comprehensive and intensive program. Its major components include basic education, vocational training, residential living (including training in social skills), health care and education, counseling, and job placement assistance. Services in each of these components are tailored to each participant.

²In total, there were 110 centers in operation, including the five centers in Alaska, Hawaii, and Puerto Rico.

³There are currently 119 centers in operation.

FIGURE II.1

JOB CORPUS CENTERS IN PROGRAM YEAR 1995, BY REGION



5

- Indicates one of the 105 Job Corps Centers in the contiguous 48 States and the District of Columbia.

Education. The goal of the education component is to enable students to learn as fast as their individual abilities permit. Education programs in Job Corps are individualized and self-paced, and they operate on an open-entry and open-exit basis. The programs include remedial education (emphasizing reading and mathematics), world of work (including consumer education), driver education, home and family living, health education, programs designed for those whose primary language is not English, and a General Educational Development (GED) program of high school equivalency for academically qualified students. About one-fourth of the centers can grant state-recognized high school diplomas.

Vocational Training. The vocational training programs at Job Corps, like the education component, are individualized and self-paced and operate on an open-entry and open-exit basis. Each Job Corps center offers training in several vocations, typically including business and clerical, health, construction, culinary arts, and building and apartment maintenance. National labor and business organizations provide vocational training at many centers through contracts with the Job Corps national office.

Residential Living. Residential living is the component that distinguishes Job Corps from other publicly funded employment and training programs. The idea behind residential living is that, because most participants come from disadvantaged environments, they require new, more supportive surroundings to derive the maximum benefits from education and vocational training. All students must participate in formal social skills training. The residential living component also includes meals, dormitory life, entertainment, sports and recreation, center government, center maintenance, and other related activities. Historically, regulations had limited the number of nonresidential students to 10 percent, but Congress raised that limit to 20 percent in 1993.

Health Care and Education. Job Corps centers offer comprehensive health services to both residential and nonresidential students. Services include medical examinations and treatment; biochemical tests for drug use, sexually transmitted diseases, and pregnancy; immunizations; dental examinations and treatment; counseling for emotional and other mental health problems; and instruction in basic hygiene, preventive medicine, and self-care.

Counseling and Other Ancillary Services. Job Corps centers provide counselors and residential advisers. These staff help students plan their educational and vocational curricula, offer motivation, and create a supportive environment. Support services are also provided during recruitment, placement, and the transition to regular life and jobs following participation in Job Corps.

3. Placement

The final step in the Job Corps program is placement, which helps students find jobs in training-related occupations with prospects for long-term employment and advancement. Placement contractors may be state employment offices or private contractors, and sometimes the centers themselves perform placement activities. Placement agencies help students find jobs by providing assistance with interviewing and resume writing and services for job development and referral. They are also responsible for distributing the readjustment allowance, a stipend students receive after leaving Job Corps.

4. Characteristics of Youths Served by Job Corps

To participate in Job Corps, youths must be legal U.S. residents ages 16 to 24. Males 18 or older must be registered with the Selective Service Board, and minors must have the consent of a parent or guardian. Youths must also be disadvantaged (defined as living in a household that

receives welfare or has income below the poverty level) and living in a debilitating environment that substantially impairs prospects for participating in other programs. Youths must need additional education, training, and job skills and possess the capacity and aspirations to benefit from Job Corps. They must also be free of serious behavioral and medical problems, and they must have arranged for adequate child care (if necessary) when they participate in Job Corps.

The detailed information from the study's baseline interview provides insights about the backgrounds of eligible Job Corps applicants (Schochet 1998a). Most eligible applicants are male (60 percent), and most are younger than 20 (40 percent are 16 or 17 years old, and nearly one-third are 18 or 19). About 40 percent live in the South, and more than 70 percent are members of racial or ethnic minority groups: 50 percent are African American, 18 percent are Hispanic, 4 percent are Native American, and 2 percent are Asian or Pacific Islander. Most (nearly 80 percent) do not have a high school credential. About 18 percent have children, and nearly 60 percent received some form of public assistance during the year prior to random assignment. About one-quarter reported that they had ever been arrested, and about 30 percent reported using illegal drugs in the year prior to random assignment.

The characteristics of eligible applicants differ by gender and age. Female applicants tend to be older than male applicants, and a higher percentage have children (29 percent, compared to 11 percent). Consequently, a much higher percentage of females (and especially females with children) are assigned to the nonresidential component. Females are more likely to have a high school credential (27 percent, compared to 17 percent of males) at the time of program application, in part because they are older. Females are also less likely to report having used illegal drugs in the prior year (25 percent, compared to 35 percent of males) or ever having been arrested (17 percent, compared to 33 percent of males).

Many of the differences across age groups would be expected. For example, older applicants are much more likely than younger applicants to have been recently employed and to have a high school credential (50 percent of those ages 20 to 24 have a credential) and are much less likely to have recently participated in an education program.

Younger eligible applicants exhibit several characteristics that suggest they may be more disadvantaged and harder to serve than older applicants. A higher proportion of younger applicants report having used drugs, having ever been arrested, and having recently been arrested. Furthermore, younger applicants are more likely to come from single-parent households and from families that received public assistance in the prior year.

5. Policy Changes Related to Violence and Drugs

In response to congressional concerns about the operation of the Job Corps program, new zero-tolerance (ZT) policies for violence and drugs were instituted in March 1995--early in the sample intake period for the National Job Corps Study. The new policies were instituted to ensure full and consistent implementation of existing policies for violence and drugs. According to the new, stricter ZT policy, students accused of specific acts of violence (possession of a weapon, assault, sexual assault, robbery, extortion, or arson) or arrested for a felony are to be removed from the center immediately and terminated from the program if fact-finding establishes they committed the alleged offenses. The ZT policy for drugs uses the same procedures for students accused of possession or sale of illegal drugs or alcohol on center or convicted of a drug offense.

The policies were intended to facilitate the rapid removal of offending students and to eliminate any discretion of staff regarding termination. Most Job Corps staff reported that the new policies substantially improved the quality of life on centers (Johnson et al. 1999). Thus, the new policies could have affected program impacts. Consequently, as discussed in Chapter III, we computed

separate impact estimates for sample members who applied to Job Corps before and after the new ZT policies became effective.

B. OVERVIEW OF THE NATIONAL JOB CORPS STUDY

The National Job Corps Study addresses six major research questions:

1. How effective is Job Corps overall at improving the employability of disadvantaged youth?
2. Does the effectiveness of Job Corps differ for youths with different personal characteristics or experiences before application to Job Corps? Do impacts vary by gender, age, the presence of children, education level, race and ethnicity, or arrest history?
3. Do program impacts differ for centers with different characteristics? Do impacts vary by CCC or center contractor type, center size, center performance level, or region?
4. Do program impacts differ for enrollees with different program experiences? Do impacts differ by residential status or program completion status?
5. What is the Job Corps program “model,” and how well is it implemented in practice?
6. Is Job Corps cost-effective?

The study consists of an impact analysis (to address Questions 1 to 4), a process analysis (to address Question 5), and a benefit-cost analysis (to address Question 6).

This report presents impact estimates for the full sample and for subgroups defined by youth characteristics (to address the first two research questions). This analysis forms the core of the 48-month impact analysis because it provides information about the effectiveness of Job Corps overall and identifies groups of the eligible population that benefit most (and least) from the program. The report also assesses the effectiveness of the residential and nonresidential components. This facet of the overall evaluation is of considerable policy interest for two reasons: (1) the residential component is the distinguishing feature of Job Corps, and (2) previous studies (for example, the

JTPA and JOBSTART evaluations) indicate that disadvantaged youths do not benefit significantly from participation in training programs that offer basic education and job-training services in a nonresidential setting.

Separate reports present impacts for subgroups defined by key center characteristics (to address Question 3; Burghardt et al. 2001) and program completion status (to address the rest of Question 4; Gritz et al. 2001). The purpose of these analyses is to identify program features and components that are particularly effective, so that policymakers can improve program operations and direct future program expansions.

In the rest of this section, we first provide an overview of the sample design for the impact analysis. Second, we review the evidence that the random assignment design was successfully implemented, which would suggest that program impacts can be effectively estimated. More details on these topics are provided in the report on study implementation (Burghardt et al. 1999). Finally, we briefly discuss key features of the process and benefit-cost analyses.

1. Impact Analysis

The central feature of the study design was the random assignment of all youths found eligible for Job Corps, either to a program group whose members were permitted to enroll in Job Corps or to a control group whose members were not. DOL considered both random assignment and nonexperimental design options in the initial design stages of the study. Because of the need for reliable, credible information about program impacts, a study advisory panel, which included representatives of Job Corps, concluded that a random assignment design was feasible and should be used for the study.

a. Sample Design

Sample intake occurred between November 1994 and February 1996. With few exceptions, all youths who applied to Job Corps for the first time between November 16, 1994, and December 17, 1995, and were found eligible for the program were included in the study--a total of 80,883 eligible applicants. During the sample intake period, 5,977 Job Corps-eligible applicants were randomly selected to the control group. Approximately 1 eligible applicant in 14 (seven percent of 80,883 eligible applicants) was assigned to the control group.

During the same 16-month period, 9,409 eligible applicants were randomly assigned to the research sample as members of the program research group (hereafter referred to as the program group).⁴ Because random assignment occurred after youths were determined eligible for Job Corps (and *not* after they enrolled in Job Corps centers), the program group includes youths who enrolled in Job Corps (about 73 percent of eligible applicants), as well as those who did not enroll, the so-called “no-shows” (about 27 percent of eligible applicants). Although the study’s research interest focuses on enrollees, all youths who were randomly assigned, including those who did not enroll at a center, were included in the analysis to preserve the benefits of the random assignment design.

Control group members were not permitted to enroll in Job Corps for a period of three years, although they were able to enroll in other programs available to them. Thus, the outcomes of the control group represent the outcomes that the program group would have experienced if they had not been given the opportunity to enroll in Job Corps. Because control group members were allowed to enroll in other education and training programs, the 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. The impact estimates do not represent

⁴The remaining 65,497 eligible applicants were randomly assigned to a program nonresearch group. These youths were allowed to enroll in Job Corps but are not in the research sample.

the effect of the program relative to no education or training; instead, they represent the incremental effect of Job Corps.

The National Job Corps Study is based on a fully national sample. With a few exceptions, the members of the program and control groups were sampled from *all* OA agencies located in the contiguous 48 states and the District of Columbia, rather than from only some OA agencies in certain areas.⁵ This design feature allows us to obtain impact estimates that are more precise than those that could be obtained from a clustered sample of the same size. In addition, the nonclustered design spread the burden of random assignment across all OA agencies and Job Corps centers, which reduced the burden on any one agency or center.

The sampling rates to the control and program groups differed for some population subgroups for both programmatic and research reasons. For example, OA agencies experienced difficulties recruiting females for residential slots, and Job Corps staff were concerned that the presence of the control group would cause these slots to go unfilled. Therefore, sampling rates to the control group were set lower for females in areas from which high concentrations of residential students come. Because of differences in sampling rates across population subgroups, all analyses were conducted using sample weights so that the impact estimates can be generalized to the intended study population: applicants in the 48 contiguous states and the District of Columbia who applied to Job Corps during the 13-month period between November 17, 1994, and December 16, 1995, and who were determined to be eligible for the program.⁶

⁵Youths who previously participated in Job Corps (“readmits”) or who applied for one of seven small, special Job Corps programs were excluded from the study (see Burghardt et al. 1999).

⁶The study population also included only those whose random assignment forms were received by MPR before March 1, 1996. This restriction did not exclude many eligible applicants who applied to the program during the 13-month period, because the time between program application and eligibility determination is typically very short.

b. Implementation of Random Assignment

As expected, random assignment produced equivalent groups, because the distribution of the characteristics of program and control group members prior to random assignment was similar (Schochet 1998b). However, our ability to draw valid inferences from a random assignment study depends on three conditions: (1) that all members of the study population were subject to random assignment, (2) that control group members did not enroll in the program, and (3) that operations of the program were not materially affected by the study.

To identify center enrollees in the study population who were not randomly assigned and to ensure that control group members did not enroll, we examined weekly extracts from the Job Corps Student Pay, Allotment, and Management Information System (SPAMIS) on all new center enrollees.

Our monitoring indicates that Job Corps staff implemented random assignment procedures well. Less than 0.6 percent of youths in the study population were not randomly assigned. In addition, only 1.4 percent of control group members enrolled in Job Corps before the end of the three-year period during which they were not supposed to enroll.⁷ Hence, we believe that the research sample is representative of the youths in the intended study population and that the bias in the impact estimates due to contamination of the control group is very small.

In general, the study did not appear to alter program operations substantially, which suggests that the study is evaluating Job Corps as it would have normally operated in the absence of the study. We found from the process analysis that the effects of the random assignment process on OA counselors' activities and on the composition of students coming to the program appear to have been modest. For example, few OA counselors said they started new outreach activities, spent more time

⁷An additional 3.2 percent of control group members enrolled in Job Corps after their three-year restriction period ended and before four years after random assignment (see Chapter III).

on outreach, or lost referral sources because of the study. In addition, OA counselors do not appear to have provided substantially more assistance in finding alternative training opportunities to the control group than they provided for other applicants who could not enroll in Job Corps.

The study, however, contributed somewhat to the decrease in the number of center slots that were filled (that is, in center on-board strength) in early 1995, because control group members were removed from the pool of potential center enrollees. We estimate, however, that the introduction of the new ZT policies had a much larger effect on the decrease in center on-board strength. Nonetheless, the study could have had some effect on the training experiences of program group members, as centers served fewer students without reducing center staff.

2. Process Analysis

The purpose of the process study was to describe the key elements of the Job Corps program model and to document how they were implemented during calendar year 1996--roughly the period when study program group members were enrolled in Job Corps centers (Johnson et al. 1999). The process study collected a large amount of information about OA practices, center operations, and placement from (1) a telephone survey of Job Corps OA counselors, (2) a mail survey of all Job Corps centers, and (3) visits to 23 centers.

The analysis found that Job Corps uses a well-developed program model and is successful in implementing it. Job Corps students are receiving substantial, meaningful education and training services. We refer to process analysis findings in this report because they provide important contextual information to help interpret findings from the impact analysis.

3. Benefit-Cost Analysis

The primary purpose of the benefit-cost analysis is to assess whether the benefits of Job Corps are commensurate with the substantial public resources invested in it. The most important benefits that are valued are (1) increased output that may result from the additional employment and productivity of program participants; (2) increased output produced by youths while in Job Corps; (3) reduced criminal activity; and (4) reduced use of other services and programs, including welfare and other educational programs. The most important Job Corps costs include program operating costs and the earnings forgone while the youth attended Job Corps.

The results of the benefit-cost analysis are presented in a companion report (McConnell et al. 2001).

III. DATA SOURCES, OUTCOME MEASURES, AND ANALYTIC METHODS

We conducted the impact analysis using survey data collected during the 48 months after random assignment. We used data on the experiences of sample members during the follow-up period to construct outcome measures so that the analysis could address the following research questions:

- Do participants receive more education and vocational training than they would have if they had not participated in Job Corps? Are they more likely to obtain a high school credential or vocational certificate?
- Does participation in Job Corps increase productivity and, hence, time spent employed and earnings?
- Does participation in Job Corps reduce dependence on welfare and other public transfers?
- Does Job Corps reduce the incidence and severity of crimes committed by program participants, both during and after the program? Does Job Corps reduce crimes committed against participants?
- Are participants less likely to use tobacco, alcohol, and illegal drugs?
- Does Job Corps reduce the likelihood of bearing or fathering children while unmarried and increase the likelihood of forming a stable, long-term relationship?
- Do participants move to areas that offer opportunities different from those in the areas they came from?

To address these questions, we estimated program impacts by comparing the distribution of outcomes of program and control group members. Program impacts were estimated for the full sample and for key subgroups defined by youth characteristics (using baseline interview data) and whether the youth was designated for a residential or nonresidential slot (using program intake data).

A. DATA SOURCES

We used four main categories of data for the impact analysis presented in this report:

1. ***Follow-Up Interview Data Collected 12, 30, and 48 Months After Random Assignment.*** We used these data, which contain information on the employment-related and other experiences of sample members during the follow-up period, to construct outcome measures for the impact analysis. Each follow-up interview contains information on the experiences of sample members since the previous interview. We used these data to construct longitudinal outcome measures so that we could examine changes in program impacts over time.
2. ***Baseline Interview Data.*** This information was collected soon after random assignment and contains background information on sample members and their experiences prior to the baseline interview. We used these data to create subgroups defined by youth characteristics at random assignment. We also used them to construct outcome measures that pertain to the period between the random assignment and baseline interview dates.
3. ***Data from Job Corps Intake (ETA-652) Forms.*** These are the standard intake forms that OA counselors and program applicants fill out as part of the application process. They contain basic demographic information on applicants. MPR received these forms as part of the random assignment process and data-entered the information into the computer for those in the research sample. Because this information is available for *all* research sample members, we used it in the nonresponse analysis to compare the characteristics of interview respondents and nonrespondents, and to adjust sample weights to account for the possible effects of interview nonresponse on the impact estimates.
4. ***Data from the Supplemental ETA-652 Forms.*** These forms, which were created for the study, were filled out by OA counselors as part of the application process and were sent to MPR as part of the random assignment process. The forms collected information on whether the youth was likely to be assigned to a residential or a nonresidential slot. As described in more detail later in this chapter, we used this information to estimate program impacts for residential and nonresidential students. The forms also collected information on the center to which a youth was likely to be assigned. We used these data in a separate report that presents program impact estimates for subgroups defined by key center attributes (for example, CCC or contract center type, center performance level, center size, and region).

The impact analysis also uses other data. Functional literacy test score data on a random subsample of the research sample were collected in conjunction with the 30-month interview.

Impact results using these data are presented in Glazerman et al. (2000) and are referred to in this report. In addition, we collected official crime records data from North Carolina and Texas covering the 30-month period after random assignment, and compared crime levels and impacts based on these records to those based on the follow-up interview data (Needels et al. 2000). We also refer to these findings in this report. Future reports will present impact results using administrative data on social security earnings on all sample members and Unemployment Insurance (UI) administrative records from 17 randomly selected states.

The rest of this section provides an overview of the survey design, the interview response rates, and the analysis samples. A separate methodological report (Schochet 2001) discusses these topics in more detail.

1. Design of the Baseline and Follow-Up Interviews

Baseline interviewing took place between mid-November 1994 and July 1996. We contacted all sample members by telephone soon after they had been subject to random assignment. We used detailed tracking information (contained in program intake forms sent to MPR as part of the random assignment process) to help locate youths. In randomly selected areas, we attempted in-person interviews with sample members not reachable by telephone within 45 days. To contain data collection costs, we subsampled youths for intensive in-person interviewing.

The target sample for the 12-month follow-up interview included (1) all sample members selected for in-person interviews at baseline (whether interviewed or not), and (2) those not eligible for in-person interviews at baseline who completed the baseline interview by telephone within 45 days after random assignment. Thus, youths who resided in areas not selected for in-person interviews and who did not complete a baseline interview by telephone within 45 days were not

eligible for 12-month (and subsequent) interviews. At the end of the 12-month interview, we administered an abbreviated baseline interview to those 12-month respondents in the in-person areas who had not completed the full baseline interview.

We attempted a 30-month interview with all sample members who completed either the baseline or the 12-month interview. Youths eligible for a 48-month interview were those who completed any previous interview. However, to reduce data collection costs, we randomly selected for 48-month interviewing about 93 percent of program group members who were eligible for 48-month interviews. We asked respondents to the 30- and 48-month interviews about their experiences since their previous interview.

For the 12-, 30-, and 48-month interviews, we first attempted interviews by telephone and then, if we were unsuccessful, in person. In contrast to the in-person interviewing at baseline, there was no clustering of in-person interviews in the follow-up interviews. We conducted the 12-month interview between March 1996 and September 1997, the 30-month interview between September 1997 and February 1999, and the 48-month interview between December 1998 and May 2000.

We offered a \$10 incentive fee to control group members and hard-to-locate program group members (who were not at a Job Corps center) to induce them to complete each interview. In June 1999, however, we increased the incentive fee to \$25 to boost the response rate to the 48-month interview.

2. Response Rates and Data Quality

The response rate to the baseline interview for sample members in all areas was 93.1 percent. We completed interviews with 14,327 of the 15,386 youths in the research sample, most by telephone soon after random assignment. Furthermore, the difference in completion rates between the program and control groups was only 1.5 percentage points (93.8 percent program, 92.3 control). The response rate for sample members in the areas selected for in-person interviewing--the *effective*

response rate--was 95.2 percent (95.9 percent program, 94.3 percent control). Response rates to the baseline interview were high for all key subgroups. Item nonresponse was infrequent for nearly all data items.

We completed 13,383 12-month interviews, 11,787 30-month interviews, and 11,313 48-month interviews. As Table III.1 shows, the effective response rate to the 12-month follow-up interview was 90.2 percent (91.4 percent program, 88.4 percent control), to the 30-month interview 79.4 percent (80.7 percent program, 77.4 percent control), and to the 48-month interview 79.9 percent (81.5 percent program, 77.8 percent control).¹

The response rates differed somewhat across some key subgroups. For example, the 48-month interview response rate was higher for females than for males (85 percent, compared to 76 percent) and for those never convicted prior to program application than for those ever convicted (80 percent, compared to 76 percent). Thus, we adjusted the sample weights to help reduce the potential bias in the impact estimates due to interview nonresponse.² As with the baseline interview, nonresponse to follow-up interview data items was infrequent.

We completed the average 12-month interview in month 14, and more than three-quarters by month 15 (not shown). Similarly, we completed the average 30-month interview in month 32.5, and

¹The effective response rate is the response rate for youths in areas selected for in-person interviews at baseline. This is the relevant response rate for the study, because we did not attempt follow-up interviews with youths who were ineligible for in-person interviews at baseline and who did not complete a baseline interview by telephone within 45 days after random assignment.

²The methodological report (Schochet 2001) provides a detailed discussion of interview nonresponse, including the methods used to adjust the sample weights to account for interview nonresponse. This analysis shows that for each research group there are some differences in the average baseline characteristics of respondents to the 48-month interview and the full sample of respondents and nonrespondents. There are fewer differences, however, in the average baseline characteristics of program group respondents and control group respondents.

TABLE III.1

EFFECTIVE RESPONSE RATES TO THE 12-MONTH, 30-MONTH, AND 48-MONTH FOLLOW-UP INTERVIEWS,
BY RESEARCH STATUS AND KEY SUBGROUP
(Percentages)

Subgroup	Effective Response Rate								
	12-Month Interview			30-Month Interview			48-Month Interview		
	Program Group	Control Group	Combined Sample	Program Group	Control Group	Combined Sample	Program Group ^a	Control Group	Combined Sample
Full Sample	91.4	88.4	90.2	80.7	77.4	79.4	81.5	77.8	79.9
Gender									
Male	90.8	86.8	89.1	77.9	74.3	76.3	78.2	73.7	76.2
Female	92.2	91.0	91.8	84.2	82.7	83.7	85.6	84.6	85.2
Age at Application									
16 to 17	92.2	90.5	91.5	81.5	79.6	80.7	81.4	79.2	80.4
18 to 19	90.9	87.6	89.6	79.9	77.4	78.9	81.9	77.3	80.0
20 to 21	91.4	87.6	89.8	81.2	75.5	78.9	81.0	76.8	79.2
22 to 24	90.3	84.2	87.9	79.5	72.4	76.8	81.1	75.6	78.9
Race/Ethnicity									
White, non-Hispanic	89.9	87.0	88.7	80.1	77.4	79.0	80.6	78.9	79.9
Black, non-Hispanic	91.8	89.4	90.9	80.7	78.0	79.6	82.3	78.6	80.8
Hispanic	91.2	85.9	89.0	80.1	75.3	78.1	79.6	73.5	76.9
Other	94.6	90.6	92.9	86.1	78.0	82.8	80.7	77.4	79.2
Education Level at Application									
Completed 12th grade	92.4	89.6	91.3	83.0	81.2	82.0	84.4	79.0	82.2
Did not complete 12th grade	91.2	88.1	89.9	80.1	76.5	78.8	80.6	77.6	79.3
Conviction History at Application									
Ever convicted or adjudged delinquent	91.1	88.6	90.0	77.5	72.5	75.4	78.2	72.7	75.8
Never convicted or adjudged delinquent	91.4	88.3	90.1	81.0	77.6	79.6	81.6	78.2	80.2
Residential Designation Status									
Resident	91.1	87.6	89.7	80.1	76.2	78.5	81.1	76.6	82.6
Nonresident	92.7	91.2	92.1	82.8	82.1	82.5	82.9	82.1	79.2
Sample Size in In-Person Areas^b	6,206	4,242	10,448	6,182	4,223	10,405	5,725	4,212	9,937

TABLE III.1 (*continued*)

SOURCE: 12-month, 30-month, and 48-month interview data, and ETA-652 data.

NOTE: The effective response rate is the response rate for sample members eligible for in-person interviews at baseline (that is, those who lived in the in-person areas at application to Job Corps). Youths not in the in-person areas who did not complete baseline interviews by telephone within 45 days after random assignment were not eligible for follow-up interviews.

^aTo reduce data collection costs, 93 percent of program group members eligible for 48-month interviews were randomly selected for 48-month interviewing.

^bFigures exclude those who died during the follow-up period and 63 cases (31 control group and 32 program group members) in the in-person areas who were determined to have enrolled in Job Corps prior to random assignment and were thus ineligible for the study.

about 78 percent by month 34. Finally, we completed the average 48-month interview in month 49.8, and more than 78 percent by month 51. These figures are similar for program and control group members. Thus, the recall period was similar across sample members and did not differ, on average, by research status.

On the basis of these results, we believe that the interview response rates and data quality are high enough to produce credible impact estimates for the full sample and for key subgroups.

3. Analysis Samples

The primary sample used for the analysis includes the 11,313 youths (6,828 program group members and 4,485 control group members) who completed 48-month interviews. About 88 percent of this sample also completed 30-month interviews, and 95 percent completed 12-month interviews. More than 85 percent completed both the 12- and the 30-month interviews, and only 2 percent completed neither. Furthermore, baseline interview data are available for everyone in this sample, because all youths completed either the full baseline interview or the abbreviated baseline interview in conjunction with the 12-month interview.³ Thus, complete data are available for most of the analysis sample.

The short-term impact report (Schochet et al. 2000) presents impact estimates covering the 30-month period after random assignment using the 11,787 youths who completed 30-month follow-up interviews. These results are very similar to the corresponding estimates covering the 30-month period obtained using the 48-month sample. Thus, we present results covering the entire follow-up period using the 48-month sample only.

The follow-up period for the analysis sample covers the period from November 1994 (the first month after random assignment--month 1--for those randomly assigned in November 1994) to

³About 210 cases in the analysis sample completed an abbreviated baseline interview.

February 2000 (month 48 for those randomly assigned in February 1996). This was a period of strong economic growth. For example, the unemployment rate for the civilian population of those 16 and older was about 5.5 percent in late 1994, about 50 percent in 1997, and about 4 percent in early 2000. Similarly, the unemployment rate for those 16 to 19 decreased from about 17 percent in late 1994 to under 14 percent in early 2000. As discussed in Chapter VI, it is difficult to determine the effects of the strong economy on the impact estimates. However, these potential effects should be kept in mind when interpreting the impact results.

B. OUTCOME MEASURES

Three criteria guided specification of the major outcome measures for the impact analysis: (1) selecting outcomes that are likely to be influenced significantly by Job Corps participation, (2) selecting outcomes that have policy relevance, and (3) measuring outcomes reliably. Next, we discuss the primary outcome measures, our hypotheses about how they are likely to be affected by Job Corps participation, and their construction. Table III.2 displays the outcome measures used in the analysis.

1. Primary Outcome Measures

The primary outcome measures can be grouped into six areas:

Education and Training. The major goal of Job Corps is to provide intensive academic classroom instruction and vocational skills training to increase the productivity, and hence the future earnings, of program participants. The typical Job Corps student stays in the program for an extended period (about eight months on average), and most enroll after leaving school. Thus, participation in Job Corps probably leads to increases in the amount of education and training youths

TABLE III.2

OUTCOME MEASURES DEFINED OVER SPECIFIC PERIODS

Education and Training

All Programs

- Ever enrolled
- Number attended
- Weeks attended
- Hours per week attended

Specific Programs

- Ever enrolled in the following programs: Job Corps; high school; GED; ABE or ESL; vocational, technical, or trade; two-year college; four-year college
- Weeks attended, by type of program
- Hours attended, by type of program

Academic Classes

- Ever took
- Weeks took
- Hours per week took
- Types of programs where took

Vocational Training

- Ever received
- Weeks received
- Hours per week received
- Types of programs where received

Educational Attainment

- Degrees, diplomas, and certificates
(high school diploma,^a GED certificate,^a vocational, technical, or trade certificate or diploma; associate degree; four-year college degree)
- Highest grade completed

Employment, Earnings, and Job Characteristics

Employment

- Ever employed
- Number of jobs
- Weeks employed
- Hours per week employed

TABLE III.2 (continued)

Employment, Earnings, and Job Characteristics (continued)

Earnings

Distribution of earnings

Characteristics of the Most Recent Job in Quarter 10 and in Quarter 16

Had a job

Months on job

Usual hours worked per week

Hourly wage

Weekly earnings

Occupation

Type of employer (private company, military, federal employee, state employee, local government employee, self-employed)

Job benefits available (health insurance, paid sick leave, paid vacation, child care assistance, flexible hours, employer-provided transportation, retirement pension benefits, dental plan, tuition reimbursement)

Education and Employment Activities

Ever participated in any activity

Weeks participated

Hours per week participated

Receipt of Public Assistance and Other Sources of Income

Public Assistance

Received benefits (AFDC/TANF, food stamps, General Assistance, SSI/SSA, WIC)

Months received benefits, by type

Amount of benefits received, by type

Covered by public health insurance (such as Medicaid) at the 12-, 30-, and 48-month interview

Lived in a public housing project at the 12-, 30-, and 48-month interview

Other Sources of Income

Received income (UI child support, from friends, other income)

Weeks received UI

Amount received, by type

TABLE III.2 (continued)

Crime, Alcohol and Illegal Drug Use, and Health

Criminal Activities

- Ever arrested or charged with a delinquency or criminal complaint
- Number of times arrested
- Months from random assignment until first arrested for those ever arrested
- Most serious charge for which arrested (murder or assault, robbery, burglary, larceny or other property crimes, drug law violations, other personal crimes, other miscellaneous crimes)
- All charges for which arrested
- Convicted, pled guilty, or adjudged delinquent
- Number of times convicted
- Made a deal or plea-bargained
- Most serious charge for which convicted
- All charges for which convicted
- Served time in jail for convictions
- Number of months in jail for convictions
- Put on probation or parole
- Number of times crimes were committed against sample members, by type of crime

Tobacco, Alcohol, and Illegal Drug Use in the 30 Days Prior to the 12-, 30-, and 48-Month Interviews

- Smoked cigarettes
- Consumed alcoholic beverages
- Tried marijuana or hashish
- Snorted cocaine powder
- Smoked crack cocaine or freebased
- Used speed, uppers, or amphetamines
- Used hallucinogenic drugs
- Used heroin, opium, methadone, or downers
- Used other drugs
- Injected drugs with a needle or syringe

Drug and Alcohol Treatment

- In a drug or alcohol treatment program
- Weeks in drug treatment
- Place where treatment was received

Health

- Health status at 12, 30, and 48 months
- At 12, 30, and 48 months, had physical or emotional problems that limited the amount of work or other regular daily activities that could be done
- Type of serious health problem
- Weeks had serious health problem since random assignment

TABLE III.2 (continued)

Family Formation

Had children during follow-up period
Number of children had during follow-up period
Had children out of wedlock during follow-up period
Percentage of females pregnant
Had children at 30 and 48 months (including those born before and after random assignment)
Percentage of children living with sample member (for parents)
Percentage of absent children who lived with their other parent^b
Time spent with children in the past three months^b
Currently provided support for children (food, child care items, household items, clothing, toys, medicine, babysitting, money)^b
Gave money in the past month^b
Gave money occasionally or on a regular basis^b
Amount of money gave in the past month^b
Ever used any child care
Type of child care used (child's parent, child's grandparent, other relative, nonrelative, day care center, other)
Weeks used child care
Hours per week used child care
Household membership (living with either parent, another adult relative, adult nonrelatives, or no other adults)
Whether sample member is the head of the household
Number in household
Marital status at 30 and 48 months (never married and not living together; married; living together; separated, divorced, or widowed)

Mobility

Distance in miles between zip codes of residence at application to Job Corps and at the 30-month interview
Lived in the same state at application to Job Corps and the 48-month interview
Characteristics of the counties of residence at application to Job Corps and the 48-month interview

SOURCE: Baseline, 12-month, 30-month, and 48-month interviews.

^aOutcomes defined only for those who did not have a high school credential at random assignment.

^bOutcomes defined for those not living with all their children.

receive while enrolled (as measured by increases in hours and weeks received academic classroom instruction and vocational skills training). These increases in education and training could lead to increases in educational attainment (as measured by the receipt of a GED or vocational certificate). Participation in Job Corps may also lead to increases in postsecondary school enrollment (such as two- and four-year colleges, the military, and vocational schools) after Job Corps. Participation in Job Corps, however, is expected to lead to reductions in time spent in alternative programs (such as high school and GED programs outside Job Corps). The effects on high school graduation status, however, are unclear, because about one-fourth of Job Corps centers can grant state-recognized high school diplomas.⁴

Employment, Earnings, and Job Characteristics. The primary hypothesis is that, if all other things are equal, youths who obtain Job Corps education and training will become more productive and, hence, will have greater employment opportunities and higher earnings than those who do not. This increased productivity is expected to enhance employability (as measured by increases in labor force participation, employment, hours worked per week, and the proportion of weeks worked) and to increase wage rates, earnings, and fringe benefits available on the job. Furthermore, because the Job Corps program provides placement assistance to participants when they leave the program, program group members should be more likely than control group members to find jobs and to find jobs that match their skills.

We expect, however, that Job Corps participation will reduce employment and earnings during the period of enrollment, because some participants would hold jobs if they had not gone to Job Corps. However, as program participants finish their participation, we expect employment and

⁴Job Corps participation could also lead to improvements in literacy skills, either directly, through participation in Job Corps basic education, or indirectly, by causing more students than would otherwise have done so to engage in skill-enhancing activities like work and further schooling. Program impacts on participants' literacy skills are presented in Glazerman et al. (2000).

earnings to rise after a period of readjustment. In light of the variation in the duration of program participation, it is difficult to predict how long after random assignment positive employment and earnings gains will emerge.

Receipt of Public Assistance and Other Sources of Income. A set of hypotheses closely related to labor market activities involves the effects of the Job Corps program on welfare dependence. Job Corps participants may experience a reduction in welfare receipt while they are in the program (to the extent that they would have been recipients were they not in the program). In addition, because their postprogram earnings may increase, they are expected to receive fewer public transfers (including Aid to Families with Dependent Children [AFDC] or Temporary Assistance for Needy Families [TANF], General Assistance [GA], food stamps, and Special Supplemental Food Program for Women, Infants and Children [WIC]).

Crime, Alcohol and Illegal Drug Use, and Health. Job Corps seeks to help youths become more employable and productive citizens. An important aspect of this process is to teach civic awareness and respect for others. In addition, many enrollees leave their neighborhoods to attend Job Corps. Thus, Job Corps is expected to reduce the incidence and severity of crimes committed by program participants (as measured by the number of arrests and convictions, the types of crimes committed, and the time spent in jails and on probation). While students are enrolled in the program, reductions in criminal activities should be pronounced, because Job Corps participants' activities are restricted, their behavior is monitored, and their material needs are met. Furthermore, most are isolated from social and environmental pressures to engage in criminal activities. After they leave the program, reductions in crime measures are expected to continue, but at a lower rate.

Job Corps should also lead to a reduction in crimes committed against Job Corps students. While at Job Corps centers, youth are less exposed to criminals who would victimize them. In

addition, if, after they have left Job Corps, students relocate to safer neighborhoods or spend less time hanging out on the street, the incidence of crimes committed against them may also be lower.

Job Corps is also expected to reduce participants' drug and alcohol use, both during and after the program. While youths are enrolled, impacts on drug and alcohol abuse should be pronounced, for two reasons. First, Job Corps forbids the use of these substances at centers, and behavior is closely monitored. Second, Job Corps provides some drug and alcohol abuse treatment. In the postprogram period, reductions in drug and alcohol use are expected to continue, because Job Corps should have a positive impact on attitudes toward it. Psychological and financial benefits derived from the program may also induce participants to feel more hopeful and under less pressure to use these substances.

Participation in Job Corps is also expected to increase participants' overall health status, for reasons similar to those discussed earlier, and because the program offers comprehensive health services and health education.

Family Formation. Important dimensions of personal responsibility are relationships with the opposite sex and the decision to have and raise children. The Job Corps program recognizes the importance of this area by requiring all students to take education program units on social and emotional well-being, sexuality, and parenting. Perhaps more important, other aspects of center experience, as well as improved economic opportunities resulting from Job Corps participation, may lead to changes in the way a youth relates to the opposite sex and on decisions to bear and raise children. Thus, the study examines a series of six outcomes related to family formation and children: (1) the likelihood of marriage; (2) the likelihood of forming a stable, long-term relationship with a single partner; (3) the likelihood of bearing or fathering children while unmarried; (4) the likelihood of living with one's children and the level of involvement with child rearing; (5) the nature and

extent of financial and nonfinancial support for absent children; and (6) the use of child care services.

Mobility. Many youths served by Job Corps live in neighborhoods where poverty rates are high and job opportunities are scarce. A core element of the philosophy motivating the residential component of Job Corps is that, for some, insurmountable barriers to succeeding in training in the youth's environment require removal from the home. Indeed, living in a debilitating environment that precludes participation in other education and training programs is a key Job Corps eligibility criterion.

This element of Job Corps raises the question of whether participation promotes mobility of students. Participation in Job Corps could affect the types of areas where students live after they leave the program, because of job placement and location assistance and because of the higher earnings that could make some neighborhoods more affordable. Thus, we examine the extent to which students return to the same areas that they lived in at the time of application, and the characteristics of the areas that they lived in at the 48-month interview.

2. Construction of Outcome Measures

Our analytic approach for the impact analysis focused on estimating period-specific impacts (that is, differences in outcomes between program and control group members by period). We constructed period-specific outcome measures using information on the dates that events occurred.⁵ For example, we constructed timelines to determine whether a sample member was working or in school or training in a given week or was receiving various types of public assistance (such as AFDC/TANF or food stamps) in a given month. As another example, we used self-reported crime

⁵A methodological appendix (Schochet 2001) provides a detailed discussion of the construction of outcome measures, including the treatment of missing values and outliers.

data to determine the timing of arrests and used fertility information to determine the timing of births. We also constructed period-specific measures about the characteristics of each activity. For example, we constructed measures of sample members' earnings, number of hours worked or in school, degrees received, public assistance benefit levels, and types of arrest charges over a given period.

Outcome measures were defined for the following periods after random assignment: (1) each quarter, (2) each year, and (3) the entire 48 months. The quarterly measures were used to examine changes in impact estimates over time and were constructed for key employment- and education-related outcomes. We used the yearly measures to summarize activities during the "in-program" and "postprogram" periods for many outcomes. As described in Chapter IV, the first year after random assignment was a period of intensive Job Corps participation for those in the program group who enrolled in centers, and the second year was a period of still significant but less intensive Job Corps participation. The last two years during the 48-month period were largely a postprogram period, because most program group members were no longer enrolled in Job Corps. We also constructed outcome measures that summarized sample member experiences over the entire 48-month period.

Some outcome measures pertain only to the time of the interview. For example, the follow-up interviews gathered data about tobacco, alcohol, and illegal drug use in the past 30 days and obtained information on the respondent's highest grade completed, overall health status, address, and living arrangements at the time of the interview.

C. ANALYTIC METHODS

The random assignment design ensures that no systematic observable or unobservable differences between program and control group members existed at the point of random assignment, except for the opportunity to enroll in Job Corps. Thus, simple differences in the distributions of outcomes between program and control group members are unbiased estimates of program impacts for eligible applicants.

Two important points about the interpretation of these impact estimates warrant discussion. First, as noted earlier, these impact estimates represent the effects of Job Corps relative to other employment and training programs in the community, and not relative to no training. Thus, the impact estimates represent the *incremental* effect of Job Corps relative to other programs in which control group members participated. Consequently, in order to interpret the impact estimates, it is crucial to examine the employment and training experiences of control group members to understand the “counterfactual” for the evaluation.

Second, the comparison of the outcomes of all program and control group members yields *combined* impact estimates for the 73 percent of program group members who enrolled in Job Corps centers and the 27 percent who did not. Policymakers, however, are more concerned with the effect of Job Corps on those who enrolled in a center and received Job Corps services. This analysis is complicated by the fact that the straightforward comparison of the outcomes of Job Corps participants in the program group and all control group members does not yield the desired impact for program participants. Ideally, we would like to compare the outcomes of program group participants with control group members who would have shown up at a center had they been in the program group. However, we cannot identify these control group members. Nevertheless, as discussed in these sections, we can overcome these complications if we assume that Job Corps has no impact on eligible applicants who do not enroll in centers.

In this section, we discuss our analytic approach for estimating impacts per eligible applicant and per Job Corps participant only, for the full sample and for key population subgroups. In addition, we discuss our approach for adjusting the impact estimates for the small number of control group members who enrolled in Job Corps. Finally, we discuss how the results are presented and interpreted.

1. Estimating Impacts per Eligible Applicant

We obtained the estimates of Job Corps impacts per eligible applicant by computing differences in average outcomes between all program and control group members (that is, using a differences-in-means approach). This approach yields unbiased estimates of the effect of Job Corps for program applicants who were determined to be eligible for the program. We used the associated t-tests (for variable means) and chi-squared tests (for distributions of categorical variables) to test the statistical significance of the impact estimates. We conducted the analysis using the 11,313 youths (6,828 program group members and 4,485 control group members) who completed 48-month interviews. We calculated all figures using sample weights to account for the sample and survey designs and for the effects of interview nonresponse, so that we could generalize the estimates to the intended study population. Standard errors of the estimates account for design effects due to unequal weighting of the data and to clustering caused by the selection of areas slated for in-person interviewing at baseline.⁶

We also estimated “regression-adjusted” impact estimates using multivariate models that control for other factors measured at baseline that affect the outcome measures. This approach increases the precision of the estimated program impacts and the power of significance tests relative to the

⁶The report containing methodological appendixes (Schochet 2001) describes the construction of sample weights and standard errors.

differences-in-means approach. In addition, the use of multivariate models can adjust for any random residual differences in the observable baseline characteristics of program and control group members.

Obtaining unbiased impact estimates using the regression approach, however, is computationally difficult because of the study's complex sample and survey designs, which generated a large number of strata (weighting cells). As discussed in more detail in Schochet (2001), the usual procedure of regressing outcomes on a program status indicator variable (which is 1 for program group members and 0 for control group members) and other explanatory variables can yield biased estimates of program impacts (that is, biased coefficient estimates on the program status indicator variable) because the estimates may be "weighted" incorrectly. Furthermore, estimating weighted regressions does not solve the problem (DuMouchel and Duncan 1983). To obtain unbiased impact estimates, separate regression-adjusted estimates must be obtained in each of the 48 weighting cells (many of which contain only a small number of sample members), and the weighted average of these 48 separate estimates must be calculated. Having small numbers of sample members in some weighting cells necessitates aggregating across weighting cells, which could introduce some bias if impacts differ across the weighting cells.

The results obtained using the differences-in-means approach and the regression approach are similar, and the same policy conclusions can be drawn from both sets of estimates (Schochet 2001). We present the differences-in-means estimates in this report for several reasons. The gains in precision from the regression approach are small for most outcome measures and subgroups. In addition, we can be sure that the differences-in-means estimates are unbiased (because sample weights can be used in this context to account for the sample design and interview nonresponse) and relatively precise (because the samples are large). Finally, few differences existed in the average

baseline characteristics of program and control group members, so controlling for these differences does not change the impact estimates materially.

We also present program and control group differences for some outcomes that are conditional on other outcomes. For example, we compared hourly wage rates and fringe benefits received on the most recent job for program and control group members who worked in months 46 to 48. As another example, we compared the financial support provided by program and control group members to their children who did not live with them. These estimates may not be unbiased estimates of program impacts, because they are based on potentially nonrandom subsets of program and control group members (that is, those who worked or were noncustodial parents). The baseline characteristics (both measured and unmeasured) of those in these subsets may have differed by research status because of potential program effects on the composition of youths in the subsets. However, these comparisons provide important insights into the differences between the outcomes of program and control group members.

2. Estimating Impacts per Job Corps Participant

Program impact estimates for program group members who enrolled in Job Corps--*participants*--were obtained by dividing the program impact estimates per eligible applicant by the proportion of program group members who enrolled (Bloom 1984). To illustrate how this works, we can express the impact of the Job Corps program per eligible applicant as a weighted average of the program impact for those eligible applicants who would enroll in Job Corps, given the chance, and the program impact for those eligible applicants who would not enroll, with weights p and $(1 - p)$, where p is the proportion of eligible applicants who enroll (73 percent).⁷ We do not know which

⁷In mathematical terms, $I_E = p * I_S + (1-p) * I_{NS}$, where I_E is the impact on eligibles, I_S is the impact on those who showed up at a center (that is, the difference between the average outcomes of program (continued...)

control group members would have enrolled if they had been assigned to the program group, or which control group members would not have enrolled. However, this information is not necessary if we assume that all impacts for the full program group were due to those who showed up at a center, and that *the impacts on no-shows are zero*. With this assumption, the impact per eligible applicant reduces to p times the impact per participant. Thus, the impact per participant can be computed by dividing the impact estimates based on *all* program and control group members by the proportion of program group members who actually enrolled in a center.⁸

The key assumption that makes this procedure work is that the program has no effect on no-shows. Although this assumption is reasonable, the offer of a Job Corps slot might affect the behavior of eligible applicants who do not enroll at a center. For example, after being determined eligible for Job Corps, no-shows might alter their job search behaviors because they have the option of enrolling. In particular, reservation wages might increase relative to what they would have been if a youth did not have the opportunity to enroll in Job Corps. Although it is unlikely that the offer of a Job Corps slot without active participation will have an appreciable effect on long-term outcome measures, it may have an effect on job search and employment in the short term. These issues are explored further in a separate report (Gritz et al. 2001).

⁷(...continued)

group participants and control group members who would have participated if given the chance), and I_{NS} is the impact on no-shows (that is, the difference between the average outcomes of program group no-shows and control group members who would have been no-shows if they were in the program group).

⁸The standard error of the impact estimate for participants was inflated to account for the estimation error in the show rate (Schochet 2001).

3. The Adjustment for Crossovers in the Control Group

About 1.4 percent of all control group members (and 1.2 percent of control group members in the 48-month sample) enrolled in Job Corps before their three-year restriction period ended. We refer to these youths as “early crossovers.” In addition, 3.2 percent of control group members in the 48-month sample enrolled in Job Corps between three and four years after random assignment (that is, after their restriction period ended). We refer to these youths as “late crossovers.” Crossovers were treated as control group members in the analysis to preserve the integrity of the random assignment design. Thus, impact estimates that do not account for these crossovers could be biased. Next, we discuss our approach for adjusting the impact estimates for early and late crossovers.

a. The Adjustment for Early Crossovers

A small number of control group members enrolled in Job Corps before their three-year embargo period ended. As described in the report on study implementation (Burghardt et al. 1999), the Job Corps national office allowed most of these youths to remain at centers, but held OA and center staff accountable for these errors. The average duration of stay in Job Corps for these youths (7.6 months) was very similar to the average duration of stay for program group enrollees (8.0 months). Thus, impact estimates on employment and earnings in the postprogram period that do not adjust for these crossovers could be slightly biased downwards if these crossovers benefited from participation in Job Corps.

The procedure to estimate impacts per participant can be extended to accommodate early control group crossovers (Angrist et al. 1996). As described in Schochet (2001), the modified procedure involves dividing the estimated impacts per eligible applicant by the difference between the Job Corps enrollment rate for the program group (73 percent) and the early crossover rate for the control group (1.2 percent). These impacts pertain to eligible applicants who would enroll in Job Corps if

they were assigned to the program group, but who would not enroll if they were instead assigned to the control group. Thus, the impacts pertain to a subset of all participants.⁹ However, because the crossover rate is very small, the adjustment procedure has very little effect on the estimates.

b. The Adjustment for Late Crossovers

Control group members were allowed to enroll in Job Corps after their three-year restriction period ended. About 3.2 percent of control group members enrolled in the program between their third and fourth years after random assignment. The enrollment rate was 4.6 percent for those 16 and 17 at application to Job Corps, 2.7 percent for those 18 and 19, and 1.1 percent for those 20 to 24. About 55 percent of these late crossovers were enrolled in Job Corps during the last quarter of the four-year period.

The approach to accommodate the *early* crossovers cannot be used to accommodate the *late* crossovers. The adjustment procedure for *early* crossovers assumes that the average outcomes of early crossovers in the control group were the same as those in the program group who would have been early crossovers had they instead been assigned to the control group (whom we label “would-be” early crossovers). This assumption is reasonable, because most early crossovers in the control group enrolled in Job Corps soon after random assignment and thus were in Job Corps at roughly the same time as the would-be early crossovers in the program group. Thus, it is likely that average earnings during the postprogram period were similar for the two groups.

The *late* crossovers, however, enrolled in Job Corps more than three years after random assignment, whereas nearly all program group participants enrolled within one year. Thus, we cannot assume that the average outcomes of late crossovers in the control group were similar to those

⁹In the literature, these impacts are referred to as impacts per “complier.” However, we sacrifice technical accuracy for clarity and refer to them as impacts per participant.

of would-be late crossovers in the program group. Instead, average earnings late in the observation period were probably much lower for the late control group crossovers than for their program group counterparts, because more than half these control group members were enrolled in Job Corps during this period, and those who had left Job Corps had been out for only a short period. Consequently, impact estimates on postprogram employment and earnings that do not adjust for these late control group crossovers would probably be biased slightly upwards.

Our procedure to adjust for the late control group crossovers was to “assume” that these crossovers never enrolled in Job Corps, and to impute their employment and education outcomes covering the last five quarters of the 48-month period. We conducted the imputation procedure in two stages. In the first stage, we identified noncrossovers in the control group whose average demographic characteristics and employment and education experiences during the first two years after random assignment were similar to those of the late crossovers.¹⁰ Second, we imputed the employment and education outcomes of late crossovers using the average outcomes of noncrossovers in the matched sample (by age and gender).¹¹

4. Subgroup Analysis

Program impact estimates for the full sample may conceal important differences in impacts across subgroups of program participants. If impacts do exist overall, they might be heavily concentrated in or much larger for some subgroups. Conversely, if impacts do not exist overall, they

¹⁰We used propensity score procedures to select the matched sample. The probability that a control group member was a late crossover was regressed on a set of explanatory variables, and a predicted probability (propensity score) was calculated for each control group member. We then selected the matched sample of noncrossovers as those with the closest propensity scores to those of the crossovers.

¹¹We did not impute other outcomes (such as crime, welfare, and family formation measures) for the late crossovers.

might exist for some subgroups. If a subgroup is small, the impact on it might not be large enough to yield a statistically significant difference in the overall sample.

This report addresses two important questions about impacts for subgroups:

1. Is Job Corps more effective for some groups of youths defined by personal characteristics or experiences before program application than for other groups?
2. Are the residential and nonresidential components effective for the students they serve?

a. Subgroups Defined by Youth Characteristics

It is important to identify groups of Job Corps students who benefit from program participation, so that policymakers can improve program services and target them appropriately. In consultation with the study advisory panel (which included representatives of Job Corps), we identified groups of students whose backgrounds, training needs, and program experiences typically differ in important ways. The selected groups often enroll in different types of centers and program components, and they experience a different mix of vocational skills and academic classroom training while enrolled.

Using baseline interview data, we estimated program impacts on seven sets of subgroups defined by youth characteristics at random assignment:¹²

1. **Gender.** The training needs and the barriers to successful employment of young women who enroll in Job Corps are different from those of young men who enroll. As discussed in Chapter II, the average characteristics of female students differ from those of male students (for example, female students tend to be older, to have completed high school, and to have children). In addition, female students are more likely to be nonresidential students and are less likely to be in CCCs. Thus, in light of the different programmatic needs and program experiences of males and females, an important policy issue is the extent to which Job Corps is effectively serving each of these groups.
2. **Age at Application to Job Corps.** The broad age range Job Corps serves means that the program must serve adolescents and young adults together. This poses a significant challenge for the program, because the training needs and backgrounds of younger

¹²Appendix Table A.1 displays sample sizes for the subgroups.

students differ from those of older students. For example, younger students tend to have lower education levels (and thus are much more likely to require education services in Job Corps), less work experience, and fewer children. In addition, younger students exhibit some characteristics (for example, higher arrest rates and incidence of drug use) that suggest that they may be more disadvantaged than older applicants. Moreover, findings from the process analysis reveal widespread concern among Job Corps staff that the younger students are often disruptive and harder to serve than the older students. Thus, an important policy objective is to assess whether Job Corps participation improves the outcomes of these relatively diverse groups. Separate impact estimates are presented for those (1) 16 and 17 years old, (2) 18 and 19 years old, and (3) 20 to 24 years old.¹³

3. ***Educational Attainment.*** Approximately 8 out of 10 Job Corps students lack a GED or high school diploma at the time of entry. Most students without a high school credential begin their Job Corps program with a balanced schedule of one-half academic course work and one-half vocational course work. These students do not normally focus on their vocational trades until they receive their GEDs; hence, most receive intensive academic education while in the program. On the other hand, students with a high school credential usually complete their academic requirements quickly and move toward a full-time vocational schedule. In light of the differences in the mix of vocational and academic classroom experiences in Job Corps and in the characteristics of those with and without a high school credential, we present separate impact estimates for each group.
4. ***Presence of Children for Females.*** The barriers to successful employment for female Job Corps enrollees with children are particularly acute. At application to Job Corps, females with children (who represent about 30 percent of all female students) are highly dependent on public assistance (for example, about 70 percent of these mothers received AFDC/TANF benefits or were part of families that received these benefits in the previous year) and have lower earnings and employment rates than other students. Furthermore, these young mothers are much less likely to live with other adults than other students, suggesting that many lack adequate support systems. Many have problems establishing suitable child care arrangements. Consequently, an important policy issue is the extent to which Job Corps can increase employment and earnings and reduce the chances that these youth become reliant on public assistance.

In addition, a large percentage of females with children are in the nonresidential component. For example, nearly 65 percent of females with children in our sample were designated for nonresidential slots, and nearly half of all nonresidential designees were females with children. Thus, policy concerns about the effectiveness of the nonresidential program and increasing the recruitment of young females are linked to

¹³The age categories were defined in this way because the factors associated with enrolling in a center and graduating from the program were similar for program group members within each group (Johnson et al. 2000).

the effectiveness of Job Corps in serving females with children. Thus, separate impact estimates are presented for females with and without children.

5. ***Arrest History.*** To be eligible for Job Corps, applicants must be free of behavioral problems that would prevent them from adjusting to the Job Corps standards of conduct. Job Corps seeks to offer youths who may have been in trouble with the law the opportunity to turn their lives around. On the other hand, an applicant cannot currently be under the control of the criminal or juvenile justice system. Furthermore, the program is not equipped to handle youths who pose a threat of violence to themselves or others. Thus, youths with prior involvement with the criminal justice system are carefully screened by the OA agency and sometimes by the regional office.¹⁴

The baseline data indicate that over one-quarter of eligible applicants were ever arrested or charged with a delinquency or criminal complaint, and that about five percent were charged with serious crimes, such as aggravated assault, murder, robbery, or burglary. Consequently, an important policy question is the extent to which Job Corps can effectively serve those with previous problems with the law, especially under the new strict ZT policies. In the analysis, we obtained separate impact estimates for those who were (1) never arrested, (2) ever arrested for nonserious crimes only, and (3) ever arrested for serious crimes.

6. ***Race and Ethnicity.*** The backgrounds of Job Corps students differ markedly by race and ethnicity. Whites are more likely than other groups to be male (67 percent, compared to about 56 percent for other groups). Whites tend to have had more work experience, even though the age distribution is similar by race and ethnicity. In addition, whites are less likely to have children, to have received public assistance in the prior year, or to be high school dropouts.

Program experiences are also likely to differ by race and ethnicity. There are large differences in the racial and ethnic composition across regions (and across centers within regions), and Job Corps operations differ somewhat across regions. For example, about 60 percent of eligible applicants in Regions 2, 3, 4, and 5 are African American, whereas most youths in Regions 1, 7/8, and 10 are white. More than one-third of youths are Hispanic in Regions 2, 6, and 9. Furthermore, whites are much more likely to be in CCC slots and much less likely to be in the nonresidential component. Thus, differences in background characteristics and program experiences by race and ethnicity could lead to differences in program impacts across these groups. Four subgroups defined by race and ethnicity were used in the analysis: (1) white, non-Hispanic; (2) African American, non-Hispanic; (3) Hispanic; and (4) other (including American Indian, Alaskan Native, Asian, and Pacific Islander).¹⁵

¹⁴Findings from the process analysis indicate that nearly all OA counselors (accounting for 96 percent of applicants) require local criminal justice records of all applicants.

¹⁵Sample sizes for American Indians, Alaskan Natives, Asians, and Pacific Islanders were too
(continued...)

7. *Job Corps Application Date and the New Job Corps Policies.* As discussed, in response to congressional concerns about the operation of the Job Corps program, new ZT policies were instituted in March 1995--during the sample intake period for the study. The process analysis found that the new policies had a profound positive effect on behavior management and the general climate at centers.¹⁶ Thus, to assess the extent to which the new policies had an effect on program impacts, we present separate impact estimates for those who applied to Job Corps before and after March 1, 1995.¹⁷ Because the ZT policies are still in effect, the post-ZT estimates are more likely to be representative of the current Job Corps program.

We also estimated program impacts for finer subgroups formed by combining groups across these seven categories. This analysis was conducted to help disentangle the subgroup findings, because many of the subgroups are correlated with each other. For example, nearly all those 16 and 17 years old did not have a high school credential at random assignment, compared to 50 percent of those 20 or older. Thus, impact estimates for those without a high school credential are heavily weighted by the outcomes of the younger sample members. Consequently, we obtained separate impact estimates for the younger dropouts and the older dropouts to better understand the extent to which Job Corps helps those with low levels of education.

This finer subgroup analysis was often limited by small sample sizes, which sometimes led to unstable results. However, the analysis provided important insights about the pattern of program effects across key subgroups.

¹⁵(...continued)
small to support separate impact estimates for these groups.

¹⁶The policies, however, did not appear to have a significant effect on the characteristics of eligible applicants (Schochet 1998a).

¹⁷Program group members in the pre-ZT group who were in Job Corps after March 1, 1995, were subject to the new rules. Thus, impact estimates pertaining to the pre-ZT period are somewhat contaminated. Furthermore, program experiences could differ by season, and because of the limited sample intake period, the data are not available to compare impacts for those in pre-ZT and post-ZT groups who were recruited during the same time of year. Thus, differences in the pre-ZT and post-ZT impact estimates are only suggestive of the effects of the new policies.

We view the subgroups defined by age, gender, and the presence of children (for females) as particularly important (along with the results for residents and nonresidents). Thus, in the report, we usually emphasize impact findings for these subgroups more heavily than for other subgroups. However, the emphasis we place on various subgroups varies somewhat, depending on the outcome measure and our hypotheses about the extent and nature of program impacts. For example, when examining impacts on education and training outcomes, we emphasize subgroups defined by age and high school credential status at baseline, because of differences in the educational needs and the expected academic classroom and vocational training experiences of both program and control group members across these subgroups. Similarly, we focus on subgroups defined by gender and the presence of children (but not age) when examining impacts on the receipt of public assistance benefits, because of large differences in the types and amounts of assistance that these gender groups typically receive. As a final example, we focus on age and gender subgroups when examining impacts on crime-related outcomes, because of subgroup differences in the level of involvement with the criminal justice system, but we do not focus on the results for females with and without children, because we had no reason to believe that crime-related impacts would differ for these two groups of females.

Estimation Issues. The random assignment design ensures that unbiased impact estimates for a subgroup defined by a youth characteristic can be obtained by comparing the distribution of outcomes of program and control group members in that subgroup. Thus, for example, impact estimates for males were obtained by comparing the outcomes of male program and control group members. Similarly, impacts estimates for those without a high school credential were computed by comparing the outcomes of program and control group members without a high school credential at random assignment.

Standard statistical tests were used to gauge the statistical significance of the subgroup impact estimates. In addition, we conducted statistical tests to determine whether program impacts were similar across levels of a subgroup. For example, we tested the hypothesis that program effects were similar for males and females and across the three age groups.

b. Impacts for Residents and Nonresidents

Residential living is the component that distinguishes Job Corps from other publicly funded employment and training programs. During our site visits to centers as part of the process analysis, staff stressed the importance of the residential component as central to helping students become more employable. Some staff believe that it is even more important than vocational training for improving the long-term outcomes of students. However, staff also stressed that the nonresidential component is important because it serves a type of student different from those in the residential component, and because nonresidents, who have outside commitments to families or children, might not enroll in Job Corps if a nonresidential option were not available.¹⁸ About 12 percent of enrollees in the study program group were nonresidents.

The process analysis found that nonresidential students are fully integrated into the academic and vocational components of Job Corps, and receive comprehensive and intensive services. However, the participation of many nonresidential students in other activities is limited, often because of family responsibilities. For example, nonresidential students are less involved in dormitory life, student government, and recreational activities. Thus, nonresidential students have a program experience that may differ from that of students who live on center.

¹⁸Most centers have some nonresidential slots, and about 25 percent of centers have at least 20 percent of their slots reserved for nonresidential students.

The estimation of separate impacts for those in the residential and nonresidential components is of considerable policy interest for two reasons. First, as discussed, the residential and nonresidential components serve students with different characteristics and needs, and program experiences may differ by residential status. Second, previous studies (for example, the JTPA and JOBSTART evaluations) have found that disadvantaged youths do not benefit significantly from participation in training programs that offer basic education and job-training services in a nonresidential setting. Thus, there is great interest in measuring impacts of Job Corps on nonresidential students, to help guide design decisions not only about Job Corps, but also about other programs to support youths' labor market participation.

However, the Job Corps nonresidential component is very different from most other nonresidential training programs. As discussed, nonresidential students in Job Corps receive services that are similar in many ways to those received by residential students. In fact, the program cost per nonresidential student is only about 12.5 percent less than the program cost per residential student (McConnell et al. 2001). Thus, the nonresidential Job Corps program is more intensive and comprehensive, and hence, more expensive, than most other nonresidential programs. Furthermore, unlike most other nonresidential programs, nonresidential and residential students in Job Corps train together, because most centers with nonresidential slots also have residential slots. Thus, nonresidential Job Corps students may benefit from their contact with residential students. These qualifications suggest that we must proceed with caution when comparing impact results for nonresidential students in Job Corps and in other programs.

Estimation Issues. We estimated the impacts of the residential and nonresidential components using data on OA counselor predictions as to whether sample members would be assigned to a residential or a nonresidential slot. As part of the application process, OA counselors filled in this

information on a special form (an ETA-652 Supplement form) developed for the study. OA staff sent these forms to MPR for those youths determined to be eligible for the program, and MPR entered the information into the study's database.

The anticipated residential status information is available for both program *and* control group members because it was collected prior to random assignment. Thus, we estimated the impacts of the residential component by comparing the distribution of outcomes of program group members designated for a residential slot with those of control group members designated for a residential slot. Similarly, we estimated the impacts of the nonresidential component by comparing the experiences of program and control group members designated for nonresidential slots. We used standard statistical tests to gauge the statistical significance of these impact estimates.

We believe that the analysis produced reliable estimates of program impacts for the residential and nonresidential components, because the anticipated residential status information is available for all sample members and matches actual residential status very closely. Because it was a key data item required for random assignment, the anticipated residential status information is available for all sample members. If the information was missing, MPR contacted OA staff and did not perform random assignment until it was provided.

OA counselor projections of residential status proved to be very accurate (Schochet 1998b). Using SPAMIS information on program group members who enrolled in centers, we found that about 98 percent of program group enrollees designated for residential slots actually enrolled in them and about 88 percent of program group enrollees designated for nonresidential slots actually enrolled in those.¹⁹ Moreover, the accuracy of the predictions was high across all key subgroups. Thus, the

¹⁹In addition, a large proportion of program group members who enrolled in a particular component were designated for that component. For example, more than 98 percent of all enrollees in residential slots were designated for these slots, and about 84 percent of those in nonresidential slots were designated for those slots.

experiences of those designated for residential (nonresidential) slots were largely representative of the experiences of actual residents (nonresidents), and vice versa.²⁰

An important (yet subtle) point about the interpretation of the impact findings for residents is that they tell us about the effectiveness of the residential component *for youths who are typically assigned to residential slots* (because the results were obtained by comparing the outcomes of program and control group members who were suitable for the residential component). Similarly, the impact estimates for nonresidents tell us about the effectiveness of the nonresidential component *for youths who are typically assigned to nonresidential slots*. The results cannot necessarily be used to measure the effectiveness of each component for the *average* Job Corps student.²¹ Nor can the results be used to assess how a youth in one component would fare in the other one.

These important qualifications can be understood further by noting that the characteristics of residential and nonresidential designees differ in important ways (see Table III.3, which presents key

²⁰We attempted to improve the accuracy of the “predictions” by using multivariate techniques. We estimated logit models where the probability that a program group enrollee was assigned to the residential component was regressed on the predicted assignment measure and other explanatory variables created using baseline interview data. We then used the parameter estimates from these models to create predicted probabilities for *all* control group and program members. The sample was then split into those likely to be residents (those with high predicted probabilities) and those likely to be nonresidents (those with low predicted probabilities). We then conducted the analysis using these groups. The models did not increase the accuracy of the predictions appreciably, and the results using the multivariate procedure were similar to those obtained with the anticipated assignment information only.

²¹To address this question effectively, we would have had to randomly assign each youth in the study population to the residential or nonresidential component. We rejected this design option because it would have introduced an unacceptable degree of intrusion into normal program operations.

TABLE III.3

BASELINE CHARACTERISTICS OF RESIDENTIAL AND NONRESIDENTIAL
DESIGNEES IN AREAS WITH A LARGE CONCENTRATION OF
NONRESIDENTIAL STUDENTS, BY GENDER
(Percentages)

Baseline Characteristic	Females		Males	
	Residential Designees	Nonresidential Designees	Residential Designees	Nonresidential Designees
Age at Application				
16 to 17	50.7	24.4	48.3	31.4
18 to 19	28.7	32.3	26.9	35.4
20 to 24	20.7	43.3	24.7	33.2
Had Children	16.5	64.7	9.8	18.7
Race/Ethnicity				
White, non-Hispanic	12.1	9.6	15.9	15.5
Black, non-Hispanic	60.6	68.7	59.5	55.1
Hispanic	23.6	17.5	19.3	20.9
Other	4.3	4.2	5.3	8.5
Had a High School Diploma or GED	21.3	34.0	17.1	24.5
Received Welfare in the Past Year ^a	67.7	78.4	56.2	60.6
Had a Job in the Past Year	62.0	52.8	59.5	63.9
Was Ever Arrested	15.6	12.3	30.3	26.8
Sample Size	873	1,312	1,357	445

SOURCE: Baseline interview data and Supplemental ETA-652 data for those who completed 48-month interviews.

NOTE: Figures pertain to those who lived in one of the 57 areas sending the largest number of nonresidential students to Job Corps. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

^aWelfare receipt includes AFDC/TANF, food stamps, or other public assistance.

baseline characteristics by residential designation status and gender in areas with large concentrations of nonresidential students). For both males and females, nonresidential designees are much more likely than residential designees to be older, to have children, and to have a high school credential, and are less likely to have ever been arrested. Thus, the residential and nonresidential program components serve very different students, and our design can address only the extent to which each component effectively serves students suited for it.

Our analysis findings suggest that there are some differences in the impact estimates for residents and nonresidents by gender and, for females, by the presence of children. Thus, we focus on these finer subgroup results in the report.

5. Presentation of Results

We present analysis findings using a series of figures, charts, and tables. The tables (which form the basis for the figures and charts) display the following seven pieces of information for each outcome measure:

1. ***The Control Group Mean for Eligible Applicants.*** This figure was calculated using the entire control group and represents the mean outcome of program group members if they had not been offered a Job Corps slot.
2. ***The Program Group Mean for Eligible Applicants.*** We calculated this mean using the full program group (participants and no-shows).
3. ***The Impact Estimate per Eligible Applicant.*** This estimate is the difference between the mean outcomes for program and control group members.
4. ***The Mean for Program Group Members Who Participated in Job Corps.*** This mean was used to examine the outcomes of program group members who enrolled in Job Corps (and who would not have enrolled in Job Corps if they had instead been assigned to the control group).²²

²²The qualification in parentheses results from our approach for adjusting the impacts to account for the small number of early crossovers in the control group, as discussed earlier in this section.

(continued...)

5. ***The Impact Estimate per Program Participant.*** This estimate is the impact estimate per eligible applicant divided by the difference between the program group participation rate in Job Corps (73 percent) and the control group early crossover rate (1.2 percent). The participation and crossover rates differed somewhat across subgroups.
6. ***The Percentage Gain Due to Participation in Job Corps.*** This estimate represents the percentage change in the mean outcome for participants relative to what it would have been if the participants had not enrolled in Job Corps. The figure is estimated by dividing the impact estimate per program participant by an estimate of the mean for control group members who would have enrolled in Job Corps if they had instead been assigned to the program group (and who were not crossovers). This control group mean was estimated as the difference between the mean for program group participants and the impact estimate per participant.
7. ***An Indication of the Statistical Significance of the Impact Estimates.*** Two-tailed statistical tests were performed to test the null hypothesis of no program impact. We indicate whether the null hypothesis was rejected (that is, whether the impact is statistically significant) at the 1 percent, 5 percent, or 10 percent level. Standard errors used in these test statistics were adjusted for design effects due to unequal weighting and clustering of the in-person sample at baseline. The standard errors of the estimated impacts per participant were also inflated to account for the estimation error in the Job Corps enrollment rate. For the subgroup analysis, we also indicate whether differences in impacts across subgroups are statistically significant.

Policymakers are likely to be more interested in the effects of Job Corps for program participants than for eligible applicants. However, we present findings for eligible applicants in addition to those for program participants, for two main reasons. First, random assignment was performed at the point that applicants were determined to be eligible for the program; hence, the average characteristics of eligible applicants in the program and control groups were equivalent at random assignment. Therefore, impact estimates per eligible applicant are pure experimental estimates. Impacts per participant, however, were obtained from the impact estimates per eligible applicant under the assumption that the program has no effect on no-shows. While this assumption

²²(...continued)

Schochet (2001) discusses how this unobserved mean for program group compliers was computed using observed sample means.

is reasonable, it is difficult to test. Thus, we cannot place as much confidence in these estimates as we can in the impact estimates per eligible applicant.

Second, an important objective of the analysis is to understand the counterfactual for the study by examining the experiences of control group members. When we use the entire control group, this analysis is straightforward, because we can observe their outcomes. Furthermore, we can be confident that these outcomes represent the true counterfactual for the full program group. This analysis is more complicated, however, if we focus on program participants only, because we cannot directly observe the outcomes of those in the control group who would have enrolled in Job Corps had they been given the chance. The average outcomes of these control group members can be estimated as the difference between the average outcomes of program group members who enrolled in Job Corps and the impact estimates per participant. However, these estimated control group means are based on assumptions about the effects of the program on no-shows. Thus, we cannot be sure that they represent the true outcomes of program group enrollees if they had not participated in Job Corps. Consequently, we use the entire control group of eligible applicants to describe the counterfactual for the evaluation, given the importance of this analysis.

6. Interpretation of Estimates

The impact analysis generated impact estimates on a large number of outcome measures and for many subgroups. We conducted formal statistical tests to determine whether program and control group differences existed for each outcome measure. However, an important challenge for the evaluation is to interpret the large number of impact estimates to assess whether Job Corps makes a difference and for whom it works.

The initial guide we use to determine whether Job Corps has an impact on a particular outcome measure is the p-value associated with the t-statistic or chi-squared statistic for the null hypothesis

of no program impact on that outcome measure. However, we need more stringent criteria than the p-values to identify “true” program impacts, because we are likely to produce significant test statistics by chance (even when impacts may not exist) as a result of the large number of outcomes and subgroups under investigation. For example, in tests of program and control group differences for statistical significance at the 5 percent level, 1 out of 20 independent tests will be significant when in fact no real difference exists.

We also apply three additional criteria to identify potential program impacts:

1. We examine the magnitude of the significant impact estimates to determine whether the differences are large enough to be policy relevant. This is important, as small impacts might be statistically significant because of large sample sizes. For example, for a control group mean of 50 percent, an impact is statistically significant if it is about 2 percentage points or less.
2. We categorize outcomes and subgroups, and look for patterns of significant impacts within and across the categories at each follow-up point and over time. That is, we check that the sign and magnitude of the impact estimates are similar for related outcome measures and subgroups.
3. We determine whether the sign and magnitude of the impact estimates are robust to alternative model specifications and estimation techniques. For example, we conduct sensitivity tests by removing outlier observations, employ different weighting schemes, and estimate impacts using the differences-in-means and regression approaches.

Finally, it is important to recognize that the impact estimates represent the effects of Job Corps for eligible applicants who applied to the program between November 1994 and December 1995. Since most program group members who enrolled in Job Corps were in centers in 1995 and 1996, the estimates may not be representative of the effectiveness of the program as it operates today.