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**National Job Corps  
Study: Findings Using  
Administrative Earnings  
Records Data**

*Final Report*

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## ABSTRACT OF FINDINGS

Job Corps stands out as the nation's largest, most comprehensive education and job training program for disadvantaged youths. It serves disadvantaged youths between the ages of 16 and 24, primarily in a residential setting. It provides comprehensive services—basic education, vocational skills training, health care and education, counseling, and residential support. Each year, Job Corps serves more than 60,000 new participants in about 120 centers nationwide, at a cost of about \$1.5 billion.

The National Job Corps Study has been conducted since 1993 under contract with the U.S. Department of Labor (DOL). It is intended to provide Congress and program managers with the information they need to assess how well Job Corps attains its goal of helping students become more responsible, employable, and productive citizens.

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 (who could enroll in Job Corps) or a control group (who could not). Random assignment took place between late 1994 and early 1996. In previous reports, we presented impact and benefit-cost estimates by comparing the experiences—and in particular, the earnings—of the program and control groups using data from follow-up *interviews* conducted during the four years after random assignment.

This report presents findings from an analysis of *administrative* earnings records. These data allow us to address two questions: (1) Do survey and administrative earnings data yield similar impact estimates on earnings during the periods covered by both data sources? and (2) What are estimated impacts on earnings in the two and a half years beyond the four-year period covered by the survey? Two sources of administrative data were collected for the study: (1) annual social security earnings (SER) data reported by employers to the Internal Revenue Service (IRS), and (2) quarterly wage records reported by employers to state unemployment insurance (UI) agencies in 22 randomly selected states. The SER and UI data cover nearly all workers in formal jobs. Our findings using these data are summarized below.

***The pattern of the estimated impacts using the survey and administrative data are similar in periods covered by both data sources.*** According to both the survey and administrative records data, the estimated earnings impacts are negative in the first and second years after random assignment (when the program group was enrolled in Job Corps) and positive and statistically significant in the third and fourth years after random assignment.

***However, the survey-based impact estimates are larger and more often statistically significant.*** Two factors account for the larger survey-based impacts. First, reported earnings *levels* are much higher according to the survey data, due in part to social security numbers that may have been incorrectly reported by employers or sample members, the noncoverage of informal and some formal jobs in the administrative records data, and the likely overreporting of hours worked in the survey data. Second, according to the administrative records data, earnings impacts are larger for survey respondents than nonrespondents, suggesting that the survey-based impact estimates are slightly biased upward.

***Based on the administrative data, we find no impacts of Job Corps for the full sample on employment or earnings after the four-year period covered by the survey.*** The estimated impacts in years 5 to 7 after random assignment are all near zero, and none are statistically significant.

***The earnings impacts for 20- to 24-year-olds at program application appear to have persisted.*** We find no beneficial earnings impacts in the post-survey period that are statistically significant for any subgroup. However, positive earnings gains for those 20 to 24 and those with a high school credential at program application persisted with little decay.

***The revised benefit-cost estimates suggest that the benefits to society of Job Corps are smaller than the substantial program costs.*** For the initial benefit-cost analysis based on the survey data only, we assumed that the survey-based impact estimates found in the fourth year after random assignment would persist without decline. This assumption generates program benefits that exceed program costs from society's perspective. The administrative-based impact findings, however, do not support the assumption that the earnings impacts will persist without decline or with only modest decline after the survey observation period. The revised benefit-cost analysis, which assumed a higher decay rate in the earnings impacts, produced substantially smaller estimates of program benefits. These estimated benefits to society are lower than program costs. The revised benefit-cost analysis, however, finds that Job Corps benefits exceed costs from the perspective of *participants*, suggesting that the program has important distributional effects.

***Job Corps, however, may be cost-effective for the 20- to 24-year-olds at program application whose earnings impacts persisted during the post-survey period.*** The findings for the older youth can help guide future program improvement. Job Corps appears to have a longer-term beneficial effect on the earnings of older students than younger ones (who had temporary earnings gains only). Older students remain in Job Corps longer than younger ones, receive more hours of vocational training while enrolled, and are more highly motivated and well-behaved (as reported by program staff). Furthermore, many of the youngest sample members in the control group returned to high school after being rejected from Job Corps, whereas fewer older control group members enrolled in alternative education and training programs. These findings suggest that to improve overall program effectiveness, Job Corps needs to fully address differences by age in program structure and experience, and perhaps, to reassess the target population served by the program.

***Only further long-term followup would eliminate all uncertainties about the effectiveness of the program.*** Intensive, costly programs like Job Corps can only be expected to show benefits that exceed costs over a relatively long time horizon. Unfortunately, the foundation of empirical evidence to make long-term extrapolations of the profile of earnings in response to programs such as Job Corps simply does not exist. We have observed impacts for only seven years after program application. Consequently, we cannot discount the possibility that positive earnings impacts might re-emerge. Job Corps may increase the long-term earnings of those who were 16 to 19 years old at program application as they mature, find stable jobs, and experience the full benefits of program participation, both from increased vocational and academic skills gained in the program, as well as from improved social skills and attitudes toward work. The persistent earnings impacts among students who were 20 to 24 at program application raise this as a possibility. Only continued followup of the study sample can answer this question.



## EXECUTIVE SUMMARY

Job Corps stands out as the nation's largest, most comprehensive education and job training program for disadvantaged youths. It serves disadvantaged youths between the ages of 16 and 24, primarily in a residential setting. It provides comprehensive services—basic education, vocational skills training, health care and education, counseling, and residential support. Each year, Job Corps serves more than 60,000 new participants in about 120 centers nationwide, at a cost of about \$1.5 billion.

The National Job Corps Study has been conducted since 1993 under contract with the U.S. Department of Labor (DOL). It is intended to provide Congress and program managers with the information they need to assess how well Job Corps attains its goal of helping students become more responsible, employable, and productive citizens.

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 (who could enroll in Job Corps) or a control group (who could not). The research sample consists of approximately 9,400 program group members and 6,000 control group members randomly selected from among nearly 81,000 applicants nationwide. Random assignment took place between late 1994 and early 1996. The survey data for the evaluation come from interviews conducted at baseline (shortly after random assignment), and at 12, 30, and 48 months after random assignment. The response rate to the 48-month interview was about 80 percent (81 percent for the program group and 78 percent for the control group). Program impacts were estimated by comparing the mean outcomes of program and control group members.

The survey data indicate that Job Corps generated positive impacts on earnings—the key outcome for the study—beginning in the third year after random assignment, and the impacts persisted without decline through the end of the four-year follow-up period. Beneficial program impacts were found broadly across youth subgroups. A benefit-cost analysis based on impact estimates from the survey data found that the benefits to society from the program exceed its costs. However, this finding requires a key assumption—that the earnings gains observed during the last year of the observation period will persist with little decay.

This report presents findings from an analysis of *administrative* earnings records. These data allow us to address two questions:

1. *Do survey and administrative earnings data yield similar impact estimates on employment and earnings during the periods covered by both data sources?*
2. *What are estimated impacts on earnings and employment in the two and a half years beyond the four-year period covered by the survey?*

Two sources of administrative data were collected for the study: (1) annual social security earnings (SER) data reported by employers to the Internal Revenue Service (IRS) and Social Security Administration (SSA), and (2) quarterly wage records reported by employers to state

unemployment insurance (UI) agencies in 22 randomly selected states. The SER data cover calendar years 1993 to 2001. The years 1995 to 1998 pertain roughly to the four-year period covered by the survey, and the years 1999 to 2001 pertain to the post-survey period (that is, years 5, 6, and 7 after random assignment). The UI data cover the 1999 to 2001 period only. The SER and UI data cover nearly all workers in formal jobs. Earnings from informal jobs are not covered.

## **IMPACT FINDINGS DURING THE PERIOD COVERED BY THE SURVEY**

**The pattern of the estimated impacts using the survey and administrative data are similar in periods covered by both data sources.** According to both the survey and SER data, the estimated earnings impacts are negative in 1995 and 1996 (when the program group was enrolled in Job Corps) and positive and statistically significant in 1997 and 1998 (Table 1).

**However, the survey-based impact estimates are larger and more often statistically significant.** Reported earnings *levels* are much higher according to the survey data for a large percentage of sample members (Tables 1 and 2). We find larger differences between the earnings levels reported in the survey and administrative data than were found in previous studies using similar populations. One possible explanation for this finding is that the National Job Corps Study was conducted during a period of strong economic growth, which may have increased the earnings sample members received from informal jobs.

**Annual employment rates are similar using the survey and administrative data, but quarterly employment rates are much higher using the survey data.** The annual employment rate in 1998 is about 80 percent according to both the survey and SER data (Table 1). However, the quarterly employment rates in quarters 15 and 16 after random assignment are substantially higher using the survey than UI data (Table 2).

**Differences in the 1998 earnings gains using the survey and SER data are due in roughly equal parts to reporting differences between the two data sources and to nonresponse bias.** The estimated 1998 earnings gain is 10.4 percent according to the survey data and 3.9 percent according to the SER data (Table 1). Using the *sample of respondents to the 48-month interview only*, the SER-based earnings gain increases from 3.9 to 6.9 percent (Table 3), which is still smaller than the 10.4 percent survey-based figure. Thus, the residual is due to reporting differences between the two data sources that are slightly *greater* for the program than control group. We estimate that about 46 percent of the difference between the 1998 earnings gains using the survey and SER data is due to interview nonresponse bias, and 54 percent is due to reporting differences between the two data sources.

## **EXAMINING REPORTING DIFFERENCES BETWEEN THE SURVEY AND ADMINISTRATIVE DATA**

We have seen that the pattern of impact findings using the administrative and survey data is similar in periods covered by both data sources. However, the estimated impacts are larger using the survey data. This is due primarily to reported earnings *levels* that are substantially higher according to the survey than administrative data for most sample members.

TABLE 1  
 IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES  
 FOR THE FULL SAMPLE, BY DATA SOURCE

Outcome Measure	Data Source											
	Survey Data			Annual Social Security Earnings Records			Quarterly UI Earnings Records from 22 States			Estimated Impact <sup>a</sup>		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
Average Calendar Year Earnings (in 1995 Dollars)												
1993				1,009.6	1,013.9	-4.3						
1994				1,590.3	1,542.4	47.9						
1995				1,758.4	2,026.5	-268.1***						
1996	5,144.8	5,728.8	-584.0***	3,096.7	3,273.5	-176.8***						
1997	8,110.5	7,818.6	291.9*	4,540.1	4,368.4	171.8**						
1998	10,295.6	9,324.1	971.6***	5,803.9	5,584.1	219.8**						
1999				6,652.9	6,619.9	32.9				5,685.9	5,659.8	26.0
2000				7,526.7	7,544.1	-17.4				6,311.6	6,505.8	-194.3
2001				7,678.7	7,671.7	7.0				7,260.0	7,394.6	-134.6
Percentage Employed in Calendar Year												
1993				42.9	43.0	-0.1						
1994				59.5	58.8	0.7						
1995				89.2 <sup>b</sup>	73.3	15.9***						
1996	70.4	74.5	-4.2***	88.7 <sup>b</sup>	78.3	10.4***						
1997	77.7	76.9	0.8	83.5	81.4	2.1***						
1998	81.4	78.9	2.4***	84.5	83.2	1.3**						
1999				84.4	83.0	1.4**				78.3	77.3	1.0
2000				83.5	82.8	0.6				75.0	77.2	-2.1**
2001				80.0	79.7	0.3				79.6	82.3	-2.7**
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>9,264</b>	<b>5,874</b>	<b>15,138</b>	<b>4,613</b>	<b>2,855</b>	<b>7,468</b>	<b>4,613</b>	<b>2,855</b>	<b>7,468</b>

TABLE 1 (continued)

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records for the full research sample; and (3) quarterly UI earnings records from the following 22 randomly selected states for those who signed the records release consent form: AR, AZ, CA, FL, ID, IL, KS, LA, MD, ME, MI, MO, MS, NC, NE, NJ, OH, OK, SC, TX, VA, and WA.

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for all three data sources), (2) the survey design and interview nonresponse (for the survey data), and (3) the selection of states to the UI sample and nonresponse to the records release form (for the UI data). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline (for the survey data) and the selection of states (for the UI data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members. The UI-based impact estimates are measured as the weighted sum of the outcome measure for the full sample divided by the number of youths who lived in the 22 selected states at application to Job Corps.

<sup>b</sup>Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

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

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

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

TABLE 2  
 IMPACTS ON EARNINGS AND EMPLOYMENT USING SURVEY AND UI DATA  
 FOR THE FULL SAMPLE, BY QUARTER AFTER RANDOM ASSIGNMENT

Outcome Measure	Data Source					
	Survey Data			Quarterly UI Earnings Records from 22 States		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
Average Earnings, by Quarter After Random Assignment (in 1995 Dollars)						
1	565.4	851.4	-286.0***			
4	1,201.0	1,378.4	-177.4***			
8	1,992.8	1,909.8	83.0*			
12	2,550.7	2,321.5	229.2***			
13	2,669.4	2,444.1	225.4***			
14	2,727.6	2,524.2	203.5***			
15	2,778.3	2,564.2	214.1***	1,396.5	1,299.1	97.3
16	2,827.0	2,591.6	235.4***	1,414.8	1,382.0	32.8
17				1,449.7	1,470.9	-21.2
18				1,508.9	1,511.6	-2.7
19				1,545.6	1,553.5	-7.9
20				1,568.6	1,593.0	-24.4
21				1,632.6	1,677.3	-44.8
22				1,707.8	1,772.0	-64.3
23				1,721.7	1,775.5	-53.8
24				1,800.4	1,857.7	-57.3
25				1,856.2	1,909.0	-52.8
26				1,909.0	1,955.6	-46.6
Percentage Employed, by Quarter After Random Assignment						
1	33.2	42.1	-8.9***			
4	49.8	57.7	-7.9***			
8	59.0	57.9	1.2			
12	66.2	63.0	3.2***			
13	66.8	63.4	3.4***			
14	67.5	65.1	2.4***			
15	69.2	65.6	3.6***	55.0	52.6	2.3
16	71.1	68.7	2.4***	55.4	55.1	0.3
17				55.2	54.9	0.3
18				54.9	55.9	-1.0
19				55.2	56.3	-1.1
20				53.6	55.5	-1.8
21				53.9	55.5	-1.6
22				55.3	57.1	-1.7
23				55.5	57.4	-2.0
24				55.3	58.5	-3.3**
25				56.5	58.6	-2.1
26				58.1	61.0	-2.8
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>4,613</b>	<b>2,855</b>	<b>7,468</b>

TABLE 2 (continued)

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Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; and (2) quarterly UI earnings records from the following 22 randomly selected states for those who signed the records release consent form: AR, AZ, CA, FL, ID, IL, KS, LA, MD, ME, MI, MO, MS, NC, NE, NJ, OH, OK, SC, TX, VA, and WA.

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), (2) the survey design and interview nonresponse (for the survey data) and (3) the selection of states to the UI sample and nonresponse to the records release form (for the UI data). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline (for the survey data) and the selection of states (for the UI data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

TABLE 3

IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR RESPONDENTS  
TO THE 48-MONTH INTERVIEW, ACCORDING TO THE SURVEY AND SER DATA

Outcome Measure	Data Source											
	Survey Data				48-Month Interview Respondents				Annual Social Security Earnings Records			
	Program Group	Control Group	Estimated Impact <sup>a</sup>		Program Group	Control Group	Estimated Impact <sup>a</sup>		Program Group	Control Group	Estimated Impact <sup>a</sup>	
Average Calendar Year Earnings (in 1995 Dollars)												
1993					1,001.9	1,028.8	-27.0		1,009.6	1,013.9	-4.3	
1994					1,587.0	1,550.6	36.4		1,590.3	1,542.4	47.9	
1995					1,772.1	2,033.6	-261.5***		1,758.4	2,026.5	-268.1***	
1996	5,144.8	5,728.8	-584.0***		3,175.1	3,330.9	-155.9**		3,096.7	3,273.5	-176.8***	
1997	8,110.5	7,818.6	291.9*		4,696.4	4,443.2	253.2**		4,540.1	4,368.4	171.8**	
1998	10,295.6	9,324.1	971.6***		6,045.8	5,655.3	390.5***		5,803.9	5,584.1	219.8**	
1999					6,925.7	6,754.0	171.7		6,652.9	6,619.9	32.9	
2000					7,835.0	7,644.8	190.2		7,526.7	7,544.1	-17.4	
2001					8,017.6	7,863.7	154.0		7,678.7	7,671.7	7.0	
Percentage Employed in Calendar Year												
1993					42.6	42.6	0.0		42.9	43.0	-0.1	
1994					60.0	58.7	1.3		59.5	58.8	0.7	
1995					90.0 <sup>b</sup>	73.0	17.0***		89.2 <sup>b</sup>	73.3	15.9***	
1996	70.4	74.5	-4.2***		89.6 <sup>b</sup>	78.7	10.9***		88.7 <sup>b</sup>	78.3	10.4***	
1997	77.7	76.9	0.8		85.1	82.7	2.4***		83.5	81.4	2.1***	
1998	81.4	78.9	2.4***		85.9	84.4	1.5**		84.5	83.2	1.3**	
1999					86.0	84.6	1.4**		84.4	83.0	1.4**	
2000					85.2	85.0	0.2		83.5	82.8	0.6	
2001					81.4	81.7	-0.3		80.0	79.7	0.3	
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>		<b>6,772</b>	<b>4,451</b>	<b>11,223</b>		<b>9,264</b>	<b>5,874</b>	<b>15,138</b>	

TABLE 3 (continued)

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records for the full research sample.

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design, (2) the survey design and interview nonresponse (for the survey sample). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline.

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>Employment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

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

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

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



There are several possible explanations for the higher reported earnings levels in the survey data. First, informal and some formal jobs are not covered by the administrative records data but may be captured in the survey data. Second, some survey respondents may have over-reported their earnings and employment levels due to recall error or other reasons. Third, some employers may have inaccurately reported (or not reported) sample members' earnings to the government. Finally, the administrative records data may have missed earnings from sample members with SSNs (or other identifying information) that were incorrectly reported by employers or sample members.

To examine these explanations, we compared individual employment and earnings measures based on the UI and survey data in quarter 16 after random assignment (the most recent overlapping period). We did not use the SER for these analyses, because SSA does not release earnings records for individuals, but only for groups of individuals.

The analysis focused on the following questions: (1) Why are quarter 16 employment levels 13 percentage points higher and the number of jobs per worker 20 percent higher in the survey data than in the UI data? and (2) Why are quarter 16 earnings levels nearly 40 percent higher in the survey data than in the UI data, even for those with the same number of reported jobs according to both data sources?

**Errors in sample members' Social Security Numbers (SSNs) partly account for the higher employment levels in the survey than the UI data.** Unlike SSA, UI agencies do not verify reported SSNs before matching to their earnings records. This problem is exacerbated by the fact that about 12 percent of our sample members reported multiple SSNs over the course of the study. Thus, the UI wage records could miss earnings from persons with SSNs that were incorrectly reported by employers or sample members. Our finding that employment rates and mean earnings are somewhat lower in the UI than the SER data support this explanation.

**The non-coverage of some formal jobs under the UI program appears to account for only a small portion of the gap between the employment rate as measured by the survey and UI data.** The UI data do not cover workers in all formal jobs (for example, federal workers, military staff, self-employed persons, some agricultural labor, and domestic service workers). Using workers in the survey data, we find that those who were likely to be in non-covered formal jobs are somewhat less likely to have a record in the UI data than those who were likely to be in covered formal jobs. However, many of those likely to be in covered jobs do *not* have a record in the UI data. Thus, differences in survey and UI match rates across occupations are smaller than expected. These findings could be due in part to errors in classifying jobs reported in the survey into occupational categories, a result of limited survey information on the nature and title of jobs held by sample members.

**The survey data provides only weak evidence that the higher employment rate is due to informal jobs.** Sample members with informal (casual or cash-only) jobs were asked to report them in the survey, but these jobs were not likely to have been reported in the UI data. To examine the extent to which informal jobs explain the higher survey-based employment rate, we compared the characteristics of jobs reported in both the survey and UI data with the characteristics of jobs reported in the survey data only. As expected, average hourly wages and the likelihood of having available fringe benefits on the job were slightly lower for the survey-

only group. However, job tenure and usual hours worked were similar for the two groups of workers. Consequently, the differences in the characteristics of jobs held by the two groups of workers were smaller than expected.

**Substantial unobserved factors account for the employment rate differences according to the survey and UI data.** Few explanatory variables have predictive power in a multivariate regression model of whether survey-based jobs are reported in the UI data. Age, fertility status, marital status, health status, education level, welfare receipt status, and crime and drug use experiences do not significantly affect whether survey-reported jobs are reported in the UI data. Furthermore, only a few of the employment-related variables are statistically significant.

**The likely over-reporting of hours worked in the survey data plays an important role in explaining the higher *earnings per job* levels in the survey than UI data.** The level of earnings over a given period is the product of (1) the number of weeks worked on the job during the period, (2) the usual hours per week worked, and (3) the hourly wage rate. An examination of the association between each of these earnings components (as measured by the survey) and the *ratio* of average survey-to-UI earnings found that the survey-to-UI ratios increase with the number of hours worked as reported in the survey, but not with hourly wage rates or weeks worked. Moreover, the average worker reported working about 42 hours per week on their most recent job in quarter 16, and more than three-quarters reported working at least 40 hours—figures that are *higher* than the corresponding figures for all U.S. workers.

**Some evidence suggests that earnings differences between the survey and UI data are smaller for those in stable jobs than less stable ones.** We found some support for the hypothesis that earnings differences using the survey and UI data are smaller for sample members who held stable jobs. Earnings differences are much smaller for those with longer job tenure. Furthermore, the differences are somewhat larger for those in occupations that are more likely to have irregular hours (such as construction and private household occupations).

**Few differences in findings occur between the program and control groups.** Reporting differences between the survey and UI data are slightly larger for the program than control group, resulting in percentage earnings gains that are slightly larger according to the survey than UI data. However, no evidence was found that the program group was more likely than the control group to hold informal jobs or formal jobs not covered by the UI program; the distribution of the occupations of the jobs held by program and control group members in quarter 16 is very similar. Furthermore, there is no evidence that the program group was more likely than the control group to over-report hours worked on their jobs.

## **INTERVIEW NONRESPONSE BIAS**

As discussed, we found using the SER data that post-program earnings impacts for 48-month interview respondents are larger than for interview nonrespondents. These results suggest that the survey-based earnings impact estimates are biased upwards. What accounts for the interview nonresponse bias? The two possible explanations are:

1. ***Differences in the baseline characteristics of respondents in the program and control groups that are correlated with earnings.*** If interview respondents in the program group were drawn from a somewhat more advantaged subpopulation of the full program group than was the case for interview respondents in the control group, the survey-based impact estimates would be biased upwards.
2. ***True differences in the earnings impacts for survey respondents and survey nonrespondents.*** If earnings impacts are truly larger for survey respondents than survey nonrespondents, the survey-based earnings impacts would be biased upwards even if the observable and unobservable characteristics of respondents in the program and control groups are similar.

**While it is difficult to disentangle these two possible explanations, the data support more strongly the explanation that the bias is caused by true differences in the earnings impacts for survey respondents and nonrespondents.** Several pieces of evidence indicate that respondents in the program and control groups are comparable, suggesting that the former explanation cannot fully account for the nonresponse bias. First, the 48-month interview response rates are similar for the program and control groups. Second, the distributions of a large number of observable baseline characteristics and of the number of months until the 48-month interview was completed are similar for respondents in the program and control groups. Third, impact estimates based on the survey data are similar for subsamples of interview respondents that were formed to *equalize* the interview response rate for the program and control groups by selecting those who completed interviews *first*. Finally, impact estimates based on the SER data are similar using 12-month and 48-month interview respondents, even though the response rate was much higher to the 12-month interview.

The available evidence suggests also that earnings impacts truly differ for interview respondents and nonrespondents, supporting the second explanation for nonresponse bias. Observable baseline characteristics differ somewhat for respondents and nonrespondents, and mean earnings levels using the SER data were larger for respondents than nonrespondents during the post-program period. Most importantly, respondents had somewhat higher Job Corps participation levels than nonrespondents and stayed in the program for nearly one month longer on average than nonrespondents.

## **IMPACT FINDINGS AFTER THE PERIOD COVERED BY THE SURVEY**

**Based on the administrative data, we find no impacts of Job Corps for the full sample on employment or earnings after the four-year period covered by the survey.** The estimated impacts on calendar year earnings in 1999 to 2001 are all near zero and none are statistically significant (Tables 1 and 2). The earnings impacts in the post-survey period for 48-month interview respondents only are also not statistically significant (Table 3).

**However, the SER-based earnings impacts for 20- to 24-year-olds at program application appear to have persisted.** We find no beneficial SER-based earnings impacts in 2000 and 2001 that are statistically significant for any subgroup. However, positive earnings gains for those 20 to 24 and those with a high school credential at program application persisted with little decay.

## BENEFIT-COST ANALYSIS

As Job Corps is an intensive program that aims to make long-term impacts on the lives of the youth it serves, it is important to consider the benefits that may occur after the four-year survey observation period. In our initial benefit-cost analysis based only on survey data, we found that benefits exceed costs by \$17,000 per participant (Table 4). A key assumption underlying this finding was that the impacts on earnings in the observation period would persist without decay for the rest of the average participant's working lifetime. The impact findings using the administrative data, however, place the validity of this assumption in question.

TABLE 4  
INITIAL AND REVISED ESTIMATES OF BENEFITS AND COSTS OF JOB CORPS  
(1995 Dollars)

	Initial Estimates: Used Survey Data and Earnings Impacts Assumed Not to Decay	Revised Estimates: Used Adjusted Survey Data and Earnings Impacts <sup>a</sup> Assumed to Decay at Rate Observed in SER Data <sup>b</sup>	
	Full Sample	Full Sample	20-24 Year Olds
<b>Total Benefits</b>	30,957	3,695	14,696
Increased Output	27,531	269	17,547
Years 1-4	753	-60	588
After Year 4	26,778	329	16,959
Other Benefits	3,426	3,426	-2,850
<b>Program Costs</b>	-14,128	-13,844	-15,193
<b>Net Benefits</b>	16,829	-10,150	-496

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

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

<sup>b</sup>The rate of decay in the SER earnings impacts from the fourth year after random assignment to the seventh year after random assignment is 68.3 percent for the full sample and 5.9 percent for the 20 to 24 year olds.

**The revised benefit-cost estimates suggest that the benefits to society of Job Corps are smaller than the substantial program costs.** The revised estimates are based on the estimated *survey* earnings impacts that are adjusted downward to account for nonresponse bias and the likely overreporting of hours. We assume also that the earnings impacts decay at the same rate after the observation period as the impacts based on the SER data—68.3 percent per year. Under these assumptions, costs exceed benefits by \$10,200 per participant (Table 4). This change in

findings is due to the replacement of the assumption that earnings impacts persist with an assumption, more consistent with the administrative data, that they will decay rapidly. The finding that costs exceeds benefits for the full sample holds under a wide range of reasonable assumptions.

**Job Corps may be cost-effective for the older youth whose earnings impacts persisted during the post-survey period.** We find that benefits to society are only \$500 lower than program costs for the youth who were 20- to 24-years old at program application, under the assumption that the positive earnings impacts in 1998 to 2001 will decay at the same rate as they do in the SER data for this subgroup (Table 4). While this is our best estimate of the benefits and costs, the finding that costs exceed benefits is sensitive to small changes in assumptions. For example, if we treat the positive impact on arrests for murder for this subgroup as an anomaly, benefits would exceed costs.

**Job Corps is still worthwhile for its participants.** Job Corps is a good deal for program participants because the value of pay, food, and clothing they receive in the program offsets the earnings forgone while they are enrolled in Job Corps. Thus, the program has important distributional effects.

## CONCLUSIONS

Intensive, costly programs like Job Corps can only be expected to show benefits that exceed costs over a relatively long time horizon. Unfortunately, the foundation of empirical evidence to make long-term extrapolations of the profile of earnings in response to programs such as Job Corps simply does not exist. Nonetheless, we are forced to make extrapolation assumptions about future earnings. When we conducted the initial benefit-cost analysis, we assumed that the earnings impact in the last year of the observation period would persist with little decay, and found that program benefits to society exceed program costs. The analysis of the administrative data, however, casts doubt on the validity of this assumption. If true earnings impacts decay at the same rate as observed in the administrative data, our initial conclusion is reversed—the costs of Job Corps exceed its benefits for the full sample, although the program may be cost-effective for the older youth. Job Corps is too costly a program for short-term benefits to exceed costs. However, we still have observed earnings for only five years after the program group left Job Corps. Only further long-term follow up would eliminate all uncertainties about the effectiveness of the program.

The impact findings for the older youth can help guide future program improvement. Job Corps appears to have a longer-term beneficial effect on the earnings of older students than younger ones. Older students remain in Job Corps longer than younger ones, receive more hours of vocational training while enrolled, and are more highly motivated and well-behaved (as reported by program staff). Furthermore, many of the youngest sample members in the control group returned to high school after being rejected from Job Corps, whereas fewer older control group members enrolled in alternative education and training programs. These findings suggest that to improve overall program effectiveness, Job Corps needs to fully address differences by age in program structure and experience, and perhaps, to reassess the target population served by the program.

Finally, it is important to emphasize that the findings presented in this report pertain to the Job Corps program as it operated in 1995 and 1996 (when our program group members were enrolled in Job Corps), and not necessarily to the program as it operates today. There have been a number of significant changes that Job Corps has recently implemented in response to WIA provisions and other factors. For example, more Job Corps centers are now accredited to award high school diplomas, and Job Corps is more focused on providing longer-term support and placement services for their former students. These changes may have improved program effectiveness.

## I. INTRODUCTION

Job Corps stands out as the nation's largest, most comprehensive education and job training program for disadvantaged youths. It serves disadvantaged youths between ages 16 and 24, primarily in a residential setting. The program's goal is to help youths become more responsible, employable, and productive citizens. Each year, it serves more than 60,000 new participants at a cost of about \$1.5 billion, which is more than half of all funds spent by the U.S. Department of Labor (DOL) on youth training programs. Because Job Corps is one of the most expensive education and training programs currently available to youths, DOL sponsored the National Job Corps Study, which has been conducted since 1993, to examine the effectiveness of the program.<sup>1</sup>

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 could enroll in Job Corps; control group members could not, but they could enroll in other training or education programs. The research sample for the study consists of about 9,400 program group members and 6,000 control group members. These youths were randomly selected from among the nearly 81,000 first-time applicants nationwide who applied to Job Corps from November 17, 1994, through December 16, 1995, and who were found eligible for the program by February 29, 1996.

The impact findings presented in the main report for the study (Schochet et al. 2001) are based on the comparisons of the average outcomes of program and control group members using *survey* data collected during the four years after random assignment. These data indicate that Job

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<sup>1</sup> The evaluation was conducted by Mathematica Policy Research, Inc. (MPR) and its subcontractors, Battelle Human Affairs Research Centers and Decision Information Resources, Inc.

Corps generated positive impacts on earnings—the key outcome for the study—beginning in the third year after random assignment, and the impacts persisted through the end of the four-year follow-up period. Earnings gains were found for the full sample, as well as broadly across subgroups defined by youth characteristics at baseline and the characteristics of centers to which the youths were assigned. As discussed in our benefit-cost report (McConnell and Glazerman 2001), program benefits exceed program costs under the assumption that the earnings gains observed during the observation period will persist with little decay.

*Administrative* earnings records provide an alternative data source for obtaining information on the employment and earnings of program and control group members. These data allow us to estimate longer-term earnings impacts than could be obtained using the survey data, as well as to test our assumption used in the benefit-cost analysis about the persistence of future earnings impacts. Two sources of administrative data were collected for the evaluation:

1. ***Annual Social Security Earnings Data Reported by Employers to the Internal Revenue Service (IRS)***. These Summary Earnings Record (SER) data were used to estimate annual earnings impacts between 1993 and 2001.
2. ***Quarterly Wage Records Reported by Employers to State Unemployment Insurance (UI) Agencies in 22 Randomly Selected States***. These data were used to estimate quarterly earnings impacts from 1999 to 2001.

The administrative earnings records data were used in the Job Corps evaluation for two main reasons. First, they were used to assess whether administrative and survey earnings data yield similar estimates of earnings impacts during periods covered by both data sources. The two sets of impact estimates could differ because of survey nonresponse bias or reporting differences in the two types of data. Second, they were used to obtain estimated impacts for an additional two and a half years beyond the four-year follow-up period covered by the survey. This postsurvey period is too short to make definitive conclusions about the long-term effects of Job Corps on



participants' earnings. However, the data can be used for initial assessment of the appropriateness of the key assumption made in the benefit-cost analysis that the economic return to Job Corps participation will persist without decline for the rest of a youth's working lifetime (McConnell and Glazerman 2001).

This report presents impact findings using the administrative earnings data. We find that the pattern of earnings impacts using the administrative and survey data are similar in periods covered by both data sources, although the survey-based impact estimates are larger and more often statistically significant. This occurs primarily because reported earnings levels are substantially higher according to the survey data. Based on the administrative records data, we find no statistically significant beneficial earnings impacts after the four-year period covered by the survey. There is some evidence, however, of persistent earnings gains for those who were 20 to 24 years old at program application. Finally, program costs exceed program benefits to society under revised assumptions about the decay of future earnings impacts based on the impact findings using the administrative records data. However, the program appears to be cost-effective for the 20- to 24-year-olds, under the assumption that the positive earnings impacts in 1998 to 2001 persist with a slight decline for this group. Job Corps is also beneficial from the perspective of program participants.

This report contains six chapters. The rest of this chapter provides an overview of Job Corps and the sample and survey designs for the National Job Corps Study. Chapter II discusses features of the UI and SER data, including the jobs they cover, our design for collecting these data, and the analytic methods used to estimate administrative-based earnings impacts. Chapter III presents the impact estimates using the administrative earnings data and compares them to the impact estimates using the survey data. Chapter IV presents results from analyses that we conducted to help understand reporting differences between the survey and

administrative earnings data. Chapter V presents estimates of program benefits and costs that incorporate the impact findings using the administrative data. Chapter VI contains concluding remarks.

## **A. OVERVIEW OF JOB CORPS**

The Job Corps program, established by the Economic Opportunity Act of 1964, operated under provisions of the Job Training Partnership Act (JTPA) of 1982 during the study period.<sup>2</sup> Job Corps uses a well-defined program model (documented in Johnson et al. 1999), which had been refined continually over 30 years at the time our study sample attended in 1995 and 1996, and which has continued to evolve since the study period. Because many Job Corps centers are some distance away from the home areas of the students who attend the centers, different organizations have traditionally performed three key programmatic functions. These functions are (1) recruiting and screening students, (2) operating center programs, and (3) helping youths find jobs or further training after they leave Job Corps.

A complex operational structure supports the program. This structure has many levels of administrative accountability 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. One example of a national office initiative is the continual development of the Job Corps performance measurement system, which has been in place for nearly two decades.

Regional offices of DOL procure and administer contracts and perform oversight activities, such as reviews of center performance. DOL uses a competitive bidding process to contract out

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<sup>2</sup> Beginning in July 2000, Job Corps has operated under provisions of the Workforce Investment Act of 1998.

center operations, recruitment 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 competitive contracts. In addition, the U.S. Department of Agriculture and the U.S. Department of the Interior operated 30 Civilian Conservation Centers (CCCs) under interagency agreements with DOL.<sup>3</sup> Job Corps centers are in all regions of the country and in most states. There were 105 Job Corps centers that were operating in the 48 contiguous states when program group members were enrolled in Job Corps.<sup>4</sup>

Next, we briefly describe the three main program elements: (1) outreach and admissions, (2) center operations, and (3) placement.

## **1. Outreach and Admissions**

Outreach and admissions (OA) agencies provide information to the public through outreach activities, describe the program to youths who apply, and screen youths to ensure that they meet the eligibility criteria. They also assign youths to centers (when the regional office delegates this function) and arrange for transportation to centers. OA agencies include private nonprofit firms, private for-profit firms, state employment agencies, and the centers themselves. At the time of the study, 41 percent of all students were screened by private organizations that were not centers, 30 percent were screened by centers that also held an OA contract, and 29 percent were screened by state employment security agency personnel. The use of these various types of OA agencies varied widely across regions (see Johnson et al. 1999).

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<sup>3</sup> Currently, 90 contract centers and 28 CCCs are providing Job Corps training.

<sup>4</sup> Five centers in Alaska, Hawaii, and Puerto Rico were not part of the study.

## **2. Center Operations**

Centers are the heart of the Job Corps program. Each center provides comprehensive, intensive services that include basic education, vocational training, residential living, health care and education, and counseling.

**Education.** 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's 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-fifth of the centers can grant state-recognized high school diplomas.

**Vocational Training.** As with the education component, the vocational training programs at Job Corps are individualized and self-paced and operate on an open-entry, 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. Union members teach these classes at the centers.

**Residential Living.** The residential living component distinguishes Job Corps from all 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. About 12 percent of Job Corps study program group participants were nonresidential students.

**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 members 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; 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 also distribute the readjustment allowance, a stipend students receive after leaving Job Corps.

## **B. OVERVIEW OF THE SAMPLE AND SURVEY DESIGNS FOR THE NATIONAL JOB CORPS STUDY**

In this section, we highlight key features of the sample and survey designs for the Job Corps evaluation, including the labor market information collected during the interviews (for a detailed

discussion, see Schochet et al. 2001 and Schochet 2001). This information is needed to fully understand the design for the administrative earnings records study discussed in Chapter II.

## **1. Sample Design**

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. 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 (7 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 called the program group).<sup>5</sup> Because random assignment occurred after youths were determined eligible for Job Corps (and *not* after they enrolled in Job Corps centers), the program group included 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 focused 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.

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<sup>5</sup> The remaining 65,497 eligible applicants were randomly assigned to a program nonresearch group. These youths were allowed to enroll in Job Corps but were not in the research sample.

Control group members were not permitted to enroll in Job Corps for 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.

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.<sup>6</sup> This design feature allowed 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 had difficulty 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

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<sup>6</sup> Youths who previously participated in Job Corps (“readmits”) or applied for one of seven small, special Job Corps programs were excluded from the study (see Burghardt et al. 1999 for further discussion of these groups).

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.<sup>7</sup>

As expected, random assignment produced program and control groups whose distributions of characteristics prior to random assignment were similar (Schochet 1998). In addition, Job Corps staff implemented random assignment procedures well (Burghardt et al. 1999). Weekly extracts from the Job Corps management information system on all new center enrollees showed that less than 0.6 percent of enrollees arrived at a center without having been previously randomly assigned; thus, nearly all those in the study population were subject to random assignment. Furthermore, only 1.4 percent of control group members enrolled in Job Corps before the end of the three-year period during which they were not supposed to enroll.<sup>8</sup>

## **2. Survey Design**

Interviews with the research sample were conducted at baseline (shortly after random assignment) and at 12, 30, and 48 months after random assignment. Interviews were conducted by telephone and in person for those not reachable by telephone. During the baseline interview, to conserve data collection costs, in-person interviews were conducted in randomly selected

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<sup>7</sup> The 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.

<sup>8</sup> An 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.



areas only, which resulted in a slightly clustered survey sample (Schochet 2001). Youths in areas not selected for in-person interviewing at baseline and who did not complete baseline interviews were *not* eligible for follow-up interviews (because they were fully represented by those in the in-person areas who completed in-person interviews). For the follow-up interviews, however, in-person interviews were conducted with eligible sample members in *all* areas.

The sample used in the main impact report includes 11,313 youths (6,828 program group and 4,485 control group members) who completed a 48-month interview. The effective response rate to the 48-month interview (that is, the response rate in areas selected for in-person interviewing at baseline) was 79.9 percent (81.5 percent for the program group and 77.8 percent for the control group).<sup>9</sup> The distributions of baseline characteristics of program and control group members in the 48-month sample are similar (Schochet 2001).<sup>10</sup>

### **3. Labor Market Information Collected in the Surveys**

Each interview requested detailed information on the labor market experiences of respondents since the previous interview.<sup>11</sup> We collected labor market information for up to 2 jobs that were in progress at the last interview date, and up to 10 new jobs that started since the last interview date. We asked respondents to provide information on paid full-time or part-time

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<sup>9</sup> Of the 9,937 youths in the in-person areas at baseline who were eligible for 48-month follow-up interviews, 7,940 completed 48-month interviews.

<sup>10</sup> The distribution of baseline characteristics, however, differ slightly for those in the 48-month sample and the nonrespondents. Thus, we adjusted the sample weights using propensity scoring procedures so that the observable baseline characteristics of the 48-month sample matched those of the full sample (Schochet 2001).

<sup>11</sup> The baseline interview collected labor market information since the year prior to random assignment.

jobs that they had, including odd jobs, paid baby-sitting jobs, military service, work in their own businesses, and other types of jobs they may have had on a regular basis.

The surveys requested the following key labor market information for *each* job respondents held since the previous interview:

- Start and end dates
- Periods of two weeks or longer when they were not working and not getting paid
- Days per week usually worked
- Hours per day usually worked, including overtime hours
- Kind of company worked for (coded into three-digit industry [SIC] codes)
- Nature of the work and job title (coded into three-digit occupation [SOC] codes)
- Type of employer—whether (1) working for a private company; (2) on active military duty; (3) a federal, state, or local government employee; (4) self-employed; (5) working without pay or in a family business or as a favor, or (6) working for another type of employer
- Most recent hourly rate of pay before taxes and deductions, including tips, commissions, and regular overtime pay
- Earnings per pay period for those not paid by the hour
- Benefits available on the job (such as health insurance, paid sick leave, paid vacation, and retirement or pension benefits)

To obtain consistent data across interviews, we asked the same questions at each follow-up interview.

Using data on the start and end dates of jobs, we constructed weekly employment timelines to determine whether a sample member was working in a given week during the 208-week (that is, four-year) follow-up period. We then used these employment timelines and information on hours worked and hourly wages received on each job to construct weekly hours and earnings timelines. The labor market outcome measures used in the impact analysis were constructed using these timelines.

## II. DESIGN OF THE ADMINISTRATIVE EARNINGS RECORDS STUDY

We used two data sources for the administrative earnings record study: (1) social security earnings data reported by employers to the IRS (SER data), and (2) wage records reported by employers to state UI agencies. We used these data to address the following questions:

- Do administrative and survey earnings data yield similar impact estimates on employment and earnings during the periods that both data sources cover?
- Are the administrative and survey impact estimates more similar for some key population subgroups (defined, for example, by gender, age, race/ethnicity, educational level, arrest history, and residential/nonresidential status) than for others?
- What are estimated impacts on earnings and employment in the two and a half years beyond the four-year period covered by the survey?

The impact results using the survey and administrative data could differ for several reasons. First, nonresponse to the baseline and follow-up surveys could affect the survey-based earnings impact estimates. The response rate to the 48-month follow-up interview was similar for the program and control groups (81.5 percent for the program group and 77.8 percent for the control group), and the distributions of observable baseline characteristics of survey respondents in the two research groups are also similar (Schochet 2001). However, survey nonresponse bias could arise because of potential differences in (1) true earnings impacts for survey respondents and survey nonrespondents, or (2) unobservable baseline characteristics between survey respondents in the program and control groups that are correlated with earnings. We examined the extent of survey nonresponse bias by comparing impact estimates for respondents and nonrespondents using the administrative records data.

Second, the impact results using the survey and administrative records data could differ because of reporting differences in the two types of data. Administrative records do not cover all types of jobs. Furthermore, during the survey, some sample members may not have accurately

recalled their earnings over a long follow-up period, and some may have systematically over- or underreported them. To examine the extent of reporting differences by data source, we compared earnings levels as measured in the survey and administrative data in overlapping periods using the sample of respondents to the 48-month interview.

Because of several important differences in the way that SER and UI data are stored and released, we used different data collection designs for each data source. SER data are stored centrally, and UI data are stored separately by state. Thus, the SER data contain sample members' earnings from all states, whereas, to conserve costs, the UI data contain sample members' earnings from 22 randomly selected states only. In addition, for confidentiality reasons, the Social Security Administration (SSA) releases summary earnings measures only for groups of people—it does not release earnings records for individuals. Thus, we could not conduct detailed analyses comparing SER to survey data at the individual level. State UI agencies, however, release wage records for individual sample members, so that analyses comparing UI to survey data could be conducted at the individual level.

These important differences in the data collection designs for the SER and UI data influenced the types of analyses we could conduct and the analytic methods we could use to estimate earnings impacts using each data source. We discuss these issues in the rest of this chapter.

## **A. SOCIAL SECURITY EARNINGS DATA**

Each year, employers are required to report to the IRS the total earnings of each worker for whom social security withholdings must be made. The data reported are total earnings subject to social security for the calendar year. The SSA receives these data from the IRS to determine eligibility for social security. The SER—the data used for the study—is an extract from the Master Earnings File (MEF) maintained by the SSA. The MEF contains earnings records for

each worker with a social security number (SSN) who has worked in covered employment. The primary source for the earnings data is the W-2 form, which is sent directly to the SSA and updated weekly. The SER provides Federal Insurance Contribution Act (FICA)-covered wages (wages in covered employment up to the annual taxable maximum).<sup>12</sup>

Annual earnings data for a calendar year are available approximately 11 months after the end of the respective tax year. Thus, for example, data pertaining to calendar year 1999 became available in late November 2000, and data pertaining to calendar year 2000 became available in late November 2001. The SSA also stores and releases retrospective earnings data.

## **1. Coverage and Accuracy**

About 96 percent of all workers in employment or self-employment are covered under the Old-Age, Survivors, and Disability Insurance (OASDI) program (Social Security Administration 2001). Workers not covered fall into five categories: (1) civilian federal employees hired before 1984, (2) railroad workers, (3) some employees of state and local governments who are covered under their employers' retirement systems, (4) people with net annual earnings from self-employment below \$400, and (5) domestic workers and farm workers with low earnings.<sup>13</sup> Clearly, earnings from casual jobs or the underground economy also are not covered in the SER data, although they may be reported in the surveys.

On the basis of these reporting rules, the SER data contain earnings from most formal jobs our sample members held. The follow-up survey data indicate that, among the most recent jobs held by sample members in quarters 10 and 16 after random assignment, only 5 percent reported

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<sup>12</sup> The maximum is updated each year. In 2001, it was \$80,400.

<sup>13</sup> In 2001, domestic workers must have earned \$1,300 annually from any single employer before FICA taxes were withheld. The corresponding figure for agricultural workers was \$2,500.

that they were self-employed, 7 percent reported working in private household occupations, 6 percent reported working in state or local government, and less than 2 percent reported working in agricultural occupations (Schochet et al. 2001). These figures are similar for the program and control groups. Furthermore, most self-employed, domestic, and agricultural workers had enough earnings to be covered under the OASDI program; average weekly earnings reported in the survey were about \$375 for self-employed workers, \$230 for those in private household occupations, and \$300 for those in agricultural occupations. Thus, the earnings of only a small number of sample members who held formal jobs should be excluded from the SER data.

The SER data are likely to be accurate, because there are financial incentives for employers to report earnings to the IRS correctly. Earnings employers report to the IRS can be counted as a business expense that can be deducted from the firm's earnings and, thus, will lower its income tax.<sup>14</sup> Furthermore, few sample members had reported annual earnings that were capped at the annual taxable maximum for any years between 1993 and 2001.

## **2. Obtaining SER Data**

To protect confidentiality, the SSA does not release earnings data for individuals. Instead, it provides summary earnings statistics for *groups* of people. Accordingly, we requested that the SSA run computer programs that we provided to estimate earnings impacts (that is, differences in mean earnings between program and control group members) and their associated levels of statistical significance for the full sample and for key subgroups of the Job Corps population. The SSA sent us the output from these computer runs. This procedure ensured that the information that the SSA provided could not be associated with an individual sample member,

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<sup>14</sup> There could, however, be financial incentives for a firm to underreport earnings to the IRS if that firm evades paying taxes.

because all “group” means that we requested were estimated using at least 25 sample members, and most were estimated using at least 1,000 sample members.<sup>15</sup>

We sent the SSA a data file containing identifying information (SSN, name, date of birth, and sex) and other data needed to run our computer program (such as sample weights and subgroup indicator variables) for the *full* research sample of 15,301 youths (9,365 program group and 5,936 control group members).<sup>16</sup> We obtained the identifying information from the Job Corps intake (ETA-652) forms and the interviews. Because the identifying information sometimes changed over time, the data file contained, for each youth, one record for every possible combination of SSN, name, and date of birth that the youth ever reported.<sup>17</sup> We used this procedure to increase match rates to the SSA data system.

SSA procedures require that SSNs be verified before being matched to the SER data. Thus, the SSA used the identifying information in our data file to check the validity of each SSN using its Enumeration Verification System (EVS). Because our data file sometimes contained more than one record per sample member (as discussed above), we asked the SSA to determine the

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<sup>15</sup> We also gave the SSA an alternative data collection option, where we would provide the SSA with an earnings group number (1 to 71) for each sample member, and it would provide us with mean earnings and sum of squared earnings for *each* of the 71 earnings groups. The 71 earnings groups were constructed so that we could estimate earnings impacts and their standard errors that account for unequal weighting of the data. SSA staff, however, preferred that we send them the computer programs. We also preferred this option, because it allowed us to obtain impact estimates for a much larger set of subgroups than could be obtained under the alternative option. This is because the earnings groups were formed primarily so that the impact estimates could be weighted correctly. Thus, we could not form earnings groups using many subgroup indicator variables, because some of the resulting cell sizes would have been too small.

<sup>16</sup> The initial research sample contained 15,386 youths. However, we excluded 77 readmits (40 program and 37 control) who were determined after random assignment to have enrolled in Job Corps prior to random assignment and thus were excluded from the study universe. We also excluded an additional 8 youths for whom we did not have an SSN.

<sup>17</sup> The data file contained 20,586 observations. About 77 percent had only one record in the file, 18 percent had two records, and 5 percent had three or more records.

best match for each person and to store only *one* record per person (that is, one record per unique ID that MPR assigned to each youth at random assignment) when matching to the SER database.

The EVS system validated SSNs for 98.9 percent of the sample (98.9 percent for the program group and 99.0 percent for the control group). In total, matches were found for 15,138 of the 15,301 youths in our sample. Because of the sophisticated validation process the SSA used and the high match rate, we believe that the correct SSNs for our sample were matched to the SER database.

After merging the SER data to the data file containing sample weights and subgroup indicator variables, the SSA ran our computer programs to estimate impacts on annual earnings between 1993 and 2001. To ensure that random assignment had produced program and control groups with similar mean earnings levels during the period *prior* to random assignment, we requested figures for 1993 and 1994. The estimated impacts for 1995 to 2001 pertain to the post-random assignment period and are the focus of this report. SSA staff ran our computer programs in mid-2002 (after the 2000 SER data were finalized) and in mid-2003 (after the 2001 SER data were finalized).

### **3. Outcome Measures**

We used the SER data to obtain estimated impacts on annual earnings and employment rates in *calendar* years 1993 to 2001 (where annual earnings measures were scaled into 1995 dollars using the GDP price deflator).<sup>18</sup> Random assignment for the evaluation, however, took place during a 14-month period between mid-November 1994 and February 1996. Thus, outcomes measured in “calendar time” differ from outcomes measured in “random assignment time” (that

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<sup>18</sup> Earnings were put into 1995 dollars, because many program group members were enrolled in Job Corps in 1995. Thus, most program costs were incurred in 1995. Consequently, program benefits were scaled also into 1995 dollars in the benefit-cost analysis.



is, in the time since random assignment). For example, calendar year earnings in 1996 represent earnings accrued between 7 and 18 months after random assignment for those who were randomly assigned in June 1995, but they represent earnings accrued between 1 and 12 months after random assignment for those who were randomly assigned in December 1995.

This timing problem somewhat complicates the interpretation of the SER-based earnings impact estimates, because at a particular calendar date, the time of “exposure” to the intervention differed across sample members. However, it is fairly accurate to consider calendar year 1996 to be the “first” year (“year 1”) after random assignment for most sample members (because about 55 percent of the sample was randomly assigned after June 1995) and part of year 2 for some. Hence, calendar year 2001 can be considered year 6 for most sample members and part of year 7 for others.

This categorization is roughly consistent with our definition of the “in-program” and “postprogram” periods used in the main impact report. In that report, we showed that years 1 and 2 after random assignment were a period of intensive Job Corps participation for the program group, and years 3 and 4 were largely a postprogram period. Similarly, many program group members reported in the survey that they ever enrolled in Job Corps in 1995 and 1996 (about 72 percent in 1995 and 45 percent in 1996). Thus, 1995 and 1996 can be considered in-program periods. Job Corps enrollment rates were about 11 percent in 1997, 3 percent in 1998, and less than 2 percent in 1999. Thus, the period starting in mid-1997 was a postprogram period for nearly all program group members.

In the main impact report, the survey-based earnings impacts were estimated for earnings measured in random assignment time. Thus, to directly compare the survey-based and SER-based impact estimates, we also constructed survey-based annual earnings measures in calendar time. These calendar time measures, however, could be constructed only using those whose

follow-up period covered by the 48-month interview included the calendar year under investigation. For example, we could construct total 1999 earnings using the survey data only for those who were (1) randomly assigned in 1996, or (2) randomly assigned before 1996 but whose 48-month interviews were conducted in 2000.

Survey-based calendar year measures for 1996 to 1998 could be constructed for nearly all sample members (see Table II.1). However, the 1995 and 1999 measures could be constructed only for a small number of sample members; therefore, we do not present impact estimates for them.

#### **4. Analytic Methods**

The computer programs sent to the SSA estimated impacts on earnings and employment outcomes by computing differences in mean outcomes between all program and control group members. The random assignment design ensures that this approach yields unbiased estimates of the effect of Job Corps for program applicants who were determined eligible for the program. We calculated all estimates using sample weights to account for the sample design. The standard errors were inflated to account for design effects due to unequal weighting of the data. The impact estimates were not regression-adjusted for simplicity, because the baseline characteristics of the program and control groups are very similar, and because of large sample sizes that yielded precise impact estimates using the simple differences-in-means approach.

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 and the 27 percent who did not. In our main impact report, we present impact estimates per eligible applicant, as well as for only those who enrolled and received services (that is, for participants only). The impacts per participant were obtained by dividing the impact estimates per eligible applicant by the proportion of program group members who enrolled in Job Corps. Because the

TABLE II.1

SAMPLE SIZES FOR MEASURES BASED ON CALENDAR YEAR AND TIME SINCE  
RANDOM ASSIGNMENT, BY DATA SOURCE

Time Period	Data Source					
	Survey Data		Annual Social Security Earnings Records		Quarterly UI Earnings Records <sup>a</sup>	
	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group
<b>Calendar Year</b>						
1995	295	185	9,264	5,874	0	0
1996	6,627	4,328	9,264	5,874	0	0
1997	6,828	4,485	9,264	5,874	0	0
1998	6,808	4,474	9,264	5,874	0	0
1999	1,115	704	9,264	5,874	4,613	2,855
2000	0	0	9,264	5,874	4,613	2,855
2001	0	0	9,264	5,874	4,613	2,855
<b>Quarter/Year Since Random Assignment</b>						
<b>Quarter</b>						
13	6,828	4,485	0	0	680	387
14	6,828	4,485	0	0	1,982	1,238
15	6,828	4,485	0	0	3,095	1,893
16	6,692	4,358	0	0	3,999	2,480
17	1,738	1,079	0	0	4,613	2,855
18	576	332	0	0	4,613	2,855
19	186	94	0	0	4,613	2,855
20	68	28	0	0	4,613	2,855
21	12	3	0	0	4,613	2,855
22	0	0	0	0	4,613	2,855
23	0	0	0	0	4,613	2,855
24	0	0	0	0	4,593	2,843
25	0	0	0	0	3,933	2,468
26	0	0	0	0	2,631	1,617
27	0	0	0	0	1,518	962
28	0	0	0	0	614	375
<b>Year</b>						
4	6,692	4,358	0	0	680	387
5	68	28	0	0	4,613	2,855
6	0	0	0	0	4,593	2,843
7	0	0	0	0	614	375

Source: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records for the full research sample; and (3) quarterly UI earnings records from the following 22 randomly selected states for those who signed the records release consent form: AR, AZ, CA, FL, ID, IL, KS, LA, MD, ME, MI, MO, MS, NC, NE, NJ, OH, OK, SC, TX, VA, and WA.

<sup>a</sup>Sample pertains to those who lived in one of the 22 states selected for the UI study at application to Job Corps.

Job Corps enrollment rate was high, the estimated impacts for participants and eligible applicants are similar (Schochet et al. 2001).

We could not estimate impacts per participant using the SER data, however, because information on Job Corps enrollment was obtained from the survey and hence is unavailable for survey nonrespondents. Thus, we present SER-based impacts per eligible applicant only.

We used the SER data to estimate impacts for the full sample (that is, for all program and control group members) and for the following key population subgroups:

- ***Respondents and Nonrespondents to the 48-Month Interview.*** We conducted this analysis to examine the extent to which interview nonresponse affected the survey-based impact estimates.
- ***Subgroups Defined by Key Youth Characteristics at Baseline.*** These characteristics are (1) gender, (2) age at application to Job Corps, (3) race and ethnicity, (4) arrest history at random assignment, (5) educational level at random assignment, and (6) the presence of children (for females).
- ***Residents (the 88 Percent of Students Who Live at a Job Corps Center) and Nonresidents (the 12 Percent of Students Who Live at Home but Attend Job Corps During the Day).*** We estimated impacts by residential status using data on the predictions of Job Corps OA counselors as to whether sample members would be assigned to a residential slot or to a nonresidential one. This information was collected at program application (which was *before* random assignment) and thus is available for both program and control group members. These predictions closely matched actual residential status (Schochet et al. 2001).
- ***Subgroups Defined by the Characteristics of Centers the Youths Attended.*** These center characteristics are type of operator (CCC or contract centers), size, region, and performance ranking (for further description of center characteristics, see Burghardt and Schochet 2001). These impacts were obtained using data on the predictions of OA counselors as to which center each youth was likely to attend. We examined impacts for small centers (225 slots or fewer; serving 20 percent of students), medium centers (226 to 495 slots; serving 46 percent of students), and large centers (more than 495 slots; serving 34 percent of students). High-performing centers were defined as those that were in the top third of the performance ranking during program years 1994 and 1995. Similarly, low-performing centers are those that were in the bottom third of the performance ranking in both years. The high- and low-performing groups each included just under one-fourth of centers. The remaining centers were designated medium-performing centers.

Appendix Table B.1 displays subgroup sample sizes using the survey and SER data.

We estimated impacts for a subgroup by comparing the distribution of outcomes of program and control group members in that subgroup. For example, we obtained impact estimates for males by comparing the mean outcomes of male program and control group members. Similarly, we obtained impact estimates for residents by comparing the mean outcomes of program and control group members who were designated for residential slots.

To examine reporting differences in the survey and SER data, our computer programs calculated, for each 48-month survey respondent, the difference between calendar-year earnings according to the survey and SER data. The program produced summary statistics on these differences separately for the program and control groups.

## **B. UI WAGE RECORDS**

UI data provide an alternative data source for obtaining information on the employment and earnings of youths in the National Job Corps Study. Employers in most states are required to maintain and submit earnings records to the state's UI system for workers in jobs covered by UI.<sup>19</sup> These records, which are maintained in machine-readable form, are used to determine individual workers' eligibility for unemployment compensation if they are laid off.

Wage records contain calendar-quarter information, by employer, on earnings. Because it takes time to collect and process the data, wage records for a given calendar quarter are, however, generally not complete for about six months. Thus, data pertaining to the last quarter of 1999, for example, became available in mid-2000. Furthermore, because the data needed to determine UI eligibility pertain to a base period that is usually defined as the first four of the last

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<sup>19</sup> Employers in all states submit wage records. However, in a few states, wage records are not used to determine UI eligibility. In these cases, the UI system does not maintain wage records.

five completed calendar quarters, wage records are typically maintained online for, at most, six or seven quarters. Data for earlier quarters are archived and generally less readily available.

For analyses, wage records have some advantages over the SER data. First, UI wage records cover calendar quarters rather than full calendar years, so they can be used to construct earnings measures over periods since random assignment (that is, in random assignment time). Second, unlike the SER data, the UI agencies provide data for individuals, so the UI data can be used to conduct detailed analyses examining individual differences in UI-based and survey-based earnings measures.

The main disadvantage of the UI wage records data is that they are not stored centrally across states; instead, they must be collected separately from each state. Making requests to every state and dealing with individual state access protocols make acquisition costs very expensive relative to the SER data. Therefore, we selected a sample of states for the UI study. Hence, sample sizes for the analysis of UI data were smaller than for the analysis of SER data, resulting in less precise impact estimates using the UI data. For this reason, we used the UI data to obtain impact estimates for the full sample but not for subgroups of youths.

Another disadvantage of the UI wage records is that states often require written consent before releasing individual UI wage records. Thus, as described below, we obtained UI wage data only for the 80 percent of sample members who signed a consent form for records release when they applied to Job Corps. Thus, earnings impacts estimated using the UI wage records may not be generalizable to the full population of eligible applicants included in the National Job Corps Study, because of potential differences in the unobservable characteristics of those who signed the consent form and those who did not.

## 1. Coverage and Accuracy

UI wage records consist of total quarterly earnings reported by employers to state UI agencies for each employee. By law, most employers are subject to a state UI tax and must report what is paid to each employee, including regular earnings, overtime, and tips and bonuses. In most states, the Federal Unemployment Tax Act (FUTA) applies to employers who (1) paid wages of \$1,500 or more during any calendar quarter in the current or preceding calendar year, or (2) employed at least one worker for at least one day in each of the 20 weeks during the current or preceding calendar year.

Most workers are covered under FUTA, and coverage provisions are similar to those under the OASDI program. The major difference between coverage in the two programs is that UI wage records do not cover federal workers, military staff, or self-employed people (the SER data cover self-employed people who have annual net earnings of at least \$400).<sup>20</sup> Another difference is that the SER data do not cover some state and local employees, whereas the UI wage records cover all state and local employees. Other workers excluded from coverage under the FUTA provisions include railroad employees, workers in service for relatives, most agricultural labor (except workers on large farms), domestic service workers whose employers paid less than \$1,000 in wages in any calendar quarter, part-time employees of nonprofit institutions, some students employed by their schools, insurance and real estate agents on commission, and casual labor not in the course of the employer's business (U.S. Department of Labor 2002). Many of these same workers are also excluded from the OASDI program. UI wage records cover about 94 percent of workers (U.S. General Accounting Office 2002). Thus, we expect that the UI data

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<sup>20</sup> Federal workers and military staff are eligible to receive UI benefits; however, their earnings are not contained in the UI wage records.

contain earnings information on most formal jobs held by our sample members in the states selected for UI data collection.

Although coverage is similar for the UI and OASDI programs, the UI wage records are likely to be less accurate than the SER data, for several reasons. First, employers have financial incentives to underreport earnings to state UI programs, because earnings reported to UI agencies provide the basis for assessing a payroll tax that finances UI benefit payments.<sup>21</sup> In contrast, as discussed earlier, employers have incentives to report earnings fully to the IRS, because reported earnings can be counted as a business expense that can be deducted from the firm's earnings and hence will lower its income tax. Second, unlike the SSA, state UI agencies do not verify reported SSNs. Thus, the UI wage records could miss earnings from people with SSNs that were incorrectly reported by employers or sample members. This problem is exacerbated by the fact that about 12 percent of our sample members reported multiple SSNs over the course of the study. For these cases, we could send only one SSN to the state UI agencies to match to their data systems. Consequently, the state UI wage records could have also missed earnings for cases for whom we selected the incorrect SSN.

## **2. Obtaining UI Wage Records**

The research sample for the National Job Corps Study is based on a fully national sample of eligible Job Corps applicants; the program and control groups contain youths who lived in each of the contiguous 48 states and the District of Columbia at the time of random assignment. However, as mentioned earlier, for cost reasons, we chose only a sample of states for the UI

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<sup>21</sup> Firms often hire outside companies to report earnings to UI agencies. In these cases, the reporting of earnings may be more accurate.



study. In this section, we discuss our approach for selecting the 25 states for the UI study (22 of which provided us with wage records data), as well as the sample members included in the study.

**a. Selection of States**

Our power analysis indicated that the desired levels of precision on earnings impacts could be achieved if 17 states were selected for the UI study. The 17 states were randomly selected using systematic sampling techniques, where the probability a state was chosen was proportional to the number of 1993 Job Corps enrollees who lived in that state. Massachusetts and New York were excluded from the selection because we were informed that UI data could not be obtained from them. In addition, we did not include Alaska and Hawaii because these states (and Puerto Rico) were excluded from the National Job Corps Study. The states were grouped by region to ensure that the distribution of the chosen states would be dispersed throughout the United States. Regions 1 and 2 were grouped together, since these regions had a small number of enrollees after Massachusetts and New York were removed from the selection.

We also selected eight “replacement” states for “primary” states that could not provide the UI data. We selected one replacement state for each region (regions 1 and 2 were grouped together) using the sampling techniques discussed above. Our design specified that a replacement state in a region would be contacted if UI data could not be obtained for any of the primary states in that region.

During data collection, however, we did not follow this sequential design for obtaining UI data from replacement states, because of the time it took to determine whether primary states would provide their UI data for the study. For most states, it took some time to gain approval from the state UI offices to obtain their data and to have the UI offices match our sample members to their UI wage records. Because of the time lag in assessing whether the primary states would cooperate, we might not have been able to obtain data from the replacement states

in time for our report if we had waited until firm refusals were obtained from the primary states. Thus, we contacted the replacement states at the *same time* that we contacted the primary states. In other words, we attempted to collect data from all 25 selected states simultaneously rather than from the primary states first and from the replacement states second. Thus, our revised design does not distinguish between primary and replacement states. Instead, we treat all 25 selected states as primary states.

The following 25 states were selected for UI data collection: Arkansas, Arizona, California, Florida, Georgia, Idaho, Illinois, Kansas, Louisiana, Maryland, Maine, Michigan, Missouri, Mississippi, North Carolina, Nebraska, Nevada, New Jersey, Ohio, Oklahoma, Pennsylvania, South Carolina, Texas, Virginia, and Washington. Appendix A provides a detailed discussion of the state selection process and the selection results.

#### **b. State Response Rates**

Of the 25 states contacted about providing UI wage records, 22 agreed to provide the data. Only Georgia, Nevada, and Pennsylvania refused to release their data.

We used several alternative procedures to adjust the sample weights to account for these three states. First, we assumed that only 22 states were originally sampled rather than 25. Second, we assumed that the nonresponding states were represented by the states in their region. These procedures produced similar results; we present the results using the first adjustment procedure (see Appendix A).

#### **c. Sample Members Included in the UI Study**

In the National Job Corps Study, all applicants during the sample intake period were asked to sign a consent form to access, collect, and use information for the study from records collected by public agencies, such as public assistance programs (AFDC/TANF, Medicaid, and the Food

Stamp Program), the UI program, child-support enforcement, and the criminal justice system. Applicants who refused consent for this data collection but agreed to participate in the full study were still allowed to enroll in Job Corps and were randomly assigned in the same way as other applicants.

Many UI state agencies will release individual UI wage records only for those who provide written consent. Thus, only youths in the research sample who signed the consent form for records release were included in the UI study. As Table II.2 shows, about 79 percent of the research sample signed the consent form (78 percent for the control group and 79 percent for the program group).

Signee rates were similar by gender, age, education level, and whether the youth received public assistance at program application (see Tables II.2 and A.4). However, the rates differed substantially across regions (ranging from about 52 percent in region 1 to 93 percent in region 4), and by conviction history at program application (74 percent for those ever convicted, compared to 80 percent for those never convicted).<sup>22</sup> Furthermore, a joint statistical test of the hypothesis that the distribution of baseline characteristics of signers and the full sample of signers and nonsigners are similar was rejected at the 1 percent significance level. Finally, and most important, we found using the *SER* data that the earnings impacts in 1999 to 2001 are substantially *larger* for signers than for nonsigners. Consequently, the UI-based impact estimates calculated using the sample of signers only may not be representative of UI-based impacts for the full study population (that is, they may be subject to nonresponse bias). To help alleviate this

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<sup>22</sup> Signee rates were also somewhat higher for 48-month interview respondents than nonrespondents (77.3 percent, compared to 74.6 percent).

TABLE II.2  
 PERCENTAGE OF SAMPLE MEMBERS WHO SIGNED THE RECORDS RELEASE CONSENT FORM,  
 BY RESEARCH STATUS AND KEY SUBGROUP

Subgroup	Program Group	Control Group	Combined Sample
Full Sample	79.4	78.3	78.9
Gender			
Male	79.5	78.7	79.1
Female	79.4	77.7	78.6
Age at Application			
16 to 17	78.5	77.7	78.1
18 to 19	80.4	79.5	79.9
20 to 21	80.7	78.2	79.5
22 to 24	78.0	77.4	77.7
Race/Ethnicity			
White, non-Hispanic	81.9	80.8	81.4
Black, non-Hispanic	78.4	77.3	77.9
Hispanic	76.5	77.4	76.9
Other	81.0	79.4	80.2
Region			
1	53.5	50.9	52.2
2	62.8	59.1	60.8
3	56.5	55.7	56.1
4	92.6	93.1	92.8
5	85.3	82.8	84.0
6	87.4	88.5	87.9
7/8	76.9	77.0	76.9
9	84.1	80.4	82.2
10	86.6	83.8	85.2
Education Level at Application			
Completed 12th grade	79.6	78.7	79.2
Did not complete 12th grade	79.5	78.7	79.1
Receipt of Public Assistance			
Received assistance	80.0	79.1	79.5
Did not receive assistance	79.4	78.8	79.1
Conviction History at Application			
Ever convicted or adjudged delinquent	76.1	72.1	74.1
Never convicted or adjudged delinquent	79.7	79.2	79.5
Residential Designation Status			
Resident	79.6	78.9	79.3
Nonresident	78.6	74.3	76.5
<b>Sample Size</b>	<b>9,369</b>	<b>5,940</b>	<b>15,309</b>

Source: Records release consent forms and ETA-652 and ETA-652 Supplement Data.

Notes: All figures were calculated using sample weights to adjust for the sample design. Figures exclude 77 cases (37 control group and 40 program group members) who were determined to have enrolled in Job Corps prior to random assignment and were thus ineligible for the study.

problem, we used propensity scoring methods to adjust the sample weights for nonresponse to the consent form (see Appendix A).

Importantly, because signatures on the consent form were requested *prior* to random assignment, there are very few differences in the baseline characteristics of program and control group *signers* (Tables II.2 and A.4). The joint test of the hypothesis that the distributions of baseline characteristics between the two groups of signers are similar is not statistically significant. Thus, although we find differences between the characteristics of signers and nonsigners, the characteristics of signers in the two research groups appear to be similar.

The SSNs of *all* program and control group members who signed the consent form were sent to each of the 22 state UI agencies. Later in this section, we discuss our analytic approach for estimating UI-based earnings impacts accounting for the fact that not all sample members were in these 22 states during the follow-up period (which causes the UI earnings data to contain too many “zeroes”).

### **3. Outcome Measures**

The UI data received from the 22 states contain quarterly earnings data for each reported job the sample members held from the first quarter in 1999 to the fourth quarter in 2001. The data cover this time period, because in most states these were the wage records that were available online when the UI agencies processed our data request.

For each state and calendar quarter between 1999 and 2001, we constructed total quarterly earnings for each sample member by summing reported earnings from each of the youth’s employers (and put them into 1995 dollars using the GDP price deflator). A small number of outliers were set to missing (seven cases for the 1999 data, four for the 2000 data, and five for the 2001 data).

We also constructed quarterly UI earnings measures in *random assignment time*. As discussed, the use of random assignment time has the advantage that outcomes are measured after a fixed period since sample members were determined eligible for Job Corps. This feature eases the interpretation of the impact estimates. However, because a particular point after random assignment falls on a different calendar date across sample members, the specific quarterly measures in random assignment time that could be constructed varied across sample members. For example, the first quarter of 1999 was quarter 17 in random assignment time for a youth randomly assigned in April 1995, but it was quarter 15 for a youth randomly assigned in October 1995.<sup>23</sup>

Table II.1 displays sample sizes for the quarterly earnings measures constructed using the UI data. The figures pertain to those who lived in the 22 selected states at application to Job Corps (the *effective* sample size for the UI study). The effective sample sizes for the 1999, 2000, and 2001 calendar-time measures are 4,613 program group and 2,855 control group members. Data for the measures in random assignment time are available for the full UI sample in quarters 17 to 23, for most of the sample in quarters 15 to 16 and 24 to 26, but for only a small number of sample members in other quarters. Thus, we present UI-based impact estimates only for quarters 15 to 26 (corresponding to years 5 and 6 and the first half of year 7 after random assignment).

Because of small effective sample sizes for some population subgroups, the UI analysis focused only on estimating impacts for the full sample.

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<sup>23</sup> We defined quarter 1 in random assignment time as the calendar quarter when the youth was randomly assigned if the random assignment date was in the first half of the calendar quarter, and as the calendar quarter after the youth was randomly assigned if the random assignment date was in the second half of the calendar quarter. For example, if a youth was randomly assigned on November 14, 1995, then quarter 1 in random assignment time was the fourth quarter in 1995, but if the youth was randomly assigned on November 16, 1995, then quarter 1 in random assignment time was the first quarter of 1996.

#### **4. Analysis Samples**

To analyze the UI data, we used two analysis samples of those who signed the consent form for records release. First, we analyzed UI data on the *full* sample of signers in the research sample to obtain nationally representative UI-based earnings impact estimates. As we discuss in the next section, we used this sample to (1) assess whether the UI-based and survey-based impact estimates differ due to the combined effect of survey nonresponse bias and reporting differences between the two data sources, and (2) obtain impact estimates covering the postsurvey period (that is, in years 5 and 6 and the first half of year 7 after random assignment).

The second analysis sample includes signers who (1) completed 48-month interviews, and (2) reported in the interviews that they lived in the 22 selected states for the *entire* four-year follow-up period and did not work in other states. *Complete* UI earnings information is available for these youths. Thus, UI-based and survey-based earnings and employment measures were directly compared for individuals in this sample. Consequently, we used this sample to assess the extent to which earnings impact estimates using the two data sources differ due to reporting differences. This sample, however, could not be used to assess the extent of survey nonresponse bias.

#### **5. Analytic Methods**

To produce unbiased UI-based impact estimates that can be generalized to the study population for the National Job Corps Study, several important issues need to be considered. First, because we selected only a sample of states for the UI study, we cannot determine the exact UI-based earnings for all sample members during the follow-up period, because the youths may have had UI-based earnings from states out of the sample. A sample member with a UI record from the selected states clearly had some UI-based earnings during the follow-up period. However, the youth may have had other UI-based earnings from a nonselected state if the youth

changed states during the follow-up period (thereby causing the UI data to underestimate the individual's total earnings). Similarly, a sample member with no UI record from the selected states may have had UI-based earnings from a nonselected state.

A second issue is that we need to account for unequal weighting due to the sampling of states. In addition, the weights need to account for the three states that would not release their records data, as well as for those sample members who did not sign the records release consent form when they applied to Job Corps (see Appendix A).

As discussed, we conducted the UI analysis using two samples. First, we used the full analysis sample. Second, we used only sample members who completed 48-month follow-up interviews and who reported living in the selected states during the entire four-year follow-up period. Next, we discuss the analytic methods that we used to obtain unbiased UI-based earnings impact estimates using these two samples.

#### **a. Analysis Using the Full Sample**

The states selected for UI records collection were chosen randomly. In principle, the random selection of states ensures that the expected number of sample members who moved into the selected states during the follow-up period should be equal to the expected number of sample members who moved out of the selected states during that time. Thus, the expected number of sample members who resided in the selected states should have remained constant over time, although the distribution of specific sample members residing in these states may have changed.

We tested the accuracy of this “steady-state” assumption using survey information on the states in which sample members lived and worked during the four-year follow-up period. Table II.3 displays this mobility information for those who lived in the 22 UI states at program application and those who lived in the other (non-UI) states. We present the figures separately for the program and control groups and for males and females.



TABLE II.3

## MOBILITY ACROSS STATES, BY RESEARCH STATUS AND GENDER

Mobility Measure	Full Sample		Males		Females	
	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group
<b>Mobility Across All States</b>						
Percentage Ever Lived or Worked in a Different State to the State Lived in at Application to Job Corps						
Years 1 to 4						
Those in all states at application	34.3	28.9	38.3	33.3	28.8	22.4
Those in the 22 UI states	33.0	27.3	37.0	31.9	27.0	20.7
Those in other states	36.7	31.7	40.3	35.6	31.6	25.7
Years 3 and 4						
Those in all states at application	26.5	24.3	29.6	27.9	22.1	19.1
Those in the 22 UI states	24.9	22.3	28.1	26.1	20.1	16.9
Those in other states	29.3	27.8	32.0	30.9	25.4	23.0
Years 3 and 4 for at least 6 months						
Those in all states at application	19.8	18.2	21.4	20.3	17.4	15.3
Those in the 22 UI states	18.2	17.0	20.2	19.4	15.3	13.7
Those in other states	22.4	20.3	23.5	21.7	20.8	18.1
<b>Mobility Across the UI and Non-UI States</b>						
Weighted Number of Those in the UI States at Application Who Lived or Worked in a Non-UI State <sup>a</sup>						
Years 1 to 4	9,334	6,850	6,231	4,747	3,103	2,103
Years 3 and 4	6,433	5,264	4,303	3,721	2,130	1,543
Years 3 and 4 for at least 6 months	4,292	3,715	2,791	2,449	1,502	1,266
Weighted Number of Those in the Non-UI States at Application Who Lived or Worked in a UI State <sup>a</sup>						
Years 1 to 4	7,618	6,856	4,889	4,601	2,730	2,256
Years 3 and 4	6,019	5,750	3,803	3,849	2,216	1,901
Years 3 and 4 for at least 6 months	4,220	4,136	2,503	2,603	1,717	1,533
<b>Unweighted Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>3,741</b>	<b>2,787</b>	<b>3,087</b>	<b>1,698</b>

Source: ETA-652 data and baseline, 12-, 30-, 48-month follow-up interview data for those who completed 48-month interviews.

Note: All figures were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

<sup>a</sup>About 50,500 (or 62.5 percent) of the 80,883 youths in the sample universe for the evaluation lived in the 22 UI states at application to Job Corps.

The steady-state assumption is supported by the data. About one-third of the sample ever lived or worked in a state different from the one in which they lived at baseline, and the figure was about one-quarter during years 3 and 4 after random assignment (the postprogram period). Mobility rates were somewhat lower for control group than program group members, and for those in the 22 UI states than those in the non-UI states. However, importantly, the number of youths in the sample universe of 80,883 who initially lived in the 22 UI states but who moved to the non-UI states was similar to the number of youths who moved in the reverse direction. This result holds for both research groups and by gender. For example, in years 3 and 4 after random assignment, the program group data indicate that 6,433 eligible applicants in the sample universe moved into the 22 states, whereas 6,019 moved into the non-UI states.<sup>24</sup> Furthermore, key baseline characteristics (such as age, education level, application date, and arrest history) of those who moved into the 22 UI states are similar to those who moved into the non-UI states (not shown) and the survey-based earnings are somewhat similar for the two groups (about \$11,000 in year 4 after random assignment for those who moved out of the UI states, compared to \$10,000 for those who moved into the UI states).

Under the steady-state assumption, the mean outcome for the program group in a particular state can be estimated by summing the reported earnings for *all* program group members in that state and dividing this sum by the number of program group members who lived in that state *at application to Job Corps*, and similarly for the control group. The estimated impact per eligible applicant for a state is then obtained as the difference between the program and control group

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<sup>24</sup> The figures for the program group in years 1 to 4 are much larger, because in years 1 and 2, many program group members enrolled in a Job Corps center in a different state than the one from which they came.

means in that state, and the estimated impact for the study population is a weighted average of these state impacts (see Appendix A for more details on this estimation procedure).

The intuition behind this estimation procedure can be demonstrated using an example. Suppose a youth who initially lived in California moved out of California one year after random assignment. Then, the estimation procedure relies on the steady-state assumption that the earnings of this youth after leaving California can be represented by the earnings of another youth who initially lived outside California but moved there one year after random assignment.

It is important to reiterate that this estimation procedure cannot be used to compare survey-based and UI-based measures *for individual sample members* but can be used only to estimate aggregate UI-based impacts. The procedure cannot be used to obtain separate impact estimates for survey respondents and nonrespondents and hence cannot directly be used to assess the extent of interview nonresponse bias. Nor can the procedure be used to directly assess reporting differences between the two data sources. Instead, the procedure produces estimates that reflect the joint effects of interview nonresponse bias and reporting differences—the relative contribution of each factor cannot be disentangled.

#### **b. Analysis Using Those Who Completed the 48-Month Follow-Up Interview**

We can directly compare employment and earnings measures from the UI and survey data using the sample of youths who completed the 48-month interview and who reported in the interviews that they lived in the selected states during the *entire* four-year follow-up period and did not work in outside states. Complete UI data are available for this sample, because they had zero earnings in the states not selected for UI data collection. Thus, we can use this sample to assess the extent of reporting differences between the UI and survey data.

For this analysis, the UI-based earnings measures for each person were constructed by *summing* the person's earnings across the 22 selected states (that is, we constructed total UI-

based earnings from all the states combined). We then compared the UI-based and survey-based earnings measures at the individual and aggregate level, and the estimated earnings impacts using the two data sources.

### III. IMPACT FINDINGS

This chapter presents impact findings using the SER and UI data and compares them to impact findings using the survey data. We present estimated impacts per eligible applicant for employment and earnings outcomes measured in calendar time (for all three data sources) and random assignment time (for the survey and UI data). We present results in both calendar and random assignment time to maximize the sample available for the analysis. To roughly link the two sets of outcome measures, one can view 1996 as year 1 after random assignment for most sample members (and part of year 2 for some) and, thus, 2001 as year 6 for most (and part of year 7 for some).

In this chapter, we first present administrative-based impact results for the full sample and for 48-month interview respondents only. Impacts are presented for the four-year period covered by the survey and afterwards. Second, we present impact results for key population subgroups.

#### A. IMPACT FINDINGS FOR THE FULL SAMPLE AND FOR 48-MONTH INTERVIEW RESPONDENTS

Tables III.1 and III.2 present impact findings for the full sample. Table III.1 presents estimated impacts per eligible applicant on *calendar-year* earnings (in 1995 dollars) and *calendar-year* employment rates using the SER, UI, and survey data. The table displays means and percentages for the program and control groups, differences between these means and percentages (that is, estimated impacts per eligible applicant) and the statistical significance of the estimated impacts. Some table entries are missing for the survey and UI data, because, as discussed, *calendar-year* measures for some years could not be constructed using these data. Table III.2 displays similar statistics for quarterly earnings (in 1995 dollars) and quarterly employment rates measured in *random assignment time* that were obtained using the survey and

TABLE III.1  
 IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR THE FULL SAMPLE,  
 BY DATA SOURCE

Outcome Measure	Data Source									
	Survey Data			Annual Social Security Earnings Records			Quarterly UI Earnings Records from 22 States			
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>	
Average Calendar Year Earnings (in 1995 Dollars)										
1993				1,009.6	1,013.9	-4.3				
1994				1,590.3	1,542.4	47.9				
1995				1,758.4	2,026.5	-268.1***				
1996	5,144.8	5,728.8	-584.0***	3,096.7	3,273.5	-176.8***				
1997	8,110.5	7,818.6	291.9*	4,540.1	4,368.4	171.8**				
1998	10,295.6	9,324.1	971.6***	5,803.9	5,584.1	219.8**				
1999				6,652.9	6,619.9	32.9				26.0
2000				7,526.7	7,544.1	-17.4				-194.3
2001				7,678.7	7,671.7	7.0				-134.6
Percentage Employed in Calendar Year										
1993				42.9	43.0	-0.1				
1994				59.5	58.8	0.7				
1995				89.2	73.3	15.9***				
1996	70.4	74.5	-4.2***	88.7	78.3	10.4***				
1997	77.7	76.9	0.8	83.5	81.4	2.1***				
1998	81.4	78.9	2.4***	84.5	83.2	1.3***				
1999				84.4	83.0	1.4***				1.0
2000				83.5	82.8	0.6				-2.1**
2001				80.0	79.7	0.3				-2.7***
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>9,264</b>	<b>5,874</b>	<b>15,138</b>	<b>4,613</b>	<b>2,855</b>	<b>7,468</b>	

TABLE III.1 (continued)

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records for the full research sample; and (3) quarterly UI earnings records from the following 22 randomly selected states for those who signed the records release consent form: AR, AZ, CA, FL, ID, IL, KS, LA, MD, ME, MI, MO, MS, NC, NE, NJ, OH, OK, SC, TX, VA, and WA.

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for all three data sources), (2) the survey design and interview nonresponse (for the survey data), and (3) the selection of states to the UI sample and nonresponse to the records release form (for the UI data). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline (for the survey data) and the selection of states (for the UI data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members. The UI-based impact estimates are measured as the weighted sum of the outcome measure for the full sample divided by the number of youths who lived in the 22 selected states at application to Job Corps.

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

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

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

TABLE III.2

IMPACTS ON EARNINGS AND EMPLOYMENT USING SURVEY AND UI DATA  
FOR THE FULL SAMPLE, BY QUARTER AFTER RANDOM ASSIGNMENT

Outcome Measure	Data Source					
	Survey Data			Quarterly UI Earnings Records from 22 States		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
Average Earnings, by Quarter After Random Assignment (in 1995 Dollars)						
1	565.4	851.4	-286.0***			
4	1,201.0	1,378.4	-177.4***			
8	1,992.8	1,909.8	83.0*			
12	2,550.7	2,321.5	229.2***			
13	2,669.4	2,444.1	225.4***			
14	2,727.6	2,524.2	203.5***			
15	2,778.3	2,564.2	214.1***	1,396.5	1,299.1	97.3
16	2,827.0	2,591.6	235.4***	1,414.8	1,382.0	32.8
17				1,449.7	1,470.9	-21.2
18				1,508.9	1,511.6	-2.7
19				1,545.6	1,553.5	-7.9
20				1,568.6	1,593.0	-24.4
21				1,632.6	1,677.3	-44.8
22				1,707.8	1,772.0	-64.3
23				1,721.7	1,775.5	-53.8
24				1,800.4	1,857.7	-57.3
25				1,856.2	1,909.0	-52.8
26				1,909.0	1,955.6	-46.6
Percentage Employed, by Quarter After Random Assignment						
1	33.2	42.1	-8.9***			
4	49.8	57.7	-7.9***			
8	59.0	57.9	1.2			
12	66.2	63.0	3.2***			
13	66.8	63.4	3.4***			
14	67.5	65.1	2.4***			
15	69.2	65.6	3.6***	55.0	52.6	2.3
16	71.1	68.7	2.4***	55.4	55.1	0.3
17				55.2	54.9	0.3
18				54.9	55.9	-1.0
19				55.2	56.3	-1.1
20				53.6	55.5	-1.8
21				53.9	55.5	-1.6
22				55.3	57.1	-1.7
23				55.5	57.4	-2.0
24				55.3	58.5	-3.3**
25				56.5	58.6	-2.1
26				58.1	61.0	-2.8
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>4,613</b>	<b>2,855</b>	<b>7,468</b>

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; and (2) quarterly UI earnings records from the following 22 randomly selected states for those who signed the records release consent form: AR, AZ, CA, FL, ID, IL, KS, LA, MD, ME, MI, MO, MS, NC, NE, NJ, OH, OK, SC, TX, VA, and WA.



TABLE III.2 (continued)

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- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), (2) the survey design and interview nonresponse (for the survey data) and (3) the selection of states to the UI sample and nonresponse to the records release form (for the UI data). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline (for the survey data) and the selection of states (for the UI data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

UI data. Again, some table entries pertaining to particular quarters are missing when data were not available to construct outcome measures pertaining to these quarters.

Tables III.3, III.4, and B.2 present results from our analysis examining the extent to which differences between the findings using the survey and administrative data are due to survey nonresponse and due to reporting differences between the two data sources. Table III.3 displays impact results using the SER data on calendar year earnings for respondents to the 48-month interview (along with the full sample results), and Table B.2 displays impact results for survey nonrespondents. Table III.4 compares UI-based and survey-based earnings impacts for survey respondents who lived in the 22 states selected for the UI study for the entire four-year follow-up period and who did not work elsewhere. We have complete UI earnings data for this sample, so differences in the survey-based and UI-based earnings can be attributed solely to reporting differences between the two data sources.

Before presenting the results, it is important to recognize that differences in earnings *levels* using the administrative records and survey data will lead to differences in earnings *impacts* in *nominal* (or dollar) terms using the two data sources. However, if the ratios of survey-based to administrative-based earnings levels are similar for the program and control groups, then the earnings impacts relative to the control group means (that is, in *percentage* terms) will be similar using the two data sources. The percentage earnings gains, however, will differ if the earnings ratios differ for the program and control groups.

To illustrate these points, the earnings impact using the survey data,  $I_S$ , can be written as follows:

$$(1) \quad I_S = E_{PARP} - E_{CARC},$$

TABLE III.3

IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR RESPONDENTS  
TO THE 48-MONTH INTERVIEW, BY DATA SOURCE

Outcome Measure	Data Source																							
	Survey Data				Annual Social Security Earnings Records				Respondents and Nonrespondents															
	48-Month Interview Respondents		48-Month Interview Respondents		48-Month Interview Respondents		48-Month Interview Respondents		Program Group		Control Group		Estimated Impact <sup>a</sup>											
Average Calendar Year Earnings (in 1995 Dollars)																								
1993																								
1994																								
1995																								
1996	5,144.8	5,728.8	-584.0***		3,175.1	3,330.9	-155.9**		3,096.7	3,273.5	-176.8***		4,540.1	4,368.4	171.8**		5,803.9	5,584.1	219.8**					
1997	8,110.5	7,818.6	291.9*		4,696.4	4,443.2	253.2**		4,540.1	4,368.4	171.8**		5,803.9	5,584.1	219.8**		6,652.9	6,619.9	32.9					
1998	10,295.6	9,324.1	971.6***		6,045.8	5,655.3	390.5***		6,652.9	6,619.9	32.9		7,526.7	7,544.1	-17.4		7,526.7	7,544.1	-17.4					
1999					6,925.7	6,754.0	171.7		7,835.0	7,644.8	190.2		8,017.6	7,863.7	154.0		7,863.7	7,678.7	185.0					
2000																								
2001																								
Percentage Employed in Calendar Year																								
1993																								
1994																								
1995																								
1996	70.4	74.5	-4.2***		89.6	78.7	10.9***		88.7	78.3	10.4***		83.5	81.4	2.1***		83.5	83.2	0.3					
1997	77.7	76.9	0.8		85.1	82.7	2.4***		84.5	83.2	1.3**		84.4	83.0	1.4**		84.4	83.0	1.4**					
1998	81.4	78.9	2.4***		86.0	84.6	1.4**		85.2	82.8	2.4***		85.0	82.8	2.2**		85.0	82.8	2.2**					
1999																								
2000																								
2001																								
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>		<b>6,772</b>	<b>4,451</b>	<b>11,223</b>		<b>9,264</b>	<b>5,874</b>	<b>15,138</b>		<b>9,264</b>	<b>5,874</b>	<b>15,138</b>		<b>9,264</b>	<b>5,874</b>	<b>15,138</b>					

TABLE III.3 (continued)

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records for the full research sample.

- Notes: 1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
2. All estimates were calculated using sample weights to account for (1) the sample design, (2) the survey design and interview nonresponse (for the survey sample). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline.

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

TABLE III.4

IMPACTS ON EARNINGS AND EMPLOYMENT USING SURVEY AND UI DATA  
FOR SURVEY RESPONDENTS WHO LIVED IN THE UI STATES  
FOR THE ENTIRE 48-MONTH FOLLOW-UP PERIOD,  
BY QUARTER AFTER RANDOM ASSIGNMENT

Outcome Measure	Data Source					
	Survey Data			Quarterly UI Earnings Records from 22 States		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
Average Earnings, by Quarter After Random Assignment (in 1995 Dollars)						
1	577.0	856.0	-279.0***			
4	1,191.7	1,369.1	-177.4***			
8	1,952.4	1,839.5	112.9			
12	2,392.3	2,273.1	119.1			
13	2,517.0	2,407.1	109.8			
14	2,574.2	2,469.5	104.7			
15	2,642.1	2,466.1	175.9**	1,296.5	1,309.9	-13.5
16	2,695.6	2,507.6	188.0**	1,409.4	1,381.8	27.6
17				1,454.1	1,410.3	43.8
18				1,449.1	1,458.7	-9.5
19				1,457.2	1,476.0	-18.8
20				1,523.8	1,510.6	13.2
21				1,564.6	1,608.5	-43.9
22				1,626.5	1,660.4	-33.9
23				1,704.6	1,678.5	26.1
24				1,763.7	1,739.3	24.5
25				1,841.3	1,825.8	15.5
26				1,897.7	1,824.2	73.5
Percentage Employed, by Quarter After Random Assignment						
1	34.3	42.8	-8.5***			
4	51.0	56.6	-5.6***			
8	58.7	57.6	1.1			
12	64.6	63.5	1.1			
13	65.7	63.7	1.9			
14	66.1	64.6	1.4			
15	68.3	64.7	3.6**	55.0	55.8	-0.7
16	70.8	69.1	1.8	58.2	55.2	3.0*
17				57.7	55.8	1.9
18				56.5	57.4	-0.9
19				56.6	57.3	-0.8
20				55.6	56.0	-0.3
21				56.2	56.0	0.2
22				56.7	57.5	-0.8
23				58.4	57.9	0.5
24				57.9	57.8	0.1
25				58.7	58.2	0.5
26				58.6	58.5	0.1
<b>Sample Size<sup>b</sup></b>	<b>2,754</b>	<b>1,872</b>	<b>4,626</b>	<b>2,754</b>	<b>1,872</b>	<b>4,626</b>

TABLE III.4 (continued)

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Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; and (2) quarterly UI earnings records from the following 22 randomly selected states for those who signed the records release consent form: AR, AZ, CA, FL, ID, IL, KS, LA, MD, ME, MI, MO, MS, NC, NE, NJ, OH, OK, SC, TX, VA, and WA.

Notes: 1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.

2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), (2) the survey design and interview nonresponse (for the survey data). The estimates do not account for the selection of states to the UI sample. Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline.

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, and (3) lived and worked only in the 22 states selected for the UI study.

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

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

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

where  $E_{PA}$  is average administrative-based earnings for the program group,  $R_P$  is the ratio of survey-to-administrative average earnings for the program group, and  $E_{CA}$  and  $R_C$  are the corresponding values for the control group, respectively. If the reporting ratios are similar for the program and control groups (that is, if  $R_P = R_C = R$ ), then the survey-based impact in equation (1) reduces to  $(E_{PA} - E_{CA})R$ , and the control group mean using the survey data becomes  $E_{CA}R$ . In this case, the survey-based impact estimate,  $(E_{PA} - E_{CA})R$ , will differ from the administrative-based impact estimate,  $(E_{PA} - E_{CA})$ , by the reporting factor  $R$ , but the two-sets of impacts relative to their control group means will be the same (that is, they both can be expressed as  $[(E_{PA} - E_{CA})/E_{CA}]$ ). Clearly, this argument does not hold if the reporting ratios differ by research status (that is, if  $R_P$  differs from  $R_C$ ).

## **1. Impact Findings During the Period Before Random Assignment**

As demonstrated in Schochet (1998), the random assignment process generated program and control groups with similar observable characteristics in the period prior to random assignment. This analysis was conducted using demographic data from Job Corps intake (ETA-652) forms (which were collected at the time of program application and are available for all sample members) and detailed baseline interview data (which are available for 93 percent of sample members who completed the baseline interview).

The 1993 and 1994 SER data were used to further check that the random assignment process was implemented correctly. Random assignment started in mid-November 1994, and thus 1993 is a pre-random-assignment period for all sample members. Calendar year 1994 is a pre-random-assignment period for the 96.5 percent of sample members who were randomly assigned in 1995 or 1996, and is largely a pre-random-assignment period for the 3.5 percent of sample

members who were randomly assigned in late 1994.<sup>25</sup> Consequently, we expect differences in the average SER earnings of program and control group members in 1993 and 1994 to be statistically insignificant.

The data support these expectations. Earnings differences between the program and control groups in 1993 and 1994 are small and statistically insignificant at the 10 percent level for the full sample and for all key population subgroups except for male nonresidents (Table III.1 and Tables B.3 to B.16).

Annual earnings levels and employment rates were low in these years, because many sample members were in school then because of their young ages (in 1993, sample members were between ages 14 and 22). In 1993, only about 43 percent of sample members were employed, and the average sample member earned only about \$1,000 (which translates into \$2,325 per worker; Table III.1). In 1994, average earnings increased to only slightly more than \$1,500.

## **2. Impact Findings During the Four-Year Period Covered by the Survey**

In this section, we present impact findings during the four-year, post-random-assignment period covered by the survey. First, we present impact findings using the administrative data for the full sample of 48-month interview respondents and nonrespondents. Second, we present the administrative-based impact findings using interview respondents only.

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<sup>25</sup> Among program group members who enrolled in Job Corps, the average time between random assignment and program enrollment was 1.4 months. Thus, many enrollees who were randomly assigned in 1994 did not enroll in Job Corps until 1995. Thus, 1994 can be considered a preprogram period for nearly all enrollees.



**a. Impact Findings for the Full Sample of Survey Respondents and Nonrespondents**

The *pattern* of the estimated impacts using the survey and administrative data is similar in periods covered by both data sources. However, the survey-based impact estimates are *larger* and more often statistically significant (Table III.1).

The estimated impacts on annual earnings in 1995 (according to the SER data) and in 1996 (according to both the survey and SER data) are negative and statistically significant, because many program group members were enrolled in Job Corps then. The estimated earnings impacts became positive and statistically significant in 1997 according to both data sources as program group members left Job Corps and found jobs. In 1998, the earnings impacts remained positive and statistically significant for both data sources, although the survey-based impact estimate is larger (\$972 or a 10.4 percent earnings gain relative to the control group mean using the survey data, compared to \$220 or a 3.9 percent earnings gain relative to the control group mean using the SER data). Similarly, the estimated impact on the annual employment rate in 1998 is larger using the survey data (2.4 percentage points, compared to 1.3 percentage points).<sup>26</sup>

The impact estimates according to the survey data are also larger than those according to the UI data (Table III.2). While the survey-based impact estimates in quarters 15 and 16 after random assignment are positive and statistically significant, the corresponding UI-based impact estimates are smaller and not statistically significant. Similarly, the employment rate impact in quarters 15 and 16 are statistically significant using the survey data but not using the UI data.

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<sup>26</sup> The estimated impacts on the 1995 and 1996 employment rates based on the SER data are large and positive, because wages that Job Corps pays to enrollees (that is, student pay) are reported to the IRS. Thus, the reported employment rates for the program group were very high during these in-program years, because most Job Corps enrollees receive student pay.

The estimated impacts in *nominal* (or dollar) terms are larger according to the survey than the administrative records data, because average reported earnings levels are substantially higher in the survey data for both the program and control groups. For example, in 1998, average earnings for the program group are about \$10,300 (or \$12,650 per worker) using the survey data but only \$5,800 (or \$6,850 per worker) using the SER data.

The impacts in *percentage* terms are also somewhat larger using the survey data during the postprogram period, because percentage differences in earnings levels according to the survey and administrative data are slightly larger for the program than the control group. For example, average reported calendar-year earnings in 1998 are about 77 percent higher according to the survey data than the SER data for the program group, whereas the corresponding figure for the control group is 67 percent (Table III.1). Similarly, average quarterly earnings in quarter 16 after random assignment are twice as large using the survey data than the UI data for the program group, but are only 88 percent larger using the survey data for the control group (Table III.2).

It is important to emphasize that differences in the survey-based and administrative-based earnings levels are due to the *combined* effects of survey nonresponse and reporting differences between the two types of data sources. We attempt to isolate each of these effects in the next section, where we compare earnings levels as measured by the survey and administrative data using comparable samples of survey respondents only.

Finally, average earnings are about 17 percent higher according to the SER data than the UI data.<sup>27</sup> As discussed in Chapter II, coverage is similar for the UI and OASDI programs, and both programs cover nearly all formal jobs. Differences in the earnings levels in the SER and UI data

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<sup>27</sup> We find a similar result using the SER data for only those who signed the records release consent form (so that the UI-based and SER-based estimates are directly comparable).

probably reflect the lower accuracy of the UI data, because, as discussed (1) employers have financial incentives to underreport earnings to state UI programs but to fully report earnings to the IRS; (2) the SSA verifies SSNs before inputting reported earnings data or matching people to their earnings records, whereas UI agencies do not; and (3) the UI data contain information on only the 80 percent of sample members who signed the records release consent form, whereas the SER data pertain to all sample members. The 17 percent SER-to-UI average earnings ratio is similar to the ratios reported for youths in the National JTPA Study (Kornfeld and Bloom 1999).

**b. Impact Findings for 48-Month Survey Respondents Only**

As discussed, the *percentage* earnings gains in 1998 are somewhat larger using the survey than using SER data. In this section, we address the following question: To what extent are the larger percentage earnings gains in the survey than administrative data due to survey nonresponse, and to what extent are they due to reporting differences between the two data sources that are correlated with research status?<sup>28</sup>

To help disentangle these effects, we present earnings impacts using the administrative records data *for the sample of 48-month survey respondents only*, so that the impact estimates using the survey and administrative data can be compared directly. We also discuss differences in earnings *levels* as measured by the survey and administrative records data using comparable samples of interview respondents.

**Earnings Impact Findings.** We find that differences in the percentage earnings gains in 1998 using the survey and SER data are due in roughly equal parts to interview nonresponse bias and to reporting differences between the two data sources (Table III.3). The estimated 1998 earnings gain is 10.4 percent according to the survey data and 3.9 percent according to the SER

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<sup>28</sup> The percentage earnings gains in 1997, however, are similar using the two data sources.

data. Using the sample of respondents to the 48-month interview only, the SER-based earnings gain increases from 3.9 to 6.9 percent, which is still smaller than the 10.4 percent survey-based figure. Thus, the remaining difference between the survey and full-sample SER impacts is due to reporting differences between the two data sources that are slightly greater for the program than control group.

To quantify the contribution of each factor, we can decompose the difference between the survey-based and full-sample SER-based earnings impacts (in percentage terms) as follows:

$$(2) (S_R - A) = (S - A) + (S_R - S),$$

where  $S_R$  is the survey-based impact (percentage earnings gain) for survey respondents,  $A$  is the administrative-based impact for the full sample, and  $S$  is the survey-based impact if *all* sample members had responded to the survey. The expression  $(S-A)$ , then, represents the extent of reporting differences between the two data sources, and  $(S_R-S)$  represents the extent of survey nonresponse bias. Equation (2) is operational if we assume that  $(S-A)$  can be estimated by  $(S_R-A_R)$ , and that  $(S_R-S)$  can be estimated by  $(A_R-A)$ , where  $A_R$  is the administrative-based impact for survey respondents.

Using equation (2), we estimate that, in 1998, about 46 percent of the difference between the survey-based and full sample SER-based earnings impacts (in percentage terms) is due to interview nonresponse bias, and 54 percent is due to reporting differences between the two data sources.

The interview nonresponse bias is caused by larger impacts for survey respondents than nonrespondents, which suggests that the survey-based earnings impact estimates are somewhat biased *upward* (Table B.2). The interview nonresponse bias can be attributed to two potential sources. First, it could be due to differences between the baseline characteristics of respondents

in the program and control groups that are correlated with earnings. If interview respondents in the program group were drawn from a somewhat more advantaged subpopulation of the full program group than was true for interview respondents in the control group, then the survey-based impact estimates would be biased upward. Second, it is possible that respondents in the program and control groups are similar but that earnings impacts are actually larger for survey respondents than for survey nonrespondents. In this case, the impacts based on survey respondents are unbiased, but they are not generalizable to the full study population. As discussed in Chapter IV, we believe that both factors account for the interview nonresponse bias, although the latter factor is more important.

Results using the UI data provide additional evidence on reporting differences between the survey and administrative data that are larger for the program than control group. Using the sample of survey respondents who lived in the 22 UI states for the entire four-year follow-up period and who did not work elsewhere, we find that the survey-based earnings impacts in percentage terms are larger than the UI-based ones in quarters 15 and 16 after random assignment (Table III.4). For example, in quarter 16, the survey data yield a statistically significant earnings gain of 7.1 percent, whereas the UI data yield a statistically insignificant earnings gain of only 2.0 percent.

**Differences in Earnings Levels, by Data Source.** We find that average earnings *levels* continue to be much larger using the survey than administrative records data using the survey sample only. Average reported earnings levels in the survey data are about 70 percent higher than for survey respondents in the SER data. Similarly, average survey-based earnings are about 80 to 90 percent higher than average UI-based earnings using a comparable sample. As discussed in Chapter IV, these reporting differences may be due to earnings from informal (cash-only) jobs

or from formal jobs not covered in the administrative records data. Alternatively, they may be due to survey respondents reporting more earnings to interviewers than they actually earned.

The differences in reporting levels found in the survey and administrative data are somewhat larger than those found in previous studies using similar populations. For example, Kornfeld and Bloom (1999) found that mean quarterly earnings were about 35 to 70 percent higher according to the survey data than the UI data for youths in the National JTPA Study. Similarly, Cave (1995) found that the survey to UI ratios ranged from about 1.05 to 1.80 from several studies that examined the earnings of welfare recipients in welfare-to-work demonstration programs. These studies also found that the extent of underreporting was similar for the program and control groups, so the impact estimates based on the administrative records and survey data were usually similar.

One possible explanation for the lower reported earnings levels in the administrative data in our study than the National JTPA Study is that the National Job Corps Study was conducted during a period of stronger economic growth. The unemployment rate for the civilian population of those age 16 and older was about 7.0 in the early 1990s (when the JTPA evaluation was conducted) but decreased to 5.5 percent in late 1994, to 4.5 percent in mid-1998, and to 4 percent in 2000 (although it increased to about 5 percent by the end of 2001). Similarly, the unemployment rate for those ages 16 to 19 decreased from 17 percent to under 14 percent during the same period. In addition, inflation was low. Thus, in the tight labor market, our sample members may have been more likely to collect “under-the-table” earnings from casual or cash-only jobs that are reported in the survey but not in the administrative data. In addition, because the program group had higher skills than the control group (as measured by higher GED completion rates, many more hours spent in academic classes and vocational training, and higher

functional literacy test scores [Schochet et al. 2001; and Glazerman et al. 2000]), the program group may have earned more from these informal jobs than the control group.

Interestingly, the calendar year employment rates computed using the survey and administrative data are similar, and are even somewhat lower according to the survey data (Table III.3). These findings further support our hypothesis that earnings levels are higher in the survey data because sample members were employed in second jobs or earned additional wages on their jobs that were not reported to the government.

Reported *quarterly* employment rates, however, are much higher in the survey data than the UI data (Table III.4), which suggests that the administrative data are not capturing short-term informal jobs held by sample members. These findings also suggest that sample members reported in the interviews that they were employed on jobs longer than they actually were (perhaps because of recall error), resulting in annual employment rates that are similar using the two data sources but quarterly employment rates that are higher using the survey data. The lower quarterly employment rates in the UI data may also be due to errors in the matching process used to obtain the UI data, because of inaccurate SSNs.

### **3. Impact Findings After the Period Covered by the Survey**

Based on the administrative data, we find no impacts of Job Corps on employment or earnings *after* the four-year period covered by the survey. The estimated impacts on calendar year earnings in 1999, 2000, and 2001 (using the SER data) and on quarterly earnings in quarters 17 to 26 after random assignment (using the UI data) are all near zero and not statistically significant (Tables III.1 and III.2).

Interestingly, the earnings impacts in the postsurvey period using the survey respondents only are also not statistically significant (Tables III.3 and III.4). The SER-based impacts using the survey respondents are larger than the SER-based impacts using the full sample because of

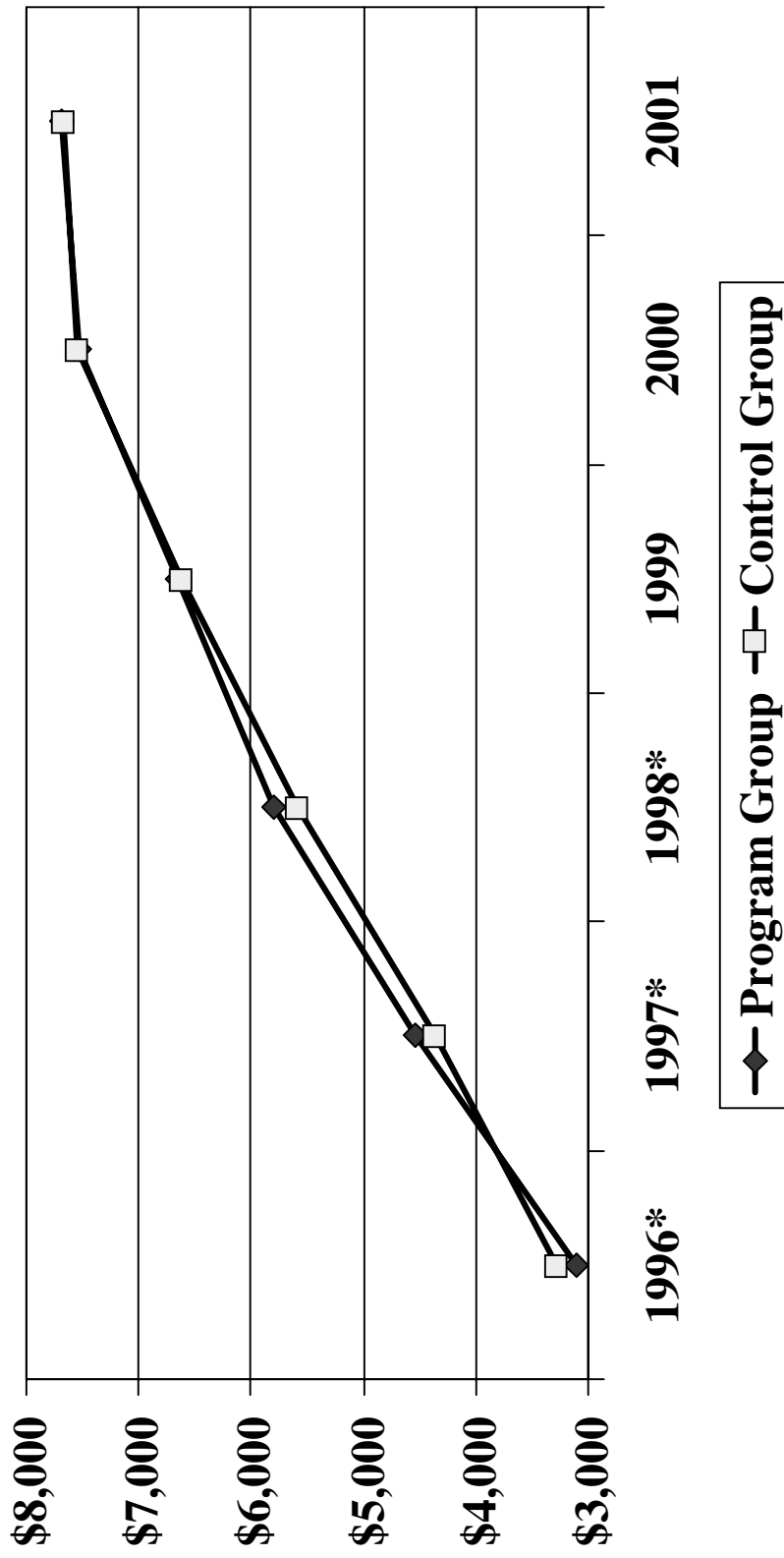
interview nonresponse bias. However, the SER-based earnings gains in 1999, 2000, and 2001 using the survey respondents are still very small (only about 2.5 percent) and are not statistically significant.

As Figure III.1 shows, the control group caught up to the program group in 1999. The mean earnings of the program group based on the SER data grew sharply between 1996 and 1998, but grew at a slower rate between 1999 and 2001. The mean earnings of the control group grew more uniformly throughout the follow-up period.

These findings are surprising. In our benefit-cost analysis, we assumed, based on the available evidence from the survey data, that the dollar value of the annual earnings impact in the fourth year after random assignment would persist without decline for the rest of a youth's working lifetime (McConnell and Glazerman 2001). This extrapolation assumption was made for four main reasons. First, the impacts of Job Corps did not decline during years 3 and 4. In long-term studies of the returns to training, if returns decline, the decline occurs within two or three years after a trainee leaves the program (Lillard and Tan 1992; and Ashenfelter 1978). Second, Job Corps teaches many skills. The employment and training programs that have been found to have long-term impacts tend to be ones that teach a wide range of skills, such as the National Supported Work Demonstration (Couch 1992) and the Center for Employment Training (Zambrowski and Gordon 1993). On the other hand, those that have short-lasting impacts, such as on-the-job training and other private-sector training (Lillard and Tan 1992) and classroom training under the Manpower Demonstration Training Act (Ashenfelter 1978) tend to teach specific skills. Third, we found that Job Corps improves literacy and numeracy skills, which we would expect to have long-lasting impacts. Finally, the 12 percent earnings gains in year 4 based on the survey data are similar to the returns to a year of school found in studies based on returns over the entire working lives of individuals (Card 1999).



FIGURE III.1  
 AVERAGE EARNINGS, BY CALENDAR YEAR



Source: Annual social security earnings records

\*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level.

Why, then, did the control group catch up? Although it is too early to conclude that these findings will be permanent, we surmise that the earnings impacts decreased as the gap between the job-specific skills of program and control group members closed as the youths gained work experience. Job Corps participants experienced an initial boost in their employment and earnings soon after they left the program (that is, in years 3 and 4 after random assignment). This boost was caused by increases in participants' skill levels, as measured by (1) large impacts on hours spent in education and training (equivalent to about one full school year), (2) large impacts on the attainment of GED and vocational certificates (about 20 to 30 percentage points), and (3) small impacts on functional literacy skills assessments at 30 months. The boost also may have been due to job placement services Job Corps offered. During this initial postprogram period, however, the differences in employment rates between the program and control groups were small (about 2 to 3 percentage points per quarter in year 4). Furthermore, although some evidence exists that the program group obtained better jobs, the differences were not large; most workers in both groups held low-paying jobs with high turnover. For example, the survey data indicate that, in their most recent job in quarter 16, workers in the program group earned an average of only about \$0.22 more per hour than workers in the control group (\$7.55, compared to \$7.33), and only a slightly higher percentage of program group workers had health insurance benefits available on the job (57 percent program, compared to 54 percent control). In addition, job tenure was less than one year for both research groups, and the distribution of job occupations was similar for the two groups.<sup>29</sup>

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<sup>29</sup> In quarter 16, about 22 percent worked in service occupations (such as food and health service); 20 percent worked in construction occupations; 11 percent worked in sales; 13.5 percent worked as mechanics, repairers, or machinists; 12.5 percent were in clerical occupations; and less than 8 percent were in private household occupations (such as building and apartment maintenance, baby-sitting, and child care) or agricultural or forestry trades.

Thus, it appears that the modest program-control group differences in labor market activities that occurred during the initial postprogram period did not put the program group on a different earnings *trajectory* than the control group. Instead, the earnings differences faded as both groups gained job-specific skills through increased work experience. The earnings of both groups increased over time, but youths in both groups continued to have low-paying, intermittent jobs, as demonstrated by the very low earnings levels in the administrative records data (about \$9,000 per worker in 2000 and \$9,600 per worker in 2001, according to the SER data).

Another potential explanation for the diminishing impacts is that most program group members returned to their home communities after leaving Job Corps. At the 48-month follow-up interview point, about 73 percent of program group members (and 75 percent of control group members) lived less than 10 miles from where they lived at application to Job Corps. Thus, Job Corps does not have a large effect on student mobility to areas that offer opportunities different from those in areas where students come from. Consequently, the earnings impacts may have decreased because program and control group members tended to face the same labor market conditions and opportunities.

The control group also may have caught up because more program than control group members were enrolled in formal education and training programs in 1999 to 2001, due to additional schooling sought by former Job Corps participants.<sup>30</sup> In this case, the earnings impacts would be small temporarily because students typically have low earnings. Data are not available to test this explanation. However, we do not believe that the earnings impacts were greatly affected by differences across the research groups in school enrollment rates, because only about

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<sup>30</sup> Job Corps helps place terminating students in additional education and training programs.

13 percent of *both* the program and control groups were enrolled in school at 48 months after random assignment (and the school enrollment rates were similar by age group).

Note that the impacts in 1999 to 2001 may be somewhat reduced because some *control* group members enrolled in Job Corps. Control group members were not allowed to enroll in Job Corps for three years after random assignment, but they could enroll afterwards. Because of administrative error, about 1.4 percent of all control group members enrolled in Job Corps before their three-year restriction period. In addition, about 4.2 percent of control group members enrolled in Job Corps afterward (5.3 percent for those ages 16 and 17 at application to Job Corps, 3.2 percent for those ages 18 and 19, and 1.5 percent for those ages 20 to 24). To the extent that these control group members benefited from Job Corps, the impact estimates pertaining to 1999, 2000, and 2001 may be slightly biased downward.

As discussed in Schochet et al. (2001), it is possible to adjust for these control group “crossovers” by dividing the impacts per eligible applicant by the difference between the Job Corps enrollment rate for the program group and the crossover rate for the control group. 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 assigned to the control group. However, because the crossover rate is small and the earning impacts in 1999, 2000, and 2001 are very small, the adjustment procedure has little effect on the estimates. It does not change the overall conclusion that the impacts in these years are small and not statistically significant.

Importantly, we believe that it is too early to conclude that the zero earnings impacts will persist. Little research exists on the long-term earnings growth of youths similar to those served by Job Corps and on the long-term earnings impacts of youth training programs. Consequently, we cannot discount the possibility that positive earnings impacts might re-emerge. As discussed below, there is some evidence that beneficial earnings impacts persisted for those ages 20 to 24

at application to Job Corps, but not for those younger. Thus, participation in Job Corps may increase the long-term earnings of the younger students (who were only in their early to mid-20s in 2001) as they mature, find stable jobs, and experience the full benefits of program participation, both from increased vocational and academic skills gained in the program and from improved social skills and attitudes towards work. Furthermore, the literature on the economic returns to schooling, which formed the basis for our extrapolation assumption used in the benefit-cost analysis, typically focuses on earnings gains from additional schooling over a relatively *long* period, rather than over a very short one. This literature also focuses on nationally representative samples of the U.S. population and on traditional schooling. Thus, it is too early to conclude that this literature is not relevant to the type of training Job Corps offers and to the population Job Corps serves.

It is also possible that positive earnings impacts may re-emerge as economic conditions change. The National Job Corps Study was conducted during a period of strong economic growth, with low unemployment and inflation. Between 1995 and 2001, the unemployment rate for the civilian population of those age 16 and older ranged from 4 to 5.5 percent. The literature contains some evidence that those with lower skills benefit more from a strong economy (that is, a tight labor market) than those with higher skills (Hoynes 1999; and Katz and Krueger 1999). Thus, although both program and control group members earned low wages, the strong economy may have favored the lower-skilled control group. Earnings impacts, however, remained small in 2001 as the economy started to weaken and the employment rate decreased for both the program and control groups (Table III.1). Thus, the early evidence suggests that the long-term earnings impacts probably will remain small. However, additional follow-up data need to be collected before definitive conclusions can be reached.

At this juncture, however, we need to reassess the key assumption used in the benefit-cost analysis that earnings gains observed during the four-year survey period will persist without decline. In Chapter V, we recalculate program benefits based on assumptions about the decay in future earnings impacts that incorporate the impact findings using the administrative records data.

Finally, it is impossible to say whether estimated impacts based on survey data would have also disappeared in 1999, 2000, and 2001. Earnings levels are substantially higher according to the survey than administrative records data. Thus, if additional follow-up survey data had been collected, it is possible that the survey-based impact estimates may not have decayed as rapidly in 2000 and 2001 as those based on the administrative records data. We believe this is unlikely, however, for several reasons. First, the *pattern* of earnings impacts using the SER and survey data are similar during the period covered by the survey, which suggests that impact estimates using the survey data would have also decreased. Second, it would be difficult to interpret a finding of positive impacts according to the survey data, but not according to the administrative records data, because that would suggest that Job Corps participation has an effect on earnings from informal jobs that are not covered by the administrative records data, but no effect on earnings from formal jobs.

Clearly, we will never know the extent to which the survey-based impacts would have persisted. However, understanding the sources of differences between reported earnings levels in the administrative and survey data can help address this issue. We address this topic in Chapter IV.

## **B. IMPACT FINDINGS FOR SUBGROUPS**

A key finding in the main impact report is that beneficial survey-based earnings impacts in years 3 and 4 after random assignment were found broadly across most groups of students and

types of settings. Earnings gains very similar for males and females. Positive impacts were found for groups at special risk of poor outcomes (such as very young students, females with children, and older youths who did not have a high school credential at baseline). They were also found for groups at lower risk, such as older participants with a high school credential at baseline. Earnings gains were found for whites and African Americans, for residents and nonresidents, and for most groups defined by center characteristics (such as center size, type of operator, and performance level). No impacts, however, were found for Hispanic youths and 18- and 19-year-olds.

As discussed, no beneficial earnings impacts based on the SER data were found in 1999 to 2001 for the *full* sample. However, it is possible that the beneficial impacts found during the four-year survey period persisted for some population subgroups.

Table III.5 summarizes the impact findings for key subgroups of students using the survey and SER data, and Table III.6 presents results for subgroups defined by key center characteristics. For each subgroup, the tables present the ratio of average survey-to-SER earnings in calendar year 1998, SER-based estimated earnings impacts in calendar years 1998 to 2001, and survey-based estimated earnings impacts in calendar year 1998 and in year 4 after random assignment. Tables B.3 to B.16 display more complete subgroup results for the primary subgroups defined by gender, age, and residential status.

As we discuss next, the impact findings for subgroups are similar to those for the full sample, with the exception of the 20- to 24-year-olds.

## **1. Impact Findings for Subgroups During the Period Covered by the Survey**

In general, the pattern of the estimated subgroup impacts using the survey and SER data are similar in periods covered by both data sources. However, as with the full sample, the survey-

TABLE III.5

RATIO OF SURVEY-TO-SER MEAN EARNINGS AND EARNINGS IMPACTS  
ACCORDING TO THE SER AND SURVEY DATA, BY SUBGROUP

Subgroup	Ratio of Survey-to-SER Mean Earnings in 1998		Estimated Impacts on Earnings (in 1995 Dollars) <sup>a</sup>					Survey Data	
	Program Group	Control Group	1998	1999	2000	2001	1998	1998 Intake	Year 4 After Intake
Full Sample	1.77	1.67	219.8**	32.9	-17.4	7.0	971.6***	827.5***	
Age at Application									
16 to 17	2.11	1.94	154.2	-3.0	-47.0	-36.2	1,014.7**	694.0**	
18 to 19	1.66	1.65	60.2	-284.6	-341.7	-295.4	205.4	202.6	
20 to 24	1.60	1.46	475.9**	428.6*	374.8	397.0	1,777.5***	1,743.3***	
Gender									
Male	1.85	1.78	386.8***	127.0	-57.7	57.2	1,139.7***	920.7***	
Female	1.63	1.48	-10.6	-90.5	60.6	-51.2	775.0***	733.5***	
Race									
White, non-Hispanic	1.87	1.73	445.5**	13.1	123.8	233.9	1,713.9***	1,676.2***	
Black, non-Hispanic	1.81	1.72	259.2**	100.6	46.0	184.7	858.1***	864.2***	
Hispanic	1.57	1.51	-180.7	-285.8	-585.5	-742.0*	135.5	-568.5	
Arrest History									
Never arrested	1.68	1.61	291.5**	149.4	114.4	60.6	892.9***	806.3***	
Ever arrested for nonserious crimes only <sup>b</sup>	1.96	1.83	302.0	112.2	21.8	179.5	1,227.2***	824.7*	
Ever arrested for serious crimes <sup>b</sup>	2.34	1.91	-603.1	-1,151.3**	-1,777.0***	-1,354.6**	724.0	303.7	
Educational Level									
Had a high school diploma or GED	1.47	1.39	107.3	362.0	293.3	295.2	864.7**	1,139.4***	
Had neither	1.92	1.82	299.3***	3.1	-12.4	17.9	1,026.1***	740.5***	



TABLE III.5 (continued)

Subgroup	Ratio of Survey-to-SER Mean Earnings in 1998		Estimated Impacts on Earnings (in 1995 Dollars) <sup>a</sup>					Year 4 After Intake
	Program Group	Control Group	Annual Social Security Earnings Records					
			1998	1999	2000	2001	1998	
Residential Designation Status								
Residents								
All	1.80	1.70	232.3**	53.8	-36.2	40.5	1,010.2***	817.2***
Males	1.86	1.77	320.2**	53.9	-134.6	-5.2	1,124.4***	841.9***
Females without children	1.66	1.54	144.4	153.2	137.3	308.7	820.8**	759.8**
Females with children	1.77	1.43	-126.5	-207.3	153.4	-637.8	1,371.3**	1,021.9
Nonresidents								
All	1.60	1.52	149.9	-85.9	114.3	-182.9	729.9*	883.9*
Males	1.82	1.99	1,290.9***	1,113.6**	1,025.0	978.8	1,408.3	2,002.5**
Females without children	1.45	1.24	-1,242.9**	-1,401.4**	-323.0	-1,085.4	-303.1	-624.0
Females with children	1.53	1.42	258.7	-19.9	58.5	-0.6	1,018.4*	1,115.1*

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

Notes: All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline (for the survey data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>Serious crimes include murder, assault, robbery, and burglary. Nonserious crimes include larceny, vehicle theft, other property crimes, drug law violations, other personal crimes, and other miscellaneous crimes.

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

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

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

TABLE III.6

## RATIO OF SURVEY-TO-SER MEAN EARNINGS AND EARNINGS IMPACTS ACCORDING TO THE SER AND SURVEY DATA, FOR CENTER SUBGROUPS

Subgroup	Ratio of Survey-to-SER Mean Earnings in 1998		Estimated Impacts on Earnings (in 1995 Dollars) <sup>a</sup>					Survey Data	
	Program Group	Control Group	1998	1999	2000	2001	1998	1998	Year 4 After Intake
Full Sample	1.76	1.70	466.6***	242.5	169.5	269.4	1,115.6***	1,115.6***	824.4***
Center Type									
Contract centers	1.72	1.66	402.4***	251.8	216.7	120.3	1,041.7***	1,041.7***	954.8
CCC centers	1.83	1.80	453.2	120.5	81.9	514.4	1,019.9*	1,019.9*	670.4
Center Size									
Small centers (<226 slots)	1.81	1.79	219.7	175.0	105.5	316.0	536.2	536.2	583.7
Medium centers (226 to 495 slots)	1.71	1.66	614.5***	316.4	309.7	333.9	1,305.1***	1,305.1***	918.5***
Large centers (>495 slots)	1.78	1.62	250.2	-25.5	-77.9	-296.5	1,357.6***	1,357.6***	1,431.0***
Region									
1	1.67	1.47	-124.3	-445.7	-408.6	-307.8	1,090.9	1,090.9	189.4
2	1.93	1.73	491.6	957.9	911.8	985.8	1,916.8	1,916.8	787.3
3	1.89	1.71	-587.7*	-871.8**	-471.1	-761.1*	-153.5	-153.5	-170.4
4	1.78	1.70	632.3**	776.5***	627.3*	538.6	1,523.3***	1,523.3***	1,542.5***
5	1.96	1.81	584.5*	21.7	-59.4	559.3	1,933.1***	1,933.1***	1,738**
6	1.64	1.74	685.9**	265.3	427.2	451.9	642.8	642.8	607.1
7/8	1.73	1.74	136.9	-13.6	-240.3	48.9	162.9	162.9	580.0
9	1.58	1.66	583.6	524.8	538.5	330.2	471.4	471.4	247.8
10	1.68	1.63	948.0*	28.4	-319.1	-186.8	1,870.7**	1,870.7**	1,271.7
Performance Level									
High	1.65	1.64	554.8	71.5	-212.4	-273.1	929.7	929.7	1,087.8*
Medium	1.75	1.68	480.1***	163.3	158.1	245.3	1,218.7***	1,218.7***	880.5***
Low	1.92	1.85	76.7	496.5	563.6	605.3	491.1	491.1	669.8

TABLE III.6 (continued)

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

Notes: 1. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline (for the survey data).

2. Impact estimates for the center subgroups were obtained by assigning equal weight to each *center* (that is, the center rather than the eligible applicant was the unit of analysis). Thus, the full sample results displayed in the table differ from the full sample results displayed in previous tables.

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>Serious crimes include murder, assault, robbery, and burglary. Nonserious crimes include larceny, vehicle theft, other property crimes, drug law violations, other personal crimes, and other miscellaneous crimes.

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

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

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

based impact estimates are larger and more often statistically significant, which results in several notable differences between the subgroup findings using the two data sources.

In particular, the impact estimates in 1998 for those ages 16 and 17 at program application and for females are statistically significant according to the survey data but not according to the SER data. A possible explanation for the 16- and 17-year-olds is that these youths were more likely than those older to have earnings from jobs not covered in the SER data. This hypothesis is supported by the fact that the survey-to-SER earnings ratios are larger for the 16- and 17-year-olds than for older sample members, perhaps because these youths were more likely to hold informal (cash-only) jobs (see Chapter IV). The findings for females are puzzling, because the survey-to-SER earnings ratios are smaller for females than males.

Consistent with the full-sample findings, the subgroup impact estimates measured in nominal terms are also larger using the survey than SER data, because average reported earnings levels are larger in the survey data for *every* subgroup. Interestingly, the survey-to-SER earnings ratios are largest for the 16- and 17-year-olds (as already noted) and for those who were arrested prior to random assignment. The ratios are also larger for males than females and for whites and African Americans than Hispanics.

We also find that the survey-to-SER earnings ratios are larger for the program group than the control group for all subgroups except one. Consequently, the survey-based impact estimates in *percentage* terms are also consistently larger than the SER-based ones during comparable periods (not shown).

## **2. Impact Findings for Subgroups After the Period Covered by the Survey**

We do not find any beneficial SER-based earnings impacts in 2000 and 2001 that are statistically significant for any subgroup. There is some evidence, however, of positive earnings gains for those ages 20 to 24 and those with a high school credential at program application.

The findings for 20- to 24-year-olds are consistent with previous project findings. Older students remain in Job Corps longer than younger ones. Furthermore, our interviews with program staff indicate that the 20- to 24-year-olds are more highly motivated and well-behaved than those younger. Moreover, younger students exhibit several baseline characteristics that suggest they are more disadvantaged and harder to serve than older students. In the baseline interview, a higher proportion of younger students reported having used drugs, having ever been arrested, living in single-parent households, and coming from families that receive public assistance. The younger students also enter the program with lower education levels (about 50 percent of those age 20 to 24 have a high school credential, compared to only 13 percent of those younger). Moreover, the estimated impacts on total hours spent in education and training during the four-year survey period are *larger* for the older students than the younger ones, because older students stayed longer in Job Corps, and because a large percentage (nearly half) of the younger control group members attended high school after being rejected for Job Corps (Schochet et al. 2001). Finally, the older Job Corps students typically received more hours of vocational training while in the program than the younger ones, which may have led to more beneficial employment outcomes for the older students.

Thus, it is not surprising that the earnings gains were larger and more persistent for the 20- to 24-year-olds than for those younger (Tables B.3 to B.5). Positive earnings gains were found for the 16- and 17-year-olds soon after they left the program, but these initial gains soon disappeared. No earnings gains were ever found for those ages 18 and 19. Thus, Job Corps seems to be most effective for the oldest group of students.

The positive findings for those with a high school credential at baseline are consistent with the findings for the 20- to 24-year-olds, because of the considerable overlap between youths in

these two subgroups. About 60 percent of those who completed high school or had earned a GED at baseline were ages 20 to 24 at baseline.

We also find positive impact findings for the small sample of male nonresidents (Tables III.5 and B.14). However, we believe that these findings are anomalous, for several reasons. First, the mean SER earnings of the program group were significantly higher than those of the control group in the period prior to random assignment (that is, in 1993 and 1994; Table B.14). If we net out this preexisting difference, the impact estimates for the nonresidential males become smaller and statistically insignificant. Second, previous studies (for example, the JTPA and JOBSTART evaluations) have found that disadvantaged male youths do not experience earnings gains from participation in education and job-training programs that offer services in a nonresidential setting. Thus, we believe that our findings for male nonresidents are due to small sample sizes.

We find a similar pattern of findings for subgroups defined by center characteristics as for the youth subgroups (Table III.6).<sup>31</sup> While there is some evidence of positive earnings gains for those in medium-sized centers, in regions 2, 4, 6, and 9, and in low-performing centers, the earnings gains across subgroups generally decreased from 1998 to 2001.

In sum, the results suggest, that based on the administrative records data, the beneficial subgroup impacts found in 1997 and 1998 did not persist in 1999, 2000, and 2001 for most subgroups, with the exception of the 20- to 24-year-olds. However, longer-term administrative data will need to be collected before firm conclusions can be reached about the long-term effects

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<sup>31</sup> We obtained impact estimates for the center subgroups by assigning equal weight to each *center* (that is, the center rather than the eligible applicant was the unit of analysis). Thus, the full sample results displayed in Table III.6 differ from the full sample results displayed in previous tables.

of Job Corps on key subgroups. It still remains to be determined whether the positive earnings gains for the 20- to 24-year-olds will persist and whether earnings gains for the younger sample members will emerge as they reach their later 20s and beyond.





#### IV. RECONCILING DIFFERENCES BETWEEN FINDINGS USING THE SURVEY DATA AND ADMINISTRATIVE RECORDS DATA

As discussed in the last chapter, the pattern of impact findings using the administrative and survey data are similar in periods covered by both data sources. However, the estimated impacts are larger using the survey data. To what broad factors can these differences be attributed?

First, the differences can be attributed to reporting differences between the two data sources. Using comparable samples, average reported earnings levels in the survey data are nearly double the levels reported in the SER and UI data. Thus, the impacts—in *nominal* terms—using the survey data are much larger than those using the administrative records data. In addition, the reporting differences are, on average, slightly larger for the program than control group, which contributes to the larger impacts in *percentage* terms using the survey data.

Second, the differences in the findings using the two data sources can be attributed to interview nonresponse bias. The impacts using the SER data are larger for survey respondents (about 80 percent of the sample) than for survey nonrespondents, which results in impact estimates based on the survey sample that are somewhat biased upward.

We estimate that about half the difference between the percentage earnings gains in 1998 based on the survey and SER data is due to reporting differences and about half is due to interview nonresponse bias.

This chapter examines further the sources of differences between the survey-based and administrative-based estimates. The purpose of this analysis is to help further understand the impact findings discussed in the previous chapter. The chapter contains two sections. First, we examine sources of reporting differences between the two data sources. Second, we examine potential reasons for interview nonresponse bias.

## **A. EXAMINING REPORTING DIFFERENCES**

There are several possible explanations for the higher reported earnings levels in the survey data than in administrative records data. First, informal and some formal jobs are not covered by the administrative records data but may be captured in the survey data. Second, some survey respondents may have over-reported their earnings and employment levels due to recall error or other reasons. Third, some employers may have inaccurately reported (or not reported) sample members' earnings to the government. Finally, the administrative records data may have missed earnings from sample members with SSNs (or other identifying information) that were incorrectly reported by employers or sample members.

To examine these explanations fully, it would be necessary to conduct a supplemental study to obtain detailed job information for sample members whose reported earnings levels differ substantially in the survey and administrative data. These interviews would collect information that, for example, could be used to distinguish between earnings from formal and informal jobs and from regular and overtime hours. Interviews with some of the youths' employers would also shed light on the earnings that employers reported to the government, as well as on potential discrepancies between hourly wages and hours worked from the perspective of sample members and their employers. Data would be collected from structured interviews and from focus groups.

Such a study is beyond the scope of this evaluation. Instead, to examine reasons for the reporting differences, we use available job information from the 48-month follow-up surveys. As discussed in Chapter II, these survey data contain some information on jobs that sample members held during the follow-up period. The survey, however, was not structured to gather sufficiently detailed information on jobs to determine earnings accurately from jobs that were and were not likely to have been reported to the government. Thus, our analysis is somewhat limited by data constraints. However, it provides important insights into the reasons that earnings

levels are so much higher in the survey than administrative data. These results can help guide future research.

We conducted our analyses using the administrative and survey data in periods covered by both data sources, and using comparable samples of youths. We compared earnings and employment in 1998 as measured by the SER and survey data using the sample of those who completed the 48-month interview. For the analysis using UI data, we compared earnings in quarter 16 after random assignment as measured by the UI and survey data using the sample of those who (1) completed the 48-month interview, (2) lived in the 22 UI states for the entire four-year period after random assignment, and (3) did not work elsewhere.

This section contains three parts. First, we provide descriptive information on the distribution of *individual* differences in earnings and employment as measured by the survey and administrative data. This analysis builds on the aggregate-level analysis presented in the previous chapter. Second, we attempt to identify reasons for the earnings differences in the survey and UI data. In particular, we compare the characteristics of jobs reported in the survey data *only* to those reported in *both* the survey and UI data, to help understand why quarterly employment rates are about 13 percentage points higher using the survey data. We also examine the extent to which differences in earnings per job across the two data sources vary by key job characteristics, to help explain why earnings levels are much higher in the survey than administrative data, even for those with the same number of reported jobs in both data sources. Finally, we summarize our results.

### **1. Distribution of Individual Differences in Reported Employment Status, Number of Jobs, and Earnings**

In this section, we present results examining individual differences in labor market outcomes as measured by the survey and administrative data. Earnings differences between the two data

sources can be attributed to differences in (1) reported employment rates, (2) the number of jobs per worker, and (3) earnings per job. Thus, we discuss each of these components in turn, where we address the following questions:

- ***What is the overlap in employment status using the survey and administrative records data?*** Were the same people employed according to both data sources?
- ***Did the number of reported jobs differ by data source?*** How often was the number of reported jobs greater in the survey than UI data?
- ***What is the distribution of individual earnings differences using the survey and administrative records data?*** Are earnings differences due to a small number of people who reported much higher earnings in the survey, or are they more common? To what extent can differences in overall reported earnings levels in the survey and UI records data be explained by (1) higher employment levels reported in the survey, (2) more jobs per worker reported in the survey, and (3) higher reported earnings per job in the survey?
- ***To what extent do answers to these questions differ for the program and control groups?*** Why are the ratios of survey-based to administrative-based earnings levels slightly higher for the program than control group?

#### **a. Differences in Reported Employment Status**

**Results Using the SER Data.** As discussed in Chapter III, the overall annual employment rate in 1998 is slightly higher according to the SER data than the survey data for both research groups. For the program group, the employment rate is 86 percent using the SER data and 81 percent using the survey data, and for the control group, it is 84 percent using the SER data and 79 percent using the survey data (Table III.3). Consequently, the higher average earnings levels reported in the survey than SER data are due to higher earnings per worker rather than to higher employment rates in the survey data.

Although the estimated annual employment rates in 1998 are similar using the survey and SER data, it may not be the case that the *same* youths were employed according to both data sources. We do find, however, considerable overlap in the youths' employment status across the two data sources (Table IV.1). In 1998, about 75 percent were employed and 10 percent were not

TABLE IV.1

OVERLAP IN EMPLOYMENT STATUS IN 1998 AS REPORTED IN THE SURVEY AND SER DATA,  
 BY RESEARCH STATUS AND SUBGROUP  
 (Percentages)

Subgroup	Employed in 1998 According to Survey Data/According to SER Data				Yes/Yes or No/No
	Yes/Yes	Yes/No	No/Yes	No/No	
Full Sample					
Program group	76.3	5.0	9.7	9.0	85.3
Control group	73.9	5.2	10.7	10.2	84.1
Males					
Program group	77.4	5.4	9.4	7.8	85.2
Control group	75.5	5.4	10.0	9.2	84.7
Females					
Program group	75.1	4.5	10.1	10.3	85.4
Control group	71.3	4.9	12.0	11.8	83.1
16 to 17 at Application					
Program group	72.6	5.7	11.5	10.2	82.8
Control group	72.0	5.4	11.8	10.8	82.8
18 to 19 at Application					
Program group	76.5	5.0	9.2	9.3	85.8
Control group	74.4	5.6	9.7	10.4	84.8
20 to 24 at Application					
Program group	81.4	4.0	7.8	6.8	88.2
Control group	76.3	4.4	10.3	9.1	85.4
White, Non-Hispanic					
Program group	82.3	4.4	8.1	5.2	87.5
Control group	81.9	3.5	8.6	6.0	87.9
Black, Non-Hispanic					
Program group	73.7	5.2	10.6	10.5	84.2
Control group	69.0	6.0	12.9	12.1	81.1
Hispanic					
Program group	75.9	4.7	9.6	9.9	85.8
Control group	76.0	5.3	8.4	10.4	86.4
<b>Full Sample Size (Program/Control)</b>	<b>6,772/4,451</b>	<b>6,772/4,451</b>	<b>6,772/4,451</b>	<b>6,772/4,451</b>	<b>6,772/4,451</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews, and 1998 social security earnings records for those who completed 48-month interviews.

Note: All figures are unweighted.

employed according to both data sources, resulting in an “*agreement rate*” of about 85 percent.<sup>32</sup> About 5 percent were employed according to the survey data only, and about 10 percent were employed according to the SER data only. Stated differently, about 94 percent of those who were employed based on the survey data were also employed according to the SER data, and about 88 percent of those who were employed according to the SER data were also employed according to the survey data.

These high agreement rates suggest that both data sources captured the broad employment experiences of the sample. They also reinforce our conclusion that the correct SSNs of our sample members were matched to the SER data system.

The overall agreement rate is similar for the program and control groups, although it is slightly higher for the program group (85.3 percent program, 84.1 percent control; Table IV.1). In addition, the agreement rate does not differ substantially by gender, age, and race and ethnicity. It is slightly higher, however, for those 20 to 24 years old at program application (who were 23 to 27 years old in 1998) than for those younger, and for whites than their counterparts. The results for the 20- to 24-year-olds are not surprising, because these people typically had more stable, higher-paying jobs than younger workers and thus were more likely to have earnings reported in the administrative records data.

**Results Using the UI Data.** As discussed, the employment rate in quarter 16 after random assignment is substantially higher according to the survey than UI data (Table III.4). For the program group, the employment rate is about 71 percent using the survey data, compared to only 58 percent using the UI data. For the control group, it is 69 percent using the survey data,

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<sup>32</sup> The expected agreement rate would be about 73 percent if employment status in the survey data were independent of employment status in the SER data.

compared to only 55 percent using the UI data. Thus, quarterly earnings differences between the survey and UI data are due partly to higher quarterly employment rates using the survey data.

It is not possible to directly compare *annual* employment rates using the survey and UI data, because the periods the two data sources cover do not overlap enough. However, there is indirect evidence that the annual employment rates using the two data sources are similar. First, the annual employment rate in 1998 using the survey data is similar to the annual employment rate in 1999 using the UI data (Table III.1). Second, the difference between the combined *six-month* employment rate in quarters 15 and 16 (the maximum period of overlap) using the survey and UI data is much smaller than the difference between the two employment rates in quarter 16 only, which suggests that the difference in annual employment rates would be even smaller.

Our finding that quarterly employment rates are higher using the survey than using administrative data, but that annual employment rates are similar, suggests that the administrative data did not capture earnings from short-term informal (second) jobs held by the sample. These findings also suggest that sample members reported during the survey that they were employed in jobs longer than they actually were (perhaps because of recall error). The reporting differences may also be due in part to errors in matching our sample members to the state's UI data systems because of inaccurate SSNs.

The *agreement rate* in employment status in quarter 16 according to the survey and UI data is about 70 percent (Table IV.2). According to both data sources, about 50 percent of sample members were employed and 20 percent were not.<sup>33</sup> This figure is lower than the 85 percent agreement rate for annual earnings using the SER data. However, about 85 percent of workers in

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<sup>33</sup> If employment status in the survey data were independent of employment status in the UI data, the expected agreement rate would be about 53 percent.

TABLE IV.2

OVERLAP IN EMPLOYMENT STATUS IN QUARTER 16 AFTER RANDOM ASSIGNMENT AS  
 REPORTED IN THE SURVEY AND UI DATA, BY RESEARCH STATUS AND SUBGROUP  
 (Percentages)

Subgroup	Employed in Quarter 16 According to Survey Data / According to UI Data				Yes/Yes or No/No
	Yes/Yes	Yes/No	No/Yes	No/No	
Full Sample					
Program group	50.0	21.1	8.8	20.1	70.1
Control group	47.1	22.0	9.0	21.9	69.0
Males					
Program group	47.4	23.8	9.2	19.6	67.0
Control group	45.8	24.5	9.4	20.3	66.1
Females					
Program group	52.9	18.0	8.4	20.7	73.6
Control group	49.1	18.4	8.3	24.2	73.3
16 to 17 at Application					
Program group	45.0	21.1	9.9	23.9	69.0
Control group	44.5	23.4	8.6	23.5	68.0
18 to 19 at Application					
Program group	51.9	20.4	8.2	19.5	71.3
Control group	49.6	20.7	8.7	21.0	70.5
20 to 24 at Application					
Program group	54.5	21.7	8.0	15.8	70.3
Control group	48.4	21.4	9.7	20.4	68.8
White, Non-Hispanic					
Program group	53.5	23.8	8.4	14.3	67.9
Control group	53.6	22.3	8.0	16.1	69.7
Black, Non-Hispanic					
Program group	50.7	17.0	9.7	22.6	73.3
Control group	45.0	18.3	11.4	25.3	70.4
Hispanic					
Program group	41.4	30.2	7.0	21.4	62.8
Control group	45.7	30.9	3.9	19.6	65.3
<b>Full Sample Size (Program/Control)</b>	<b>2,256/1,508</b>	<b>2,256/1,508</b>	<b>2,256/1,508</b>	<b>2,256/1,508</b>	<b>2,256/1,508</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview; (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures are unweighted.



the UI wage records were also employed according to the survey data. Thus, there is considerable overlap in employment status using the survey and UI data *among* those identified as workers in the UI wage records.

Finally, the quarter 16 agreement rate is similar for the program and control groups (Table IV.2). Furthermore, the agreement rate does not differ substantially across subgroups, although it is slightly lower for males than females and for Hispanics than whites and African Americans.

**Summary.** In sum, we find using the annual SER data that most sample members who were workers according to survey data were also workers according to the SER data. There is less overlap in employment status in quarter 16 according to the survey and UI data, because the quarterly employment rate is much higher using the survey than UI data. However, most workers in the UI data are also considered to be workers in the survey data. Finally, the overlap in employment status is very similar for program and control group members and for key subgroups. There is some evidence, however, that agreement rates are slightly higher for older sample members than younger ones (according to the SER data) and for females than males (according to the UI data).

#### **b. Differences in the Reported Number of Jobs**

The higher reported earnings levels in the survey than UI data may be due not only to differences in reported employment rates, but also to differences in the reported number of jobs held by workers. The UI wage records contain earnings for each reported *job* held by sample members. Thus, we compared the number of jobs held by sample members in quarter 16 using the survey and UI data. The sample for this analysis included those who were employed according to *both* data sources.

Substantially more jobs are reported in the survey data than in the UI data (Table IV.3). The average number of jobs held per worker is 1.2 jobs according to the survey data, compared to 1.0 job according to the UI data. Similarly, for about 20 percent of workers, the number of jobs reported in the survey is greater than the number reported in the UI wage records. More jobs are reported in the UI data for only about 2 percent of workers. These figures are very similar by research status and subgroup.

These findings suggest that a nontrivial percentage of sample members had earnings from second jobs that were not reported to the state UI agencies, and that these jobs accounted for some of the differences in earnings levels as measured by the survey and UI data.

### **c. Differences in Reported Earnings**

Table IV.4 displays summary statistics for differences between *individual* earnings as reported in the survey and administrative data. We display mean differences, the standard deviation of the differences, and percentiles of the distribution of differences. We also display mean earnings *levels* as measured by the administrative data, so that the earnings differences can be put into percentage terms. The figures are presented for the full sample, for those employed according to both data sources, and for workers with the same number of reported jobs. The statistics are presented separately for the program and control groups. Because the goal of this descriptive analysis is to examine reporting differences at the individual level, sample weights were *not* used in the analysis.

Earnings levels are substantially higher using the survey data than the administrative records data for most sample members (Table IV.4). For the full sample, mean annual earnings in 1998 are about \$3,500 to \$4,000 higher according to the survey data than the SER data, and the differences are about \$4,700 to \$5,000 using the sample of those who were employed according to *both* data sources. Similarly, mean earnings in quarter 16 are about \$1,200 higher in the

TABLE IV.3

NUMBER OF JOBS WORKED IN QUARTER 16 ACCORDING TO THE SURVEY AND UI DATA, AMONG  
THOSE EMPLOYED ACCORDING TO BOTH DATA SOURCES,  
BY RESEARCH STATUS AND SUBGROUP

Subgroup	Average Number of Jobs Worked		Difference Between the Number of Jobs Reported in the Two Data Sources (Percentages)		
	Survey Data	UI Data	Same Number in Both Data Sources	More in Survey Data	More in UI Data
<b>Full Sample</b>					
Program group	1.2	1.0	78.9	19.7	1.4
Control group	1.2	1.0	76.9	21.1	1.9
<b>Males</b>					
Program group	1.2	1.0	79.5	18.8	1.7
Control group	1.3	1.0	75.0	22.9	2.1
<b>Females</b>					
Program group	1.2	1.0	78.1	20.8	1.1
Control group	1.2	1.0	79.7	18.6	1.7
<b>16 to 17 at Application</b>					
Program group	1.2	1.0	78.7	19.7	1.6
Control group	1.3	1.0	76.7	22.9	0.4
<b>18 to 19 at Application</b>					
Program group	1.2	1.0	81.6	17.0	1.4
Control group	1.2	1.0	76.5	20.0	3.5
<b>20 to 24 at Application</b>					
Program group	1.3	1.0	76.3	22.6	1.1
Control group	1.2	1.0	77.8	20.0	2.2
<b>White, Non-Hispanic</b>					
Program group	1.3	1.0	74.2	24.2	1.7
Control group	1.3	1.0	74.1	25.9	0.0
<b>Black, Non-Hispanic</b>					
Program group	1.2	1.0	78.4	19.9	1.7
Control group	1.3	1.0	74.6	22.7	2.7
<b>Hispanic</b>					
Program group	1.2	1.0	84.3	15.1	0.5
Control group	1.1	1.0	83.4	13.8	2.8
<b>Full Sample Size (Program/Control)</b>	<b>1,128/711</b>	<b>1,128/711</b>	<b>1,128/711</b>	<b>1,128/711</b>	<b>1,128/711</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) were employed according to both data sources; (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures are unweighted.

TABLE IV.4

## SUMMARY STATISTICS FOR DIFFERENCES BETWEEN EARNINGS AS REPORTED IN THE SURVEY AND ADMINISTRATIVE DATA, BY RESEARCH STATUS

Summary Statistic (Unweighted)	Difference Between Survey and SER Earnings in 1998 (in 1995 Dollars)		Difference Between Survey and UI Earnings in Quarter 16 After Random Assignment (in 1995 Dollars)	
	Program Group	Control Group	Program Group	Control Group
<b>Full Sample</b>				
(Mean SER or UI Earnings <i>Level</i> )	(5,825.1)	(5,636.7)	(1,432.2)	(1,417.2)
Mean Earnings Difference	3,983.8	3,501.3	1,287.1	1,139.8
Standard Deviation of Difference	8,111.6	7,670.4	2,606.1	2,645.2
Percentile of the Distribution of Differences				
10th	-2,149.9	-2,246.9	-647.7	-694.9
25th	0.0	-45.5	0.0	0.0
50th	1,859.6	1,559.0	468.6	368.4
75th	6,390.8	5,827.0	2,376.7	2,241.5
90th	13,437.8	12,438.3	4,414.9	4,307.4
<b>Sample Who Were Employed According to Both Data Sources</b>				
(Mean SER or UI Earnings <i>Level</i> )	(7,428.8)	(7,155.9)	(2,640.1)	(2,709.6)
Mean Earnings Difference	5,057.8	4,682.9	1,268.5	1,062.2
Standard Deviation of Difference	7,848.7	7,944.2	2,464.3	2,214.3
Percentile of the Distribution of Differences				
10th	-1,411.4	-1,713.2	-610.8	-747.8
25th	602.3	466.3	42.2	33.8
50th	3,247.2	2,938.1	850.4	794.5
75th	7,733.8	7,397.6	2,105.9	2,000.7
90th	14,193.1	13,761.4	3,868.3	3,334.6
<b>Sample Who Were Employed and Had the Same Number of Jobs According to Both Data Sources</b>				
(Mean SER or UI Earnings <i>Level</i> )	NA	NA	(2,646.3)	(2,709.3)
Mean Earnings Difference	NA	NA	1,084.4	926.3
Standard Deviation of Difference	NA	NA	2,340.8	1,859.9
Percentile of the Distribution of Differences				
10th	NA	NA	-644.5	-747.8
25th	NA	NA	-4.2	-27.9
50th	NA	NA	684.8	564.1
75th	NA	NA	1,779.0	1,658.9
90th	NA	NA	3,350.9	2,917.7
<b>Sample Size</b>	<b>6,772</b>	<b>4,451</b>	<b>2,645</b>	<b>1,737</b>

Source: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) 1998 social security earnings records for those who completed 48-month interviews; and (3) quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states using those who signed the records release consent form, lived in the 22 states for the entire 48 months after random assignment, and did not work outside the 22 states.

Notes: All figures are unweighted.

NA = Not applicable because the SER data do not contain earnings from individual jobs, but only combined earnings from all jobs.

survey than UI data for each of the analysis samples.<sup>34</sup> Median differences are smaller than mean differences, because reporting differences are large and positive for a substantial fraction of sample members (that is, the distribution of differences is skewed to the right).

Most important, we find that survey-based earnings per job are larger than administrative-based earnings for about *75 percent of workers*. This result is similar for the program and control groups. Thus, differences in reported earnings are *common*. They are not due to a small number of people who reported much higher earnings in the survey.

#### d. Decomposition of UI and Survey Earnings Differences into Their Component Parts

Differences in quarter 16 earnings levels based on the survey and UI data can be decomposed into differences due to (1) earnings per job, (2) the number of jobs per worker, and (3) employment levels. To calculate the relative contribution of each of these three components, we express the overall mean difference in survey-based and UI-based earnings as follows:

$$(3) \quad (\bar{E}_S - \bar{E}_U) = \frac{\bar{E}_S}{\bar{J}_S} \frac{\bar{J}_S}{\bar{W}_S} \bar{W}_S - \frac{\bar{E}_U}{\bar{J}_U} \frac{\bar{J}_U}{\bar{W}_U} \bar{W}_U,$$

where  $E_S$  is mean earnings using the survey data,  $E_U$  is mean earnings using the UI data, the  $J_i$  ( $i = S, U$ ) variables represent the mean number of reported jobs from the two data sources, and the  $W_i$  variables represent employment rates according to the survey and UI data. After adding and subtracting relevant terms, the overall mean earnings difference can be expressed as a weighted sum of the ratios in the right-hand side of equation (3) as follows:

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<sup>34</sup> The full sample figures are similar to the figures for the sample of workers only. This is because in the calculation of the full sample figures, the positive earnings differences for the 20 percent of the sample who are workers according to the survey but not the UI data are offset by the zero differences for the 20 percent who are not workers according to both data sources.

$$(4) \quad (\bar{E}_S - \bar{E}_U) = \left( \frac{\bar{E}_S}{\bar{J}_S} - \frac{\bar{E}_U}{\bar{J}_U} \right) p_1 + \left( \frac{\bar{J}_S}{\bar{W}_S} - \frac{\bar{J}_U}{\bar{W}_U} \right) p_2 + (\bar{W}_S - \bar{W}_U) p_3,$$

where,

$$(5) \quad p_1 = \frac{\bar{J}_S}{\bar{W}_S} \bar{W}_S, \quad p_2 = \frac{\bar{E}_U}{\bar{J}_U} \bar{W}_U, \quad p_3 = \frac{\bar{E}_U}{\bar{J}_U} \frac{\bar{J}_S}{\bar{W}_S}.^{35}$$

Equations (4) and (5) can then be used to decompose overall mean earnings differences into its component parts.

We find that *all* three components play an important role in explaining the overall mean earnings difference as measured by the survey and UI data, and this important result holds for both the program and control groups (Table IV.5). For the program group, about 45 percent of the overall mean earnings difference is due to differences in mean earnings per job, 25 percent is due to differences in the mean number of jobs per worker, and 30 percent is due to differences in employment rates. The corresponding figures for the control group are 39, 24, and 38 percent, respectively. Thus, the earnings per job component is a somewhat more important factor for the program than control group, whereas the employment rate component is a somewhat more important factor for the control group.

All three factors also play an important role in explaining the overall earnings differences across key subgroups, although there are some notable differences by age and race and ethnicity (Table IV.5). The earnings per job component is less important for the older sample members

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<sup>35</sup> Equation (5) displays one of six possible combinations of  $p_1$ ,  $p_2$ , and  $p_3$  that satisfy equation (4). Our decomposition results discussed below are based on the average of the results based on each of the six combinations. (Each combination, however, produced very similar results.)

TABLE IV.5

DECOMPOSITION OF THE DIFFERENCE IN MEAN EARNINGS IN QUARTER 16 BASED ON  
THE SURVEY AND UI DATA, BY RESEARCH STATUS AND SUBGROUP

Subgroup	Percentage of the Difference in Mean Earnings in Quarter 16 Based on the Survey and UI Data That Is Due to Differences in:		
	Employment Rates	Mean Number of Jobs Per Worker	Mean Earnings Per Job
Full Sample			
Program group	30	25	45
Control group	38	24	39
Males			
Program group	32	21	47
Control group	37	21	42
Females			
Program group	28	33	39
Control group	43	31	27
16 to 17 at Application			
Program group	26	21	53
Control group	35	21	44
18 to 19 at Application			
Program group	31	21	48
Control group	32	23	44
20 to 24 at Application			
Program group	36	33	31
Control group	49	26	25
White, Non-Hispanic			
Program group	32	22	46
Control group	33	31	36
Black, Non-Hispanic			
Program group	19	27	54
Control group	27	29	44
Hispanic			
Program group	61	21	17
Control group	62	9	28
<b>Full Sample Size (Program/Control)</b>	<b>2,754/1,872</b>	<b>2,754/1,872</b>	<b>2,754/1,872</b>

Source: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) 1998 social security earnings records for those who completed 48-month interviews; and (3) quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states using those who signed the records release consent form, lived in the 22 states for the entire 48 months after random assignment, and did not work outside the 22 states.

Notes: All figures were calculated using sample weights to adjust for the sample and survey designs.

than the younger ones, perhaps because the older sample members were more likely to work jobs with stable hours. In addition, the earnings per job component is much less important for Hispanics than the employment rate component, and the reverse holds for African Americans.

## **2. Explanations for Higher Employment Rates in the Survey Data than in the UI Data**

We have shown that estimated employment levels in quarter 16 are about 13 percentage points higher according to the survey than UI data for both the program and control groups. What caused these reporting differences?

We have already discussed two partial explanations. First, the UI-based employment rates could be lower because of financial incentives for employers to underreport earnings to state UI programs. Second, the UI-based rates could be lower because of errors in matching our sample members to the states' UI data systems. These explanations are supported somewhat by the figures in Table III.1, which show that annual employment rates in 1999, 2000, and 2001 are about 5 percentage points higher according to the SER data than the UI data, even though job coverage is similar for the OASDI and UI programs. These explanations, however, account for only a portion of the difference between the quarter 16 employment rate as measured by the survey and UI data.

We offer two other possible explanations for the lower UI-based employment rate. First, some *formal* jobs are not covered under the UI program. Hence, the earnings of sample members who held such jobs may not have been reported to the UI agencies but may have been reported in the survey. Second, *informal* jobs are not covered under the UI program, although earnings from these casual or cash-only jobs may have been reported in the survey.

Next, we examine these two hypotheses in more detail. Specifically, we address the following questions:



- *To what extent are higher employment rates in the survey data due to formal jobs that are not covered by the UI program?* Does the overlap in employment status using the survey and UI data (that is, agreement rates) differ by occupation?
- *To what extent are higher employment rates in the survey data due to informal jobs?* What are the characteristics of jobs (such as job tenure, hourly wages, weekly earnings, usual hours worked, available fringe benefits) reported in the survey data but not in the UI data? What are the demographic characteristics and employment experiences of those who held these informal jobs?
- *To what extent do the answers to these questions differ for the program and control groups?* Is there any evidence that the program group was more likely than the control group to have held informal jobs and formal jobs not covered by the UI program?

**a. Examining Formal Jobs Not Covered in the UI Wage Records**

The UI wage records do not cover workers in some formal jobs. These workers include federal workers, military staff, self-employed people, agricultural labor (except workers on large farms), and domestic service workers (whose employers paid less than \$1,000 in wages in any calendar quarter).<sup>36</sup>

About 17 percent of workers in both the program and control groups reported in the survey that they worked in these “uncovered” sectors during their most recent jobs in quarter 16. These workers were identified using survey information on reported job occupations (which were open-ended responses coded into three-digit SOC codes) and on reported types of employers.<sup>37</sup> About 2 percent of workers in the survey sample reported working for the federal government, about 1 percent were in the military, 5 percent were self-employed, 2 percent worked in agricultural occupations, 7 percent worked in private household occupations, and 1 percent worked without

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<sup>36</sup> Federal workers and military staff are eligible to receive UI benefits. Their earnings are not reported to state UI agencies, however, and so are not in the UI wage records.

<sup>37</sup> The type of employer data provided information on whether the respondent was working for a private company; on active military duty; a federal, state, or local government employee; self-employed; working without pay or in a family business or as a favor, or other.

pay (see columns 2 and 4 in Table IV.5).<sup>38</sup> The presence of these “uncovered” workers would completely explain the gap between the survey-based and UI-based employment rates if earnings for these workers were not reported in the UI data (and if earnings for all other workers were reported in the UI data).

Before proceeding, we note that, based on national data, we expect that some sample members in these “uncovered” jobs were actually covered by the UI program. UI wage records cover about 94 percent of workers nationally (U.S. General Accounting Office 2002), but U.S. workers in the “uncovered” sectors described above comprise more than 6 percent of all U.S. workers.<sup>39</sup> Thus, some of these “uncovered” U.S. workers must have actually been covered by the UI program. For example, some farmers and domestic workers are covered by the UI program, although it is not possible to determine from published statistics (or our survey data) the number of such workers. Furthermore, there is often ambiguity about reported self-employment status.

To examine the extent to which uncovered formal jobs explain the lower UI-based than survey-based employment rate, we calculated the percentage of workers according to the survey data who were also workers according to the UI data (that is, *agreement rates* for survey-based workers) by occupation, type of employer, and research status. Table IV.6 presents these figures for the full sample, and Table C.1 presents them for subgroups.

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<sup>38</sup> The sum of the percentages in each type of uncovered job (19.3 percent) is greater than the 17 percent figure who worked in any of them, because some youths who worked in private household and agriculture occupations reported that they were self-employed.

<sup>39</sup> In 1999, 2.1 percent of all workers nationally reported working for the federal government, 7 percent were self-employed, 3.5 percent worked in agricultural-related occupations, and 1 percent worked in private household occupations (Statistical Abstract of the United States 2000).

TABLE IV.6

AGREEMENT RATES BETWEEN THE SURVEY AND UI DATA BY OCCUPATION AND TYPE OF  
EMPLOYER IN THE MOST RECENT JOB HELD IN QUARTER 16, BY RESEARCH STATUS

Job Characteristic According to Survey	Program Group Members Employed According to the Survey Data		Control Group Members Employed According to the Survey Data	
	Percentage with Job Characteristic	Percentage Also Employed According to the UI Data	Percentage with Job Characteristic	Percentage Also Employed According to the UI Data
<b>Occupation</b>				
Services	22.0	72.5	21.0	75.1
Sales	10.1	74.6	12.8	67.9
Construction	20.3	66.0	19.4	66.8
Private household	6.5	59.6	7.8	52.8
Clerical	12.3	73.6	13.0	73.6
Mechanics/repairers/ machinists	14.3	68.8	13.4	63.7
Agriculture	2.2	60.6	2.3	63.2
Other	12.3	68.5	10.4	68.3
n				
<b>Type of Employer</b>				
Private company	80.5	73.7	80.7	69.7
Military	1.4	12.9	0.5	0.0
Federal government	2.3	57.4	2.1	71.8
State government	4.5	66.7	4.9	70.9
Local government	3.7	75.0	4.0	74.6
Self-employed	5.3	41.8	5.3	32.9
Working without pay in a family business or as a favor	1.0	30.3	1.2	60.7
Other	1.4	35.1	1.2	84.0
<b>Sample Size</b>	<b>1,870</b>	<b>1,870</b>	<b>1,198</b>	<b>1,198</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

The overall agreement rate for all survey-based workers is slightly less than 70 percent for both the program and control groups. Stated differently, 70 percent of those classified as workers according to the survey data are also classified as workers according to the UI data.

Agreement rates are somewhat lower for workers in the “uncovered” sectors than for “covered” workers, but the differences are smaller than expected (Table IV.6). For example, among the program group, the agreement rate for survey-based workers is about 60 percent for those employed in private household and agriculture occupations, 60 percent for federal workers, 42 percent for those self-employed, and only 13 percent for those in the military. Nonetheless, many of these “uncovered” workers had reported earnings in the UI data. Furthermore, the agreement rate for “covered” workers is only about 72 percent. Stated differently, about 28 percent of “covered” workers do *not* have reported earnings in the UI data. The pattern of findings is similar for the program and control groups. Furthermore, despite differences in the types of jobs held by males and females and younger and older sample members, the pattern of estimated agreement rates is similar across subgroups (Table C.1).

These disappointing findings could be due in part to errors in misclassifying jobs reported in the survey into occupational categories, due to limited verbatim survey information on the nature and title of jobs that sample members held. However, because the agreement rates do not differ substantially for the “covered” and “uncovered” sectors, it appears that the noncoverage of formal jobs in the UI data accounts for only a small portion of the gap between the quarter 16 employment rate as measured by the survey and UI data.

#### **b. Examining Informal Jobs**

Another possible explanation for the lower quarter 16 employment rate in the UI data is that earnings from informal (casual or cash-only) jobs are covered in the survey data but not in the UI data. Under-the-table or off-the-books payments from informal jobs were likely to have been

reported in the survey, because survey respondents were asked to provide information on “paid full-time or part-time jobs that they may have had, including odd jobs, paid baby-sitting jobs, military service, work in their own businesses, or other types of jobs they may have had on a regular basis.” Some survey respondents might not have reported earnings from informal jobs that were performed on an *irregular* basis (because the survey asked for information on jobs performed on a *regular* basis). We believe this was uncommon, however, because many sample members reported earnings from jobs that were performed only once or twice (for example, baby-sitting or lawn-mowing).

In partial support of the informal-job explanation, the agreement rates for survey workers were somewhat lower in occupations in which we might expect informal jobs to be more common (Table IV.6). For example, the agreement rate for those in construction and mechanical-related trades was about 65 percent, compared to about 72 percent for those in service, sales, and clerical occupations.

To examine further the extent to which informal jobs explain the lower UI-based employment rate, we compared the characteristics of jobs reported in *both* the survey and UI data with the characteristics of jobs reported in the survey data *only*.<sup>40</sup> Job characteristics were obtained from the survey data, and pertain to the most recent job held in quarter 16 after random assignment. We expect that the survey-only jobs were more likely to have been informal jobs than those reported in both data sources. Thus, we anticipate that the survey-only workers had

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<sup>40</sup> For this analysis, we omitted those who had UI-based jobs but *more* survey-based jobs, because it was often difficult to accurately determine which survey-based job corresponds to which UI-based job and, thus, which survey jobs are the “matched” jobs and which are the “unmatched” ones.

(1) shorter job tenure, (2) fewer hours worked per week, (3) lower hourly wages, (4) lower weekly earnings, and (5) fewer benefits available on the job.

These hypotheses are only weakly supported by the data (Table IV.7). The survey-only group was more likely to have had less than one month of job tenure, but it was also more likely to have at least one year of job tenure. Consequently, average job tenure was similar for the two groups. Similarly, the distributions of usual hours worked per week were similar for the two groups of workers. However, average hourly wages were somewhat lower for the survey-only group (\$7.20, compared to \$7.30 to \$7.60 for those employed according to both data sources), and survey-only workers were much more likely to have hourly wages below \$4.50. In addition, survey-only workers were much less likely to have health insurance, paid vacation, and retirement benefits available on the job. We find a similar pattern for the program and control groups and by age and gender (Table C.2).

These results provide some evidence that the survey-only jobs were more likely than the jobs reported in both data sources to be informal jobs. However, the differences in the characteristics of jobs held by these two groups of workers are not as large as expected.

### **c. Multivariate Analysis**

Thus far, we have examined job characteristics *one at a time* to assess the extent to which informal and uncovered formal jobs account for the much lower quarterly employment rate using the UI than survey data. These job characteristics, however, are correlated with each other and with other worker characteristics. Thus, we also estimated multivariate logistic regression models to examine the effects of worker and job characteristics on the probability that a job reported in the survey data is also reported in the UI data, controlling for the effects of other characteristics. The sample for this analysis included those who were employed in quarter 16 according to the survey data. The dependent variable was set to 1 for those who also had a job

TABLE IV.7

JOB CHARACTERISTICS IN QUARTER 16 FOR THOSE EMPLOYED ACCORDING TO BOTH  
THE SURVEY AND UI DATA AND ACCORDING TO THE SURVEY DATA ONLY,  
BY RESEARCH STATUS  
(Percentages)

Job Characteristic According to Survey Data	Program Group		Control Group	
	Employed in Both Survey and UI Data <sup>a</sup>	Employed in Survey Data Only	Employed in Both Survey and UI Data <sup>a</sup>	Employed in Survey Data Only
<b>Number of Months on Job</b>				
Less than 1	4.5	10.6	5.2	11.9
1 to 3	33.2	30.8	32.6	30.4
3 to 6	21.8	20.1	22.7	17.6
6 to 12	25.0	20.0	24.8	21.4
12 or more	15.5	18.6	14.8	18.7
(Average months)	12.5	12.7	12.2	12.8
<b>Usual Hours Worked per Week</b>				
Less than 20	2.7	4.3	2.2	6.9
20 to 30	5.8	8.3	8.5	5.1
30 to 39	12.7	14.4	11.8	13.5
40	41.9	32.4	40.5	29.6
More than 40	36.9	40.5	37.1	44.8
(Average hours)	42.4	43.0	42.4	42.9
<b>Hourly Wage</b>				
Less than \$4.50	2.9	10.7	3.3	7.6
\$4.50 to \$6.00	26.2	26.6	30.8	32.0
\$6.00 to \$7.50	27.7	21.6	30.1	23.0
\$7.50 to \$9.00	24.3	20.2	19.2	18.1
\$9.00 or more	18.9	21.0	16.6	19.3
(Average hourly wage in 1995 dollars)	7.50	7.20	7.30	7.20
<b>Average Weekly Earnings (in 1995 dollars)</b>				
	323.10	310.40	308.60	315.10
<b>Benefits Available on Job</b>				
Health insurance	63.7	47.4	60.4	46.0
Paid vacation	67.6	56.3	63.6	60.7
Retirement or pension benefits	53.6	40.3	45.0	37.5
<b>Sample Size</b>	<b>860</b>	<b>475</b>	<b>524</b>	<b>332</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

<sup>a</sup>Includes only those with the *same* number of reported jobs in both data sources.

reported in the UI data and zero for those who did not. We excluded from the analysis those who had a UI-based job but *more* survey-based jobs, because it was difficult to accurately match specific survey-based jobs to specific UI-based jobs.

The regression models produced regression-adjusted *agreement* rates for subgroups of survey-based workers. The explanatory variables in the models included a large number of worker characteristics across which we posited agreement rates might vary, as well as job characteristics. Five categories of explanatory variables were included in the models: (1) demographic characteristics, (2) education and training experiences, (3) employment and earnings experiences during the four-year follow-up period, (4) welfare receipt during the follow-up period, and (5) arrest and drug use experiences during the follow-up period. Separate regression models were estimated for the program and control groups.

Table IV.8 displays estimated marginal probabilities and their significance levels for each explanatory variable (relative to the pertinent omitted variable). Thus, the marginal probabilities represent differences in regression-adjusted agreement rates between various groups. The overall agreement rate is 63 percent for the program group and 61 percent for the control group.

The results from the multivariate models are similar to the univariate results presented above for both the program and control groups. Agreement rates are significantly lower for workers who are (1) self-employed (because these jobs are not covered by the UI program); (2) in jobs without available health insurance benefits, and (3) in private household occupations (although this effect is not statistically significant). Agreement rates are also significantly lower for males than females and for Hispanics than other racial and ethnic groups.

Importantly, however, few of the other explanatory variables included in the models have predictive power in determining which survey-based jobs are covered in the UI data and which are not. Agreement rates do not differ significantly by age, fertility status, marital status, health



TABLE IV.8

MARGINAL EFFECTS FROM LOGITISTIC REGRESSION MODELS PREDICTING THE PROBABILITY THAT A JOB REPORTED IN THE SURVEY DATA IN QUARTER 16 IS ALSO REPORTED IN THE UI DATA, BY RESEARCH STATUS

Explanatory Variable (Left-Out State)	Marginal Effect (Percentage Points)	
	Program Group	Control Group
Mean of Binary Dependent Variable (Overall Agreement Rate) <sup>a</sup>	63.3	61.2
<b>Demographic Characteristics</b>		
Female	9.6***	8.9*
Age at Application to Job Corps (16 to 17)		
18 to 19	1.7	4.2
20 to 24	-0.8	1.6
Race and Ethnicity (White, non-Hispanic)		
Black, non-Hispanic	5.6	1.0
Hispanic	-9.6**	-9.0
Other	12.7**	1.6
Size of Area of Residence at Application (not a PMSA or MSA)		
PMSA	-1.2	2.0
MSA	1.6	1.9
Quarter of Application to Job Corps (11/1/94 to 2/28/95)		
3/1/95 to 6/30/95	1.7	4.1
7/1/95 to 9/30/95	1.8	5.2
10/1/95 to 12/31/95	4.6	7.6
Had Children at 48 Months After Random Assignment	0.2	6.0
Lived with all Children at 48 Months	-5.1	14.8**
Lived with a Partner (Married or Unmarried) at 48 Months	-0.3	-2.0
Head of Household at 48 Months	0.8	4.3
Had Physical or Emotional Problems that Limited the Nature and Amount of Work that Could Be Done	2.6	-1.5
<b>Education and Training</b>		
Had a High School Diploma or GED at 48 Months	-1.0	1.3
Had a Vocational Certificate at 48 Months	-1.3	-4.9
Had a Two-Year or Four-Year College Degree at 48 Months	1.2	5.8
	0.1	0.1

TABLE IV.8 (continued)

Explanatory Variable (Left-Out State)	Marginal Effect (Percentage Points)	
	Program Group	Control Group
<b>Welfare Receipt</b>		
Received Welfare for Most of the Time When Growing Up	-2.4	5.1
Ever Received AFDC/TANF, Food Stamp, General Assistance, or SSI Benefits During the 48-Month Period	0.0	-5.2
Amount of AFDC/TANF, Food Stamp, General Assistance, or SSI Benefits Received During the 48-Month Period	0.0	0.0
In Public Housing at 48 Months	-5.2	1.7
<b>Employment and Earnings</b>		
Total Hours Worked per Week During the 48 Months After Random Assignment	-0.1	0.3
Number of Jobs Held During the 48-Month Period	0.2	1.3
Occupation on the Most Recent Job in Quarter 16 (Construction)		
Services	0.0	4.8
Sales	3.6	-5.2
Private household	-9.1	-13.1
Clerical	-1.5	-1.7
Mechanics/repairers/machinists	-2.4	-5.7
Other	-1.2	-0.8
Self-employed in the Most Recent Job in Quarter 16	-22.0***	-30.8***
Benefits Available on the Most Recent Job in Quarter 16		
Health insurance	12.8***	15.2***
Paid sick leave	-1.1	-4.6
Retirement or pension benefits	3.1	0.3
Months Worked on the Most Recent Job in Quarter 16	0.0	-0.2
Usual Hours Worked per Week on the Most Recent Job in Quarter 16	-0.1	-0.2
Hourly Wage on the Most Recent Job in Quarter 16	0.8	0.1
<b>Criminal Activities and Drug Use Experience</b>		
Ever Arrested During the 48-Month Period	-1.7	1.9
Ever in Jail During the 48-Month Period	-4.3	4.5
Ever in Drug Treatment	7.6	-1.8
Used Hard Drugs in the Month Before the 48-Month Interview	9.4	2.7
<b>Sample Size</b>	<b>1,335</b>	<b>856</b>

TABLE IV.8 (continued)

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Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

<sup>a</sup>The sample includes those who were employed in quarter 16 according to the survey data. The dependent variable equals 1 for those who also had a job reported in the UI data and zero for those who did not. The dependent variable was set to missing for those who had a UI-based job but *more* survey jobs.

<sup>b</sup>Hard drugs include cocaine powder; crack; speed, uppers, or methamphetamines; hallucinogens; and heroin, opium, methadone, and downers.

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

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

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

status, education level, welfare receipt status, and crime and drug use experiences. Furthermore, except for the small number of measures discussed above, none of the employment-related variables are statistically significant.

These findings suggest that substantial residual factors account for the differences in employment rates using the survey and UI data. We have identified some important associations between job characteristics and agreement rates that are consistent with our hypotheses concerning the noncoverage of informal and some formal jobs in the UI data. Clearly, however, there are important unobserved factors associated with the lower UI-based employment rate that we could not identify using our survey data.

### **3. Explanations for Higher Earnings per Job in the Survey than in the UI Data**

We have shown that overall mean earnings differences according to the survey and UI data are due not only to differences in quarterly employment rates, but also in large part to differences in earnings per job. In this section, we address the following question: Why are quarter 16 earnings levels nearly 40 percent higher in the survey than UI data, even for those with the same number of reported jobs according to both data sources?

First, we examine the extent to which earnings per job are higher in the survey data because of the overreporting in the survey of weeks worked, hours worked, or hourly wages. Second, we examine whether the survey-to-UI earnings differences vary according to job stability measures (as measured by job tenure and the availability of job benefits).

#### **a. Examining the Effects of Reported Weeks Worked, Hours Worked, and Hourly Wages**

The income that a worker earns in a job over a given period is the product of (1) the number of weeks worked on the job during the period, (2) the usual hours per week worked, and (3) the hourly wage rate. Consequently, differences in worker earnings per job using the survey and UI

data can be attributed to survey-to-UI differences in each of these three components. A critical analysis objective is to ascertain which of these components is most important in explaining the large gap in mean earnings per job as measured by the two data sources.

Ideally, we would like to compare differences in each of the three earnings components as reported by sample members and their employers. This is not possible, however, because the UI wage records contain earnings per job only, and not the components of earnings. Instead, we examined the association between each of the earnings components—as measured by the survey—and the *ratio* of average survey-to-UI earnings. Thus, we assessed the extent to which the survey-to-UI earnings ratios vary by the number of weeks worked, the number of hours per week worked, and the hourly wage rate as measured by the survey. These results provide indirect evidence as to the earnings components that matter most in explaining the large gap in mean earnings per job using the survey and UI data.

The sample for this analysis consists of those who were classified as workers and had the same number of reported jobs according to *both* data sources. For sample size considerations, the job characteristics were grouped into fewer categories than in previous analyses. For similar reasons, we present subgroup findings by age and gender using the combined program and control groups.

The ratio of survey-to-UI mean earnings is 1.42 for the full program group and 1.34 for the full control group (Table IV.9). Stated differently, mean earnings per job in quarter 16 are 42 percent higher according to the survey than the UI data for the program group and 34 percent higher for the control group. This program-control group difference contributes to the higher earnings impact in percentage terms using the survey than UI data.

Our main finding is that the likely overreporting of hours worked in the survey data plays an important role in explaining the higher earnings per job levels in the survey than UI data

TABLE IV.9

## RATIO OF SURVEY-TO-UI MEAN EARNINGS PER JOB IN QUARTER 16, BY WEEKS WORKED, HOURS PER WEEK WORKED, HOURLY WAGES, AND RESEARCH STATUS

Job Characteristic According to Survey Data	Program Group <sup>a</sup>		Control Group <sup>a</sup>	
	Percentage with Job Characteristic	Ratio of Survey-to-UI Mean Earnings Per Job	Percentage with Job Characteristic	Ratio of Survey-to-UI Mean Earnings Per Job
Full Sample	100.0	1.42	100.0	1.34
Number of Weeks Worked on Job in Quarter 16				
Less than 3	9.1	1.13	9.8	0.74
3 to 6	12.2	1.38	8.5	1.87
6 to 12	8.6	1.76	7.7	1.57
13 (all weeks)	70.1	1.41	74.0	1.33
Hours Worked per Week				
Less than 30	8.5	0.89	10.6	0.95
30 to 39	12.7	1.14	11.8	1.25
40	41.9	1.21	40.5	1.21
More than 40	36.9	1.83	37.0	1.57
Hourly Wage (in 1995 dollars)				
Less than \$6.00	28.9	1.38	33.7	1.32
\$6.00 to \$7.50	27.8	1.37	30.3	1.32
\$7.50 to \$9.00	24.4	1.33	19.3	1.38
\$9.00 or more	18.9	1.58	16.7	1.36
<b>Sample Size</b>	<b>860</b>	<b>860</b>	<b>524</b>	<b>524</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

<sup>a</sup>The sample includes those according to both the survey and UI data who were employed in quarter 16 after random assignment and who had the same number of reported jobs.

(Table IV.9). The overreporting of hourly wage rates and weeks worked during the quarter appears to play a less important role. The earnings per job ratios for both the program and control groups are less than 1 for those who reported working fewer than 30 hours per week. The ratios increase to about 1.20 for those who reported working 30 to 40 hours per week, and to 1.83 for the program group and 1.57 for the control group for the 37 percent of workers who reported working more than 40 hours per week. The earnings per job ratios, however, display a less clear trend across the hourly wage and number of weeks worked categories. We also find the same patterns across categories constructed by *combining* the hours worked, weeks worked, and wage measures to adjust for the correlation among these measures (not shown). Finally, the pattern of findings is similar across gender and age groups (Tables C.3 and C.4).

These findings are consistent with the fact that many program and control group workers reported in the survey that they worked a substantial number of hours per week. The average worker reported working about 42 hours per week on the most recent job in quarter 16, and more than three-quarters reported working at least 40 hours. These figures are *higher* than the corresponding figures for all U.S. workers. According to the 1999 Current Population Survey, U.S. workers age 16 and older worked an average of 39.6 hours per week, and 69 percent worked at least 40 hours per week (Statistical Abstract of the United States 2000). The hourly wages reported by our sample members, however, are more consistent with national figures; the average hourly wage in 1999 was \$11.43 (in 1995 dollars) for all U.S. workers, compared to about \$7.40 (in 1995 dollars) for our sample members. Thus, there is evidence that the Job Corps sample overreported hours worked but more accurately reported their hourly wages.

Why, then, were reported hours worked in the survey so high? One possibility is that the survey questions requesting information on hours worked were unclear or misleading. We do not believe, however, that this was the case. For each job, the survey asked each worker the

following two simple questions: (1) “How many days per week do/did you usually work?” and (2) “How many hours per day do/did you usually work, including overtime hours?” These two data items were multiplied to construct the hours per week variable for each job. The data items were rarely missing, and there was no evidence that survey respondents had trouble responding to these questions.

Another possibility is that sample members reported high hours worked in the survey because of recall error. However, recall error would also affect the hourly wage variables and other job-related variables. Furthermore, it is unclear why recall error would systematically lead to overreporting of hours worked.

Still another possibility is that workers reported the number of hours that their employers *advertised* they would work rather than their actual hours. For example, some workers may have been hired as full-time workers but may have only worked part-time when demand for their services was low (for example, in “off-seasons” in retail trade occupations). Similarly, some workers may have actually worked less hours than they were supposed to have worked due to child care issues, transportation problems, or other reasons, but reported the hours they were supposed to have worked.

Of course, it is also possible that the survey data are accurate and that employers did not accurately report earnings from employees’ overtime or other hours to the government.

A complete answer to the important question of why reported hours appear to be high in the survey data can only be obtained from a study structured to address this issue. Such a study would collect detailed job information from the perspective of both employees and their employers. The information would be collected from surveys and, perhaps, from focus groups. We believe that such a study has important policy implications and should be a focus of future research funded by DOL.



To examine further the extent to which the hours worked component accounts for the gap in earnings per job using the survey and UI data, we simulated the effects of reducing hours worked on survey-based earnings levels, and hence, on the survey-to-UI earnings per job ratios. The simulations were conducted by (1) lowering the cap on hours per week worked from 84 hours to 72, 60, and 48 hours, respectively; and (2) reducing hours worked for all workers by 10, 15, 20, and 25 percent, respectively.

The simulation results show that reducing mean hours worked to realistic levels leads to significant reductions in the survey-to-UI earnings ratios, although earnings levels are still substantially higher according to the survey than UI data (Table IV.10). For example, if hours are reduced by 10 percent for all workers (which assumes that workers overreported earnings by 10 percent in the survey), mean hours worked decrease from 42 hours to 38 hours (which is close to the national average) for both research groups, and the survey-to-UI ratio decreases from 1.42 to 1.27 for the program group and from 1.34 to 1.21 for the control group. The ratios reduce to 1.0 for both research groups if hours for all workers were reduced by 25 percent (in which case mean hours worked become about 32 hours), although we believe that it is unrealistic to assume that hours worked were overreported to this extent. Finally, reducing the cap on hours worked from 84 to 60 hours has small effects, because only about 5 percent of workers reported working hours above the 60-hour threshold. However, reducing the cap to 48 hours reduces the earnings ratio from 1.42 to 1.33 for the program group, and from 1.34 to 1.26 for the control group, because about 20 percent of workers reported working more than 48 hours a week in quarter 16.

In sum, the apparent overreporting of hours worked in the survey data provides a partial explanation for the higher earnings per job levels in the survey than UI data. However, based on our simulations, reported hours would need to be reduced by nearly one-quarter to close the survey-to-UI earnings gap completely. We believe, however, that it is unlikely that the survey-to-

TABLE IV.10

SIMULATION RESULTS FROM REDUCING HOURS PER WEEK WORKED  
IN THE SURVEY DATA, BY RESEARCH STATUS

Simulation Method for Reducing Hours per Week Worked	Program Group <sup>a</sup>		Control Group <sup>a</sup>	
	Mean Hours per Week Worked According to the Survey Data	Ratio of Survey-to-UI Mean Earnings per Job	Mean Hours per Week Worked According to the Survey Data	Ratio of Survey-to-UI Mean Earnings per Job
Cap on Hours per Week Worked (Hours)				
84 (benchmark)	42.4	1.41	42.4	1.34
72	42.3	1.41	42.3	1.34
60	41.8	1.39	41.7	1.32
48	40.1	1.33	39.9	1.26
Percentage Reduction in Hours per Week Worked for All Workers				
10	38.2	1.27	38.1	1.21
15	36.1	1.20	36.0	1.14
20	33.9	1.13	33.9	1.07
25	31.8	1.06	31.8	1.00
<b>Sample Size</b>	<b>860</b>	<b>860</b>	<b>524</b>	<b>524</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

<sup>a</sup>The sample includes those according to both the survey and UI data who were employed in quarter 16 after random assignment and who had the same number of reported jobs.

UI differences in reported hours are that large. Thus, residual factors (including discrepancies in reported hourly wages and weeks worked) also account for some of the survey-to-UI earnings differences.

**b. Examining Survey-to-UI Earnings per Job Ratios by Job Stability Measures**

We hypothesize that earnings differences using the survey and UI data would be smaller for sample members who held stable jobs than for those who held less stable ones. Those who held stable jobs were probably more likely to have worked regular hours than their counterparts and thus may have more accurately recalled their usual hours worked, job start and end dates, and hourly wages. Furthermore, employers may have been more likely to report earnings for workers who held stable jobs than for those who held irregular, informal ones.

To test this hypothesis, we defined job stability measures using survey information on job tenure and the availability of health, vacation, and pension benefits on the job. We then examined survey-to-UI earnings ratios in quarter 16 across these job stability measures. We expected the ratios to be smaller for those with longer job tenure and those on jobs with available benefits than for other workers. In addition, we expected the ratios to be somewhat smaller in professions with irregular hours (such as construction and private household occupations).

The job tenure results strongly support our hypothesis that reporting differences are smaller for those in stable jobs than less stable ones (Table IV.11). The earnings per job ratios are much smaller for those with longer than shorter job tenure. For the program group, the survey-to-UI earnings ratio is 1.69 for those with less than 3 months of job tenure and only 1.18 for those with more than one year of job tenure, and the corresponding figures for the control group are 1.40 and 1.10, respectively.

Thus, it appears that the earnings differences between the two data sources stem primarily from those who held short-term jobs. Either these youths could not accurately recall their hours

TABLE IV.11

## RATIO OF SURVEY-TO-UI MEAN EARNINGS PER JOB IN QUARTER 16, BY JOB TENURE, OCCUPATION, AVAILABLE JOB BENEFITS AND RESEARCH STATUS

Job Characteristic According to Survey Data	Program Group <sup>a</sup>		Control Group <sup>a</sup>	
	Percentage with Job Characteristic	Ratio of Survey-to-UI Mean Earnings per Job	Percentage with Job Characteristic	Ratio of Survey-to-UI Mean Earnings per Job
Full Sample	100.0	1.42	100.0	1.34
Total Number of Months on Job				
Less than 3	37.7	1.69	37.8	1.40
3 to 6	21.8	1.51	22.7	1.36
6 to 12	25.0	1.33	24.8	1.48
12 or more	15.5	1.18	14.8	1.10
Occupation				
Services	21.2	1.39	24.1	1.30
Sales	11.2	1.21	13.7	1.52
Construction	20.0	1.50	18.6	1.57
Private household	5.6	1.55	6.0	1.22
Clerical	13.2	1.35	13.2	1.13
Mechanics/repairers/ machinists	14.1	1.50	11.5	1.23
Other	14.7	1.42	12.9	1.41
Benefits Available on Job				
Health insurance	63.7	1.39	60.4	1.28
Paid vacation	67.6	1.40	63.6	1.29
Retirement or pension benefits	53.6	1.38	45.0	1.29
<b>Sample Size</b>	<b>860</b>	<b>860</b>	<b>524</b>	<b>524</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

<sup>a</sup>The sample includes those according to both the survey and UI data who were employed in quarter 16 after random assignment and who had the same number of reported jobs.

and wages (perhaps because work hours were irregular) or employers did not report their earnings.

We conducted a multivariate regression analysis to compare the characteristics of workers with short and long tenure (not shown). Regression models were estimated where a binary dependent variable signifying whether the worker had more than six months of job tenure was regressed on a large number of worker and job characteristics (that were presented in the previous section). We find that older workers tended to have longer job tenure than younger ones. However, we find no other patterns across the other explanatory variables included in the models for either the program or control groups. Thus, it is difficult to isolate the characteristics of those with longer job tenure and, hence, of groups with lower survey-to-UI earnings ratios.

As expected, the earnings ratios are somewhat larger in occupations that are most likely to have irregular hours: construction, private household, and mechanical trade occupations (Table IV.11). However, the differences in ratios across occupations are not large. Furthermore, earnings ratios do not differ for those with and without available job benefits. Thus, these results provide weaker evidence than the job tenure results on the association between the earnings ratios and job stability measures.

#### **4. Summary of Findings**

We have explored potential reasons that employment rates in quarter 16 are about 13 percentage points higher using the survey than the UI data, and similarly, why average earnings per job in quarter 16 are nearly 40 percent higher according to the survey data. Because of data limitations, our analysis could not fully identify all relevant factors explaining these employment and earnings differences, especially for the employment differences. However, we were able to identify some partial explanations and to discard others. These findings could help guide future research in this area.

Our main findings can be summarized as follows:

- ***Differences in overall quarter 16 earnings according to the survey and UI data are due in roughly equal parts to differences in (1) employment levels, (2) the number of jobs per worker, and (3) earnings per job.*** Thus, all three components of earnings play an important role in explaining the overall earnings differences across the two data sources, for the full sample and for all key subgroups.
- ***The noncoverage of some formal jobs in the UI data appears to account for only a small portion of the gap between the quarter 16 employment rate as measured by the survey and UI data.*** Using workers in the survey data, we find that those who were likely to be in noncovered formal jobs are somewhat less likely to have a record in the UI data than those who were likely to be in covered formal jobs. However, many of those likely to be in covered jobs do not have a record in the UI data. Thus, differences in survey and UI match rates across occupations are smaller than expected.
- ***There is only weak evidence that the higher employment rate in the survey data is due to informal jobs.*** Comparing the characteristics of jobs reported in both the survey and UI data with the characteristics of jobs reported in the survey data only, we find that, as expected, average hourly wages and the likelihood of having available fringe benefits on the job were slightly lower for the survey-only group. However, job tenure and usual hours worked were similar for the two groups of workers. Consequently, the differences in the characteristics of jobs held by the two groups of workers are smaller than expected.
- ***There are substantial unobserved factors that account for the employment rate differences according to the survey and UI data.*** The multivariate regression model results indicate that few explanatory variables have predictive power in determining which survey-based jobs are reported in the UI data and which are not. Thus, there are important residual factors associated with the higher survey-based employment rate.
- ***The likely overreporting of hours worked in the survey data plays an important role in explaining the higher earnings per job levels in the survey than UI data.*** We find that survey-to-UI earnings ratios increase with the number of hours worked as reported in the survey but not with hourly wage rates or weeks worked. Based on our simulations, however, reported hours worked would need to be reduced by nearly one quarter to completely close the survey-to-UI earnings gap. We believe that it is unlikely that the overreporting of hours in the survey is that large. Thus, residual factors also account for some of the survey-to-UI earnings differences.
- ***There is also some evidence that earnings differences between the survey and UI data are smaller for those in stable jobs than less stable ones.*** Earnings differences are much smaller for those with longer than shorter job tenure. Furthermore, the differences are somewhat larger for those in occupations that are more likely to have irregular hours (such as construction and private household occupations) than for those in other occupations.

- *There are few differences in findings between the program and control groups.* As discussed, reporting differences between the survey and UI data are slightly larger for the program than control group, which results in percentage earnings gains that are slightly larger according to the survey than UI data. We did not, however, find any evidence that the program group was more likely than the control group to hold informal jobs and formal jobs not covered by the UI program; the distribution of the occupations of the jobs held by program and control group members in quarter 16 is similar. Furthermore, there is no evidence that the program group was more likely than the control group to overreport usual hours worked on their jobs. We surmise that our survey data do not contain sufficiently detailed job information to explain the small reporting differences between the two research groups.

## **B. EXAMINING INTERVIEW NONRESPONSE BIAS**

As discussed in Chapter III, we found that differences between the percentage earnings gains using the survey and SER data are due not only to reporting differences between the two data sources, but also in equal part to interview nonresponse bias. This interview nonresponse bias is caused by *larger* SER-based impacts for 48-month survey respondents than nonrespondents during the postprogram period, which suggests that the survey-based earnings impact estimates are somewhat biased upward (Tables III.3 and B.2). The pattern of SER-based impact findings using interview respondents and the full analysis sample, however, are similar because of the high response rate to the 48-month interview.

In this section, we address the following question: What are possible explanations for the interview nonresponse bias? First, we discuss the nature of the nonresponse bias. Second, we discuss possible explanations for it. Finally, we discuss the implications of the interview nonresponse bias for the impact estimates on other key outcomes measured using the survey data.

### **1. The Nature of the Interview Nonresponse Bias**

Differences in the SER-based impacts between the interview respondents and nonrespondents became noticeable starting in 1997 when the earnings impacts became positive

(Table B.2). This pattern was not noticeable in 1993 or 1994 (the period before random assignment). It was also not noticeable in 1995 and 1996, when many program group members were enrolled in Job Corps and thus when the earnings impacts were negative. However, starting in 1997, the earnings impacts for respondents were *positive*, while the earnings impacts for nonrespondents were *negative* (and statistically significant at the 10 percent level). Furthermore, the differences in impacts between respondents and nonrespondents grew somewhat over time as the impacts for nonrespondents became increasingly negative.

Starting in 1997, the average annual earnings *levels* were larger for interview respondents than nonrespondents for both the program and control groups. However, the respondent-nonrespondent differences were much larger for the program group. For example, in 2001, the average earnings of respondents was 27 percent higher than for nonrespondents in the program group (\$8,018 for respondents and \$6,323 for nonrespondents), compared to only 14 percent higher in the control group (\$7,864 for respondents and \$6,904 for nonrespondents).

## 2. Possible Explanations for the Interview Nonresponse Bias

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

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

It is difficult to disentangle these two effects. While both explanations are likely to account for the bias, we believe that the data more strongly support the latter explanation—that the bias is



caused by larger impacts for survey respondents than nonrespondents. Next, we discuss this evidence.

**a. Are Interview Respondents in the Program and Control Groups Similar?**

Several pieces of evidence suggest that respondents in the program and control groups are comparable. First, the response rate to the 48-month interview was similar for the program and control groups. It was 79.9 percent overall, 81.5 percent for the program group, and 77.8 percent for the control group.

Second, the baseline characteristics of 48-month interview respondents in the two research groups are similar. As Table C.5 shows, the distributions of a large number of characteristics from the baseline interview and Job Corps application forms are similar for *respondents* in the two research groups. In addition, program and control group differences in average SER earnings and employment levels in 1993 and 1994 are not statistically significant (Table B.2). Thus, the survey-based earnings impacts constructed using simple differences-in-means procedures (the ones presented in this report) are similar to those constructed using regression models that control for observable baseline differences between the program and control groups (Schochet 2001). Similarly, the earnings impact results are similar, whether or not the estimates were obtained using sample weights that were adjusted for interview nonresponse (Schochet 2001). While it is possible that there are *unobservable* differences between the characteristics of respondents in the program and control groups that are correlated with earnings, we do not believe that these differences alone are sufficient to account for the large differences in the SER-based earnings impacts between interview respondents and nonrespondents.

A third piece of evidence that suggests that the program and control group respondents are similar is that the distribution of the number of months until the 48-month interview was completed was similar for those in the two research groups. The average 48-month interview was

completed in month 50 for both the program and control groups. Similarly, 78 percent of interviews were completed within 3 months for the program group, compared to 79 percent for the control group. These results are not surprising, since similar search methods were used to locate program and control group members for the 48-month interview, similar percentages of program and control group members received incentive fees to induce them to complete the interview, and our interviewers did not systematically report having more difficulty locating members of one research group than another.

We conducted several analyses to examine whether the small differences in interview response rates and completion times across the research groups had an effect on the impact estimates. First, we calculated 1998 earnings impact estimates using the *survey* data for various subgroups of respondents that were defined to help “balance” the two research groups. We subsampled interview respondents so that the resulting response rates in *both* the program and control groups were the same (50 percent, 60 percent, 70 percent, and 77.8 percent [the response rate for the control group], respectively). For this analysis, we selected those who completed interviews *first* to further balance the program and control group samples. We also obtained separate impact estimates for those who completed interviews within a certain number of months (3, 6, 9, and 12 months, respectively) after they were released for 48-month interviews.

The 1998 mean earnings levels and earnings impact estimates are remarkably similar across the various subgroups of respondents (Table IV.12). In all cases, the impacts are about \$1,000 per eligible applicant and statistically significant at the 1 percent level. Thus, balancing the program and control groups in terms of their time until interview completion has no effect on the impact results, which further suggests that the program and control group respondents are comparable.

TABLE IV.12

SURVEY-BASED 1998 EARNINGS IMPACT ESTIMATES USING SUBGROUPS OF  
48-MONTH INTERVIEW RESPONDENTS DEFINED BY THEIR TIME  
UNTIL INTERVIEW COMPLETION

Subgroup	Program Group	Control Group	Estimated Earnings Impact in 1998 <sup>a</sup>
All Respondents	10,295.6	9,324.1	971.6***
Response Rate Assumption Where Those Who Completed Interviews First Were Selected (Percentage)			
50.0	10,433.7	9,486.0	947.7***
60.0	10,457.6	9,347.4	1,110.2***
70.0	10,348.9	9,346.7	1,002.2***
77.8 (the full control group response rate)	10,322.6	9,324.1	998.5***
Months Until Interview Was Completed from the 48-Month Interview Release Date			
3 or less	10,408.0	9,361.5	1,046.6***
6 or less	10,312.5	9,328.7	983.8***
9 or less	10,304.5	9,322.7	981.9***
12 or less	10,294.3	9,319.7	974.5***
<b>Full Sample Size</b>	<b>6,808</b>	<b>4,474</b>	<b>11,282</b>

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

Notes: All estimates were calculated using sample weights to account for the sample design, the survey design, and interview nonresponse.

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

A second analysis that we conducted was to estimate separate *SER-based* earnings impacts for those who completed the 12-, 30-, and 48-month interviews. The response rate for the 12-month interview was 90.2 percent (91.4 percent program, 88.4 percent control), which is higher than the 79.9 percent figure for the 48-month interview. Thus, if interview nonresponse bias were caused by differences in the characteristics of respondents in the program and control groups, we would expect that the interview nonresponse bias would be smaller using the larger 12-month interview sample than the smaller 48-month one. We also estimated separate impacts for 30-month interview completers, although the response rate for the 30-month interview (79.4 percent overall, 80.7 percent program, and 77.4 percent control) was similar to the response rate to the 48-month interview.

As Table IV.13 shows, the estimated earnings impacts between 1993 and 2001 are similar using the samples of respondents to the 12-, 30-, and 48-month interviews. While there is some evidence that the post-1996 impacts are slightly larger using the sample of 48-month respondents than 12-month respondents, the size of the estimated impacts and levels of statistical significance are similar across the respondent groups. These results again suggest that there are not substantial differences between the unobservable characteristics of respondents in the program and control groups.

As discussed next, despite these findings, we believe that the interview nonresponse bias is due to some extent to differences in respondent characteristics across the two research groups. This is because, while postprogram earnings impacts for interview nonrespondents may be smaller than those for interview respondents, it is difficult to explain why the impacts for nonrespondents are *negative*.

TABLE IV.13

SER-BASED EARNINGS IMPACT ESTIMATES FROM 1993 TO 2001 USING RESPONDENTS TO THE 12-, 30-, AND 48-MONTH INTERVIEWS

Outcome Measure	Estimated Earnings Impact <sup>a</sup>		
	12-Month Interview Respondents	30-Month Interview Respondents	48-Month Interview Respondents <sup>b</sup>
Calendar Year Earnings (in 1995 Dollars)			
1993	12.7	14.6	3.5
1994	58.0	52.8	47.4
1995	-277.7***	-275.0***	-260.8**
1996	-172.9**	-152.5**	-121.2**
1997	209.3**	282.4***	274.5***
1998	276.0**	361.2***	394.4***
1999	114.2	188.6	175.0
2000	114.6	186.3	207.0
2001	130.2	131.1	184.7
<b>Program/Control Group Sample Size</b>	<b>8,220/5,047</b>	<b>7,254/4,442</b>	<b>6,772/4,451<sup>c</sup></b>

Source: Annual social security earnings records for the full research sample, and 12-, 30-, and 48-month follow-up interview completion data.

Notes: All estimates were calculated using sample weights to account for the sample design, but not the survey design.

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>The impact estimates for the 48-month respondents differ slightly from those displayed in Tables III.3 and B.2 because the estimates in those tables were constructed using weights that adjusted for the sample and survey designs, whereas the estimates in this table adjust for the sample design only.

<sup>c</sup>To conserve project resources, program group members were randomly subsampled for 48-month interviews.

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

**b. Do Earnings Impacts Truly Differ for Interview Respondents and Nonrespondents?**

There is some evidence that the survey nonresponse bias can be attributed to larger earnings impacts for 48-month interview respondents than nonrespondents. What is this evidence?

First, earnings impacts and earnings levels for respondents and nonrespondents might differ because of observed differences in their baseline characteristics. As Table C.5 shows, females and younger sample members were significantly more likely than their counterparts to complete a 48-month interview. In addition, response rates were significantly higher (1) for those in less populated areas than for those in more populated areas (such as PMSAs or MSAs); (2) for those with children at program application than for those without children; (3) for those who had completed high school at program application than for those without a high school degree; (4) for those never convicted prior to application than for those convicted; and (5) for nonresidential designees than for residential designees. Finally, a test of the hypothesis that the characteristics of the respondents and nonrespondents are jointly similar is statistically significant at the 1 percent level for both the program and control groups.

When constructing the impact estimates using the survey data, we attempted to account for differences in the baseline characteristics of respondents and nonrespondents by adjusting the sample weights using propensity scoring procedures (Schochet 2001). However, we believe that this procedure did not adequately account for the nonresponse bias.

A second piece of evidence that supports the theory that impacts are larger for respondents than nonrespondents is that, among program group members, respondents had somewhat higher Job Corps participation levels than nonrespondents. Using enrollment data from the Job Corps Student Pay, Allotment, and Management Information System (SPAMIS), we find that, among the program group, about 77.2 percent of interview respondents ever enrolled in a Job Corps center, compared to only 69.5 percent of interview nonrespondents. Similarly, among program

group enrollees, respondents stayed in the program for nearly one month longer on average than nonrespondents (6.9 months, compared to 6.0 for nonrespondents), and a smaller percentage of respondents left the program within 6 months (54 percent, compared to 60 percent for nonrespondents).

Importantly, Gritz and Johnson (2001), using survey data, found that nearly all the positive earnings impacts in quarter 16 accrued to those who completed Job Corps (that is, those who completed a vocational program or received a GED). Program completion status is highly correlated with length of stay in the program. Thus, impacts for interview respondents might be larger than for interview nonrespondents, because respondents usually remained in Job Corps for a longer period than nonrespondents and thus were more likely to have completed the program.

While these explanations can account somewhat for the larger earnings impacts for respondents than for nonrespondents, we do not believe that they can account fully for the *negative* postprogram earnings impacts for nonrespondents. Consequently, the interview nonresponse bias must also be due in part to interview respondents in the program group being drawn from a somewhat more advantaged subpopulation of the full program group than was true for interview respondents in the control group.

### **c. Implications of the Nonresponse Bias for the Impact Findings on Other Outcomes**

Our finding that the survey-based earnings impacts may be slightly biased *upward* suggests that the survey-based impacts on other outcomes may also be slightly biased in favor of finding beneficial program effects. In particular, the estimated impacts on education and training outcomes may be slightly biased upward, and the impacts on criminal behavior may be slightly biased downward.

Using the survey data, we found very large impacts of Job Corps on hours spent in education and training (about 1,000 hours per Job Corps participant during the 48-month follow-up period)

and on the attainment of a GED (about 21 percentage points). Thus, we believe that a small amount of survey nonresponse bias would not change our basic conclusions about the effectiveness of Job Corps on these outcomes. However, we believe that the small, but statistically significant, impacts that were found on functional literacy test scores at 30 months may have been affected by survey nonresponse. Job Corps raised participants' average test scores by about four points on the prose scale, two points on the document scale, and five points on the quantitative scale (Glazerman et al. 2001). These small impacts, however, may have disappeared in the absence of interview nonresponse bias.

Similarly, using the survey data, we found that Job Corps had modest, but statistically significant, effects on reducing arrest and conviction rates, as well as on time spent incarcerated. About 33 percent of control group members were arrested during the 48-month follow-up period, compared to 29 percent of program group members. More than 25 percent of control group members were convicted during the follow-up period, compared to 22 percent of program group members. In addition, Job Corps reduced the percentage incarcerated by 2 percentage points and the average time spent incarcerated by about six days. Again, it is possible that these modest impacts may have been smaller in the absence of interview nonresponse bias.

Without additional data, it is impossible to determine the extent to which the estimated impacts on the education- and arrest-related outcomes suffer from interview nonresponse bias. Our finding that the survey-based earnings impacts are slightly biased upward, however, suggests that the survey-based impacts on other outcomes must be interpreted cautiously.



## V. BENEFIT-COST ANALYSIS RESULTS

One of the major objectives of the National Job Corps Study is to determine whether the benefits of Job Corps exceed the costs of the program. To address this question, we conducted a detailed analysis of the benefits and costs of the program using survey data (McConnell and Glazerman 2001). Based on this benefit-cost analysis, we concluded that the benefits of Job Corps to society as a whole exceeded its costs by about \$17,000 per participant.

A key assumption underlying the estimates of the benefits was that the earnings impacts observed in the last year of the observation period would continue for the rest of the youths' working lifetimes. As discussed in earlier chapters of this report, the analysis of the administrative data shed doubt on the validity of that assumption. Under assumptions more consistent with the findings from the administrative data, we find that the benefits of Job Corps to society do not exceed its costs for all youth, but that its benefits may exceed its costs for older youth.

This chapter discusses the implications of the findings from the administrative data for the results of the benefit-cost analysis. The chapter is in six parts. The first two sections provide a short description of the benefit-cost methodology and review the earlier findings of the benefit-cost analysis using the survey data. We then discuss the revisions in the assumption of the persistence of earnings impacts and other revisions to the estimates of benefits and costs. The last two sections present revised estimates of the benefits and costs for the full sample and for a subgroup of older youth and provide some concluding thoughts.

## A. BENEFIT-COST METHODOLOGY

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

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

1. The benefits of increased output resulting from the additional productivity of Job Corps participants. This is measured from the impact on earnings plus the cost of fringe benefits (compensation) net of the cost of any additional child care.
2. The benefits from reduced use of other programs and services, including other education and training, public assistance, and substance abuse treatment programs.
3. The benefits from reduced crime committed by participants as well as the benefits from reduced crime committed *against* participants.
4. Program costs, including reported program operating costs, costs not reported on Job Corps' financial reports (such as the costs of administering the national and regional offices and donated goods and services), and the economic costs of the capital—land, buildings, furniture, and equipment—used by Job Corps.

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

Society gains from the increased output, reduced use of programs and services, and reduced crime committed by the youth. Society also bears most of the costs of Job Corps. The allowances and allotments students are paid while in Job Corps and the cost of their food and clothing are treated as a transfer from the rest of society to the participants and are hence considered a cost to the government but not a cost to society. Similarly, the increased taxes paid as earnings increase is a cost to participants and a benefit to the rest of society, but neither a cost nor a benefit to society.

Job Corps is an intensive program that aims to make long-term changes in the lives of the youths it serves. The survey data, however, follow the youth for only four years after random assignment and only about three years after they left Job Corps. Hence, it was necessary to make an assumption about the size and pattern of program impacts after the four-year survey observation period. In our initial analysis, we assumed that only benefits that did not decline during the observation period would continue after it. Impacts on crime and the use of other programs and services declined during the observation period; therefore, we did not include the possible future benefits of these impacts after the follow-up period. As the earnings impacts did not decline, we assumed they would persist. All benefits that occur after the first year of the study are discounted at an annual rate of 4.0 percent.

To estimate the benefits from increased output after the survey observation period, we assumed the dollar value of the impact on output in year 4 of the observation period would persist for the rest of the average participant's working lifetime. We made this extrapolation assumption for four main reasons. First, the impacts of Job Corps did not decline during years 3 and 4. In long-term studies of the returns to training, if returns decline, the decline occurs within two or three years after a trainee leaves the program (Lillard and Tan 1992; and Ashenfelter 1978). Second, Job Corps teaches many skills. The employment and training programs found to

have long-term impacts tend to be ones that teach a wide range of skills, such as the National Supported Work Demonstration (Couch 1992) and the Center for Employment Training (Zambrowski and Gordon 1993). On the other hand, those that have short-lived impacts, such as on-the-job training and other private-sector training (Lillard and Tan 1992) and classroom training under the Manpower Demonstration Training Act (Ashenfelter 1978), tend to teach specific skills. Third, we found that Job Corps improves literacy and numeracy skills, which we would expect to have long-lasting impacts (Glazerman et al. 2000). Finally, the 12 percent earnings gains in year 4 based on the survey data are similar to the returns of a year of school found in studies based on returns over people's entire working lives (Card 1999).

## **B. BENEFIT-COST ANALYSIS BASED ON SURVEY DATA ONLY**

In our initial benefit-cost analysis based on the survey data alone (McConnell and Glazerman 2001), the total benefits to society of Job Corps were estimated to be about \$31,000 per participant (Table V.1, column 1). As Job Corps costs society about \$14,000 per participant, these benefits exceeded costs by about \$17,000 per participant.

By far the largest component of the benefits was the increased output produced by Job Corps youth and measured by the increase in their earnings plus an estimate of the cost of fringe benefits and any output they produced during vocational training. This benefit comprised \$27,500 per participant, or 89 percent of the total value of the benefits. The benefits to society from the reduced use of other programs and services were about \$2,200 per participant and the benefits to society from reduced crime about \$1,200 per participant (Table D.1).

The assumption that the earnings impacts persist is key to the conclusions of this benefit-cost analysis. Within the four-year survey observation period, the analysis of the survey data concluded that Job Corps yielded total benefits to society of only \$4,200 per participant. As program costs are incurred during the first year or so of the observation period, program costs

TABLE V.1

BENEFITS AND COSTS UNDER DIFFERENT ASSUMPTIONS ABOUT THE SIZE OF THE EARNINGS  
IMPACTS AND THEIR DECAY, FOR THE FULL SAMPLE, BY DATA SOURCE  
(1995 Dollars)

	Unadjusted Survey Data		Annual Social Security Earnings Records		Adjusted Survey Data <sup>a</sup>	
	No Decay in Earnings Impacts	Decay Rate Observed in SER Data <sup>b</sup>	No Decay in Earnings Impacts	Decay Rate Observed in SER Data <sup>b</sup>	No Decay in Earnings Impacts	Decay Rate Observed in SER Data <sup>b</sup>
<b>Total Benefits</b>	30,957	4,751	10,244	3,587	19,168	3,695
Increased Output	27,531	1,326	6,818	161	15,742	269
Years 1-4	753	753	32	32	-60	-60
After Year 4	26,778	572	6,786	130	15,802	329
Other Benefits	3,426	3,426	3,426	3,426	3,426	3,426
<b>Program Costs</b>	-14,128	-14,128	-14,128	-14,128	-13,844	-13,844
<b>Net Benefits</b>	16,829	-9,377	-3,884	-10,541	+5,324	-10,150

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

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

<sup>b</sup>Assumes that impacts on earnings and child-care expenses decay at 68.3 percent per year—the rate of decay in the SER earnings impacts from the fourth year after random assignment to the seventh year after random assignment.

exceed benefits during the observation period by about \$9,900. The benefits that were expected to occur after year 4—\$26,800 per participant—are large in comparison.

Our conclusion that benefits to society exceed costs requires that the dollar value of the earnings impact in the last year of the observation period will persist for at least nine years without any decline. Alternatively, it requires that the earnings impact will decline at less than 8 percent each year until retirement.

The survey data suggest that Job Corps is a good deal for participants. They gain \$20,000 each, on average, mostly from increased earnings and fringe benefits net of increased taxes and child care costs (Table D.1). Even if Job Corps had no impact on post-program earnings, the value of the pay, food, and clothing Job Corps participants receive while enrolled in Job Corps generally offset the earnings and fringe benefits forgone while attending Job Corps.

The survey data also suggest that while the rest of society pays for Job Corps, it also shares in the benefits. While the government spends about \$16,500 on each participant,<sup>41</sup> most of these costs are offset by the increased taxes participants pay; the reduced use of education, training, and public assistance programs; and the reduced costs of crime. After these benefits are realized, the net cost of the program to the rest of society is only \$3,200 per participant (Table D.1).

### **C. REVISING THE ASSUMPTION OF PERSISTENCE OF EARNINGS IMPACTS**

As discussed in Chapter III, the administrative data do not support our initial assumption that the earnings impacts will persist. The earnings impacts estimated using both the SER and the UI data decline precipitously between the fourth and fifth year after random assignment, and continue to decline through the end of the seventh year (the last year for which we have data).

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<sup>41</sup> The program's cost to the government includes the cost of student pay, food, and clothing, which are treated as transfers and not included in the program's cost to society.

Between the fourth and seventh year, the earnings impacts for the full sample estimated using the SER data declined by about 68.3 percent per year.

Without a complete understanding of the reasons for the differences between the earnings impacts estimated from the administrative and survey data sources, we do not know whether the impacts estimated using survey data would have declined in the same way as the impacts estimated using the administrative data. It is also possible that positive earnings impacts may re-emerge as the sample matures or economic conditions change.

However, we believe it is realistic at this juncture to assume higher decay rates than we initially assumed for future earnings impacts. The pattern of impacts estimated using the administrative and survey data are similar in overlapping periods, and we have no reason to expect this pattern to diverge. In addition, if the difference between reported survey earnings and SER earnings is due to earnings from informal jobs, a decay in the earnings impacts estimated from administrative data without a decay in the earnings impacts estimated from the survey data would suggest a growing impact on earnings from informal jobs. It is difficult to envisage a situation in which Job Corps would have a growing impact on earnings from informal jobs at the same time as it had a declining impact on earnings from formal jobs.

Assuming that the survey impact estimates follow the same pattern of decline as those based on the administrative records data, the estimated benefits of Job Corps based on the survey earnings impacts do not exceed its costs. If the survey-based earnings impacts are assumed to decline by 68.3 percent per year—the same rate as observed in the SER data—the costs of Job Corps would exceed its benefits by about \$9,400 (Table V.1, column 2).<sup>42</sup>

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<sup>42</sup> In the initial benefit-cost analysis, we used estimates of child-bearing probabilities to extrapolate how the impacts on child-care costs would change after the observation period. However, with a rapid decline in the impacts on earnings, it is unrealistic to assume impacts on

Even if the earnings impacts based on the administrative records data did not decay, they are too small to justify the costs of the program. The earnings impacts estimated using both the SER and the UI data are smaller than the impacts estimated using the survey data. Even if we assume that the impacts in 1998 estimated using SER data persist without decay, costs would still exceed benefits by \$3,900 per person (Table V.1, column 3).<sup>43</sup> If, more realistically, we assume that the SER impact decays after the observation period by 68.3 percent per year for the rest of the person's working lifetime, estimates based on the SER data imply that costs exceed benefits by \$10,500 per participant (Table V.1, column 4).

#### **D. REVISIONS TO THE BENEFIT-COST ANALYSIS**

The analysis of the administrative data shed doubt on some key assumptions made in the initial benefit-cost analysis. Hence, we reestimated the benefits and costs of Job Corps in the light of the new information. Although we still base the estimates of the benefits from increased output on the earnings impacts based on the survey data, we adjust the estimates for nonresponse and overreporting of hours worked. We also replace the assumption of no decay in earnings impacts after the observation period with an assumption that the survey earnings impacts decay at the same rate as the SER earnings impacts.

##### **1. Adjusting for Nonresponse and Overreporting of Hours Worked**

Large differences exist between the impacts estimated using the survey and administrative data (Chapter III). As discussed in earlier chapters, considerable uncertainty surrounds the

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*(continued)*

child-care costs would decline at a much slower rate. Hence, for this analysis, we assume that child-care costs decline at the same rate as earnings impacts.

<sup>43</sup> For this exercise, we do not use the SER earnings impacts after 1998, but extrapolate from the 1998 earnings impacts.



causes of the differences between the earnings reported on the survey and on the administrative databases. As the earnings reported on the survey include earnings from a broader set of jobs, we believe that the earnings impacts based on the survey data, on balance, more accurately reflect the actual earnings of sample members than those based on administrative data. However, evidence also exists that the earnings impacts estimated from the survey data are too high as a result both of nonresponse and of overreporting of hours.

**Adjusting for Nonresponse.** Using the SER data, we found that the average earnings of survey respondents were higher than the average earnings of the full sample (Table III.3). We assumed that the average earnings of survey respondents would be higher than the average earnings of the full sample if there had been a 100 percent response rate, and by the same percentage as the SER earnings differed between the respondents and the full sample. Given this assumption, the survey earnings can be adjusted for nonresponse by multiplying the survey earnings for each research group, in each year after random assignment, by the ratio of SER earnings for the full sample by the SER earnings for the survey respondents (Table V.2).<sup>44</sup> For example, the survey earnings of the program group in year 1 were multiplied by 0.992—the average SER earnings for the full sample in 1995 (\$1,758.4) divided by the average SER earnings for the 48-month survey respondents in 1995 (\$1,772.1).<sup>45</sup> Reflecting the differences in the response rates between the program and control group and the lower response rates obtained in later follow-up surveys, the ratios of earnings for the full sample and survey respondents decrease over time and are slightly smaller in the program group than in the control group.

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<sup>44</sup> For the estimates for the older youth, we estimated the ratios using the subsample of older youth.

<sup>45</sup> See Table III.3.

TABLE V.2

RATIOS OF AVERAGE EARNINGS FOR THE FULL SAMPLE TO  
AVERAGE EARNINGS FOR SURVEY RESPONDENTS IN THE SER DATA

Year After Random Assignment	Program Group (Percentage)	Control Group (Percentage)
1	99.2	99.7
2	97.5	98.3
3	96.7	98.3
4	96.0	98.7

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records for the full research sample.

To illustrate the effect of this adjustment on the impacts on compensation (earnings plus the cost of fringe benefits), Table V.3 presents estimates of the impacts on compensation for the fourth year after random assignment. The first row shows the impacts on compensation based on unadjusted survey data. The second row shows the estimated impact on compensation in year 4 adjusted for survey nonresponse, which is \$1,030, 66.5 percent of the unadjusted estimate.

TABLE V.3

IMPACT ESTIMATES ON COMPENSATION IN THE FOURTH YEAR AFTER RANDOM ASSIGNMENT,  
BY DATA SOURCE AND ASSUMPTION  
(1995 Dollars)

Data Source/Assumption	Full Sample	20- to 24-Year-Olds
Survey	1,550	3,148
Survey, adjusted for nonresponse	1,030	2,352
Survey, adjusted for nonresponse and overreporting of hours	927	2,117
SER	414	857

SOURCES: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed the 48-month interview and annual social security earnings records for the full research sample.

Survey respondents were also in Job Corps longer on average than survey nonrespondents. The ratio of the length of stay for the full sample and the respondents was 98 percent. Adjusting the length of stay for this nonresponse decreases program costs per participant by about \$300 and has a small impact on the estimate of the value of output produced during vocational training.

**Adjusting for Overreporting of Hours Worked.** As discussed in the previous chapter, we suspect that survey respondents overreported the hours they worked. To adjust for this, we assume that members of both the program and control group overreport the hours they worked by 10 percent. Although further research into why the hours reported on the survey are higher than average is needed to derive a more precise estimate of the overreporting, we assumed an overreporting of 10 percent because this reflects the difference between the average hours reported by survey respondents and the national average. Adjusting for the overreporting of hours as well as nonresponse decreases the estimated impact on compensation in the fourth year after random assignment by a further 10 percent (Table V.3).

## **2. Revised Assumption on the Rate of Decay of Earnings Impacts**

The benefit-cost analysis based on survey data assumed that the impact on earnings in year 4 would persist for the rest of the youths' working lifetime. The findings from the administrative data shed doubt on this assumption. The revised estimates of the benefits and costs assume that the earnings impacts decay at the annual rate that is observed in the SER data between 1998 and 2001. For the full sample, this is 68.3 percent.

## **E. REVISED ESTIMATES OF BENEFITS AND COSTS**

The revised estimates of the benefits and costs differ significantly from the initial estimates derived using survey data only. The revisions to the benefit-cost analysis affect the estimate of the benefits of increased output, taxes paid, and program costs, but leave the estimates of the

other benefits unchanged. The major change is due to the changes in the estimate of the increased output after the observation period.

Details of the revised estimates of benefits and costs from the perspective of society, the participants, and the rest of society, for the full sample and for youth ages 20 to 24 at program application, are presented in Tables D.2 and D.3.

### **1. Benefits and Costs of Job Corps for the Full Sample**

After we adjust the earnings impacts and assume that they decay by 68.3 percent per year, benefits no longer exceed costs (Table V.1, column 6). Our best estimate is that the costs to society of Job Corps exceed its benefits to society by \$10,200 per participant. The rate of decay of the earnings impacts is key. If we assumed, unrealistically, that the adjusted earnings impacts would not decline after the observation period, benefits *exceed* costs by \$5,300 per participant (Table V.1, column 5).

The finding that costs exceed benefits for the full sample is not sensitive to small changes in assumptions. Even if we assumed there was no nonresponse bias or overreporting of hours, costs would exceed benefits (Table V.1, column 2). Costs would still exceed benefits if we had underestimated the benefits not related to increased output, the “other benefits,” by one-third. Further, the impacts on earnings need to decay by more than only 2.5 percent a year for costs to exceed benefits.

Even with earnings impacts that decay over time, the benefits of Job Corps still exceed the costs from the perspective of its *participants*. Using the adjusted survey data and assuming a decay rate of 68.3 percent, the benefits of Job Corps exceed its costs to participants by \$1,900 per participant (Table D.2). Even though our estimates of the impact on post-program earnings decreased, participants still benefit from Job Corps because the value of pay, food, and clothing

they receive in the program offset the earnings forgone while they are enrolled in Job Corps. The cost of Job Corps to the rest of society is \$12,000 per participant (Table D.2).

## **2. Benefits and Costs of Job Corps for the Older Youth**

In the initial benefit-cost analysis based only on the survey data, we concluded that benefits exceeded costs for most subgroups of interest (McConnell and Glazerman 2001). However, as the earnings impacts decay rapidly in the administrative data for nearly all subgroups, under the revised assumptions, costs exceed benefits for most subgroups.

A possible exception is the subgroup of youth who were 20 to 24 years old at the time of program application. The impacts on compensation estimated from both the SER and the survey data are about twice as large for this group than for the entire sample (Table V.3). Moreover, the SER earnings impacts for the 20- to 24-year-olds decay much less rapidly than for other youth—by only 5.9 percent a year.

Benefits other than increased output tend to be smaller for the older youth. The benefit of reduced use of high school is \$1,200 for the full sample but negligible for the older youth. Even the benefits of the reduction in the use of education and training programs other than high school are smaller for the older youth, \$630 per participant, compared with \$880 per participant for the full sample. Surprisingly, the “benefit” of reduced crime is negative for the older youth (−\$3,800, compared with +\$1,200 for the full sample), as the impact on murder for the older group is positive.

By construction, program cost estimates vary only by the centers attended by the youth, whether the youth is residential or nonresidential, and the length of time he or she is enrolled in Job Corps. Because older youth stay longer in Job Corps, estimated program costs to society of serving them are slightly higher than for the full sample—\$15,500 per participant for the older

youth (Table V.4), compared with \$14,100 per participant for the full sample (Table V.1),<sup>46</sup> despite the fact that the older youths are more likely to be nonresidential.

Our cost estimates may overstate the additional costs of serving older youth, however. Our estimates of costs did not vary between older and younger students who attended the same center and spent the same length of time there. The older youths who, according to the findings of the process analysis, are easier to serve than the younger youth (Johnson et al. 1999) may be also less costly to serve.

When the survey estimate of earnings impacts is adjusted for nonresponse and overreporting of hours worked, the revised estimates of benefits fall short of the costs of the program for this group, but just barely. They are only \$500 per participant less than costs (Table V.4, column 2).

Unlike the findings from the full sample, the finding that costs exceed benefits for the older youth is sensitive to small changes in assumptions. For example, if we adjust the impact estimates for nonresponse only, benefits exceed costs by \$1,500 (Table V.5, column 3). In fact, benefits still exceed costs if we assumed hours were overreported by only 5 percent (not shown). In addition, if we fully adjust earnings impacts but lower the assumed discount rate from 4.0 percent to 3.7 percent, benefits would exceed costs (not shown).

The revised estimate of benefits for the older group is also sensitive to the estimate of the impact of Job Corps on murder. For the older youth, we estimated that Job Corps *increases* the likelihood of being arrested for murder by 5.13 per 1,000 persons (p-value = 0.043). Six older youth in the program group were arrested for murder, compared to zero control group members. Our judgment is that this positive impact occurred by chance—it is difficult to believe that participation in Job Corps actually increases the probability of a youth committing a murder.

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<sup>46</sup> These figures refer to the unadjusted estimates of program costs to society.

TABLE V.4

BENEFITS AND COSTS UNDER DIFFERENT ASSUMPTIONS ABOUT THE SIZE OF THE EARNINGS IMPACTS AND THEIR DECAY, FOR YOUTH 20 TO 24 YEARS OF AGE AT PROGRAM APPLICATION (1995 Dollars)

	Unadjusted Survey Data	Survey Data Fully Adjusted <sup>a</sup>	Survey Data Adjusted Only for Nonresponse	Survey Data Fully Adjusted <sup>a</sup> and Zero Murder Impacts
	Earnings Impact Decay Rate Observed in SER Data <sup>b</sup>	Earnings Impact Decay Rate Observed in SER Data <sup>b</sup>	Earnings Impact Decay Rate Observed in SER Data <sup>b</sup>	Earnings Impact Decay Rate Observed in SER Data <sup>b</sup>
<b>Total Benefits</b>	24,141	14,696	16,712	19,034
Increased Output	26,991	17,547	19,562	17,547
Years 1-4	1,502	588	657	588
After Year 4	25,489	16,959	18,905	16,959
Other Benefits	-2,850	-2,850	-2,850	1,487
<b>Program Costs</b>	-15,493	-15,193	-15,193	-15,193
<b>Net Benefits</b>	8,648	-496	+1,519	+3,841

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

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

<sup>b</sup>Assumes that impacts on earnings decay at 5.9 percent per year—the rate of decay in the SER earnings impacts from the fourth year after random assignment to the seventh year after random assignment for the youth 20-24 years old at program application.

Each murder imposes a large cost on society—we estimated it to be \$676,300. Hence, the size of the impact on murder makes a large difference to the overall estimate. If we set the impact of Job Corps on murder to zero, estimated benefits of the program would exceed costs by \$3,800 (Table V.4, column 4).

## **F. CONCLUSIONS**

The original benefit-cost analysis, which was based on survey data only, concluded that the benefits to society from Job Corps exceed its costs. The administrative data provided additional information, which we used to revise the analysis. Our revised analysis, which used survey estimates of the impact on compensation that are adjusted for nonresponse and overreporting of hours worked and assumed a faster decay in earnings impacts, concluded that the costs of Job Corps exceed its benefits for the full sample. Benefits may exceed costs for the older youth who attend Job Corps.

Because Job Corps is an expensive program, it must have long-term effects for its benefits to exceed its costs. The key reason that the findings changed so dramatically is the replacement of the assumption that the earnings impacts persist after the observation period with the assumption that they decay rapidly—the latter assumption being supported by the findings from the administrative data. While we believe the revised assumption to be more realistic given the findings from the administrative data, the trajectory of future earnings for members of the program and control group is still unknown.

However, Job Corps should be judged not merely by the size of its quantified benefits and costs. The analysis was designed to quantify many benefits and costs, but, like all such analyses, it could not be comprehensive. For example, we do not quantify the benefits to the general public of less crime or the benefits to participants of any improvement in quality of life. The benefit-cost analysis also examines the program from the point of view of efficiency—a dollar to



taxpayers is valued the same as a dollar to Job Corps participants—and does not take into account any beneficial distributional effects of the program.



## VI. CONCLUDING REMARKS

This report has examined the following two questions pertaining to the impacts of Job Corps on the labor market outcomes of program participants:

1. Do administrative (SER and UI) and survey data yield similar impact estimates on employment and earnings during the four-year follow-up period covered by the survey?
2. Using the administrative data, what are estimated impacts on employment and earnings in the two and a half years beyond the four-year period covered by the survey?

The answer to the first question is that the *pattern* of the estimated earnings impacts using the survey and administrative data is similar in overlapping periods. According to each data source, the impacts are negative in 1995 and 1996 (when the program group was enrolled in Job Corps) and positive and statistically significant in 1997 and 1998. However, the survey-based impact estimates are larger and more often statistically significant. This occurs primarily because reported earnings *levels* are much higher according to the survey data for a large percentage of sample members.

We also find that the estimated 1998 earnings *gains* (in percentage terms relative to the control group means) are larger using the survey than administrative data. This occurs because (1) reporting differences between the two data sources are slightly larger for the program than control group, and (2) the survey estimates are slightly biased upward due to interview nonresponse bias. The available evidence suggests that the interview nonresponse bias is not due to differences in the baseline characteristics of program and control group respondents that are correlated with earnings, but to *true* differences in the earnings impacts between interview respondents and nonrespondents.

The answer to the second question is that, based on the administrative data, we find no impacts of Job Corps on employment or earnings after the four-year period covered by the survey. The estimated impacts on calendar year earnings in 1999 to 2001 and on quarterly earnings in quarters 17 to 26 after random assignment are all near zero, with none statistically significant. The earnings impacts in the postsurvey period for 48-month interview respondents only are slightly larger but are also not statistically significant. These impact findings hold for the full sample, as well as for key student subgroups. There is, however, some evidence of persistent positive earnings gains for those ages 20 to 24 and those with a high school credential at program application.

Revised benefit-cost estimates, which incorporate the impact findings using the administrative records data, suggest that the benefits to society of participating in Job Corps are smaller than the substantial program costs, under most reasonable assumptions. These revised estimates are based on the assumption that earnings impacts will decay more rapidly than we assumed in our previous benefit-cost analysis based on the survey data (McConnell and Glazerman 2001). For that initial analysis, our best estimate assumed that the survey-based impact estimates found in years 3 and 4 after random assignment would persist without decline. This assumption generates program benefits that exceed program costs from society's perspective. Sensitivity tests showed that benefits would exceed costs, even with reductions in the earnings impact as large as 8 percent per year. The administrative-based impact findings in 1999, 2000, and 2001, however, do not support the assumption that the earnings impacts will persist without decline or with only modest decline. Thus, our revised analysis assumed a higher decay rate in the earnings impacts, which produced substantially smaller estimates of program benefits.

Job Corps, however, may be cost-effective for the 20- to 24-year-olds, whose earnings impacts persisted during the three-year postsurvey period. We find that benefits to society are similar to program costs for this group, under the assumption that the positive earnings impacts in 1998 to 2001 persist with a 6 percent annual decay (which is the decrease in the SER impacts observed for these youths between 1998 and 2001).

The revised benefit-cost analysis finds also that Job Corps benefits exceed costs from the perspective of *participants*. Job Corps is a good deal for participants because the increases in their postprogram earnings and fringe benefits, and the value of pay, food, and clothing they receive in the program offsets the earnings forgone while they are enrolled in Job Corps. This finding is important, because the benefit-cost analysis from society's perspective examines the program from an efficiency point of view—a dollar to taxpayers is valued equally to a dollar to Job Corps participants—but does not take into account any distributional effects of the program.

We stress that it is too early to conclude that the zero earnings impacts for the full sample will persist. Little research exists on the long-term earnings growth of youths similar to those served by Job Corps or on the long-term earnings impacts of youth training programs. Consequently, we cannot discount the possibility that positive earnings impacts might re-emerge. Job Corps may increase the long-term earnings of those who were 16 to 19 years old at the time of program application as they mature into their late 20s and 30s, find stable jobs, and experience the full benefits of program participation, both from increased vocational and academic skills gained in the program, as well as from improved social skills and attitudes towards work. The persistent earnings impacts among students who were 20 to 24 at the time of program application raise this as a possibility. Again, we stress that no solid empirical evidence exists about the time patterns of earnings and longer term earnings impacts of an intensive training program like Job Corps.

Economic conditions might also influence the earnings impacts. The National Job Corps Study was conducted during a period of strong economic growth with low unemployment and inflation. Because the literature contains some evidence that those with lower skills benefit more from a strong economy (that is, a tight labor market) than those with higher skills, the strong economy may have favored the lower-skilled control group. This raises the possibility that impacts might be larger during a period of slower economic growth or recession.

The limited available evidence, however, does not support this hypothesis. Earnings impacts remained small in 2001 as the economy started to weaken and the employment rate declined for both the program and control groups. These findings suggest that the earnings impacts will not be much affected by economic downturns. However, additional follow-up data need to be collected before definitive conclusions are warranted.

We stress also that uncertainty remains about whether estimated impacts based on survey data would have also disappeared in the postsurvey period, because of substantial differences between reported earnings levels as measured by the survey and administrative records data. As discussed, the higher reporting levels in the survey data yield estimated impacts that are larger using the survey data. Thus, if additional follow-up survey data had been collected, the survey-based impact estimates might not have decayed as rapidly in 2000 and 2001 as those based on the administrative records data.

We believe it is unlikely, however, that additional follow-up survey data would have yielded qualitatively different conclusions than the administrative data, for several reasons. First, the *pattern* of earnings impacts using the SER and survey data are similar during the period covered by the survey, suggesting that impact estimates using the survey data would have also decreased. Second, it would be difficult to interpret a finding of positive impacts according to the survey data, but not according to the administrative records data; such a finding would imply that Job

Corps participation has an effect on earnings from informal jobs that are not covered in the administrative records data, but has no effect on earnings from formal jobs that are covered in these data.

Clearly, we will never know the extent to which the survey-based impacts would have persisted. However, understanding the sources of differences between reported earnings levels in the administrative and survey data can help address this issue. We identified important factors associated with the much higher quarter 16 employment rate as reported in the UI than survey data, including SSNs that may have been incorrectly reported by employers or sample members, the noncoverage of some formal jobs under the UI program, and informal jobs captured in the survey but not the UI data. In addition, we found that the likely overreporting of hours worked in the survey data is a key factor associated with the higher reported earnings per job levels in the survey data. Because of data constraints, however, there remain substantial unobserved factors that account for the employment and earnings differences across the two data sources.

We believe that the impact findings for the older youth can help guide future program improvement. Job Corps appears to have a longer-term beneficial effect on the earnings of older students than younger ones. The positive earnings gains for those ages 20 to 24 persisted during the postsurvey period. Positive earnings gains were found for the 16- and 17-year-olds soon after they left the program, but these initial gains soon disappeared. No earnings gains were ever found for those ages 18 and 19.

What factors led to the differences in the impact findings by age? One important factor is that the 20- to 24-year-olds are more highly motivated, mature, and well-behaved than those younger (according to program staff). Younger students also exhibit several characteristics at program entry that suggest they are more disadvantaged and harder to serve than older students. For example, a higher proportion of younger students reported in the baseline interview having

used drugs, having ever been arrested, living in single-parent households, and coming from families that receive public assistance. Consequently, it is not surprising that older students typically remain in Job Corps longer than younger ones.

Another potentially important factor is that because older students enter the program with higher education levels than younger ones (about 50 percent of those age 20 to 24 have a high school credential, compared to only 13 percent of those younger), the older youth focus more on vocational training while in Job Corps and less on academic classroom education (and, in particular, GED preparation classes). In addition, many of the younger sample members in the control group returned to high school after being rejected from Job Corps, whereas fewer older control group members enrolled in alternative education and training programs. Consequently, impacts on time spent in education and training are larger for the older than younger sample members.

These results suggest that to improve overall program effectiveness, Job Corps needs to fully address differences by age in program structure and experience, and perhaps, to reassess the target population served by the program. These program improvements could lead to more persistent earnings impacts for the youngest students.

Finally, it is important to emphasize that the findings presented in this report pertain to the Job Corps program as it operated in 1995 and 1996 (when our program group members were enrolled in Job Corps), and not necessarily to the program as it operates today. There have been a number of significant changes that Job Corps has recently implemented in response to WIA provisions and other factors. For example, more Job Corps centers are now accredited to award high school diplomas, and Job Corps is more focused on providing longer-term support and placement services for their former students. These changes may have improved program effectiveness.



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## **APPENDIX A**

### **SELECTING STATES AND SAMPLE MEMBERS FOR THE UI STUDY AND OBTAINING UI-BASED IMPACT ESTIMATES USING THE FULL SAMPLE**



This appendix first discusses the process of selecting states for UI data collection and the results of this selection process. Second, it discusses our method for estimating UI-based earnings impacts and their variances using the full sample that adjusts for the fact that some sample members did not live in the selected states during the follow-up period. Finally, it presents the full nonresponse analysis comparing the baseline characteristics of sample members who did and did not sign the records release consent form.

## 1. Selecting States for UI Data Collection

We randomly selected 25 states for the UI study using systematic sampling techniques, where the probability a state was chosen was proportional to the number of 1993 Job Corps enrollees who lived in that state. Table A.1 displays (1) the state distribution of Job Corps enrollees in 1993; (2) sampling probabilities that were used to select the 25 states; (3) the certainty states (that is, states selected with probability 1 because they contain a large number of Job Corps enrollees); and (4) the 25 selected states. The states are ordered from those with the most to the least number of Job Corps enrollees in 1993. Table A.2 displays the selected states by region and compares the regional distribution of enrollees in the selected states and in all states.

Key features of the sampling results are as follows:

- ***Eleven states were selected with certainty.*** These states would have been selected at least once using our systematic sampling procedure (that is, had selection probabilities greater than 1). These states were removed from the sampling to avoid the possibility that they could be selected more than once. Certainty states were those that (1) had initial sampling probabilities greater than 1 (as shown in column 4) or (2) had sampling probabilities greater than 1 after removing previously identified certainty states.
- ***The 25 selected states contain about 79 percent of all Job Corps enrollees (excluding those in Massachusetts and New York).*** This occurred because 20 of the 22 states with the largest Job Corps populations were selected.

TABLE A.1  
 SAMPLING PROBABILITIES FOR SELECTING STATES FOR THE UI STUDY  
 AND SELECTION RESULTS

State	State Distribution for 1993 Enrollees (Percents)	State Distribution Excluding NY and MA (Percents)	Expected Number of Selections (Probability of Selection)	Certainty State <sup>a</sup>	State Distribution for Noncertainty States (Percents)	Final Probability of Selection	State Selected
TX	8.7	9.3	2.32	Yes		1	Yes
CA	6.9	7.3	1.84	Yes		1	Yes
FL	4.8	5.1	1.28	Yes		1	Yes
NY	4.7						
PA	4.7	5.0	1.25	Yes		1	Yes
GA	3.9	4.2	1.04	Yes		1	Yes
NC	3.9	4.2	1.04	Yes		1	Yes
MO	3.7	3.9	0.99	Yes		1	Yes
MS	3.6	3.8	0.96	Yes		1	Yes
LA	3.3	3.5	0.88	Yes		1	Yes
OH	3.3	3.5	0.88	Yes		1	Yes
VA	3.1	3.3	0.83	Yes		1	Yes
WA	2.9	3.1	0.77		6.6	0.92	Yes
IL	2.7	2.9	0.72		6.1	0.86	Yes
MD	2.7	2.9	0.72		6.1	0.86	Yes
SC	2.4	2.6	0.64		5.5	0.76	Yes
OR	2.3	2.4	0.61		5.2	0.73	
MI	2.2	2.3	0.59		5.0	0.70	Yes
AL	2.1	2.2	0.56		4.8	0.67	
AR	1.9	2.0	0.51		4.3	0.60	Yes
AZ	1.9	2.0	0.51		4.3	0.60	Yes
NJ	1.9	2.0	0.51		4.3	0.60	Yes
OK	1.9	2.0	0.51		4.3	0.60	Yes
TN	1.9	2.0	0.51		4.3	0.60	
MA	1.4						
NM	1.4	1.5	0.37		3.2	0.45	
CO	1.3	1.4	0.35		3.0	0.41	
DC	1.1	1.2	0.29		2.5	0.35	
IN	1.1	1.2	0.29		2.5	0.35	
UT	1.1	1.2	0.29		2.5	0.35	
KS	1.0	1.1	0.27		2.3	0.32	Yes
MT	0.9	1.0	0.24		2.0	0.29	
IA	0.8	0.9	0.21		1.8	0.25	
ME	0.8	0.9	0.21		1.8	0.25	Yes
MN	0.8	0.9	0.21		1.8	0.25	
CT	0.7	0.7	0.19		1.6	0.22	
KY	0.7	0.7	0.19		1.6	0.22	
NE	0.7	0.7	0.19		1.6	0.22	Yes
ID	0.6	0.6	0.16		1.4	0.19	Yes
NV	0.6	0.6	0.16		1.4	0.19	Yes
SD	0.6	0.6	0.16		1.4	0.19	
WI	0.5	0.5	0.13		1.1	0.16	

TABLE A.1 (continued)

State	State Distribution for 1993 Enrollees (Percents)	State Distribution Excluding NY and MA (Percents)	Expected Number of Selections (Probability of Selection)	Certainty State <sup>a</sup>	State Distribution for Noncertainty States (Percents)	Final Probability of Selection	State Selected
WV	0.5	0.5	0.13		1.1	0.16	
DE	0.4	0.4	0.11		0.9	0.13	
ND	0.4	0.4	0.11		0.9	0.13	
NH	0.3	0.3	0.08		0.7	0.10	
RI	0.3	0.3	0.08		0.7	0.10	
VT	0.3	0.3	0.08		0.7	0.10	
WY	0.3	0.3	0.08		0.7	0.10	
<b>Total</b>	<b>100</b>	<b>100</b>	<b>25</b>	<b>11</b>	<b>100</b>	<b>25</b>	<b>25</b>

<sup>a</sup>Certainty states are states with probabilities of selection greater than 1 either from column 4 or after removing other certainty states.

- ***The states are geographically dispersed.*** As Table A.2 shows, the distribution by region of enrollees in the selected states is similar to that in all states. The proportion of states selected within each region differed somewhat, however, because of differences in state enrollments by region. For example, we selected 5 of 8 states in region 4, but only 3 of 10 states in region 7/8, because region 7/8 contains some states with a small number of Job Corps enrollees and thus had low probabilities of selection.

Finally, we used the 1993 distribution of enrollees for sampling rather than the actual state distribution of eligible applicants in the sample universe for the National Job Corps Study, because we selected the states near the beginning of the random assignment period in mid-1995. Consequently, we did not know the state distribution of eligible applicants when the states were selected. The two state distributions, however, are similar (see Table A.3). In the next section, we discuss the poststratification methods that we used to adjust the sample weights for the small differences in these distributions.

## **2. Estimating UI-Based Impacts and Variances Using the Full Sample**

The states selected for UI records collection were chosen randomly. The random selection of states ensures that, in principle, the expected number of sample members who moved into the selected states during the follow-up period should be equal to the expected number of sample members who moved out of the selected states during that time. Thus, the expected number of sample members who resided in the selected states should have remained constant over time.

As discussed in Section II, the survey data support this steady-state assumption. Thus, mean earnings for the program group in a particular state can be estimated by summing the earnings for *all* program group members in that state and dividing this sum by the number of program group members who lived in that state at application to Job Corps, and similarly for the control group. The estimated impact per eligible applicant for a state can then be obtained as the



TABLE A.2

THE SELECTED STATES FOR THE UI STUDY BY REGION, AND THE DISTRIBUTION BY REGION  
OF JOB CORPS ENROLLEES IN THE 25 SELECTED STATES AND IN ALL STATES  
(EXCLUDING NEW YORK AND MASSACHUSETTS)

Region	States Selected	Number Selected/ Number in Region	Proportion of Enrollees in 25 Selected States (Percents)	Proportion of Enrollees in All States (Percents)
1 and 2	Maine New Jersey	2 of 6	3.6	4.5
3	Maryland Pennsylvania Virginia	3 of 6	14.3	13.4
4	Florida Georgia Mississippi North Carolina South Carolina	5 of 8	25.1	24.7
5	Illinois Michigan Ohio	3 of 6	11.1	11.4
6	Arkansas Louisiana Oklahoma Texas	4 of 5	21.3	18.3
7/8	Kansas Missouri Nebraska	3 of 10	7.2	11.6
9	Arizona California Nevada	3 of 3	12.7	10.0
10	Idaho Washington	2 of 3	4.7	6.2
<b>Sample Size</b>		<b>25 of 48</b>	<b>44,424</b>	<b>56,357</b>

Note: States included in each region are as follows: Region 1: Connecticut, Maine, Massachusetts (excluded), New Hampshire, Rhode Island, Vermont; Region 2: New Jersey, New York (excluded); Region 3: Delaware, District of Columbia, Maryland, Pennsylvania, Virginia, West Virginia; Region 4: Alabama, Florida, Georgia, Kentucky, Mississippi, North Carolina, South Carolina, Tennessee; Region 5: Illinois, Indiana, Michigan, Minnesota, Ohio, Wisconsin; Region 6: Arkansas, Louisiana, New Mexico, Oklahoma, Texas; Region 7/8: Colorado, Iowa, Kansas, Missouri, Montana, Nebraska, North Dakota, South Dakota, Utah, Wyoming; Region 9: Arizona, California, Nevada; Region 10: Idaho, Oregon, Washington.

TABLE A.3

THE DISTRIBUTION OF 1993 JOB CORPS ENROLLEES AND THE DISTRIBUTION OF  
ELIGIBLE APPLICANTS IN THE SAMPLE UNIVERSE FOR THE  
NATIONAL JOB CORPS STUDY, BY STATE  
(EXCLUDING NEW YORK AND  
MASSACHUSETTS)

State	State Distribution for 1993 Enrollees (Percents)	State Distribution for Eligible Applicants in the Sample Universe (Percents)
TX	8.7	7.8
CA	6.9	7.1
FL	4.8	5.5
NY	4.7	6.0
PA	4.7	5.0
GA	3.9	4.2
NC	3.9	3.0
MO	3.7	4.8
MS	3.6	3.0
LA	3.3	2.8
OH	3.3	2.7
VA	3.1	3.5
WA	2.9	2.6
IL	2.7	3.0
MD	2.7	2.2
SC	2.4	2.8
OR	2.3	1.7
MI	2.2	1.8
AL	2.1	2.3
AR	1.9	1.1
AZ	1.9	1.8
NJ	1.9	1.5
OK	1.9	1.9
TN	1.9	1.2
MA	1.4	1.9
NM	1.4	1.3
CO	1.3	1.4
DC	1.1	1.1
IN	1.1	1.5
UT	1.1	1.0
KS	1.0	0.9
MT	0.9	0.6
IA	0.8	0.8

TABLE A.3 (continued)

State	State Distribution for 1993 Enrollees (Percents)	State Distribution for Eligible Applicants in the Sample Universe (Percents)
ME	0.8	1.0
MN	0.8	0.9
CT	0.7	0.8
KY	0.7	1.0
NE	0.7	1.1
ID	0.6	0.7
NV	0.6	0.3
SD	0.6	0.7
WI	0.5	0.6
WV	0.5	0.7
DE	0.4	0.5
ND	0.4	0.6
NH	0.3	0.3
RI	0.3	0.3
VT	0.3	0.2
WY	0.3	0.6
<b>Total</b>	<b>100</b>	<b>100</b>

difference between the program and control group means in that state, and the estimated impact for the study population is a weighted average of these state impacts.

Mathematically, an unbiased impact estimate for a UI-based outcome measure can be obtained using the following series of equations:

$$(1) I_1 = \sum_s p_{cs} I_{cs} + p_n \bar{I}_n,$$

$$(2) I_{cs} = \frac{\sum_{i \in P} w_i y_{Pi}}{\sum_{i \in P} w_i L_{csi}} - \frac{\sum_{i \in C} w_i y_{Ci}}{\sum_{i \in C} w_i L_{csi}}$$

$$(3) \bar{I}_n = \frac{1}{n_n} \sum_s I_{ns}$$

$$(4) I_{ns} = \frac{\sum_{i \in P} w_i y_{Pi}}{\sum_{i \in P} w_i L_{nsi}} - \frac{\sum_{i \in C} w_i y_{Ci}}{\sum_{i \in C} w_i L_{nsi}}$$

where,

$p_{cs}$  = the proportion of all eligible applicants who lived in *certainty* state  $s$  at application to Job Corps

$y_{Pi}$  = the outcome measure for program group member  $i$

$y_{Ci}$  = the outcome measure for control group member  $i$

$w_i$  = the original sample design weight for individual  $i$  based on the probability that the youth was selected to the program or control group, which was adjusted using propensity scoring methods to account for nonresponse to the consent form for records release (see the next section)

$L_{csi}$  = 1 if the sample member lived in certainty state  $s$  at application to Job Corps and 0 otherwise

$I_{cs}$  = the impact per eligible applicant in certainty state  $s$  and is calculated as the difference in the weighted mean outcomes between program and control group members. As discussed, the mean for the program group is calculated by taking a weighted sum of the outcome ( $y_{Pi}$ ) for the *full* sample of program group members and dividing this sum by

the weighted number of program group members *who lived in state s at application to Job Corps*, and similarly for the control group mean.

$p_n$  = the percentage of all eligible applicants who lived in *all noncertainty* states at application to Job Corps

$L_{nsi}$  = 1 if the sample member lived in noncertainty state  $s$  at application to Job Corps and 0 otherwise

$I_{ns}$  = the impact per eligible applicant in noncertainty state  $s$  and is constructed using the same procedure as for  $I_{cs}$

$n_n$  = the number of noncertainty states in the UI sample

$I_n$  = the (unweighted) average of the estimated impacts in the noncertainty states

The impact estimate is a weighted average of the (1) estimated impacts in each certainty state, and (2) the average impact in the noncertainty states. Each certainty state represents itself (because each of these states was selected with probability 1). In addition, the simple average of the estimated impacts in each of the sampled noncertainty states is an unbiased estimate of program impacts for the universe of noncertainty states, because these states were selected with probabilities proportional to size (Cochran 1982). We weight (poststratify) the various impact pieces using the actual state distribution of eligible applicants in our sample universe (that is, using  $p_{cs}$  and  $p_n$ ) rather than using the state distribution of enrollees in 1993.

To test the sensitivity of our results to alternative weighting schemes, we also calculated the following variant of equation (1):

$$(5) I_2 = \sum_s p_{cs} I_{cs} + \sum_R p_{nR} \bar{I}_{nR},$$

where  $R$  represents regions, and where  $p_{nR}$  is the proportion of all eligible applicants in the noncertainty states who are in region  $R$ . This procedure adjusts for states that refused to provide

their UI data (that is, for state nonresponse) by assuming that nonresponding states are represented by responding states in the same region. We calculated two sets of impacts using equation (5): one that excluded Massachusetts and New York, and another that included these states (so that, for example, New Jersey and Maine represent New York and Massachusetts). These results were similar to those obtained using equation (1).

Equation (1) can be used to directly estimate impacts for earnings. However, the procedure will produce an estimate of the employment rate in each state that is biased upward (that is, too large). This is because the estimated employment rate in a state represents the employment rate for those who *ever* worked in the state, rather than the desired employment rate (across all states) for those who initially lived in the state at program application. For example, suppose there was a sample member (youth A) who lived and worked in California, but who moved out of state after one year, and suppose there was another person (youth B) who moved into California after one year and was employed there. In the earnings calculations, the California earnings of youth B proxy for the out-of-state earnings of youth A. However, in the calculation of the employment rate in California, *both* youth A and youth B will be counted as having been employed, thereby artificially increasing the employment rate estimate.

To account for this problem, we used the survey data to calculate the average proportion of time that youths living in a particular state at program application remained in that state during the follow-up period. We then multiplied the binary indicator variables signifying whether the youth was employed in a particular state by these average proportions. These adjusted employment indicator variables were used in the analysis.

The variance of the impact estimate in equation (1) was estimated using the following equations:

$$(6) \text{ var}(I_1) = \sum_s p_{cs}^2 \text{ var}(I_{cs}) + p_n^2 \text{ var}(\bar{I}_n)$$

$$(7) \text{ var}(\bar{I}_n) = \frac{\sum_s (I_{ns} - \bar{I}_n)^2}{n_n(n_n - 1)},$$

where  $\text{var}(I_{cs})$  is the usual variance formula for the difference between two weighted means and the other terms are defined as above. The  $\text{var}(I_{cs})$  term for a particular certainty state was calculated using the total 22-state earnings for those who lived in the certainty state at baseline.

This variance formula presented above assumes that the UI states were selected with replacement. Thus, we also estimated the variance using the statistical package SUDAAN to account for the fact that states were actually selected without replacement (so that a finite sample correction could be used for the noncertainty states). SUDAAN was run using the sample of those who lived in the selected states at baseline and their total 22-state earnings. For this analysis, the weight for a youth was constructed to be inversely proportional to the product of (1) the probability that the youth's state was selected for the UI study, (2) the probability that the youth's state provided data (which was assumed to be 22/25), (3) the probability that the youth was selected from within the state (which was assumed to be the probability that the youth was selected into the sample for the National Job Corps Study), and (4) the probability that the youth signed the records release consent form (which was estimated using propensity scoring methods).

The estimated variances using SUDAAN were similar to those from equation (6). The significance levels of the estimated impacts presented in this report are based on the variance estimates obtained using equation (6).

### **3. Nonresponse to the Records Release Consent Form**

As discussed in Chapter II, only youths in the research sample who signed the consent form for records release were included in the UI study. About 79 percent of the research sample signed the consent form (78 percent for the control group and 79 percent for the program group). To examine the extent of nonresponse bias to the consent form, we compared the observable characteristics of signers to the full sample of signers and nonsigners for the program and control groups (Table A.4). This analysis was conducted using data from the Job Corps intake (ETA-652) forms that were completed at program application (before random assignment) and are available for both signers and nonsigners. We conducted t-tests for binary and continuous variables and chi-squared tests for categorical variables to gauge the similarity of the characteristics of signers and the full sample. We conducted separate tests for the program and control groups. We also conducted tests for the hypothesis that the characteristics of signers and the full sample are jointly similar. Finally, we conducted similar statistical tests comparing the characteristics of signers in the program and control groups.

There were some important differences in the characteristics of signers and the full sample of signers and nonsigners for both the program and control groups. For example, those in regions 1 to 3, more populated areas (such as PMSAs or superdense areas), and those ever convicted or adjudged delinquent were less likely to sign the consent form than their counterparts. Signee rates were significantly lower for blacks and Hispanics than for whites, for those who applied earlier than later, and for those in smaller families than larger ones. Furthermore, a joint statistical test of the hypothesis that the distribution of baseline characteristics of signees and the full sample are similar was rejected at the 1 percent significance level for both program and control group members. Thus, using the propensity scoring methods discussed in Schochet 2001, we adjusted the sample weights to adjust for nonresponse to the consent form, so that the UI-



TABLE A.4

COMPARISON OF THE CHARACTERISTICS OF THOSE WHO SIGNED THE RECORDS RELEASE  
CONSENT FORM AND OF THE FULL SAMPLE OF SIGNERS AND NON-SIGNERS,  
BY RESEARCH STATUS  
(Percentages)

Characteristic <sup>a</sup>	Control Group		Program Group	
	Signers of the Consent Form <sup>b</sup>	Full Sample	Signers of the Consent Form <sup>b</sup>	Full Sample
<b>Demographic Characteristics</b>				
Male	60.0	59.7	59.3	59.2
Age at Application				
16 to 17	39.8	40.1	39.2*	39.7
18 to 19	32.8	32.3	32.6	32.2
20 to 21	16.8	16.8	17.1	16.8
22 to 24	10.6	10.8	11.1	11.4
(Average age)	(18.9)	(18.9)	(19.0)	(19.0)
Race/Ethnicity				
White, non-Hispanic	27.3*	26.4	28.0***	27.1
Black, non-Hispanic	47.1	47.8	47.4	47.8
Hispanic	17.8	18.1	16.9	17.5
Other	7.8	7.7	7.8	7.6
Region				
1	3.0***	4.6	3.0***	4.4
2	5.8	7.7	5.7	7.2
3	9.2	13.0	9.3	13.0
4	27.0	22.7	27.2	23.4
5	11.1	10.5	11.1	10.3
6	16.6	14.7	16.8	15.2
7/8	11.9	12.1	12.3	12.7
9	9.8	9.6	9.5	9.0
10	5.5	5.1	5.3	4.8
Size of City of Residence				
Less than 2,500	9.1***	8.5	9.2***	8.7
2,500 to 10,000	11.9	11.3	11.8	11.2
10,000 to 50,000	20.1	19.2	20.8	19.7
50,000 to 250,000	17.1	17.6	17.8	17.6
250,000 or more	41.8	43.5	40.3	42.8
PMSA or MSA Residence Status				
In PMSA	27.8***	32.6	27.4***	31.7
In MSA	47.5	45.2	47.8	45.8
In neither	24.7	22.2	24.8	22.6
Density of Area of Residence				
Superdense	31.9***	35.1	31.0***	33.9
Dense	27.9	28.2	28.7	28.8

TABLE A.4 (continued)

Characteristic <sup>a</sup>	Control Group		Program Group	
	Signers of the Consent Form <sup>b</sup>	Full Sample	Signers of the Consent Form <sup>b</sup>	Full Sample
Nondense	40.2	36.7	40.4	37.3
Lived in 57 Areas with a Large Concentration of Nonresidential Females	29.6***	31.2	29.1***	30.8
Legal U.S. Resident*	98.9	98.8	98.6	98.6
Job Corps Application Date				
11/94 to 2/95	23.4***	22.2	23.3***	22.6
3/95 to 6/95	29.1	29.2	29.2	29.1
7/95 to 9/95	27.3	28.0	27.4	27.6
10/95 to 12/95	20.1	20.6	20.1	20.6
<b>Fertility and Family Status</b>				
Had Dependents	15.4	15.4	15.1	14.9
Family Status				
Family head	13.5	13.1	14.3***	13.8
Family member	60.8	61.3	59.8	60.5
Unrelated person	25.7	25.6	25.9	25.8
Average Family Size	3.1**	3.2	3.1***	3.2
<b>Education</b>				
Completed the 12th Grade	21.9	21.9	21.6	21.5
<b>Welfare Dependence</b>				
Public Assistance Receipt				
Received AFDC	26.0**	26.5	26.1***	26.7
Received other assistance	17.0	16.4	17.4	16.6
Did not receive	57.1	57.2	56.6	56.7
<b>Health</b>				
Had Any Health Conditions That Were Being Treated	3.3	3.4	3.2	3.3
<b>Crime</b>				
Arrested in Past Three Years	11.9	12.1	12.2	12.0
Ever Convicted or Adjudged Delinquent	5.6***	6.1	5.7**	6.0

TABLE A.4 (continued)

Characteristic <sup>a</sup>	Control Group		Program Group	
	Signers of the Consent Form <sup>b</sup>	Full Sample	Signers of the Consent Form <sup>b</sup>	Full Sample
<b>Completion Status to Previous Interviews</b>				
Baseline Interview Completion Status*				
Completed within 45 days	88.6*	88.1	89.3	89.3
Completed between 46 and 270 days	3.9	4.1	4.2	4.3
Did not complete	7.5	7.8	6.5	6.4
Completed the 12-Month Interview***	89.6	89.4	91.9	91.9
Completed the 30-Month Interview*	80.1**	79.4	81.3	81.2
Completed the 48-Month Interview***	80.3*	79.8	82.2	81.9
<b>Anticipated Program Enrollment Information</b>				
Designated for a Nonresidential Slot	13.2	13.9	13.6	13.7
Designated for a CCC Center <sup>c</sup>	15.2	14.7	15.6***	15.1
Designated for a High- or Medium-High-Performing Center <sup>c</sup>	57.4	55.5	57.8***	56.2
Designated for a Large or Medium-Large Center <sup>c</sup>	66.2	65.2	66.0***	65.3
<b>Sample Size</b>	<b>4,641</b>	<b>5,940</b>	<b>7,436</b>	<b>9,369</b>

Source: Records release consent forms and ETA-652 and ETA-652 Supplement data.

Note: 1. All figures are calculated using sample weights to account for the sample design.  
 2. Figures exclude 77 cases (37 control group and 40 program group members) who were determined to have enrolled in Job Corps prior to random assignment and were thus ineligible for the study.

<sup>a</sup>Significance levels pertain to tests of differences between signers in the program and control groups.

<sup>b</sup>Significance levels pertain to tests of differences between signers and non-signers in the respective research group.

<sup>c</sup>Figures are obtained using data on OA counselor projections about the centers that youths were likely to attend.

\*Difference between signers and the full sample is significant at the .10 level, two-tailed test.

\*\*Difference between signers and the full sample is significant at the .05 level, two-tailed test.

\*\*\*Difference between signers and the full sample is significant at the .01 level, two-tailed test.

based impact estimates can be generalized to the full study population rather than to signees only.<sup>1</sup>

Importantly, because signatures on the consent form were requested *prior* to random assignment, there were few differences in the baseline characteristics of program and control group *signees* (Table A.4). Few of the individual t-tests and chi-squared tests comparing the characteristics of signees in the two research groups are statistically significant. Furthermore, the joint test of the hypothesis that the distributions of baseline characteristics between the two groups of signees are similar is not statistically significant. Thus, although we find some differences in the characteristics of signees and nonsignees, the characteristics of signees in the two research groups appear to be similar. Thus, the UI-based impact estimates are likely to be unbiased.

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<sup>1</sup> Specifically, we (1) estimated a logit model where the probability a youth signed the consent form was regressed on baseline variables; (2) calculated the predicted probability (propensity score) for each signee from this model; (3) sorted these predicted probabilities from largest to smallest; (4) formed five propensity scoring groups of equal size; and (5) calculated the average propensity score within each of these propensity scoring groups, which was used to adjust the sampling weights.

**APPENDIX B**

**SUPPLEMENTARY TABLES TO CHAPTER III**



TABLE B.1

## SUBGROUP SAMPLE SIZES, BY RESEARCH STATUS AND DATA SOURCE

Subgroup	Percentage of the Study Population	Data Source			
		Survey Data		Annual Social Security Earnings Records	
		Program Group	Control Group	Program Group	Control Group
<b>Subgroups Defined by Youth Characteristics</b>					
Age at Application					
16 to 17	41.2	2,742	1,907	3,709	2,439
18 to 19	32.0	2,175	1,402	2,948	1,857
20 to 24	26.8	1,911	1,176	2,607	1,578
Gender					
Male	59.4	3,741	2,787	5,314	3,854
Female	40.6	3,087	1,698	3,950	2,020
Race					
White, non-Hispanic	27.0	1,793	1,193	2,474	1,558
Black, non-Hispanic	47.4	3,366	2,179	4,462	2,814
Hispanic	17.7	1,175	787	1,623	1,051
Arrest History					
Never arrested	76.6	5,020	3,225	6,437	3,960
Ever arrested for nonserious crimes <sup>a</sup>	18.7	1,158	795	1,529	984
Ever arrested for serious crimes <sup>a</sup>	4.7	294	203	392	262
Educational Level					
Had a high school diploma or GED	23.1	1,626	1,028	2,036	1,267
Had neither	77.0	5,161	3,436	6,777	4,272
<b>Residents and Nonresidents</b>					
Residential Designation Status					
Residents					
All	86.0	5,484	3,753	7,499	4,982
Males	55.3	3,373	2,581	4,798	3,566
Females without children	25.3	1,710	957	2,105	1,076
Females with children	5.4	387	206	467	237
Nonresidents					
All	14.0	1,344	732	1,765	892
Males	4.2	368	206	516	288
Females without children	3.6	350	189	439	228
Females with children	6.2	618	332	753	364

TABLE B.1 (continued)

Subgroup	Percentage of the Study Population	Data Source			
		Survey Data		Annual Social Security Earnings Records	
		Program Group	Control Group	Program Group	Control Group
<b>Subgroups Defined by Center Characteristics</b>					
Center Type					
Contract centers	85.1	5,448	3,541	7,411	4,595
CCC centers	14.9	914	617	1,240	816
Center Size					
Small centers (<226 slots)	19.9	1,272	837	1,716	1,087
Medium centers (226 to 495 slots)	45.4	2,910	1,891	3,952	2,462
Large centers (>495 slots)	34.8	2,180	1,430	2,983	1,862
Region					
1	4.7	297	190	398	251
2	5.1	299	184	421	260
3	13.4	889	564	1,187	743
4	23.2	1,497	993	1,989	1,242
5	10.0	654	403	870	541
6	15.7	989	642	1,378	845
7/8	13.6	888	587	1,210	723
9	9.0	550	367	774	514
10	5.3	299	228	424	292
Performance Level					
High	14.6	918	623	1,269	804
Medium	65.9	4,241	2,728	5,751	3,549
Low	19.5	1,203	807	1,631	1,058
<b>Sample Size</b>	<b>80,883</b>	<b>6,828</b>	<b>4,485</b>	<b>9,264</b>	<b>5,874</b>

Source: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) Job Corps intake (ETA-652) forms and Supplemental ETA-652 forms; and (3) annual social security earnings records.

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

<sup>a</sup>Serious crimes include murder, assault, robbery, and burglary. Nonserious crimes include larceny, vehicle theft, other property crimes, drug law violations, other personal crimes, and other miscellaneous crimes.



TABLE B.2

IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR RESPONDENTS  
AND NONRESPONDENTS TO THE 48-MONTH INTERVIEW USING THE SER DATA

Outcome Measure	Annual Social Security Earnings Records											
	48-Month Interview Respondents				48-Month Interview Nonrespondents <sup>a</sup>				Respondents and Nonrespondents			
	Program Group	Control Group	Estimated Impact <sup>b</sup>		Program Group	Control Group	Estimated Impact <sup>b</sup>		Program Group	Control Group	Estimated Impact <sup>b</sup>	
Average Calendar Year Earnings (in 1995 Dollars)												
1993	1,001.9	1,028.8	-27.0	1,040.4	954.3	86.1	1,009.6	1,013.9	-4.3			
1994	1,587.0	1,550.6	36.4	1,603.5	1,509.6	93.9	1,590.3	1,542.4	47.9			
1995	1,772.1	2,033.6	-261.5***	1,703.6	1,998.1	-294.5***	1,758.4	2,026.5	-268.1***			
1996	3,175.1	3,330.9	-155.9**	2,783.1	3,043.9	-260.8*	3,096.7	3,273.5	-176.8***			
1997	4,696.4	4,443.2	253.2**	3,914.9	4,069.2	-154.3	4,540.1	4,368.4	171.8**			
1998	6,045.8	5,655.3	390.5***	4,836.3	5,299.3	-463.0*	5,803.9	5,584.1	219.8**			
1999	6,925.7	6,754.0	171.7	5,561.7	6,083.5	-521.8*	6,652.9	6,619.9	32.9			
2000	7,835.0	7,644.8	190.2	6,293.5	7,141.3	-847.8***	7,526.7	7,544.1	-17.4			
2001	8,017.6	7,863.7	154.0	6,323.1	6,903.7	-580.6*	7,678.7	7,671.7	7.0			
Percentage Employed in Calendar Year												
1993	42.6	42.6	0.0	44.1	44.6	-0.5	42.9	43.0	-0.1			
1994	60.0	58.7	1.3	57.5	59.2	-1.7	59.5	58.8	0.7			
1995	90.0	73.0	17.0***	86.0	74.5	11.5***	89.2	73.3	15.9***			
1996	89.6	78.7	10.9***	85.1	76.7	8.4***	88.7	78.3	10.4***			
1997	85.1	82.7	2.4***	77.1	76.2	0.9	83.5	81.4	2.1***			
1998	85.9	84.4	1.5**	78.9	78.4	0.5	84.5	83.2	1.3**			
1999	86.0	84.6	1.4**	78.0	76.6	1.4	84.4	83.0	1.4**			
2000	85.2	85.0	0.2	76.7	74.0	2.7	83.5	82.8	0.6			
2001	81.4	81.7	-0.3	74.4	71.7	2.7	80.0	79.7	0.3			
<b>Sample Size</b>	<b>6,772</b>	<b>4,451</b>	<b>11,223</b>	<b>2,492</b>	<b>1,423</b>	<b>3,915</b>	<b>9,264</b>	<b>5,874</b>	<b>15,138</b>			

TABLE B.2 (continued)

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

<sup>a</sup>The estimated impacts for nonrespondents were obtained using the estimated impacts for respondents and the full sample, and using the relation that impacts for the full sample are a weighted average of the impacts for respondents and nonrespondents, where the weights are the survey response rate (80 percent) and the survey nonresponse rate (20 percent), respectively.

<sup>b</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

TABLE B.3

IMPACTS ON EARNINGS AND EMPLOYMENT FOR 16- AND 17-YEAR-OLDS,  
USING SURVEY AND SER DATA

Outcome Measure	Survey Data			Annual Social Security Earnings Records		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
Average Earnings (in 1995 Dollars)						
<b>Calendar Year</b>						
1993				119.3	127.1	-7.8
1994				417.2	402.0	15.2
1995				804.7	885.1	-80.4**
1996	3,999.0	4,171.1	-172.2	2,018.0	1,851.9	166.1**
1997	6,899.5	6,286.1	613.5***	3,202.0	2,892.8	309.2***
1998	8,945.8	7,931.2	1,014.7***	4,233.7	4,079.4	154.2
1999				5,109.4	5,112.4	-3.0
2000				5,886.0	5,933.0	-47.0
2001				6,017.4	6,053.6	-36.2
<b>Year After Random Assignment</b>						
1	2,609.9	3,156.3	-546.5***			
2	5,574.3	5,280.8	293.5			
3	7,984.7	7,093.3	891.4***			
4	9,781.2	9,087.1	694.0**			
Percentage Employed						
<b>Calendar Year</b>						
1993				17.0	17.3	-0.3
1994				38.5	37.9	0.6
1995				85.3	59.3	26.0***
1996	65.2	69.8	-4.6***	86.5	70.6	15.9***
1997	74.9	72.5	2.4*	81.0	77.8	3.1***
1998	78.5	77.2	1.3	82.6	81.7	0.9
1999				82.6	80.8	1.8*
2000				81.6	80.5	1.1
2001				76.9	77.0	-0.1
<b>Year After Random Assignment</b>						
1	56.2	64.9	-8.7***			
2	68.5	68.7	-0.2			
3	78.5	78.2	0.3			
4	80.5	77.8	2.7**			
<b>Sample Size</b>	<b>2,742</b>	<b>1,907</b>	<b>4,649</b>	<b>3,709</b>	<b>2,439</b>	<b>6,148</b>

TABLE B.3 (continued)

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

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

TABLE B.4

IMPACTS ON EARNINGS AND EMPLOYMENT FOR 18- AND 19-YEAR-OLDS,  
USING SURVEY AND SER DATA

Outcome Measure	Survey Data			Annual Social Security Earnings Records		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
<b>Average Earnings (in 1995 Dollars)</b>						
Calendar Year						
1993				747.3	788.6	-41.3
1994				1,598.1	1,562.3	35.8
1995				1,893.4	2,341.4	-448.0***
1996	5,317.5	6,261.4	-943.9***	3,237.2	3,706.1	-468.9***
1997	8,241.9	8,416.6	-174.8	4,714.5	4,876.2	-161.7
1998	10,242.4	10,037.0	205.4	6,159.6	6,099.4	60.2
1999				6,864.7	7,149.3	-284.6
2000				7,771.6	8,113.3	-341.7
2001				8,034.5	8,329.9	-295.4
Year After Random Assignment						
1	3,812.3	5,199.6	-1,387.3***			
2	7,118.4	7,636.1	-517.7*			
3	9,337.9	9,212.8	125.1			
4	10,940.3	10,737.6	202.6			
<b>Percentage Employed</b>						
Calendar Year						
1993				51.6	51.7	-0.1
1994				70.0	69.4	0.6
1995				91.2	81.8	9.4***
1996	73.1	77.7	-4.6***	89.3	83.2	6.1***
1997	79.0	80.9	-1.8	84.7	82.9	1.9*
1998	81.3	80.1	1.2	84.5	83.1	1.4
1999				84.7	84.3	0.4
2000				83.6	84.5	-0.9
2001				81.3	81.6	-0.3
Year After Random Assignment						
1	67.3	74.2	-6.8***			
2	75.4	77.7	-2.2			
3	82.2	81.0	1.1			
4	82.3	79.6	2.7*			
<b>Sample Size</b>	<b>2,175</b>	<b>1,402</b>	<b>3,577</b>	<b>2,948</b>	<b>1,857</b>	<b>4,805</b>

TABLE B.4 (continued)

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

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

TABLE B.5

IMPACTS ON EARNINGS AND EMPLOYMENT FOR 20- AND 24-YEAR-OLDS,  
USING SURVEY AND SER DATA

Outcome Measure	Survey Data			Annual Social Security Earnings Records		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
<b>Average Earnings (in 1995 Dollars)</b>						
Calendar Year						
1993				2,658.6	2,649.4	9.2
1994				3,351.3	3,274.3	76.9
1995				3,040.0	3,406.1	-366.1***
1996	6,666.2	7,474.7	-808.6***	4,560.7	4,943.8	-383.1**
1997	9,799.6	9,478.1	321.5	6,356.0	6,031.4	324.6*
1998	12,393.2	10,615.8	1,777.5***	7,758.8	7,282.9	475.9**
1999				8,734.9	8,306.3	428.6*
2000				9,716.9	9,342.1	374.8
2001				9,770.8	9,373.8	397.0
Year After Random Assignment						
1	4,538.0	6,379.7	-1,841.7***			
2	8,550.1	8,631.6	-81.5			
3	11,204.0	10,157.7	1,046.4***			
4	12,878.7	11,135.4	1,743.3***			
<b>Percentage Employed</b>						
Calendar Year						
1993				71.7	71.9	-0.2
1994				78.8	78.2	0.6
1995				92.7	84.5	8.2***
1996	74.8	77.9	-3.1*	91.4	84.4	7.0***
1997	80.5	79.2	1.3	86.1	85.1	0.9
1998	85.7	80.2	5.5***	87.5	85.6	1.9*
1999				86.7	84.7	2.0*
2000				86.1	84.4	1.7
2001				83.0	81.6	1.5
Year After Random Assignment						
1	69.2	74.7	-5.6***			
2	77.1	77.1	0.0			
3	84.6	81.5	3.0**			
4	85.1	79.9	5.1***			
<b>Sample Size</b>	<b>1,911</b>	<b>1,176</b>	<b>3,087</b>	<b>2,607</b>	<b>1,578</b>	<b>4,185</b>

TABLE B.5 (continued)

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

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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



TABLE B.6

IMPACTS ON EARNINGS AND EMPLOYMENT FOR MALES,  
USING SURVEY AND SER DATA

Outcome Measure	Survey Data			Annual Social Security Earnings Records		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
<b>Average Earnings (in 1995 Dollars)</b>						
Calendar Year						
1993				1,067.3	1,049.5	17.8
1994				1,687.8	1,598.1	89.8
1995				1,823.7	2,130.8	-307.1***
1996	5,794.2	6,480.6	-686.5***	3,273.0	3,499.8	-226.8***
1997	9,252.0	8,955.3	296.7	4,923.5	4,716.7	206.8*
1998	11,720.6	10,580.9	1,139.7***	6,319.1	5,932.2	386.8***
1999				7,131.6	7,004.6	127.0
2000				8,068.2	8,125.9	-57.7
2001				8,160.9	8,103.7	57.2
Year After Random Assignment						
1	3,904.2	5,321.7	-1,417.5***			
2	7,834.2	7,830.9	3.4			
3	10,628.0	9,824.9	803.0***			
4	12,467.6	11,546.9	920.7***			
<b>Percentage Employed</b>						
Calendar Year						
1993				43.4	43.9	-0.5
1994				60.1	59.0	1.0
1995				90.4	74.2	16.2***
1996	72.2	76.7	-4.5***	89.6	79.3	10.3***
1997	79.5	79.4	0.1	84.7	82.6	2.1***
1998	82.7	81.0	1.8*	84.8	83.4	1.4*
1999				84.0	83.1	0.9
2000				83.0	82.1	0.8
2001				78.9	79.4	-0.5
Year After Random Assignment						
1	64.6	72.1	-7.6***			
2	74.9	75.8	-0.9			
3	82.9	82.8	0.1			
4	83.5	80.7	2.8***			
<b>Sample Size</b>	<b>3,741</b>	<b>2,787</b>	<b>6,528</b>	<b>5,314</b>	<b>3,854</b>	<b>9,168</b>

TABLE B.6 (continued)

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

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

TABLE B.7

IMPACTS ON EARNINGS AND EMPLOYMENT FOR FEMALES,  
USING SURVEY AND SER DATA

Outcome Measure	Survey Data			Annual Social Security Earnings Records		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
<b>Average Earnings (in 1995 Dollars)</b>						
Calendar Year						
1993				926.0	961.1	-35.1
1994				1,449.0	1,459.9	-10.9
1995				1,663.8	1,871.8	-208.0***
1996	4,221.9	4,639.9	-418.0**	2,841.3	2,938.0	-96.8
1997	6,472.2	6,152.4	319.9	3,984.4	3,851.8	132.6
1998	8,250.8	7,475.8	775.0***	5,057.2	5,067.8	-10.6
1999				5,958.9	6,049.3	-90.5
2000				6,741.7	6,681.2	60.6
2001				6,979.7	7,030.9	-51.2
Year After Random Assignment						
1	2,960.4	3,713.7	-753.4***			
2	5,500.7	5,618.5	-117.8			
3	7,355.5	6,769.8	585.7***			
4	8,869.4	8,136.0	733.5***			
<b>Percentage Employed</b>						
Calendar Year						
1993				42.1	41.5	0.6
1994				58.5	58.4	0.2
1995				87.4	71.9	15.5***
1996	67.7	71.4	-3.7***	87.4	76.8	10.6***
1997	75.2	73.4	1.8	81.9	79.6	2.3**
1998	79.4	75.9	3.5***	84.2	83.0	1.2
1999				85.0	82.8	2.2**
2000				84.2	83.9	0.4
2001				81.6	80.1	1.4
Year After Random Assignment						
1	61.4	68.1	-6.7***			
2	70.4	71.0	-0.6			
3	79.0	75.9	3.1**			
4	80.6	76.4	4.1***			
<b>Sample Size</b>	<b>3,087</b>	<b>1,698</b>	<b>4,785</b>	<b>3,950</b>	<b>2,020</b>	<b>5,970</b>

TABLE B.7 (continued)

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

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

TABLE B.8

IMPACTS ON EARNINGS AND EMPLOYMENT FOR WHITE, NON-HISPANIC YOUTH,  
USING SURVEY AND SER DATA

Outcome Measure	Survey Data			Annual Social Security Earnings Records		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
<b>Average Earnings (in 1995 Dollars)</b>						
Calendar Year						
1993				1,278.9	1,226.2	52.7
1994				1,939.8	1,877.4	62.4
1995				2,083.7	2,511.2	-427.5***
1996	6,829.0	7,284.0	-455.0*	3,777.8	3,874.2	-96.4
1997	10,414.2	9,840.5	573.6*	5,486.2	5,321.3	164.9
1998	12,920.5	11,206.6	1,713.9***	6,923.1	6,477.6	445.5**
1999				7,666.9	7,653.8	13.1
2000				8,695.8	8,571.9	123.8
2001				8,812.6	8,578.7	233.9
Year After Random Assignment						
1	4,874.1	6,003.7	-1,129.6***			
2	9,001.0	8,763.1	237.9			
3	11,691.5	10,510.8	1,180.7***			
4	13,705.6	12,029.4	1,676.2***			
<b>Percentage Employed</b>						
Calendar Year						
1993				50.6	49.8	0.8
1994				69.6	68.1	1.5
1995				93.3	82.4	10.9***
1996	81.2	83.0	-1.8	92.8	85.0	7.8***
1997	84.9	83.5	1.4	89.9	88.8	1.1
1998	86.9	84.8	2.1	89.7	88.4	1.3
1999				88.8	88.7	0.1
2000				88.6	86.9	1.6
2001				85.3	83.4	1.8
Year After Random Assignment						
1	74.9	81.4	-6.5***			
2	82.0	81.9	0.1			
3	86.5	86.4	0.1			
4	88.8	84.9	3.9***			
<b>Sample Size</b>	<b>1,793</b>	<b>1,193</b>	<b>2,986</b>	<b>2,474</b>	<b>1,558</b>	<b>4,032</b>

TABLE B.8 (continued)

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

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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

TABLE B.9

IMPACTS ON EARNINGS AND EMPLOYMENT FOR BLACK, NON-HISPANIC YOUTH,  
USING SURVEY AND SER DATA

Outcome Measure	Survey Data			Annual Social Security Earnings Records		
	Program Group	Control Group	Estimated Impact <sup>a</sup>	Program Group	Control Group	Estimated Impact <sup>a</sup>
<b>Average Earnings (in 1995 Dollars)</b>						
Calendar Year						
1993				850.7	864.1	-13.3
1994				1,356.7	1,319.8	36.9
1995				1,540.5	1,745.6	-205.1***
1996	4,456.2	4,676.1	-219.9	2,601.1	2,700.9	-99.8
1997	6,907.4	6,551.4	356.0*	3,774.4	3,554.3	220.1**
1998	8,770.4	7,912.4	858.1***	4,847.8	4,588.6	259.2**
1999				5,623.2	5,522.6	100.6
2000				6,298.3	6,252.3	46.0
2001				6,355.4	6,170.7	184.7
Year After Random Assignment						
1	3,052.2	3,866.1	-813.9***			
2	5,849.9	5,778.0	71.9			
3	7,828.2	7,222.1	606.1***			
4	9,520.2	8,656.0	864.2***			
<b>Percentage Employed</b>						
Calendar Year						
1993				40.2	39.8	0.4
1994				56.0	56.0	0.0
1995				87.8	69.8	18.0***
1996	66.5	69.6	-3.0**	86.4	75.2	11.2***
1997	74.4	72.6	1.8	80.6	78.1	2.5***
1998	78.7	74.8	3.9***	81.9	80.8	1.1
1999				82.5	80.4	2.2**
2000				80.7	80.9	-0.3
2001				76.5	77.7	-1.2
Year After Random Assignment						
1	60.0	65.1	-5.1***			
2	69.4	69.4	0.0			
3	78.4	75.7	2.7**			
4	79.2	74.7	4.5***			
<b>Sample Size</b>	<b>3,366</b>	<b>2,179</b>	<b>5,545</b>	<b>4,462</b>	<b>2,814</b>	<b>7,276</b>

TABLE B.9 (continued)

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

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
  2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), and (2) the survey design and interview nonresponse (for the survey data).

<sup>a</sup>These estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

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

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

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



**APPENDIX C**

**SUPPLEMENTARY TABLES TO CHAPTER IV**



TABLE C.1

AGREEMENT RATES BETWEEN THE SURVEY AND UI DATA, BY OCCUPATION AND TYPE OF EMPLOYER IN  
THE MOST RECENT JOB HELD IN QUARTER 16 AND BY GENDER, AGE, AND RESEARCH STATUS

Job Characteristic	Program Group Members Employed According to the Survey Data		Control Group Members Employed According to the Survey Data	
	Percentage with Job Characteristic	Percentage Also Employed According to the UI Data	Percentage with Job Characteristic	Percentage Also Employed According to the UI Data
<b>Males</b>				
Occupation				
Services	19.6	71.4	20.8	74.4
Sales	4.7	64.0	5.9	70.1
Construction	30.3	63.7	29.9	66.2
Private household	3.3	54.2	4.9	51.7
Clerical	6.1	68.7	6.5	69.3
Mechanics/repairers/ machinists	18.7	68.8	17.3	57.8
Agriculture	3.3	64.6	3.4	55.7
Other	14.1	63.2	11.2	63.9
Type of Employer				
Private company	80.5	70.3	81.8	68.4
Military	2.3	12.9	0.9	0.0
Federal government	2.2	56.4	1.4	61.7
State government	3.8	65.8	4.8	61.1
Local government	3.7	79.7	3.4	61.6
Self-employed	5.3	46.8	5.4	42.9
Working without pay in a family business or as a favor	1.2	22.4	1.1	43.5
Other	1.0	21.0	1.2	70.6
<b>Females</b>				
Occupation				
Services	25.3	73.6	21.2	75.8
Sales	17.6	78.4	22.3	67.1
Construction	6.6	78.3	4.8	72.1
Private household	10.9	62.0	11.8	53.5
Clerical	20.8	75.7	21.9	75.4
Mechanics/repairers/ machinists	8.3	68.5	8.1	83.2
Agriculture	0.7	27.1	0.8	100.0
Other	9.9	78.7	9.1	76.3
Type of Employer				
Private company	80.6	78.3	79.2	71.4
Military	0.1	NA	0.0	NA
Federal government	2.3	58.9	3.2	76.7
State government	5.6	67.3	5.2	84.5
Local government	3.7	69.1	4.7	91.3
Self-employed	5.2	35.6	5.3	18.7
Working without pay in a family business or as a favor	0.8	46.5	1.3	76.2
Other	1.8	47.9	1.3	100.0

TABLE C.1 (continued)

Job Characteristic	Program Group Members Employed According to the Survey Data		Control Group Members Employed According to the Survey Data	
	Percentage with Job Characteristic	Percentage Also Employed According to the UI Data	Percentage with Job Characteristic	Percentage Also Employed According to the UI Data
<b>Age 16 to 17 at Program Application</b>				
Occupation				
Services	24.5	66.4	20.8	70.4
Sales	11.2	76.5	14.7	71.8
Construction	24.7	64.2	18.5	59.4
Private household	6.0	57.6	8.5	60.0
Clerical	9.1	77.8	11.3	68.0
Mechanics/repairers/ machinists	13.6	73.3	14.8	59.5
Agriculture	2.2	55.0	3.2	68.3
Other	8.9	57.6	8.2	74.2
Type of Employer				
Private company	79.6	72.2	77.4	67.2
Military	1.6	0.0	0.6	0.0
Federal government	2.8	49.3	2.3	79.3
State government	3.5	58.9	6.6	63.5
Local government	3.3	88.9	5.7	67.6
Self-employed	6.5	47.3	5.8	35.2
Working without pay in a family business or as a favor	1.6	31.8	0.6	0.0
Other	1.2	20.6	1.1	80.8
<b>Age 18 to 19 at Program Application</b>				
Occupation				
Services	21.1	78.7	21.9	82.6
Sales	10.2	76.0	14.4	65.8
Construction	17.6	64.6	18.2	67.7
Private household	7.3	48.5	8.8	54.1
Clerical	14.8	76.6	14.5	80.2
Mechanics/repairers/ machinists	14.3	59.2	9.5	68.0
Agriculture	2.2	70.2	1.8	65.9
Other	12.5	80.9	10.9	61.3
Type of Employer				
Private company	81.4	74.8	79.2	72.9
Military	1.2	33.4	0.9	0.0
Federal government	1.5	68.0	2.5	65.1
State government	6.7	68.1	4.1	76.8
Local government	3.3	64.1	3.0	77.9
Self-employed	3.8	32.6	6.2	32.4
Working without pay in a family business or as a favor	0.7	44.6	2.1	66.7
Other	1.5	53.8	2.0	100.0

TABLE C.1 (continued)

Job Characteristic	Program Group Members Employed According to the Survey Data		Control Group Members Employed According to the Survey Data	
	Percentage with Job Characteristic	Percentage Also Employed According to the UI Data	Percentage with Job Characteristic	Percentage Also Employed According to the UI Data
<b>Age 20 to 24 at Program Application</b>				
Occupation				
Services	20.0	74.9	20.1	72.0
Sales	8.8	70.3	8.4	63.6
Construction	17.7	70.5	22.0	75.1
Private household	6.2	75.1	5.5	35.6
Clerical	13.7	66.6	13.6	71.8
Mechanics/repairers/ machinists	15.1	73.7	16.1	66.4
Agriculture	2.2	58.1	1.6	47.9
Other	16.3	65.7	12.6	70.5
Type of Employer				
Private company	80.8	74.4	86.9	69.3
Military	1.4	16.2	0.0	NA
Federal government	2.4	59.2	1.5	64.8
State government	3.6	72.6	3.6	79.4
Local government	4.6	71.6	2.8	90.5
Self-employed	5.2	39.9	3.7	29.1
Working without pay in a family business or as a favor	0.6	0.0	1.0	63.2
Other	1.4	33.5	0.6	40.6
<b>Sample Sizes</b>				
<b>Males</b>	<b>999</b>	<b>999</b>	<b>733</b>	<b>733</b>
<b>Females</b>	<b>871</b>	<b>871</b>	<b>465</b>	<b>465</b>
<b>Age 16 to 17</b>	<b>683</b>	<b>683</b>	<b>485</b>	<b>485</b>
<b>Age 18 to 19</b>	<b>600</b>	<b>600</b>	<b>385</b>	<b>385</b>
<b>Age 20 to 24</b>	<b>587</b>	<b>587</b>	<b>328</b>	<b>328</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

TABLE C.2

JOB CHARACTERISTICS IN QUARTER 16 FOR THOSE EMPLOYED ACCORDING TO BOTH THE SURVEY AND UI DATA AND ACCORDING TO THE SURVEY DATA ONLY, BY GENDER, AGE, AND RESEARCH STATUS

Job Characteristic	Program Group		Control Group	
	Employed in Both Survey and UI Data <sup>a</sup>	Employed in Survey Data Only	Employed in Both Survey and UI Data <sup>a</sup>	Employed in Survey Data Only
<b>Males</b>				
Average Number of Months on Job	13.0	13.4	12.5	13.3
Average Hours Worked per Week	44.2	45.6	44.9	45.6
Average Hourly Wage (in 1995 dollars)	7.80	7.60	7.70	7.70
Average Weekly Earnings (in 1995 dollars)	349.30	344.50	340.40	359.90
Benefits Available on Job (Percentages)				
Health insurance	65.8	50.3	63.0	51.8
Paid vacation	67.4	57.0	67.2	63.9
Retirement or pension benefits	54.4	43.3	47.6	43.1
<b>Females</b>				
Average Number of Months on Job	11.8	11.3	11.9	12.0
Average Hours Worked per Week	40.3	38.5	39.5	38.5
Average Hourly Wage (in 1995 dollars)	7.20	6.50	6.80	6.40
Average Weekly Earnings (in 1995 dollars)	291.40	252.20	272.40	240.60
Benefits Available on Job (Percentages)				
Health insurance	61.20	42.2	57.5	35.8
Paid vacation	67.70	55.2	59.5	55.5
Retirement or pension benefits	52.70	34.8	42.0	27.7
<b>Age 16 to 17 at Program Application</b>				
Average Number of Months on Job	9.7	12.1	10.8	9.3

TABLE C.2 (continued)

Job Characteristic	Program Group		Control Group	
	Employed in Both Survey and UI Data <sup>a</sup>	Employed in Survey Data Only	Employed in Both Survey and UI Data <sup>a</sup>	Employed in Survey Data Only
Average Hours Worked per Week	43.7	41.5	42.0	43.7
Average Hourly Wage (in 1995 dollars)	7.10	6.70	7.20	6.80
Average Weekly Earnings (in 1995 dollars)	316.40	273.70	297.6	304.80
Benefits Available on Job (Percentages)				
Health insurance	56.7	44.0	59.9	44.0
Paid vacation	60.2	57.0	60.5	58.4
Retirement or pension benefits	44.3	34.0	41.5	36.6
<b>Age 18 and 19 at Program Application</b>				
Average Number of Months on Job	12.2	13.1	12.2	13.3
Average Hours Worked per Week	42.3	42.1	42.7	44.1
Average Hourly Wage (in 1995 dollars)	7.40	7.50	7.10	7.60
Average Weekly Earnings (in 1995 dollars)	318.80	322.80	306.70	347.10
Benefits Available on Job (Percentages)				
Health insurance	63.2	49.9	58.1	49.7
Paid vacation	68.9	56.7	63.2	69.6
Retirement or pension benefits	54.5	44.3	47.5	45.2
<b>Age 20 to 24 at Program Application</b>				
Average Number of Months on Job	16.1	13.0	14.1	17.6
Average Hours Worked per Week	41.0	45.8	42.4	40.4
Average Hourly Wage (in 1995 dollars)	8.10	7.40	7.50	7.50
Average Weekly Earnings (in 1995 dollars)	336.00	347.40	326.00	295.40

TABLE C.2 (continued)

Job Characteristic	Program Group		Control Group	
	Employed in Both Survey and UI Data <sup>a</sup>	Employed in Survey Data Only	Employed in Both Survey and UI Data <sup>a</sup>	Employed in Survey Data Only
<b>Benefits Available on Job (Percentages)</b>				
Health insurance	72.5	49.4	63.9	45.0
Paid vacation	74.8	55.0	68.5	54.5
Retirement or pension benefits	63.3	44.6	46.6	30.9
<b>Sample Sizes</b>				
<b>Males</b>	<b>428</b>	<b>282</b>	<b>291</b>	<b>219</b>
<b>Females</b>	<b>432</b>	<b>193</b>	<b>233</b>	<b>113</b>
<b>Age 16 to 17</b>	<b>300</b>	<b>185</b>	<b>204</b>	<b>146</b>
<b>Age 18 to 19</b>	<b>293</b>	<b>147</b>	<b>176</b>	<b>100</b>
<b>Age 20 to 24</b>	<b>267</b>	<b>143</b>	<b>144</b>	<b>86</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

<sup>a</sup>Includes only those with the *same* number of reported jobs in both data sources.



TABLE C.3

RATIO OF MEAN EARNINGS PER JOB IN QUARTER 16 USING THE SURVEY AND UI DATA,  
BY JOB CHARACTERISTIC AND GENDER

Job Characteristic According to Survey Data	Ratio of Survey-to-UI Mean Earnings	
	Males	Females
Full Sample <sup>a</sup>	1.43	1.31
Number of Weeks Worked on Job in Quarter 16		
Less than 3	0.99	0.84
3 to 6	1.53	1.57
6 to 12	1.78	1.56
13 (All weeks)	1.42	1.30
Hours Worked per Week		
Less than 30	1.05	0.86
30 to 39	1.34	1.10
40	1.19	1.24
More than 40	1.72	1.65
Hourly Wage (in 1995 dollars)		
Less than \$6.00	1.36	1.33
\$6.00 to \$7.50	1.47	1.20
\$7.50 to \$9.00	1.37	1.32
\$9.00 or more	1.51	1.43
Number of Months on Job		
Less than 3	1.63	1.41
3 to 6	1.51	1.33
6 to 12	1.44	1.34
12 or more	1.14	1.14
Occupation		
Services	1.34	1.35
Sales	1.67	1.28
Construction	1.54	1.52
Private household	1.36	1.37
Clerical	1.25	1.22
Mechanics/repairers/machinists	1.40	1.29
Other	1.46	1.35
Benefits Available on Job		
Health insurance	1.37	1.29
Paid vacation	1.38	1.30
Retirement or pension benefits	1.36	1.31
<b>Sample Size</b>	<b>719</b>	<b>665</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

TABLE C.3 (continued)

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Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

<sup>a</sup>The sample includes those according to both the survey and UI data who were employed in quarter 16 after random assignment and who had the same number of reported jobs. For sample size reasons, the program and control groups were combined for the analysis.

TABLE C.4

RATIO OF MEAN EARNINGS PER JOB IN QUARTER 16 USING THE SURVEY AND UI DATA,  
BY JOB CHARACTERISTIC AND AGE AT PROGRAM APPLICATION

Job Characteristic According to Survey Data	Ratio of Survey-to-UI Mean Earnings		
	Age 16 to 17	Age 18 to 19	Age 20 to 24
Full Sample <sup>a</sup>	1.53	1.40	1.23
Number of Weeks Worked on Job in Quarter 16			
Less than 3	1.09	0.88	0.65
3 to 6	1.54	1.49	1.64
6 to 12	1.81	1.39	1.94
13 (All weeks)	1.53	1.42	1.20
Hours Worked per Week			
Less than 30	0.97	1.19	0.73
30 to 39	1.34	1.07	1.18
40	1.30	1.26	1.09
More than 40	1.91	1.66	1.52
Hourly Wage (in 1995 dollars)			
Less than \$6.00	1.49	1.41	1.07
\$6.00 to \$7.50	1.45	1.36	1.19
\$7.50 to \$9.00	1.57	1.27	1.25
\$9.00 or more	1.63	1.61	1.30
Number of Months on Job			
Less than 3	1.68	1.52	1.38
3 to 6	1.56	1.38	1.32
6 to 12	1.47	1.44	1.29
12 or more	1.31	1.21	1.03
Occupation			
Services	1.40	1.38	1.24
Sales	1.51	1.37	1.18
Construction	1.65	1.46	1.46
Private household	1.42	1.49	1.16
Clerical	1.48	1.24	1.06
Mechanics/repairers/machinists	1.51	1.51	1.19
Other	1.66	1.50	1.21
Benefits Available on Job			
Health insurance	1.50	1.32	1.22
Paid vacation	1.53	1.36	1.20
Retirement or pension benefits	1.56	1.31	1.22
<b>Sample Size</b>	<b>504</b>	<b>469</b>	<b>411</b>

Source: Baseline and 12-, 30-, and 48-month follow-up interview data and quarterly UI earnings records in 1999 and 2000 from 22 randomly selected states. The sample includes those who (1) completed the 48-month interview, (2) signed the records release consent form, (3) lived in the 22 states selected for UI data collection for the entire 48 months after random assignment, and (4) did not work outside the 22 states.

TABLE C.4 (continued)

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Note: All figures were calculated using sample weights to adjust for the sample and survey designs.

<sup>a</sup>The sample includes those according to both the survey and UI data who were employed in quarter 16 after random assignment and who had the same number of reported jobs. For sample size reasons, the program and control groups were combined for the analysis.

TABLE C.5

COMPARISON OF THE CHARACTERISTICS OF RESPONDENTS AND NONRESPONDENTS,  
TO THE 48-MONTH INTERVIEW, BY RESEARCH STATUS  
(Percentages)

Characteristic <sup>a</sup>	Control Group		Program Group	
	Respondents <sup>b</sup>	Nonrespondents	Respondents <sup>b</sup>	Nonrespondents
<b>Demographic Characteristics</b>				
Male	60.0	59.7	59.3	59.2
Age at Application				
16 to 17	39.8	40.1	39.2*	39.7
18 to 19	32.8	32.3	32.6	32.2
20 to 21	16.8	16.8	17.1	16.8
22 to 24	10.6	10.8	11.1	11.4
(Average age)	(18.9)	(18.9)	(19.0)	(19.0)
Race/Ethnicity				
White, non-Hispanic	27.3*	26.4	28.0***	27.1
Black, non-Hispanic	47.1	47.8	47.4	47.8
Hispanic	17.8	18.1	16.9	17.5
Other	7.8	7.7	7.8	7.6
Region				
1	3.0***	4.6	3.0***	4.4
2	5.8	7.7	5.7	7.2
3	9.2	13.0	9.3	13.0
4	27.0	22.7	27.2	23.4
5	11.1	10.5	11.1	10.3
6	16.6	14.7	16.8	15.2
7/8	11.9	12.1	12.3	12.7
9	9.8	9.6	9.5	9.0
10	5.5	5.1	5.3	4.8
Size of City of Residence				
Less than 2,500	9.1***	8.5	9.2***	8.7
2,500 to 10,000	11.9	11.3	11.8	11.2
10,000 to 50,000	20.1	19.2	20.8	19.7
50,000 to 250,000	17.1	17.6	17.8	17.6
250,000 or more	41.8	43.5	40.3	42.8
PMSA or MSA Residence Status				
In PMSA	27.8***	32.6	27.4***	31.7
In MSA	47.5	45.2	47.8	45.8
In neither	24.7	22.2	24.8	22.6
Density of Area of Residence				
Superdense	31.9***	35.1	31.0***	33.9
Dense	27.9	28.2	28.7	28.8
Nondense	40.2	36.7	40.4	37.3

TABLE C.5 (continued)

Characteristic <sup>a</sup>	Control Group		Program Group	
	Respondents <sup>b</sup>	Nonrespondents	Respondents <sup>b</sup>	Nonrespondents
Lived in 57 Areas with a Large Concentration of Nonresidential Females	29.6***	31.2	29.1***	30.8
Legal U.S. Resident*	98.9	98.8	98.6	98.6
Job Corps Application Date				
11/94 to 2/95	23.4***	22.2	23.3***	22.6
3/95 to 6/95	29.1	29.2	29.2	29.1
7/95 to 9/95	27.3	28.0	27.4	27.6
10/95 to 12/95	20.1	20.6	20.1	20.6
<b>Fertility and Family Status</b>				
Had Dependents	15.4	15.4	15.1	14.9
Family Status				
Family head	13.5	13.1	14.3***	13.8
Family member	60.8	61.3	59.8	60.5
Unrelated person	25.7	25.6	25.9	25.8
Average Family Size	3.1**	3.2	3.1***	3.2
<b>Education</b>				
Completed the 12th Grade	21.9	21.9	21.6	21.5
<b>Welfare Dependence</b>				
Public Assistance Receipt				
Received AFDC	26.0**	26.5	26.1***	26.7
Received other assistance	17.0	16.4	17.4	16.6
Did not receive	57.1	57.2	56.6	56.7
<b>Health</b>				
Had Any Health Conditions That Were Being Treated	3.3	3.4	3.2	3.3
<b>Crime</b>				
Arrested in Past Three Years	11.9	12.1	12.2	12.0
Ever Convicted or Adjudged Delinquent	5.6***	6.1	5.7**	6.0

TABLE C.5 (continued)

Characteristic <sup>a</sup>	Control Group		Program Group	
	Respondents <sup>b</sup>	Nonrespondents	Respondents <sup>b</sup>	Nonrespondents
<b>Completion Status to Previous Interviews</b>				
Baseline Interview Completion Status*				
Completed within 45 days	88.6*	88.1	89.3	89.3
Completed between 46 and 270 days	3.9	4.1	4.2	4.3
Did not complete	7.5	7.8	6.5	6.4
Completed the 12-Month Interview***	89.6	89.4	91.9	91.9
Completed the 30-Month Interview*	80.1**	79.4	81.3	81.2
Completed the 48-Month Interview***	80.3*	79.8	82.2	81.9
<b>Anticipated Program Enrollment Information</b>				
Designated for a Nonresidential Slot	13.2	13.9	13.6	13.7
Designated for a CCC Center <sup>c</sup>	15.2	14.7	15.6***	15.1
Designated for a High- or Medium-High-Performing Center <sup>c</sup>	57.4	55.5	57.8***	56.2
Designated for a Large or Medium-Large Center <sup>c</sup>	66.2	65.2	66.0***	65.3
<b>Sample Size</b>	<b>4,641</b>	<b>5,940</b>	<b>7,436</b>	<b>9,369</b>

Source: 48-month follow-up interview, ETA-652 and ETA-652 Supplement data.

Note: 1. All figures are calculated using sample weights to account for the sample design.  
2. Figures exclude 77 cases (37 control group and 40 program group members) who were determined to have enrolled in Job Corps prior to random assignment and were thus ineligible for the study.

<sup>a</sup>Significance levels pertain to tests of differences between respondents in the program and control groups.

<sup>b</sup>Significance levels pertain to tests of differences between signers and non-signers in the respective research group.

<sup>c</sup>Figures are obtained using data on OA counselor projections about the centers that youths were likely to attend.

\*Difference between respondents and nonrespondents is significant at the .10 level, two-tailed test.

\*\*Difference between respondents and nonrespondents is significant at the .05 level, two-tailed test.

\*\*\*Difference between respondents and nonrespondents is significant at the .01 level, two-tailed test.





**APPENDIX D**

**SUPPLEMENTARY TABLES TO CHAPTER V**



TABLE D.1

INITIAL ESTIMATES OF BENEFITS AND COSTS OF JOB CORPS, FULL SAMPLE,  
 BASED ON SURVEY DATA ONLY AND ASSUMING NO DECAY IN THE YEAR 4  
 EARNINGS IMPACTS AFTER THE OBSERVATION PERIOD  
 (1995 Dollars)

Benefits or Costs	Perspective		
	Society	Participants	Rest of Society
<b>Benefits from Increased Output</b>	<b>27,531</b>	<b>17,773</b>	<b>9,758</b>
Year 1			
Increased Earnings and Fringe Benefits	-1,883	-1,883	0
Increased Child Care Costs	-50	-47	-4
Increased Taxes	0	309	-309
Years 2 to 4			
Increased Earnings and Fringe Benefits	2,558	2,558	0
Increased Child Care Costs	-96	-77	-19
Increased Taxes	0	-855	855
After the Observation Period			
Increased Earnings and Fringe Benefits	27,281	27,281	0
Increased Child Care Costs	-503	-398	-106
Increased Taxes	0	-9,115	9,115
Output Produced During Vocational Training in Job Corps	225	0	225
<b>Benefits from Reduced Use of Other Programs and Services</b>	<b>2,186</b>	<b>-780</b>	<b>2,966</b>
Reduced Use of High School	1,189	0	1,189
Reduced Use of Other Education and Training Programs	874	0	874
Reduced Use of Public Assistance and Substance Abuse Treatment Programs	122	-780	902
<b>Benefits from Reduced Crime</b>	<b>1,240</b>	<b>643</b>	<b>597</b>
Reduced Crime by Participants	1,240	0	1,240
Reduced Crime Against Participants	0	643	-643
<b>Program Costs</b>	<b>-14,128</b>	<b>2,361</b>	<b>-16,489</b>
Reported Program Operating Costs (Net of Transfers)	-12,540	0	-12,540
Unreported Program Operating Costs (Net of Transfers)	-551	0	-551
Capital Costs	-1,037	0	-1,037
Student Pay, Food, and Clothing (Transfers)	0	2,361	-2,361
<b>Net Benefits<sup>a</sup></b>	<b>16,829</b>	<b>19,997</b>	<b>-3,168</b>

SOURCE: McConnell and Glazerman (2001), Table 1.

<sup>a</sup>Because of rounding, net benefits may not equal the sum of the rows. Similarly, benefits to society may not precisely equal the sum of the benefits to participants and the benefits to the rest of society.

TABLE D.2

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

Benefits or Costs	Perspective		
	Society	Participants	Rest of Society
<b>Benefits from Increased Output</b>	<b>269</b>	<b>-298</b>	<b>567</b>
Year 1			
Increased Earnings and Fringe Benefits	-1,715	-1,715	0
Increased Child Care Costs	-50	-47	-4
Increased Taxes	0	268	-268
Years 2 to 4			
Increased Earnings and Fringe Benefits	1,581	1,581	0
Increased Child Care Costs	-96	-77	-19
Increased Taxes	0	-514	514
After the Observation Period			
Increased Earnings and Fringe Benefits	361	361	0
Increased Child Care Costs	-32	-25	-7
Increased Taxes	0	-129	129
Output Produced During Vocational Training in Job Corps	220	0	220
<b>Benefits from Reduced Use of Other Programs and Services</b>	<b>2,186</b>	<b>-780</b>	<b>2,966</b>
Reduced Use of High School	1,189	0	1,189
Reduced Use of Other Education and Training Programs	874	0	874
Reduced Use of Public Assistance and Substance Abuse Treatment Programs	122	-780	902
<b>Benefits from Reduced Crime</b>	<b>1,240</b>	<b>643</b>	<b>597</b>
Reduced Crime by Participants	1,240	0	1,240
Reduced Crime Against Participants	0	643	-643
<b>Program Costs</b>	<b>-13,844</b>	<b>2,314</b>	<b>-16,158</b>
Reported Program Operating Costs (Net of Transfers)	-12,285	0	-12,285
Unreported Program Operating Costs (Net of Transfers)	-543	0	-543
Capital Costs	-1,016	0	-1,016
Student Pay, Food, and Clothing (Transfers)	0	2,314	-2,314
<b>Net Benefits<sup>b</sup></b>	<b>-10,150</b>	<b>1,879</b>	<b>-12,028</b>

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

<sup>a</sup>Assumes that impacts on earnings and child-care expenses decay at 68.3 percent per year after the observation period.

<sup>b</sup>Because of rounding, net benefits may not equal the sum of the rows. Similarly, benefits to society may not precisely equal the sum of the benefits to participants and the benefits to the rest of society.

TABLE D.3

REVISED BENEFITS AND COSTS OF JOB CORPS, YOUTH AGES 20-24 AT PROGRAM APPLICATION, BASED ON ADJUSTED SURVEY DATA AND AN ASSUMPTION OF A DECAY IN EARNINGS IMPACTS<sup>a</sup>  
(1995 Dollars)

Benefits or Costs	Perspective		
	Society	Participants	Rest of Society
<b>Benefits from Increased Output</b>	<b>17,547</b>	<b>15,591</b>	<b>1,956</b>
Year 1			
Increased Earnings and Fringe Benefits	-2,381	-2,381	0
Increased Child Care Costs	-83	-73	-9
Increased Taxes	0	513	-513
Years 2 to 4			
Increased Earnings and Fringe Benefits	3,006	3,006	0
Increased Child Care Costs	-204	-185	-19
Increased Taxes	0	-394	394
After the Observation Period			
Increased Earnings and Fringe Benefits	17,516	17,516	0
Increased Child Care Costs	-557	-463	-94
Increased Taxes	0	-1,948	1,948
Output Produced During Vocational Training in Job Corps	250	0	250
<b>Benefits from Reduced Use of Other Programs and Services</b>	<b>937</b>	<b>-1,358</b>	<b>2,295</b>
Reduced Use of High School	21	0	21
Reduced Use of Other Education and Training Programs	629	0	629
Reduced Use of Public Assistance and Substance Abuse Treatment Programs	287	-1,358	1,645
<b>Benefits from Reduced Crime</b>	<b>-3,787</b>	<b>643</b>	<b>-4,430</b>
Reduced Crime by Participants	-3,787	0	-3,787
Reduced Crime Against Participants	0	643	-643
<b>Program Costs</b>	<b>-15,193</b>	<b>2,562</b>	<b>-17,754</b>
Reported Program Operating Costs (Net of Transfers)	-13,487	0	-13,487
Unreported Program Operating Costs (Net of Transfers)	-554	0	-554
Capital Costs	-1,152	0	-1,152
Student Pay, Food, and Clothing (Transfers)	0	2,562	-2,562
<b>Net Benefits<sup>b</sup></b>	<b>-496</b>	<b>17,437</b>	<b>-17,934</b>

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

<sup>a</sup>Assumes that the impacts on earnings decay at 5.9 percent per year after the observation period.

<sup>b</sup>Because of rounding, net benefits may not equal the sum of the rows. Similarly, benefits to society may not precisely equal the sum of the benefits to participants and the benefits to the rest of society.