

Contract No.: K-4279-3-00-80-30  
MPR Reference No.: 8140-530

**MATHEMATICA**  
Policy Research, Inc.

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

*June 2001*

*Peter Z. Schochet  
John Burghardt  
Steven Glazerman*

Submitted to:

U.S. Department of Labor  
Employment and Training Administration  
Office of Policy and Research  
Room N-5637  
200 Constitution Ave., NW  
Washington, DC 20210

Submitted by:

Mathematica Policy Research, Inc.  
(Prime Contractor)  
P.O. Box 2393  
Princeton, NJ 08543-2393  
(609) 799-3535

Project Officer:

Daniel Ryan

Project Director:

John Burghardt

Principal Investigators:

Terry Johnson  
Charles Metcalf  
Peter Z. Schochet

In conjunction with:

Battelle Human Affairs Research  
Centers (Subcontractor)  
4500 Sand Point Way NE, Suite 100  
Seattle, WA 98105-3949

Decision Information Resources, Inc.  
(Subcontractor)  
2600 Southwest Freeway, Suite 900  
Houston, TX 77098

This report has been produced under Contract Number K-4279-3-00-80-30 with the U.S. Department of Labor, Employment and Training Administration. The contents of the report do not necessarily reflect the views or policies of the Department of Labor, nor does mention of trade names, commercial products, or organizations imply endorsement of these by the U.S. Government.

## ACKNOWLEDGMENTS

We would like to thank the many people whose efforts have made this report possible. These include those involved in the design and implementation of random assignment, the collection of survey data, and the preparation of the document itself.

The study design was developed by a team that included Charles Metcalf, Sheena McConnell, and John Homrighausen from Mathematica Policy Research, Inc. (MPR), Terry Johnson from Battelle Human Affairs Research Centers, Mark Gritz from the Sphere Institute, Russell Jackson from Decision Information Resources, Inc. (DIR), and the first two authors of this report. The operational design and study implementation benefited greatly from the contributions of many people at the U.S. Department of Labor (DOL): Daniel Ryan, project officer for the study; Karen Greene; David Lah; Peter Rell, Job Corps Director during the period of design and early implementation; Mary Silva, Job Corps Director during the period covering the previous impact report; Richard Trigg, current Job Corps Director; Alexandra Kielty; Jenny Gallo; Brian Kennedy; Edna Primrose-Coates; Jim Woods; and the regional Job Corps Directors and regional office study coordinators in each of the nine Job Corps regions. Members of the study advisory panel also made important contributions to the design and focus of the study. In addition, John Homrighausen, Marianne Stevenson, Linda Gentzik, and Mike Watts at MPR designed and supervised the processing of information from more than 100,000 youths nationwide. We would especially like to recognize the efforts and contributions of the hundreds of Job Corps outreach and admissions counselors nationwide, who explained the study to new Job Corps applicants.

Many of these same people also made significant contributions to the content and structure of this report.

The impact analysis would not have been possible without the efforts of the many people who conducted several rounds of interviews with a large nationwide sample of mobile youths over a four-year period. John Homrighausen served as survey director throughout; Cindy Steenstra supervised telephone center interviewing and searching operations; Donna Kratzer and Bill Beecroft managed in-person interviewing for the 30- and 48-month data collection effort; Barbara Rogers performed this role on the 12-month data collection; and Sharon De Leon and Edward Freeland did so for the baseline data collection. DIR conducted in-person interviewing in the South and Southwest for all rounds of data collection. Key DIR staff were Pamela Wells and Eleanor Tongee. Todd Ensor and John Homrighausen developed the survey instruments and oversaw preparation of the CATI programs to support telephone interviewing. Mike Watts managed sample release and provided support for CATI operations and reporting. Ben Shen assisted in providing support for the data system necessary to manage field interviewing. We are grateful to the many telephone and field interviewers who were involved in the data collection effort, and finally, to the young men and women in the sample who patiently answered our many questions.

Jeanne Bellotti, Ruo-Jiao Cao, April Grady, and Melissa Seeley provided excellent programming assistance throughout the course of the study; they constructed the data files, wrote the subroutines to produce the impact estimates, and prepared the tabulations. Steve Bishop took the lead in the arduous task of hand coding much of the data on arrests. Walter Brower and Patricia Ciaccio provided valuable editorial assistance. Cathy Harper did an excellent job of producing this report, with assistance from Monica Capizzi, Jill Miller, Jennifer Chiamonti, Cindy McClure, and Jane Nelson.

## CONTENTS

Chapter		Page
	ABSTRACT OF FINDINGS .....	xxiii
	EXECUTIVE SUMMARY .....	xxv
I	INTRODUCTION .....	1
II	OVERVIEW OF JOB CORPS AND THE NATIONAL JOB CORPS STUDY .	3
	A. OVERVIEW OF JOB CORPS .....	3
	1. Outreach and Admissions .....	4
	2. Job Corps Center Services .....	4
	3. Placement .....	7
	4. Characteristics of Youths Served by Job Corps .....	7
	5. Policy Changes Related to Violence and Drugs .....	9
	B. OVERVIEW OF THE NATIONAL JOB CORPS STUDY .....	10
	1. Impact Analysis .....	11
	2. Process Analysis .....	15
	3. Benefit-Cost Analysis .....	16
III	DATA SOURCES, OUTCOME MEASURES, AND ANALYTIC METHODS .....	17
	A. DATA SOURCES .....	18
	1. Design of the Baseline and Follow-Up Interviews .....	19
	2. Response Rates and Data Quality .....	20
	3. Analysis Samples .....	24
	B. OUTCOME MEASURES .....	25
	1. Primary Outcome Measures .....	25
	2. Construction of Outcome Measures .....	33
	C. ANALYTIC METHODS .....	35
	1. Estimating Impacts per Eligible Applicant .....	36

**CONTENTS** *(continued)*

<b>Chapter</b>		<b>Page</b>
III <i>(continued)</i>	2. Estimating Impacts per Job Corps Participant . . . . .	38
	3. The Adjustment for Crossovers in the Control Group . . . . .	40
	4. Subgroup Analysis . . . . .	42
	5. Presentation of Results . . . . .	53
	6. Interpretation of Estimates . . . . .	55
IV	JOB CORPS EXPERIENCES . . . . .	57
	A. JOB CORPS PARTICIPATION AMONG ELIGIBLE APPLICANTS IN THE PROGRAM GROUP . . . . .	59
	1. Enrollment Rates . . . . .	59
	2. Timing of Job Corps Participation . . . . .	61
	B. PARTICIPATION IN JOB CORPS ACADEMIC INSTRUCTION AND VOCATIONAL TRAINING . . . . .	65
	C. STUDENTS' EXPERIENCES AND PERCEPTIONS OF SELECTED OTHER ACTIVITIES . . . . .	70
	D. CHILD CARE UTILIZATION . . . . .	74
V	EDUCATION AND TRAINING . . . . .	77
	A. IMPACTS ON PARTICIPATION AND TIME SPENT IN EDUCATION AND TRAINING PROGRAMS . . . . .	81
	1. Impacts on Participation in Education and Training Programs . . . . .	81
	2. Impacts on Time Spent in Education and Training Programs . . . . .	85
	3. Impacts on the Types of Programs Attended . . . . .	86
	4. Impacts on Participation in Academic Classes and Vocational Training . . . . .	94
	B. IMPACTS ON EDUCATIONAL ATTAINMENT . . . . .	99
	1. Impacts on the Attainment of a High School Credential . . . . .	102
	2. Impacts on the Attainment of a Vocational Certificate . . . . .	106
	3. Impacts on the Attainment of a College Degree . . . . .	106
	4. Impacts on Highest Grade Completed . . . . .	106

## CONTENTS *(continued)*

Chapter		Page
V <i>(continued)</i>	C. FINDINGS FOR SUBGROUPS .....	107
	1. Impacts by Age and High School Credential Status .....	108
	2. Impacts for Other Key Subgroups .....	114
VI	EMPLOYMENT AND EARNINGS .....	117
	A. IMPACTS ON EMPLOYMENT RATES, TIME EMPLOYED, AND EARNINGS .....	120
	1. Impacts on Employment Rates .....	120
	2. Impacts on Time Employed .....	124
	3. Impacts on Earnings .....	128
	4. Decomposition of Impacts on Earnings in Year 4 into Its Components .....	132
	5. The Overtaking Point .....	134
	6. Effects of the Strong Economy .....	135
	B. DIFFERENCES IN HOURLY WAGES AND OTHER JOB CHARACTERISTICS .....	136
	1. Differences in Job Tenure, Hours Worked, Hourly Wages, and Weekly Earnings .....	138
	2. Differences in Occupations .....	142
	3. Differences in Hourly Wages Within Occupations .....	144
	4. Differences in the Availability of Job Benefits .....	144
	C. IMPACTS ON PARTICIPATION IN ANY ACTIVITY .....	147
	D. FINDINGS FOR SUBGROUPS .....	151
	1. Impacts by Age .....	152
	2. Impacts by Gender .....	156
	3. Impacts for Residential and Nonresidential Students .....	159
	4. Impacts for Other Key Subgroups .....	166
VII	WELFARE, CRIME, ILLEGAL DRUG USE, AND OTHER OUTCOMES ..	177
	A. RECEIPT OF PUBLIC ASSISTANCE AND OTHER SOURCES OF INCOME .....	181

CONTENTS (continued)

Chapter	Page
VII (continued)	1. Full Sample Results ..... 182
	2. Subgroup Results ..... 197
B. INVOLVEMENT WITH THE CRIMINAL JUSTICE SYSTEM	..... 203
	1. Impacts on Arrest Rates ..... 204
	2. Impacts on Arrest Charges ..... 209
	3. Impacts on Convictions ..... 213
	4. Impacts on Incarcerations Resulting from Convictions and on Probation and Parole Rates ..... 217
	5. Subgroup Results ..... 217
C. CRIMES COMMITTED AGAINST JOB CORPS PARTICIPANTS	.... 227
	1. Impacts on Victimization Rates ..... 229
	2. Impacts on Victimizations by Type of Crime ..... 231
	3. Subgroup Results ..... 233
D. TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE, HEALTH, AND MORTALITY	..... 233
	1. Impacts on Tobacco Use ..... 236
	2. Impacts on Alcohol Use ..... 236
	3. Impacts on Illegal Drug Use ..... 236
	4. Impacts on Drug or Alcohol Treatment ..... 242
	5. Impacts on Health ..... 244
	6. Impacts on Mortality ..... 244
	7. Impacts for Subgroups ..... 248
E. FAMILY FORMATION AND CHILD CARE	..... 249
	1. Impacts on Fertility ..... 252
	2. Impacts on Custodial Responsibility ..... 252
	3. Impacts on Living Arrangements and Marriage ..... 256
	4. Impacts on Child Care Use ..... 263
	5. Impacts for Other Subgroups ..... 275
F. MOBILITY	..... 276

**CONTENTS** *(continued)*

<b>Chapter</b>		<b>Page</b>
VIII	SUMMARY AND CONCLUDING OBSERVATIONS .....	283
	A. SUMMARY .....	283
	B. CONCLUDING OBSERVATIONS .....	287
	REFERENCES .....	293
	APPENDIX A: SUBGROUP SAMPLE SIZES	
	APPENDIX B: SUPPLEMENTARY TABLES TO CHAPTER IV	
	APPENDIX C: SUPPLEMENTARY TABLES TO CHAPTER V	
	APPENDIX D: SUPPLEMENTARY TABLES TO CHAPTER VI	
	APPENDIX E: SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON PUBLIC ASSISTANCE OUTCOMES	
	APPENDIX F: SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON CRIME-RELATED OUTCOMES	
	APPENDIX G: SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON CRIMES COMMITTED AGAINST JOB CORPS PARTICIPANTS	
	APPENDIX H: SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE	
	APPENDIX I: SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON FAMILY FORMATION AND MOBILITY	



## TABLES

Table	Page
III.1	EFFECTIVE RESPONSE RATES TO THE 12-MONTH, 30-MONTH AND 48-MONTH FOLLOW-UP INTERVIEWS, BY RESEARCH STATUS AND KEY SUBGROUP ..... 22
III.2	OUTCOME MEASURES DEFINED OVER SPECIFIC PERIODS ..... 26
III.3	BASELINE CHARACTERISTICS OF RESIDENTIAL AND NONRESIDENTIAL DESIGNEES IN AREAS WITH A LARGE CONCENTRATION OF NONRESIDENTIAL STUDENTS, BY GENDER ..... 52
IV.1	ENROLLMENT IN JOB CORPS, TIMING OF ENROLLMENT, AND MONTHS OF PARTICIPATION FOR THE PROGRAM GROUP ..... 60
IV.2	COMBINED ACADEMIC AND VOCATIONAL TRAINING PARTICIPATION MEASURES FOR PROGRAM GROUP ENROLLEES ..... 66
IV.3	ACADEMIC EXPERIENCE IN JOB CORPS FOR PROGRAM GROUP ENROLLEES ..... 68
IV.4	VOCATIONAL TRAINING EXPERIENCES IN JOB CORPS FOR PROGRAM GROUP ENROLLEES ..... 69
IV.5	DESCRIPTION OF SELECTED JOB CORPS ACTIVITIES ..... 72
IV.6	CHILD CARE ARRANGEMENTS USED BY FEMALES WITH CHILDREN WHILE THEY WERE ENROLLED IN JOB CORPS ..... 76
V.1	IMPACTS ON PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS ..... 82
V.2	IMPACTS ON TIME SPENT IN EDUCATION AND TRAINING PROGRAMS ..... 87
V.3	IMPACTS ON PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS, BY TYPE OF PROGRAM ..... 91
V.4	PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS OTHER THAN JOB CORPS FOR JOB CORPS PARTICIPANTS AND NO-SHOWS ..... 93

**TABLES** (continued)

<b>Table</b>	<b>Page</b>
V.5	IMPACTS ON PARTICIPATION IN ACADEMIC CLASSES ..... 97
V.6	IMPACTS ON PARTICIPATION IN VOCATIONAL TRAINING ..... 100
V.7	IMPACTS ON EDUCATIONAL ATTAINMENT ..... 104
VI.1	IMPACTS ON EMPLOYMENT RATES AND THE NUMBER OF JOBS ..... 122
VI.2	IMPACTS ON THE PERCENTAGE OF WEEKS EMPLOYED ..... 126
VI.3	IMPACTS ON HOURS EMPLOYED PER WEEK ..... 127
VI.4	IMPACTS ON EARNINGS ..... 130
VI.5	EMPLOYMENT TENURE, HOURS, AND HOURLY WAGES IN THE MOST RECENT JOB IN QUARTERS 10 AND 16 ..... 139
VI.6	OCCUPATIONS AND TYPE OF EMPLOYER ON THE MOST RECENT JOB IN QUARTERS 10 AND 16 ..... 143
VI.7	HOURLY WAGES BY OCCUPATION FOR THOSE EMPLOYED IN QUARTERS 10 AND 16 ..... 145
VI.8	BENEFITS AVAILABLE ON THE MOST RECENT JOB IN QUARTERS 10 AND 16 FOR THOSE EMPLOYED ..... 146
VI.9	IMPACTS ON BEING EMPLOYED OR IN AN EDUCATION OR TRAINING PROGRAM ..... 150
VI.10	KEY BASELINE CHARACTERISTICS, BY RACE AND ETHNICITY ..... 173
VII.1	IMPACTS ON THE RECEIPT OF AFDC/TANF, FOOD STAMP, SSI/SSA, OR GA BENEFITS ..... 184
VII.2	IMPACTS ON THE RECEIPT OF AFDC/TANF BENEFITS ..... 189
VII.3	IMPACTS ON THE RECEIPT OF FOOD STAMP BENEFITS ..... 191
VII.4	IMPACTS ON THE RECEIPT OF GA AND SSI/SSA BENEFITS ..... 193

**TABLES** (continued)

<b>Table</b>		<b>Page</b>
VII.5	IMPACTS ON PUBLIC HEALTH INSURANCE COVERAGE AND THE RECEIPT OF WIC AND PUBLIC HOUSING BENEFITS .....	195
VII.6	IMPACTS ON ARRESTS .....	207
VII.7	CRIME CATEGORIES .....	210
VII.8	IMPACTS ON ARREST CHARGES .....	212
VII.9	IMPACTS ON CONVICTION RATES AND CHARGES .....	215
VII.10	IMPACTS ON INCARCERATIONS RESULTING FROM CONVICTIONS AND ON PROBATION AND PAROLE RATES .....	218
VII.11	IMPACTS ON CRIMES COMMITTED AGAINST PARTICIPANTS IN THE PREVIOUS YEAR .....	230
VII.12	IMPACTS ON VICTIMIZATION RATES IN THE PREVIOUS YEAR, BY CRIME TYPE .....	232
VII.13	TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE IN THE 30 DAYS PRIOR TO THE 12-, 30-, AND 48-MONTH FOLLOW-UP INTERVIEWS ....	238
VII.14	IMPACTS ON PARTICIPATION IN DRUG OR ALCOHOL TREATMENT PROGRAMS .....	243
VII.15	IMPACTS ON HEALTH STATUS .....	246
VII.16	IMPACTS ON FERTILITY FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT .....	254
VII.17	IMPACTS ON CUSTODIAL RESPONSIBILITY AT 48 MONTHS FOR MALES .....	258
VII.18	IMPACTS ON LIVING ARRANGEMENTS AT THE 48-MONTH INTERVIEW FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT .....	260

**TABLES** (continued)

<b>Table</b>	<b>Page</b>
VII.19 IMPACTS ON MARITAL STATUS AT 48 MONTHS FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT .....	264
VII.20 IMPACTS ON CHILD CARE UTILIZATION FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT .....	268
VII.21 IMPACTS ON CHILD CARE UTILIZATION, BY TYPE OF ARRANGEMENT AND YEAR .....	272
VII.22 IMPACTS ON MOBILITY FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT .....	278
VII.23 CHARACTERISTICS OF THE COUNTIES OF RESIDENCE AT APPLICATION TO JOB CORPS AND THE 48-MONTH INTERVIEW .....	281
A.1 SUBGROUP SAMPLE SIZES FOR THE 48-MONTH SAMPLE .....	A.3
B.1 QUARTERLY ENROLLMENT RATES IN JOB CORPS FOR PROGRAM GROUP MEMBERS .....	B.3
B.2 PARTICIPATION IN OTHER JOB CORPS ACTIVITIES FOR PROGRAM GROUP ENROLLEES .....	B.4
B.3 JOB PLACEMENT SERVICES FOR PROGRAM GROUP ENROLLEES .....	B.5
B.4 STUDENTS' ASSESSMENT OF OTHER JOB CORPS ACTIVITIES FOR PROGRAM GROUP ENROLLEES .....	B.6
B.5 JOB CORPS EXPERIENCES, BY RESIDENTIAL DESIGNATION STATUS AND GENDER .....	B.7
B.6 EXPERIENCES IN JOB CORPS, BY HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE .....	B.8
C.1 IMPACTS ON TIME SPENT IN EDUCATION AND TRAINING PROGRAMS, BY TYPE OF PROGRAM .....	C.3

**TABLES** (continued)

<b>Table</b>	<b>Page</b>
C.2	TIME SPENT IN EDUCATION AND TRAINING PROGRAMS FOR THOSE ENROLLED IN TYPE OF PROGRAM ..... C.4
C.3	TYPES OF PROGRAMS RECEIVED ACADEMIC CLASSROOM INSTRUCTION AND VOCATIONAL TRAINING ..... C.5
C.4	IMPACTS ON EDUCATION AND TRAINING OUTCOMES FOR 16- AND 17-YEAR-OLDS ..... C.6
C.5	IMPACTS ON EDUCATION AND TRAINING OUTCOMES FOR 18- TO 24-YEAR-OLDS WITHOUT A HIGH SCHOOL CREDENTIAL AT RANDOM ASSIGNMENT ..... C.8
C.6	IMPACTS ON EDUCATION AND TRAINING OUTCOMES FOR 18- TO 24-YEAR-OLDS WITH A HIGH SCHOOL CREDENTIAL AT RANDOM ASSIGNMENT ..... C.10
C.7	IMPACTS ON KEY EDUCATION AND TRAINING OUTCOMES, BY GENDER, RESIDENTIAL DESIGNATION STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE ..... C.12
D.1	IMPACTS ON THE PERCENTAGE OF WEEKS EMPLOYED OR IN AN EDUCATION PROGRAM ..... D.3
D.2	IMPACTS ON HOURS PER WEEK EMPLOYED OR IN AN EDUCATION PROGRAM ..... D.4
D.3	IMPACTS ON EMPLOYMENT AND EARNINGS FOR 16- AND 17-YEAR-OLDS ..... D.5
D.4	IMPACTS ON EMPLOYMENT AND EARNINGS FOR 18- AND 19-YEAR-OLDS ..... D.7
D.5	IMPACTS ON EMPLOYMENT AND EARNINGS FOR 20- TO 24-YEAR-OLDS ..... D.9
D.6	IMPACTS ON EMPLOYMENT AND EARNINGS FOR MALES ..... D.11
D.7	IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALES ..... D.13
D.8	IMPACTS ON EMPLOYMENT AND EARNINGS FOR MALE RESIDENTIAL DESIGNEES ..... D.15

**TABLES** (continued)

<b>Table</b>	<b>Page</b>
D.9 IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALE RESIDENTIAL DESIGNEES WITHOUT CHILDREN .....	D.17
D.10 IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALE RESIDENTIAL DESIGNEES WITH CHILDREN .....	D.19
D.11 IMPACTS ON EMPLOYMENT AND EARNINGS FOR MALE NONRESIDENTIAL DESIGNEES .....	D.21
D.12 IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALE NONRESIDENTIAL DESIGNEES WITHOUT CHILDREN .....	D.23
D.13 IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALE NONRESIDENTIAL DESIGNEES WITH CHILDREN .....	D.25
D.14 KEY EMPLOYMENT AND EARNINGS OUTCOMES, BY HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE .....	D.27
D.15 ESTIMATED IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK IN YEAR 4 ACROSS KEY SUBGROUPS, BY RACE AND ETHNICITY .....	D.28
E.1 IMPACTS ON OTHER SOURCES OF INCOME .....	E.3
E.2 IMPACTS ON THE RECEIPT OF KEY TYPES OF PUBLIC ASSISTANCE FOR MALES .....	E.5
E.3 IMPACTS ON THE RECEIPT OF KEY TYPES OF PUBLIC ASSISTANCE FOR FEMALES WITHOUT CHILDREN .....	E.8
E.4 IMPACTS ON THE RECEIPT OF KEY TYPES OF PUBLIC ASSISTANCE FOR FEMALES WITH CHILDREN .....	E.11
E.5 IMPACTS ON THE RECEIPT OF KEY TYPES OF PUBLIC ASSISTANCE, BY RESIDENTIAL DESIGNATION STATUS, AGE, HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE .....	E.14
F.1 IMPACTS ON FINER CATEGORIES OF ARREST CHARGES .....	F.3
F.2 IMPACTS ON THE NUMBER OF ARREST CHARGES, BY YEAR .....	F.5

**TABLES** (continued)

<b>Table</b>	<b>Page</b>
F.3	IMPACTS ON KEY CRIME OUTCOMES FOR 16- AND 17-YEAR-OLDS ... F.7
F.4	IMPACTS ON KEY CRIME OUTCOMES FOR 18- AND 19-YEAR-OLDS ... F.9
F.5	IMPACTS ON KEY CRIME OUTCOMES FOR 20- TO 24-YEAR-OLDS ... F.11
F.6	IMPACTS ON KEY CRIME OUTCOMES FOR MALES ..... F.13
F.7	IMPACTS ON KEY CRIME OUTCOMES FOR FEMALES ..... F.15
F.8	IMPACTS ON KEY CRIME OUTCOMES FOR MALE RESIDENTIAL DESIGNEES ..... F.17
F.9	IMPACTS ON KEY CRIME OUTCOMES FOR FEMALE RESIDENTIAL DESIGNEES ..... F.19
F.10	IMPACTS ON KEY CRIME OUTCOMES FOR MALE NONRESIDENTIAL DESIGNEES ..... F.21
F.11	IMPACTS ON KEY CRIME OUTCOMES FOR FEMALE NONRESIDENTIAL DESIGNEES ..... F.23
F.12	IMPACTS ON KEY CRIME OUTCOMES, BY THE PRESENCE OF CHILDREN, HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE ..... F.25
G.1	IMPACTS ON THE NUMBER OF VICTIMIZATIONS IN THE PREVIOUS YEAR, BY CRIME TYPE ..... G.3
G.2	IMPACTS ON KEY VICTIMIZATION OUTCOMES, BY AGE, GENDER, RESIDENTIAL DESIGNATION STATUS, HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE ..... G.4
H.1	FREQUENCY OF TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE IN THE 30 DAYS PRIOR TO THE 30-MONTH INTERVIEW ..... H.3
H.2	FREQUENCY OF TOBACCO, ALCOHOL, AND ILLEGAL DRUG-USE IN THE 30 DAYS PRIOR TO THE 48-MONTH INTERVIEW ..... H.5

**TABLES** (continued)

<b>Table</b>	<b>Page</b>
H.3 IMPACTS ON KEY ALCOHOL AND ILLEGAL DRUG USE OUTCOMES IN THE 30 DAYS PRIOR TO THE 12-MONTH INTERVIEW AND HEALTH STATUS AT 12 MONTHS, BY SUBGROUP .....	H.7
H.4 IMPACTS ON KEY ALCOHOL AND ILLEGAL DRUG USE OUTCOMES IN THE 30 DAYS PRIOR TO THE 30-MONTH INTERVIEW AND HEALTH STATUS AT 30 MONTHS, BY SUBGROUP .....	H.9
H.5 IMPACTS ON KEY ALCOHOL AND ILLEGAL DRUG USE OUTCOMES IN THE 30 DAYS PRIOR TO THE 48-MONTH INTERVIEW AND HEALTH STATUS AT 30 MONTHS, BY SUBGROUP .....	H.11
I.1 IMPACTS ON CHILD CARE UTILIZATION FOR MALES, BY TYPE OF ARRANGEMENT AND YEAR .....	I.3
I.2 IMPACTS ON CHILD CARE UTILIZATION FOR FEMALES WITHOUT CHILDREN AT RANDOM ASSIGNMENT, BY TYPE OF ARRANGEMENT AND YEAR .....	I.5
I.3 IMPACTS ON CHILD CARE UTILIZATION FOR FEMALES WITH CHILDREN AT RANDOM ASSIGNMENT, BY TYPE OF ARRANGEMENT AND YEAR .....	I.7
I.4 IMPACTS ON HOURS USED CHILD CARE UTILIZATION FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT, BY TYPE OF ARRANGEMENT .....	I.9
I.5 IMPACTS ON KEY FERTILITY, LIVING ARRANGEMENT, MARITAL STATUS, AND MOBILITY OUTCOMES, BY SUBGROUP .....	I.11



## FIGURES

Figure		Page
II.1	JOB CORPS REGIONS IN PROGRAM YEAR 1995, BY REGION .....	5
IV.1	JOB CORPS PARTICIPATION RATES FOR THE FULL PROGRAM GROUP, BY QUARTER .....	64
IV.2	OTHER ACTIVITIES IN JOB CORPS .....	73
V.1	PARTICIPATION RATES IN EDUCATION AND TRAINING PROGRAMS, BY QUARTER .....	84
V.2	AVERAGE HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS, BY QUARTER .....	88
V.3	PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS, BY TYPE OF PROGRAM .....	90
V.4	PARTICIPATION IN ACADEMIC CLASSES AND VOCATIONAL TRAINING DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT .....	96
V.5	DEGREES, DIPLOMAS, AND CERTIFICATES RECEIVED .....	103
V.6	PARTICIPATION AND HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS FOR CONTROL GROUP MEMBERS, BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE .....	109
V.7	PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS FOR CONTROL GROUP MEMBERS, BY TYPE OF PROGRAM, AGE, AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE .....	111
V.8	PARTICIPATION AND HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS, BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE .....	113
V.9	EDUCATIONAL ATTAINMENT, BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE .....	115
VI.1	EMPLOYMENT RATES BY QUARTER .....	121
VI.2	TIME EMPLOYED, BY QUARTER .....	125

**FIGURES** *(continued)*

<b>Figure</b>	<b>Page</b>
VI.3	AVERAGE EARNINGS PER WEEK BY QUARTER ..... 129
VI.4	PERCENTAGE EMPLOYED OR IN SCHOOL, BY QUARTER ..... 149
VI.5	AVERAGE EARNINGS PER WEEK (IN 1995 DOLLARS), BY QUARTER AND AGE ..... 153
VI.6	IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4, BY AGE ..... 154
VI.7	AVERAGE EARNINGS PER WEEK (IN 1995 DOLLARS), BY QUARTER AND GENDER ..... 157
VI.8	IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4, BY GENDER ..... 158
VI.9	AVERAGE EARNINGS PER WEEK (IN 1995 DOLLARS) FOR RESIDENTIAL DESIGNEES, BY QUARTER AND GENDER ..... 161
VI.10	IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4 FOR RESIDENTIAL DESIGNEES, BY GENDER ..... 162
VI.11	AVERAGE EARNINGS PER WEEK (IN 1995 DOLLARS) FOR NONRESIDENTIAL DESIGNEES, BY QUARTER AND GENDER ..... 164
VI.12	IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4 FOR NONRESIDENTIAL DESIGNEES, BY GENDER ..... 165
VI.13	IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4, BY HIGH SCHOOL CREDENTIAL STATUS AND AGE ..... 168
VI.14	IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4, BY ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE ..... 170
VII.1	RECEIPT OF AFDC/TANF, FOOD STAMP, SSI/SSA, OR GA BENEFITS, BY QUARTER ..... 183

**FIGURES** *(continued)*

<b>Figure</b>	<b>Page</b>
VII.2 RECEIPT OF AFDC/TANF AND FOOD STAMP BENEFITS, BY QUARTER .....	187
VII.3 PERCENTAGE WHO RECEIVED AFDC/TANF, FOOD STAMP, SSI/SSA, OR GA BENEFITS, FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN, BY QUARTER .....	199
VII.4 AVERAGE DOLLAR VALUE OF PUBLIC ASSISTANCE BENEFITS RECEIVED BY MALES AND BY FEMALES WITH AND WITHOUT CHILDREN, BY BENEFIT TYPE .....	200
VII.5 ARREST RATES, BY QUARTER .....	205
VII.6 CONVICTIONS AND INCARCERATIONS RESULTING FROM CONVICTIONS DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT .....	214
VII.7 PERCENTAGE EVER ARRESTED, CONVICTED, AND INCARCERATED FOR CONVICTIONS DURING THE 48-MONTH PERIOD, BY AGE .....	220
VII.8 PERCENTAGE EVER ARRESTED, CONVICTED, AND INCARCERATED FOR CONVICTIONS DURING THE 48-MONTH PERIOD, BY GENDER .....	222
VII.9 PERCENTAGE EVER ARRESTED, CONVICTED, AND INCARCERATED FOR CONVICTIONS DURING THE 48-MONTH PERIOD FOR RESIDENTIAL DESIGNEES, BY GENDER .....	224
VII.10 PERCENTAGE EVER ARRESTED, CONVICTED, AND INCARCERATED FOR CONVICTIONS DURING THE 48-MONTH PERIOD FOR NONRESIDENTIAL DESIGNEES, BY GENDER .....	225
VII.11 TOBACCO AND ALCOHOL USE IN THE 30 DAYS PRIOR TO THE 12-, 30-, AND 48-MONTH INTERVIEWS .....	237
VII.12 ILLEGAL DRUG USE IN THE 30 DAYS PRIOR TO THE 12- AND 30-, AND 48-MONTH INTERVIEWS .....	240
VII.13 HEALTH STATUS AT THE 12-, 30-, AND 48-MONTH INTERVIEWS .....	245

**FIGURES** *(continued)*

<b>Figure</b>		<b>Page</b>
VII.14	FERTILITY DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN .....	253
VII.15	THE PRESENCE OF CHILDREN AND CUSTODIAL RESPONSIBILITY AT 48 MONTHS FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT .....	257

## ABSTRACT OF FINDINGS

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

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

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

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

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

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

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

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

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

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

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

## EXECUTIVE SUMMARY

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

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

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

The report answers the following three research questions:

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

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

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

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

## STUDY DESIGN

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

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

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

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



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

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

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

## **DATA SOURCES, OUTCOME MEASURES, AND ANALYTIC METHODS**

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

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

---

<sup>1</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 are not in the research sample.

<sup>2</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.

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

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

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

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

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

---

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

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

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

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

## **JOB CORPS EXPERIENCES**

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

---

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

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

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

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

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

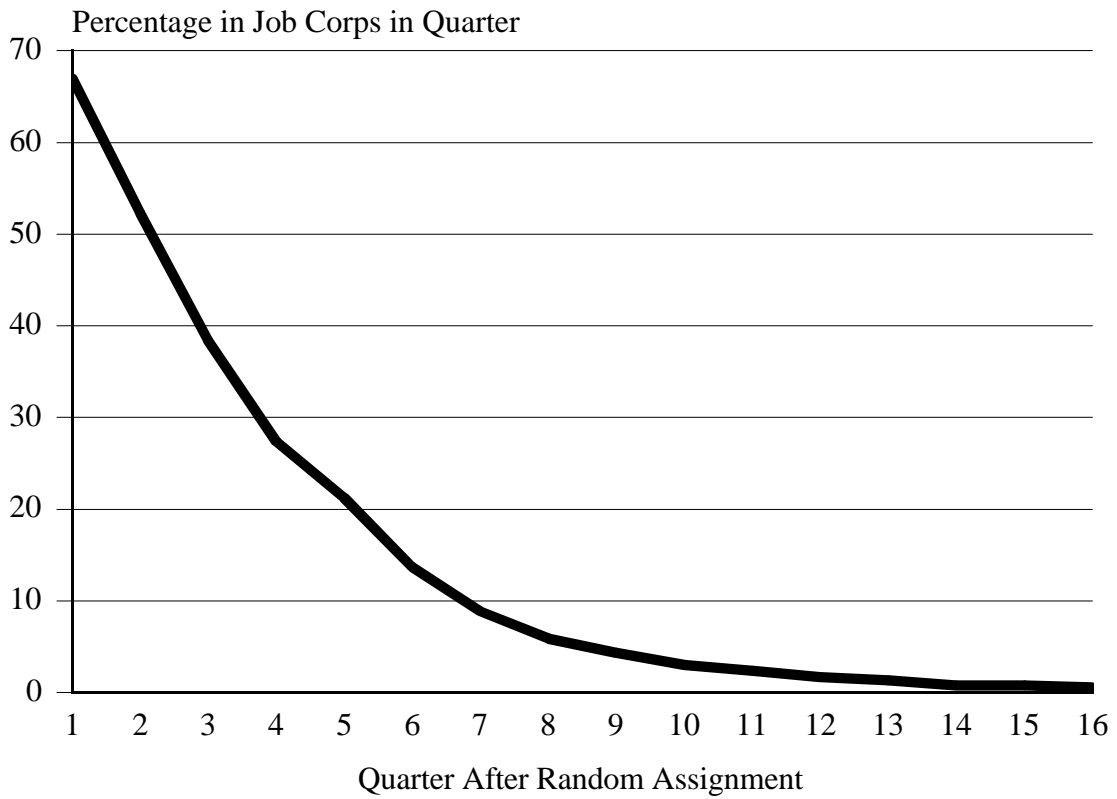
**Most participants stayed in Job Corps for a substantial period of time, although the period of participation varied considerably.** The average period of participation per enrollee was eight months. About 28 percent of all enrollees participated less than three months, and nearly a quarter participated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

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

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

FIGURE 1

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



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

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

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

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

## EDUCATION AND TRAINING

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

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

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

Our main findings can be summarized as follows:

**Many control group members received substantial amounts of education and training.** Nearly 72 percent participated in an education or training program during the 48 months after random assignment. On average, they received 853 hours of education and training, roughly equivalent to three-quarters of a year of high school. Participation rates were highest in programs that substitute for Job Corps: GED programs (37 percent); high school (32 percent); and vocational, technical, or trade schools (29 percent).<sup>5</sup> These high participation rates are not surprising, because control group members demonstrated motivation to go to Job Corps, and thus had the motivation to find other programs.

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

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

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

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

---

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

TABLE 1

IMPACTS ON PARTICIPATION AND TIME SPENT IN EDUCATION  
AND TRAINING PROGRAMS

	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Estimated Impact per Participant <sup>b</sup>
Percentage Ever Enrolled in an Education or Training Program During the 48 Months After Random Assignment	92.5	71.7	20.8*	28.9*
Average Percentage of Weeks Ever in Education or Training	24.4	18.2	6.3*	8.7*
Average Hours per Week Ever in Education or Training	7.6	4.1	3.5*	4.8*
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	

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

<sup>a</sup>Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

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

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



TABLE 2

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

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

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

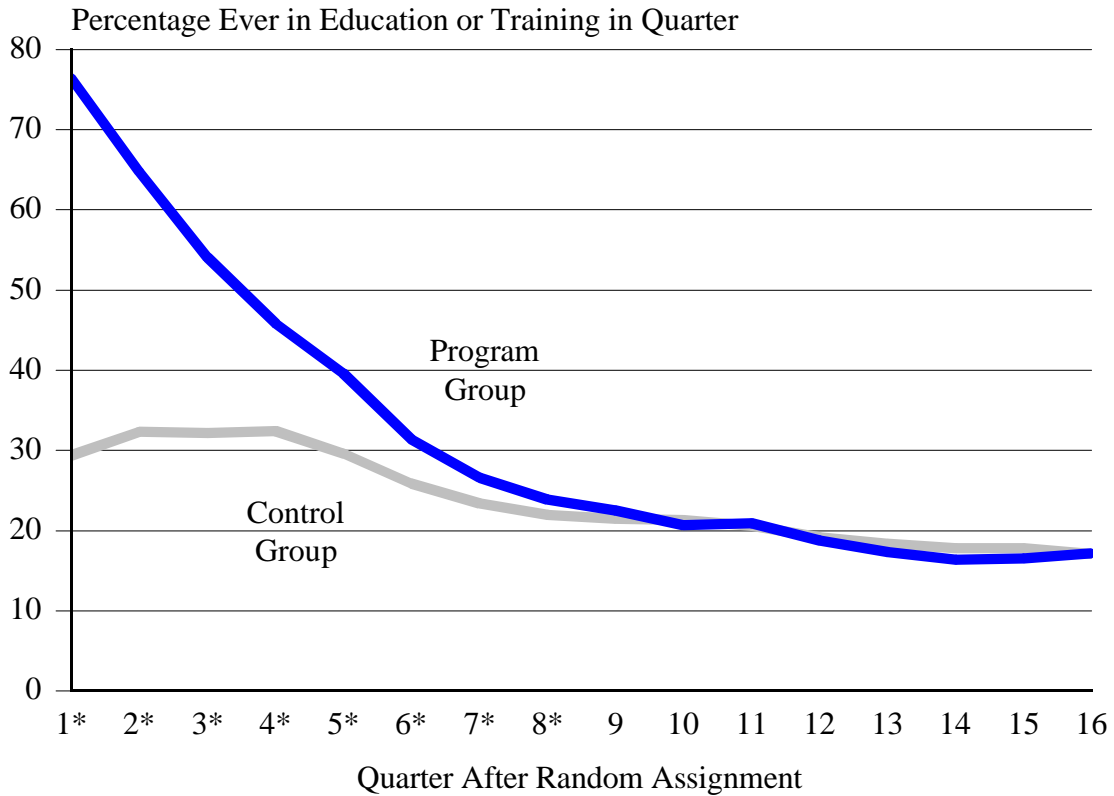
<sup>a</sup>Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

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

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

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

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



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

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

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

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

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

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

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

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

---

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

FIGURE 3

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS,  
BY TYPE OF PROGRAM

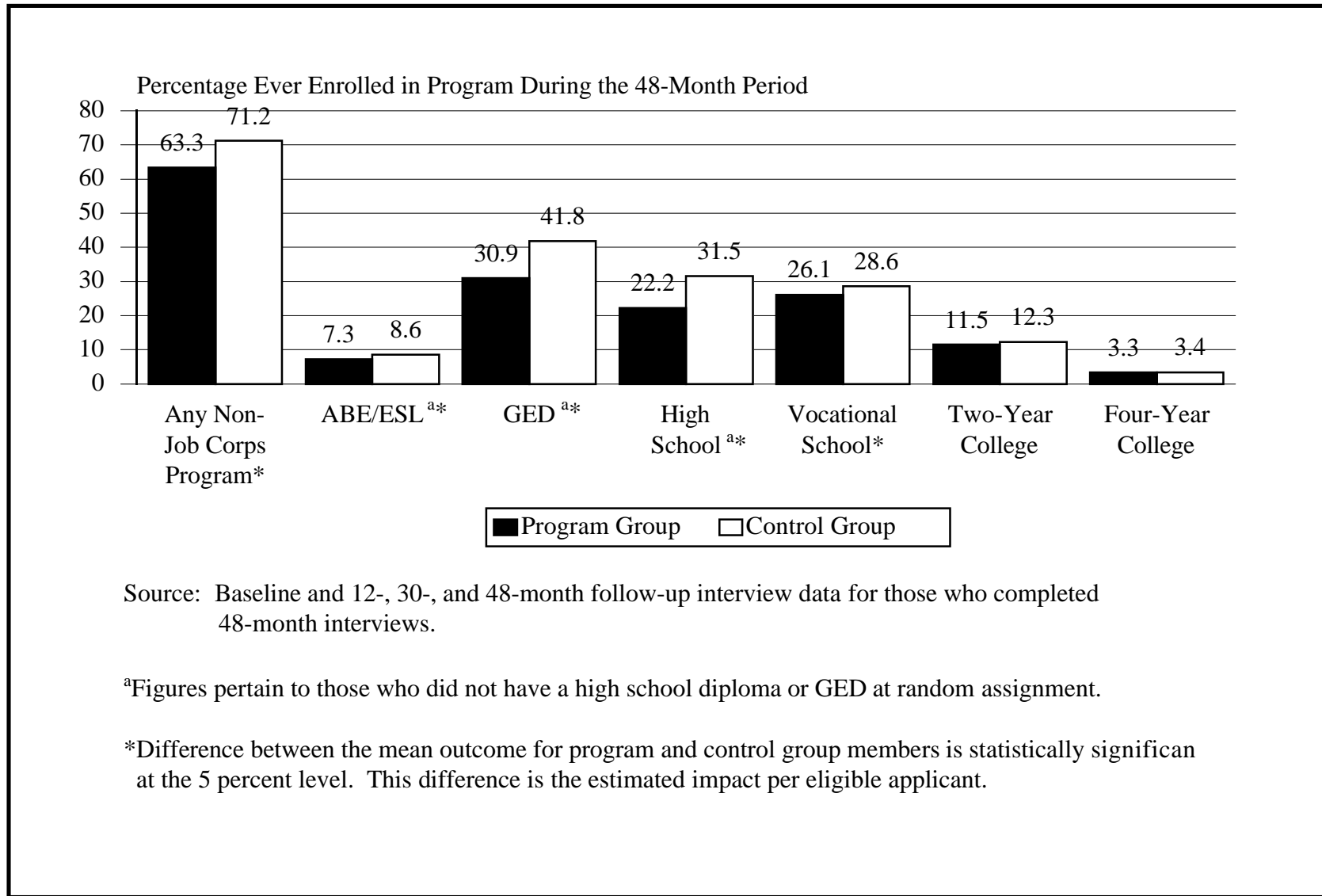
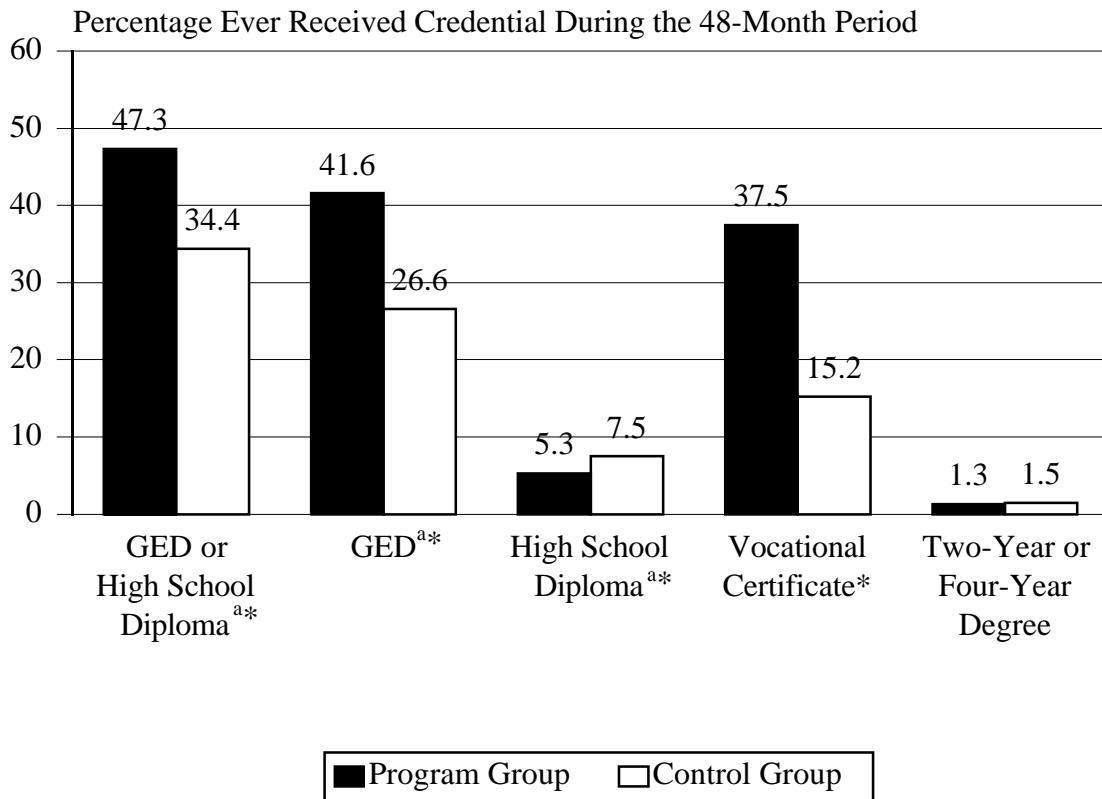


FIGURE 4

DEGREES, DIPLOMAS, AND CERTIFICATES RECEIVED



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

<sup>a</sup>Figures pertain to those who did not have a high school credential at random assignment.

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

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

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

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

## **EMPLOYMENT AND EARNINGS**

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

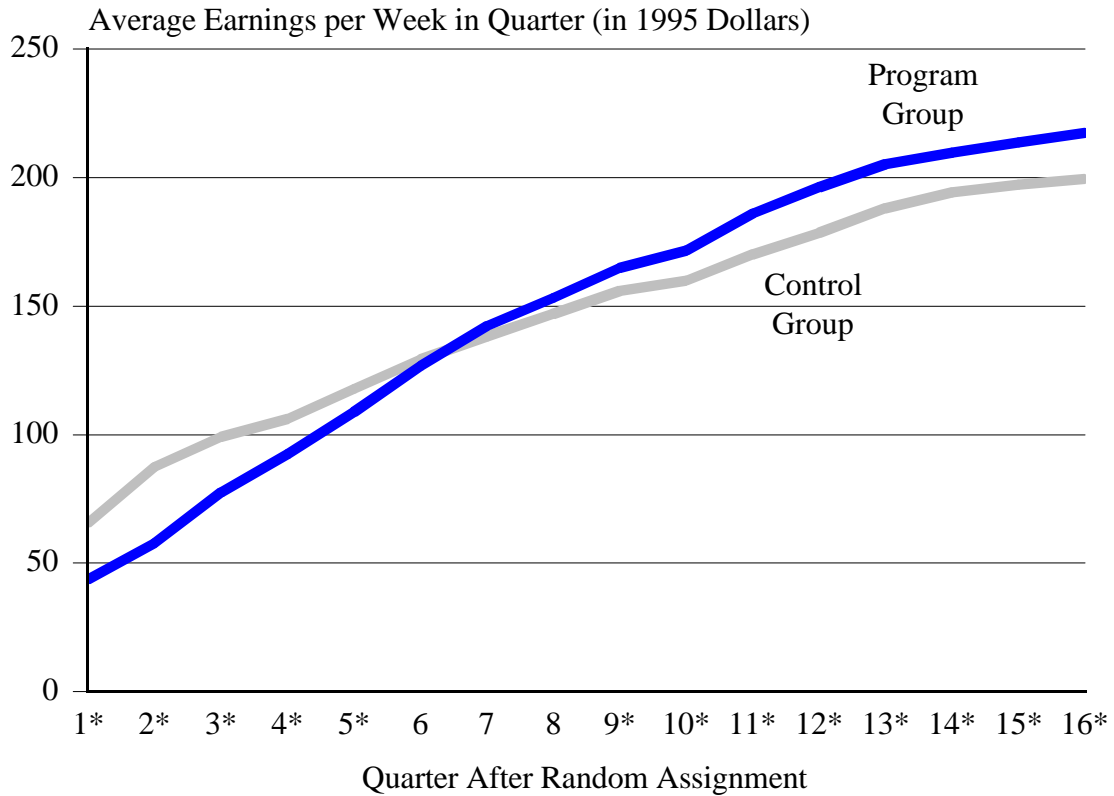
We expect negative impacts on participants' employment and earnings during the period of enrollment, because some would have held jobs if they had not gone to Job Corps. However, because of improvements in participants' skills, we expect positive impacts on employment and earnings after they leave the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

A summary of our findings is as follows:

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

FIGURE 5

AVERAGE EARNINGS PER WEEK, BY QUARTER



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

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

TABLE 3

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

	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Estimated Impact per Participant <sup>b</sup>
Average Earnings per Week, by Quarter After Random Assignment				
13	205.3	188.0	17.3*	24.1*
14	209.8	194.2	15.7*	21.8*
15	213.7	197.2	16.5*	22.9*
16	217.5	199.4	18.1*	25.2*
Percentage Employed, by Quarter				
13	66.8	63.4	3.4*	4.8*
14	67.5	65.1	2.4*	3.3*
15	69.2	65.6	3.6*	5.0*
16	71.1	68.7	2.4*	3.3*
Average Percentage of Weeks Employed, by Quarter				
13	58.6	55.7	3.0*	4.1*
14	59.6	56.8	2.9*	4.0*
15	60.9	57.7	3.2*	4.4*
16	61.8	59.0	2.8*	3.9*
Average Hours Employed per Week, by Quarter				
13	26.8	25.4	1.5*	2.0*
14	27.3	25.9	1.4*	1.9*
15	27.7	26.3	1.5*	2.0*
16	27.9	26.4	1.5*	2.0*
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	

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

<sup>a</sup>Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

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

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



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

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

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

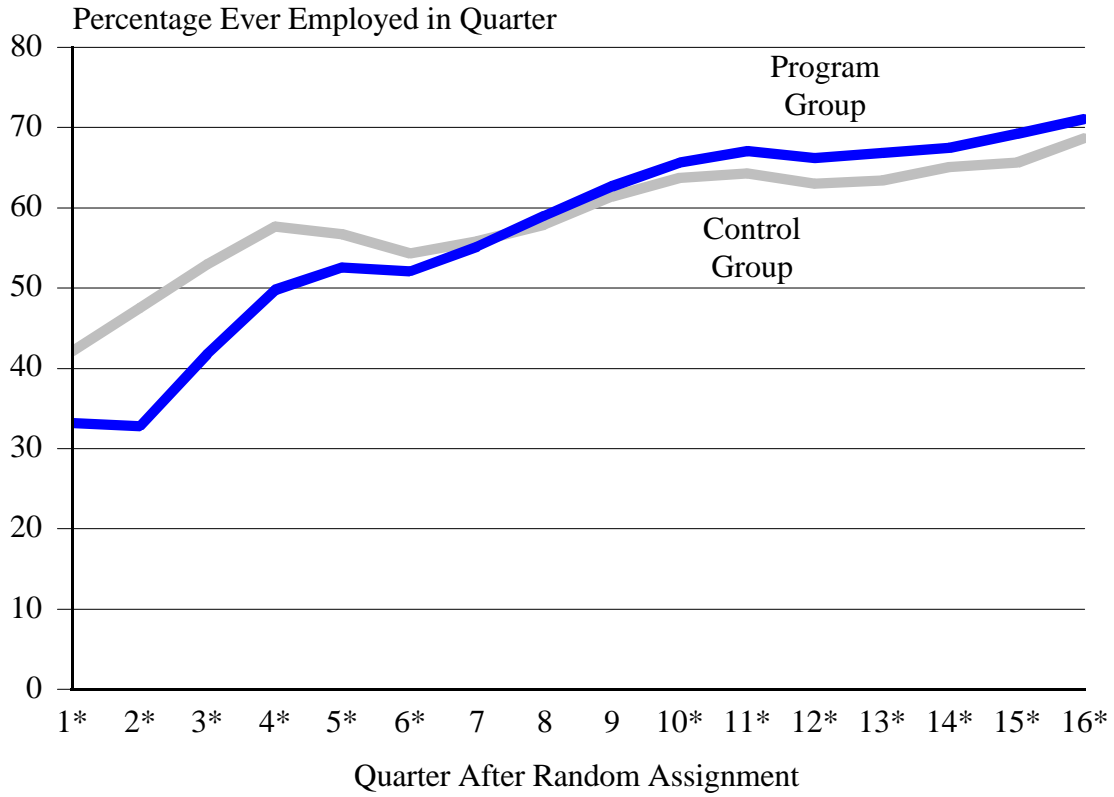
**The earnings gains late in the period were due to a combination of greater hours of work and higher earnings per hour.** Program group members earned about \$11 more per week in year 4 than control group members because they worked more hours, and they earned about \$5 more per week because they had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

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

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

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

FIGURE 6  
EMPLOYMENT RATES, BY QUARTER



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

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

policies took effect, and for whites and African Americans.

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

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

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

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

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

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

---

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

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

## **WELFARE, CRIME, ILLEGAL DRUG USE, AND OTHER OUTCOMES**

The study examined the impacts of Job Corps on several additional outcomes to help assess whether the program achieves its goals of helping students become more responsible and productive citizens. This section reports on impacts on welfare dependence; involvement with the criminal justice system; use of tobacco, alcohol, and illegal drugs; the overall health of participants; the likelihood of bearing or fathering children while unmarried; custodial responsibility; the likelihood of forming stable, long-term relationships; mobility; and the use of child care.

Our main results are as follows:

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

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

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

TABLE 4

## IMPACTS ON KEY PUBLIC ASSISTANCE AND CRIME OUTCOMES

	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Estimated Impact per Participant <sup>b</sup>
Average Amount of Benefits Received, by Year (in Dollars)				
All years	3,696.0	4,155.7	-459.8*	-638.9*
1	1,109.8	1,225.9	-116.2*	-161.4*
2	978.7	1,101.6	-122.9*	-170.8*
3	893.3	1,001.4	-108.1*	-150.2*
4	745.5	825.6	-80.1*	-111.3*
Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year				
All years	28.8	32.6	-3.7*	-5.2*
1	11.1	14.1	-3.1*	-4.3*
2	10.5	11.3	-0.8	-1.2
3	11.1	11.4	-0.4	-0.5
4	9.6	10.3	-0.7	-0.9
Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment				
	22.1	25.2	-3.1*	-4.3*
Percentage Served Time in Jail for Convictions During the 48-Month Period				
	15.8	17.9	-2.1*	-2.9*
Average Weeks in Jail for Convictions During the 48-Month Period				
	6.0	6.6	-0.6	-0.8
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	

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

<sup>a</sup>Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

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

<sup>c</sup>Benefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance.

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

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

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

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

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

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

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

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

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

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

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

## CONCLUDING OBSERVATIONS

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

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

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

---

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

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

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

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

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

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

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

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



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

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

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

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

## I. INTRODUCTION

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

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

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

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

---

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

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

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

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

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

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

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

The Job Corps program, established by the Economic Opportunity Act of 1964, operates under provisions of the Workforce Investment Act (WIA) of 1998.<sup>1</sup> The operational structure of Job Corps is complex, with multiple levels of administrative accountability, several distinct program components, and numerous contractors and subcontractors. DOL administers Job Corps through a national office and nine regional offices. The national office establishes policy and requirements, develops curricula, and oversees major program initiatives. The regional offices procure and administer contracts and perform oversight activities, such as reviews of center performance.

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

---

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

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

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

## **1. Outreach and Admissions**

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

## **2. Job Corps Center Services**

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

---

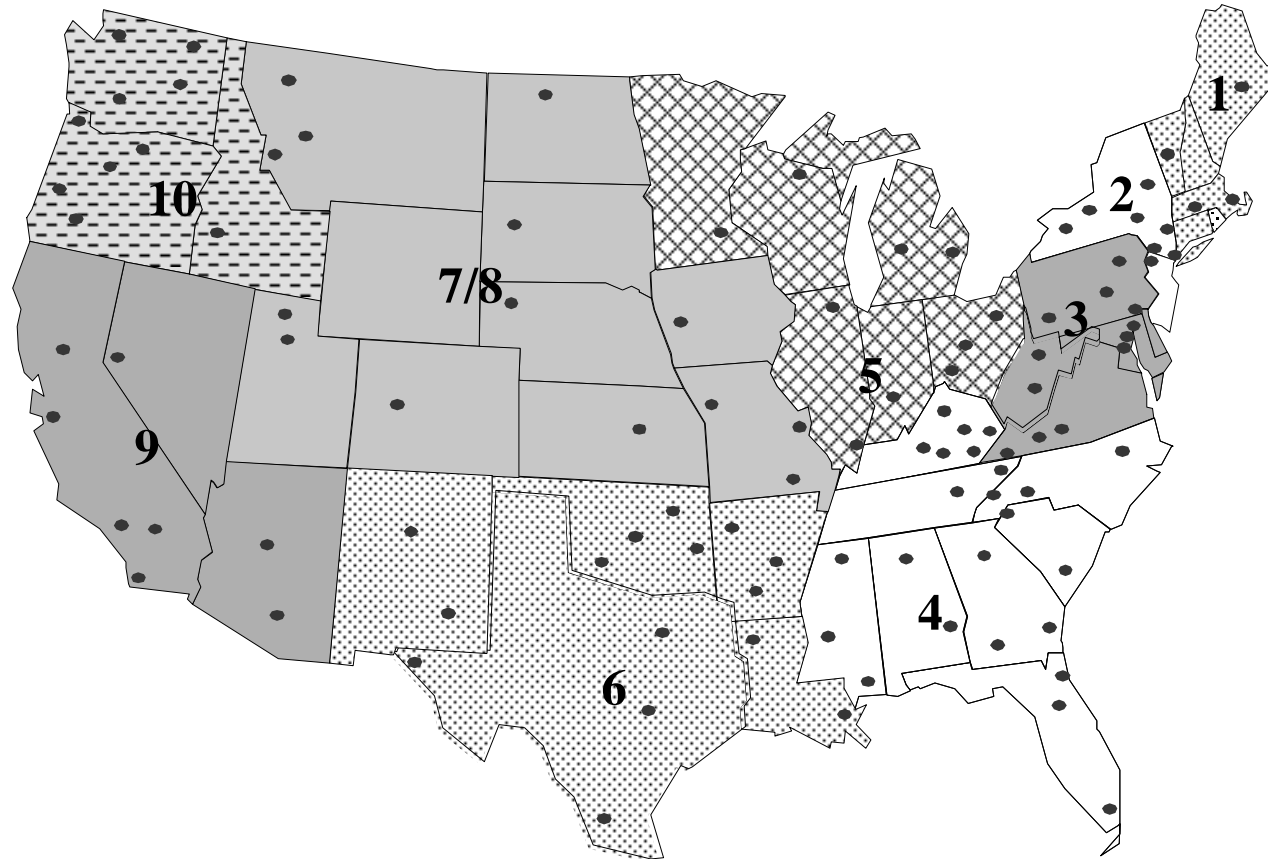
<sup>2</sup>In total, there were 110 centers in operation, including the five centers in Alaska, Hawaii, and Puerto Rico.

<sup>3</sup>There are currently 119 centers in operation.

## FIGURE II.1

### JOB CORPS CENTERS IN PROGRAM YEAR 1995, BY REGION

---



5

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

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

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

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

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

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

### **3. Placement**

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

### **4. Characteristics of Youths Served by Job Corps**

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



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

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

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

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

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

## **5. Policy Changes Related to Violence and Drugs**

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

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

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

## **B. OVERVIEW OF THE NATIONAL JOB CORPS STUDY**

The National Job Corps Study addresses six major research questions:

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

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

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

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

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

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

## **1. Impact Analysis**

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

### a. Sample Design

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

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

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

---

<sup>4</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 are not in the research sample.

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

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

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

---

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

<sup>6</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.

## **b. Implementation of Random Assignment**

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

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

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

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

---

<sup>7</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 (see Chapter III).

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

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

## **2. Process Analysis**

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

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



### **3. Benefit-Cost Analysis**

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

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

### **III. DATA SOURCES, OUTCOME MEASURES, AND ANALYTIC METHODS**

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

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

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

## A. DATA SOURCES

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

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

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

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

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

## **1. Design of the Baseline and Follow-Up Interviews**

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

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

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

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

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

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

## **2. Response Rates and Data Quality**

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

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

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

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

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

---

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

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

TABLE III.1

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

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

TABLE III.1 (continued)

---

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

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

<sup>a</sup>To reduce data collection costs, 93 percent of program group members eligible for 48-month interviews were randomly selected for 48-month interviewing.

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



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

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

### **3. Analysis Samples**

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

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

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

---

<sup>3</sup>About 210 cases in the analysis sample completed an abbreviated baseline interview.

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

## **B. OUTCOME MEASURES**

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

### **1. Primary Outcome Measures**

The primary outcome measures can be grouped into six areas:

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

TABLE III.2

OUTCOME MEASURES DEFINED OVER SPECIFIC PERIODS

---

**Education and Training**

All Programs

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

Specific Programs

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

Academic Classes

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

Vocational Training

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

Educational Attainment

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

**Employment, Earnings, and Job Characteristics**

Employment

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

TABLE III.2 (continued)

---

**Employment, Earnings, and Job Characteristics (continued)**

Earnings

Distribution of earnings

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

Had a job

Months on job

Usual hours worked per week

Hourly wage

Weekly earnings

Occupation

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

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

Education and Employment Activities

Ever participated in any activity

Weeks participated

Hours per week participated

**Receipt of Public Assistance and Other Sources of Income**

Public Assistance

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

Months received benefits, by type

Amount of benefits received, by type

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

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

Other Sources of Income

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

Weeks received UI

Amount received, by type

TABLE III.2 (continued)

---

### **Crime, Alcohol and Illegal Drug Use, and Health**

#### **Criminal Activities**

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

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

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

#### **Drug and Alcohol Treatment**

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

#### **Health**

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

TABLE III.2 (continued)

---

### **Family Formation**

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

### **Mobility**

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

---

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

<sup>a</sup>Outcomes defined only for those who did not have a high school credential at random assignment.

<sup>b</sup>Outcomes defined for those not living with all their children.

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

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

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

---

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

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

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

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

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



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

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

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

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

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

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

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

## **2. Construction of Outcome Measures**

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

---

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

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

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

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

## C. ANALYTIC METHODS

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

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

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

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

### **1. Estimating Impacts per Eligible Applicant**

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

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

---

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

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

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

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

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

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

## **2. Estimating Impacts per Job Corps Participant**

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

---

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

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

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

---

<sup>7</sup>(...continued)

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

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



### **3. The Adjustment for Crossovers in the Control Group**

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

#### **a. The Adjustment for Early Crossovers**

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

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

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

#### **b. The Adjustment for Late Crossovers**

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

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

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

---

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

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

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

#### **4. Subgroup Analysis**

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

---

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

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

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

This report addresses two important questions about impacts for subgroups:

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

#### **a. Subgroups Defined by Youth Characteristics**

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

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

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

---

<sup>12</sup>Appendix Table A.1 displays sample sizes for the subgroups.

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

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

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

---

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

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

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

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

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

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

---

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

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

7. *Job Corps Application Date and the New Job Corps Policies.* As discussed, in response to congressional concerns about the operation of the Job Corps program, new ZT policies were instituted in March 1995--during the sample intake period for the study. The process analysis found that the new policies had a profound positive effect on behavior management and the general climate at centers.<sup>16</sup> Thus, to assess the extent to which the new policies had an effect on program impacts, we present separate impact estimates for those who applied to Job Corps before and after March 1, 1995.<sup>17</sup> Because the ZT policies are still in effect, the post-ZT estimates are more likely to be representative of the current Job Corps program.

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

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

---

<sup>15</sup>(...continued)  
small to support separate impact estimates for these groups.

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

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

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

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



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

#### **b. Impacts for Residents and Nonresidents**

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

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

---

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

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

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

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

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

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

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

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

---

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

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

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

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

---

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

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

TABLE III.3

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

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

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

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

<sup>a</sup>Welfare receipt includes AFDC/TANF, food stamps, or other public assistance.

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

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

## 5. Presentation of Results

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

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

---

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

(continued...)

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

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

---

<sup>22</sup>(...continued)

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

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

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

## **6. Interpretation of Estimates**

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

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



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

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

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

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

#### **IV. JOB CORPS EXPERIENCES**

Job Corps staff have implemented a well-developed program model throughout the country. Both the model and the fidelity of its implementation are documented in a separate process analysis report (Johnson et al. 1999). For understanding of the impacts that the program may have had on employment and related outcomes of participants, this chapter uses interview data to describe the Job Corps experiences of the program group. Here we note whether program group members received services and then describe the intensity and types of those services.

This chapter answers four broad questions about program participation:

1. Did those who were randomly assigned to the Job Corps program group actually participate?
2. When did most Job Corps participation occur?
3. What were the experiences in the program of those who enrolled?
4. Do the Job Corps experiences of subgroups of interest to the study differ in important ways?

The answers to these questions led to the following conclusions.

First, the program group received extensive Job Corps services. Of those who were assigned to the program group, 73 percent enrolled in Job Corps, 72 percent of these enrollees (just over half the program group) participated in Job Corps for at least three months, and nearly one-quarter of enrollees participated for longer than a year. The average period of participation per enrollee was eight months.

Second, participants enrolled quickly, and most participation occurred during the first 12 months after random assignment. The average participant in the program group enrolled in Job Corps within

1.4 months after random assignment and spent 8 months in the program, which resulted in an average postprogram period of more than three years. Furthermore, the postprogram period was at least two years for about 92 percent of participants. Thus, the 48-month follow-up data provide a reliable indication of the medium-term, postprogram benefits of Job Corps.

Third, enrollees participated extensively in the core Job Corps activities. Most took both academic classes and vocational training, although the relative emphasis differed among individual enrollees. Also, most enrollees participated in the many socialization activities, such as parenting, education, health education, social skills, training, and cultural awareness classes. Many enrollees, however, reported that they did not receive job placement assistance from the program.

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

The rest of this chapter presents the data supporting these findings. The first section discusses rates and timing of enrollment in Job Corps for those assigned to the program group. The second section discusses the academic classroom and vocational training experiences of enrollees. Third, we discuss the enrollees' participation in other Job Corps activities, such as social skills training

(SST) and parenting classes. Finally, we discuss the child care arrangements used by female enrollees with children while they attended Job Corps. Appendix B presents supplementary tables.<sup>1</sup>

The extent, duration, and intensity of participation may have differed for different groups of students. To identify possible differences, we present tabulations for key subgroups defined by gender and parental status (males, females, and females with children) and for three groups defined by age (16 and 17 years old, 18 and 19 years old, and 20 to 24 years old). Appendix B presents selected data on the program experiences of other important subgroups.

## **A. JOB CORPS PARTICIPATION AMONG ELIGIBLE APPLICANTS IN THE PROGRAM GROUP**

### **1. Enrollment Rates**

The study's program and control groups were established at the point that each youth had been determined to be eligible for Job Corps. An applicant found eligible was assigned to a specific center, and an OA counselor arranged for transportation. However, between the time that eligibility was established and the time that transportation was arranged, some applicants decided not to enroll. Consequently, not everyone who was assigned to the Job Corps program group actually went to a center.

The overall enrollment rate in Job Corps was 73 percent (Table IV.1). This self-reported enrollment rate is practically identical to that calculated from Job Corps administrative records

---

<sup>1</sup>The 12- and 30-month follow-up interviews contain detailed questions on program group members' experiences in Job Corps. These interviews captured over 91 percent of all weeks spent in Job Corps. This information, however, was not collected at the 48-month interview. Thus, we used Job Corps administrative data from SPAMIS to measure additional program participation that occurred between the previous interview and the 48-month interview. SPAMIS, however, does not contain detailed information on Job Corps activities (such as participation in SST classes, academic and vocational courses taken, and child care). Thus, descriptive analyses for these activities were conducted using those in the analysis sample who completed 30-month interviews.

TABLE IV.1  
 ENROLLMENT IN JOB CORPS, TIMING OF ENROLLMENT, AND  
 MONTHS OF PARTICIPATION FOR THE PROGRAM GROUP  
 (Percentages)

	Gender			Age			
	Total	All Males	All Females	Females with Children	16 to 17	18 to 19	20 to 24
Enrolled in a Job Corps Center	73.2	75.8	69.6	64.1	78.8	70.6	67.9
Number of Centers Attended							
0	26.8	24.3	30.4	35.9	21.2	29.4	32.2
1	65.8	67.8	62.9	58.9	71.2	62.6	61.4
2	7.0	7.5	6.3	4.8	7.4	7.6	5.8
3	0.4	0.4	0.3	0.4	0.2	0.4	0.6
Months Between Random Assignment and Center Enrollment <sup>a</sup>							
Less than 0.5	48.7	48.0	49.9	47.5	49.2	48.1	48.8
0.5 to 1	25.8	25.6	26.2	24.3	25.7	25.9	25.8
1 to 3	17.4	18.0	16.6	18.8	16.9	18.1	17.4
3 to 6	3.7	4.3	2.8	2.6	3.7	3.7	3.8
6 or more	4.3	4.1	4.6	6.9	4.5	4.2	4.2
(Average months)	1.4	1.4	1.4	1.7	1.5	1.4	1.3
Months Enrolled <sup>a</sup>							
Less than 1	9.1	9.7	8.2	8.7	8.6	10.1	8.6
1 to 3	19.2	20.2	17.7	19.3	22.2	17.9	15.6
3 to 6	18.6	18.7	18.5	20.9	20.1	18.4	16.3
6 to 9	16.6	16.1	17.4	18.1	15.9	17.6	16.5
9 to 12	12.9	12.7	13.2	12.3	11.6	13.3	14.7
12 to 18	14.4	13.5	15.9	14.1	14.3	13.8	15.5
18 or more	9.1	9.1	9.1	6.7	7.2	9.0	12.7
(Average months)	8.0	7.8	8.4	7.6	7.4	7.9	9.2
Months Between Date Left Job Corps and 48 Months After Random Assignment <sup>a</sup>							
Less than 12	2.5	2.5	2.6	1.6	2.9	2.4	2.0
12 to 24	5.8	5.7	6	6.6	4.9	6.4	6.8
24 to 36	25.1	24.9	25.4	24	24.4	23	28.9
36 to 48	66.5	66.9	65.9	67.8	67.8	68.2	62.3
(Average months)	37.5	37.7	37.2	37.7	37.7	37.7	36.9
Enrolled at 48 Months After Random Assignment	0.3	0.3	0.4	0.3	0.4	0.3	0.1
<b>Sample Size</b>	<b>6,828</b>	<b>3,741</b>	<b>3,087</b>	<b>1,005</b>	<b>2,742</b>	<b>2,175</b>	<b>1,911</b>

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

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

<sup>a</sup>Data pertain to program group members who enrolled in a Job Corps center during the 48 months after random assignment.

(Johnson et al. 2000). Most students (90 percent) attended just one center, although 10 percent transferred to another center for regular or advanced training.

Enrollment rates over the 48-month follow-up period differed by subgroup (Table IV.1). Somewhat larger percentages of younger applicants than older applicants enrolled (79 percent compared to 68 percent), and larger percentages of males enrolled than females (76 percent compared to 70 percent). Female applicants with children at baseline had the lowest enrollment rate (64 percent). Rates of participation were somewhat lower for students who were identified at intake as likely nonresidential students than for residential students, 66 percent compared to 75 percent (Table B.5). Furthermore, this relationship between rates of participation for residential and nonresidential students is observed for males, females, and females with children in each residential group.

## **2. Timing of Job Corps Participation**

Two aspects of the timing of Job Corps participation are important for the interpretation of program impacts. First, it is useful to know *how long* participants spent in the program, because this is an important measure of *exposure* to the program and of the extent to which program group members invested in their future earning capacity. On the other hand, time spent in the program is time when students probably would have worked, and thus they earned less than they would have if they had not participated.

Second, it is important to know *when participation ended* in order to interpret the impacts on employment, earnings, and related outcomes. One hypothesis of this study is that, for key outcomes like employment and earnings, negative impacts during the in-program period will be offset by positive impacts in the postprogram period. Because Job Corps uses “open-entry” and “open-exit” instruction, the length of participation varies for each student, and no fixed “in-program” period can be identified for all students. Furthermore, waiting times until youths enrolled differed across centers. Thus, impacts defined over a specific time during the 48-month follow-up period are based on some program group members who were still enrolled in Job Corps, some who had been out of Job Corps for a short time, and some who had been out for a longer time. Data on the timing of participation help us identify “in-program” and “postprogram” periods and underscore the need for caution when interpreting impacts over 48 months.

Program group members typically enrolled in Job Corps soon after random assignment (Table IV.1). The average enrollee waited 1.4 months, or just over six weeks, to be enrolled in a Job Corps center, although nearly three-quarters of those who enrolled did so in the first month, and only four percent enrolled more than six months after random assignment.<sup>2</sup>

Once in Job Corps, enrollees participated for about eight months on average, although the period of participation varied considerably (Table IV.1). About 28 percent of all enrollees participated less than three months, and nearly a quarter participated for over a year. Differences across subgroups in average enrollment rates, duration of participation, and length of the follow-up period were generally quite small (Tables IV.1, B.5, and B.6).

---

<sup>2</sup>This statistic and all others in the rest of this chapter, except where noted, refer to Job Corps enrollees only. They do not include the 27 percent of program group members who never enrolled in the program.

Variations in the duration of participation in Job Corps resulted in some differences across participants in how much of the 48-month follow-up period was actually a postprogram period. However, most participants had been out of Job Corps for some time at the 48-month point. The average postprogram period for enrollees was 38 months (Table IV.1).<sup>3</sup> In addition, almost 67 percent of enrollees were out of Job Corps for more than three years, and nearly 92 percent were out for more than two years. Less than three percent of enrollees were out for less than one year. Thus, the 48-month employment and earnings results described in Chapter VI should be interpreted as medium-term impacts.

Rates of participation by quarter reveal patterns of participation over time that are useful for interpreting the impact findings. Figure IV.1 shows the fraction of program group members (including the no-shows) who participated in Job Corps during each quarter, measured as 13-week intervals starting from each sample member's date of random assignment.<sup>4</sup> (Table B.1 shows data by gender and age.) The participation rate declined from a peak of 67 percent in the first quarter after random assignment to 21 percent in the fifth quarter (beginning of the second year) and 3 percent in the tenth quarter. By the end of the 48-month period, almost all participants had left Job Corps. Only 0.3 percent of the program group (0.4 percent of enrollees) were in Job Corps in the final week of the 48-month follow-up period.

Based on these broad patterns of participation, we interpret the period from quarters 1 to 4 (months 1 to 12) as largely an "in-program" period. To be sure, some participants left Job Corps near the beginning of this period, and a few had not yet started their training by the end of it. Yet

---

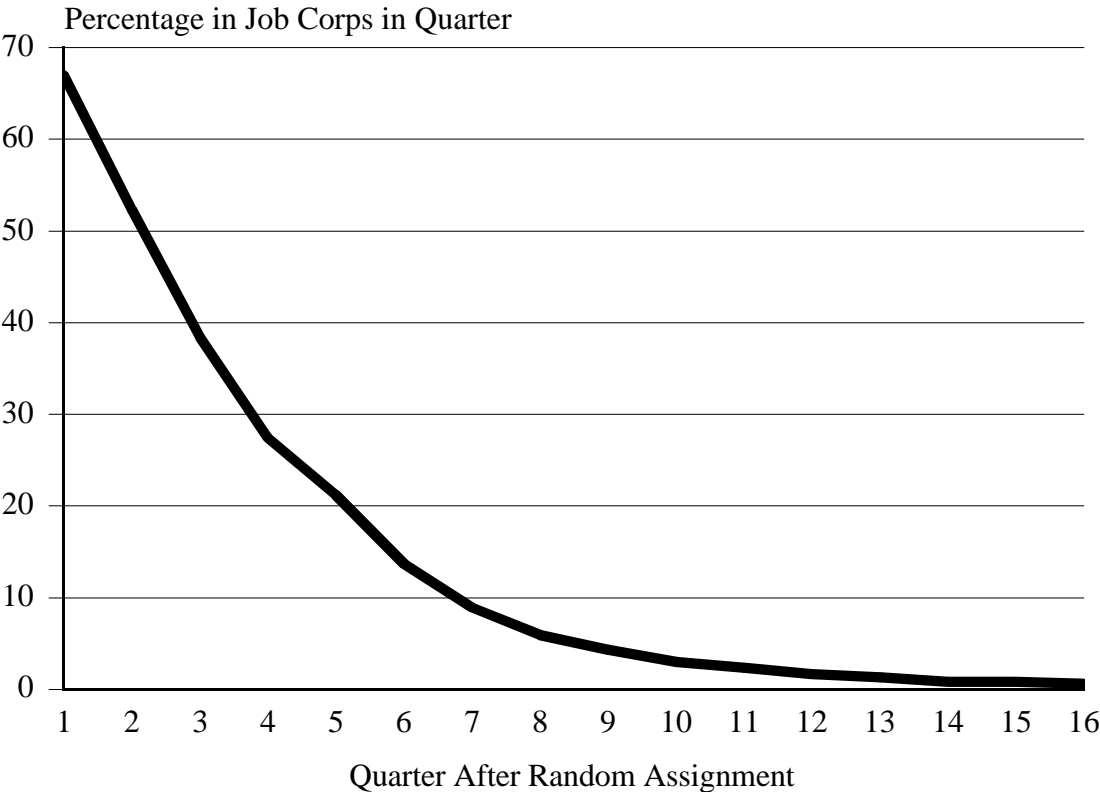
<sup>3</sup>The sum of months before, during, and after Job Corps do not add to 48 months exactly. This is because average length of stay does not include time spent in between spells in Job Corps, for those who left and reentered the program.

<sup>4</sup>Note that here, and throughout the report, quarterly statistics are based on 13-week periods beginning from each enrollee's date of random assignment and thus do not correspond to fixed calendar periods.



FIGURE IV.1

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



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

on average just less than half the sample were participating in each quarter. The period from quarters 5 to 8 (months 13 to 24) was a one of transition, in which smaller yet still substantial fractions of the program group were engaged in Job Corps training. The final two years were a postprogram period for most students, although, as noted, a small minority continued to participate in Job Corps. The use of these in-program, transition, and postprogram periods provides a framework for understanding the time profiles of employment and earnings and related impacts.

## **B. PARTICIPATION IN JOB CORPS ACADEMIC INSTRUCTION AND VOCATIONAL TRAINING**

As the program design intends, a large majority of Job Corps participants (77 percent) took both academic classes and vocational training (Table IV.2). Overall, more than 82 percent of enrollees reported taking academic classes, and nearly 89 percent received vocational training. These patterns are similar for males and females and for younger and older students. The average enrollee reported receiving 1,140 hours of academic and vocational instruction. The average number of weeks that an enrollee participated in academic classes or vocational training (or both) was about 31. A typical high school student receives approximately 1,080 hours of instruction during a school year. Thus, Job Corps provides approximately the equivalent classroom instruction of one year in school.

A few students took only academic classes (5 percent), and a few took only vocational training (12 percent). Most of these were students who participated in Job Corps for a short period, because all students eventually take vocational training and all eventually take a few required academic classes even if they already have a high school credential and solid basic skills. Some students who already had a high school credential and were able to concentrate on vocational training may not have remembered the few academic classes that they took or may not have considered

TABLE IV.2  
 COMBINED ACADEMIC AND VOCATIONAL TRAINING PARTICIPATION MEASURES  
 FOR PROGRAM GROUP ENROLLEES  
 (Percentages)

	Gender			Age			
	Total	All Males	All Females	Females with Children	16 to 17	18 to 19	20 to 24
<b>Participation in Activity</b>							
Took both academic and vocational	77.3	77.8	76.5	72.1	84.2	74.6	68.4
Took academic classes only	5.2	5.3	5.2	6.5	5.3	5.5	4.9
Took vocational training only	11.5	11.4	11.7	13	5.3	13.5	19.9
Took neither	6.0	5.6	6.6	8.5	5.2	6.5	6.8
<b>Total Hours in Academic Classes and Vocational Training</b>							
0	5.9	5.7	6.3	8.2	5.3	6.4	6.5
1 to 100	5.5	6.3	4.3	3.8	5.0	6.6	5.2
100 to 250	10.7	11.2	10	11.6	12.9	9.4	8.5
250 to 500	14.7	14.6	15	16.8	14.9	15	14.1
500 to 1,000	19.9	19.5	20.5	20.1	20.7	20.4	17.7
More than 1,000	43.2	42.7	44.0	39.4	41.2	42.1	48.0
(Average hours)	1,140.0	1,130.6	1,154.3	1,009.9	1,093.7	1,102.2	1,267.6
<b>Number of Weeks Took Academic Classes or Vocational Training</b>							
0	5.9	5.7	6.3	8.2	5.3	6.4	6.5
4 or less	7.2	8.3	5.6	4.6	7.3	8.1	5.9
5 to 13	20.5	21.1	19.6	23.1	23.1	19.4	17.1
13 to 26	19.3	19.0	19.8	18.9	20.0	19.1	18.5
26 to 39	16.9	15.8	18.5	19.5	16.5	17.9	16.2
39 to 52	12.1	12.0	12.4	11.0	11.1	12.4	13.6
52 to 78	11.8	11.9	11.7	10.3	11.6	10.5	13.7
More than 78	6.2	6.3	6.2	4.4	5.0	6.2	8.5
(Average weeks)	30.5	30.1	31.0	28.2	28.9	29.8	34.0
<b>Sample Size</b>	<b>4,925</b>	<b>2,799</b>	<b>2,126</b>	<b>637</b>	<b>2,132</b>	<b>1,518</b>	<b>1,275</b>

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

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

these to be academic classes.<sup>5</sup> A small fraction (six percent) did not participate in either academic or vocational training. These were students who left Job Corps before the end of orientation, which typically lasts two weeks.<sup>6</sup>

Job Corps enrollees received a substantial amount of academic instruction, averaging over 440 hours over 20 weeks (Table IV.3). Mathematics was the most common subject taken: 61 percent of all students said they took it. Just under half reported taking reading. Just over half of all students took GED or high school classes. Most other subjects asked about were reported by 14 to 26 percent of all students. Just three percent of students said they took ESL instruction.

A somewhat higher proportion of students reported taking vocational training (nearly 90 percent, Table IV.4) than reported taking academic instruction (82 percent, Table IV.3). Students also spent on average nearly 28 weeks in vocational training and received 700 hours of vocational instruction. The great amount of time spent in vocational training is consistent with Job Corps's practice of allowing students who enter with a high school credential and good basic skills to focus on vocational training while taking a few required academic classes (for example, health education, parenting, world of work).

Job Corps participants studied a variety of trades. The most popular categories were clerical and construction-related (about 22 percent each), followed by health (15 percent), food service (11 percent), welding (7 percent), and auto mechanics and repair (8 percent).

---

<sup>5</sup>Among students who reported only academic classes, nearly 30 percent reported participating in Job Corps for less than one month, and another 45 percent participated for one to three months. Among students who reported taking only vocational training, the distribution of length of stay was more like that for those who took both academic classes and vocational training.

<sup>6</sup>Nearly three-fourths of enrollees who reported taking neither vocational training nor academic classes were enrolled in Job Corps for less than one month.

TABLE IV.3  
ACADEMIC EXPERIENCES IN JOB CORPS  
FOR PROGRAM GROUP ENROLLEES  
(Percentages)

	Total	Gender			Age		
		All Males	All Females	Females with Children	16 to 17	18 to 19	20 to 24
Took Academic Classes	82.3	82.8	81.6	78.5	89.1	79.9	73.3
Total Hours in Academic Classes							
0	16.9	16.5	17.6	20.9	10.4	19.3	25.5
0 to 100	15.2	16.3	13.6	13.7	14.9	16.5	14.1
100 to 250	19.2	19.5	18.7	22.2	20.0	19.8	17.0
250 to 500	18.5	18.1	19.1	15.3	21.0	17.3	15.6
500 to 1,000	18.1	18.4	17.6	16.2	20.6	17.3	14.4
More than 1,000	12.2	11.3	13.5	11.7	13.2	9.7	13.4
(Average hours)	439.6	425.1	461.8	401.4	482.3	389.4	426.0
Number of Weeks Took Academic Classes							
0	17.2	16.8	17.7	20.9	10.4	19.6	26.2
4 or less	10.1	10.9	8.8	7.6	9.7	11.5	9.0
5 to 13	24.7	25.0	24.3	28.3	27.0	24.3	21.2
13 to 26	19.6	19.2	20.3	19.2	21.6	18.7	17.3
26 to 39	12.2	11.6	13.1	10.8	13.4	12.8	9.4
39 to 52	7.1	7.2	6.8	5.3	8.3	5.4	6.9
52 to 78	6.3	6.4	6.2	6.3	6.9	5.4	6.5
More than 78	2.8	2.9	2.8	1.5	2.7	2.4	3.6
(Average weeks)	20.0	20.0	20.1	17.7	21.9	18.1	19.1
Academic Subjects Taken							
Reading	45.8	46.7	44.4	41.8	51.9	42.1	39.6
Writing	26.2	26.0	26.5	22.8	27.2	24.5	26.5
English language skills	23.1	25.8	19.2	18.2	27.0	20.7	19.4
ESL	3.3	3.2	3.5	1.4	2.0	2.5	6.7
GED	48.1	49.6	46.0	44.8	58.7	46.1	32.1
High school	3.5	3.6	3.3	2.4	4.1	3.2	2.9
Mathematics	61.4	62.3	60.2	57.4	66.3	59.2	55.6
Science	13.6	15.5	10.8	7.1	18.2	11.9	7.7
Other	22.6	23.9	20.5	21.6	24.5	20.2	21.9
<b>Sample Size</b>	<b>4,925</b>	<b>2,799</b>	<b>2,126</b>	<b>637</b>	<b>2,132</b>	<b>1,518</b>	<b>1,275</b>

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

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

TABLE IV.4

VOCATIONAL TRAINING EXPERIENCES IN JOB CORPS FOR PROGRAM GROUP ENROLLEES  
(Percentages)

	Gender				Age		
	Total	All Males	All Females	Females with Children	16 to 17	18 to 19	20 to 24
Took Vocational Training	88.4	88.6	87.9	84.9	89.1	87.7	87.9
Total Hours in Vocational Training							
0	11.0	10.7	11.4	14.8	10.5	11.4	11.2
1 to 100	11.1	11.1	11.0	10.4	12.6	11.4	7.8
100 to 250	14.1	14.4	13.6	15.2	16.2	12.6	12.1
250 to 500	16.4	16.3	16.4	16.5	17.0	16.9	14.6
500 to 1,000	21.6	21.2	22.3	21.8	21.9	20.8	22.1
More than 1,000	25.9	26.2	25.5	21.4	21.7	26.9	32.2
(Average hours)	700.4	705.5	692.5	608.4	611.4	712.8	841.6
Number of Weeks Took Vocational Training							
0	11.0	10.7	11.4	14.8	10.5	11.4	11.2
4 or less	6.6	7.3	5.7	4.2	6.8	7.6	5.1
5 to 13	19.4	19.9	18.6	21.9	22.0	18.4	16.0
13 to 26	19.8	19.0	21.1	20.0	20.5	19.1	19.5
26 to 39	16.8	16.1	17.9	18.6	16.4	17.8	16.3
39 to 52	11.1	11.2	11.0	9.4	9.7	11.6	13.2
52 to 78	10.6	11.1	9.8	7.6	10.4	9.6	12.1
More than 78	4.7	4.8	4.7	3.6	3.7	4.5	6.7
(Average weeks)	27.5	27.7	27.3	24.4	26.0	27.1	30.8
Vocational Trades Taken							
Clerical	21.8	11.5	37.0	39.2	18.1	22.9	26.7
Health	15.0	5.8	28.5	28.5	14.3	14.4	16.8
Auto mechanics and repair, heavy equipment operator	7.5	11.0	2.2	1.5	8.8	5.6	7.4
Welding	7.1	10.1	2.6	1.7	8.2	6.0	6.4
Electrical	3.1	4.7	0.7	0.3	3.4	2.7	3.0
Other construction trades	21.3	30.2	8.0	5.1	25.6	20.0	15.4
Food service	10.8	10.1	11.9	8.6	13.1	10.4	7.3
Electronics	0.9	1.3	0.3	0.3	0.6	1.2	1.0
Other	21.7	25.1	16.7	13.9	20.4	23.3	22.2
Schedule of Classes							
Every week	56.5	51.2	64.5	64.8	48.5	60.1	66.3
Alternate weeks	41.7	46.9	33.9	34.2	50.3	38.0	31.2
Other	1.8	1.9	1.5	1.0	1.2	1.9	2.5
<b>Sample Size</b>	<b>4,925</b>	<b>2,799</b>	<b>2,126</b>	<b>637</b>	<b>2,132</b>	<b>1,518</b>	<b>1,275</b>

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

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

The most notable difference among subgroups is that the youngest students, nearly all of whom did not possess a high school diploma or GED at enrollment, were more likely than older students to say they took both academic classes and vocational training (Table IV.2). Moreover, the younger students reported more hours of academic classes than older students (482 compared with 389 and 426, Table IV.3) and fewer hours of vocational training (611 compared with 713 and 842, Table IV.4). Patterns similar to those of the younger students are also found for older students who enrolled in Job Corps without already holding a high school credential. These patterns of participation reflect the program's emphasis on improving academic skills and achieving a credential for students who come with poor skills, at the same time providing vocational training. Students who already have a high school credential and good skills are encouraged to concentrate on vocational training (though all must take a few key academic classes).<sup>7</sup> Also noteworthy is that, within each age and gender group, the experiences of students designated for residential slots and those designated for nonresidential slots were very similar (Table B.5).

### **C. STUDENTS' EXPERIENCES AND PERCEPTIONS OF SELECTED OTHER ACTIVITIES**

In addition to formal academic and vocational instruction, Job Corps offers a broad range of activities that are designed to promote health, life skills, and workplace success. While we did not gather detailed data on all domains of center experience, we did ask survey respondents about their experiences with selected activities beyond the core academic classroom instruction and vocational training.<sup>8</sup> Our primary purpose was to assess whether students participated in these activities and

---

<sup>7</sup>See Johnson et al. 1999.

<sup>8</sup>Data on these activities were not collected at the 48-month interview. Thus, results presented in this section pertain to those in the 48-month sample who completed 30-month interviews.

whether they thought the activities were useful. (Table IV.5 describes the activities.) Although we asked about academic classes and vocational training in both Job Corps and other programs, we did not ask about these other activities for programs other than Job Corps.

Most enrollees said they participated in most of the key activities we asked about. Figure IV.2 shows participation levels for each activity (Table B.2 shows data by gender and age). Almost 82 percent of enrollees reported having attended Progress/Performance Evaluation Panels (P/PEPs). Three-fourths said they took World of Work (WOW), SST, and health classes. Nearly two-thirds of enrollees reported taking cultural awareness and parenting classes. Just less than half of all enrollees took part in the drug and alcohol programs (AODA).

Job placement services was the one area in which well under half of enrollees said they received services (see also Table B.3). Only 40 percent said Job Corps center staff or placement contractor staff had helped them look for a job. This relatively low percentage is consistent with findings on placement services reported in the process report. Johnson et al. (1999) reported that placement contractor staff resources were spread very thin because placement counselors were supposed to serve all students leaving Job Corps for a period of six months. Placement contract managers estimated that their counselors spent half to three-fourths of their time trying to contact former students, many of whom are very mobile, difficult to find, and not interested in receiving placement assistance services. This left very little time for working directly with former students to help them find jobs.<sup>9</sup>

Of those students who reported receiving job placement assistance, just over 41 percent said they got a job as a result of the help they received (Table B.3). Thus, only about 16 percent of all

---

<sup>9</sup>Since the period of the study, Job Corps has changed the requirement to serve all terminees, thereby allowing placement contractors to focus efforts on fewer former students.



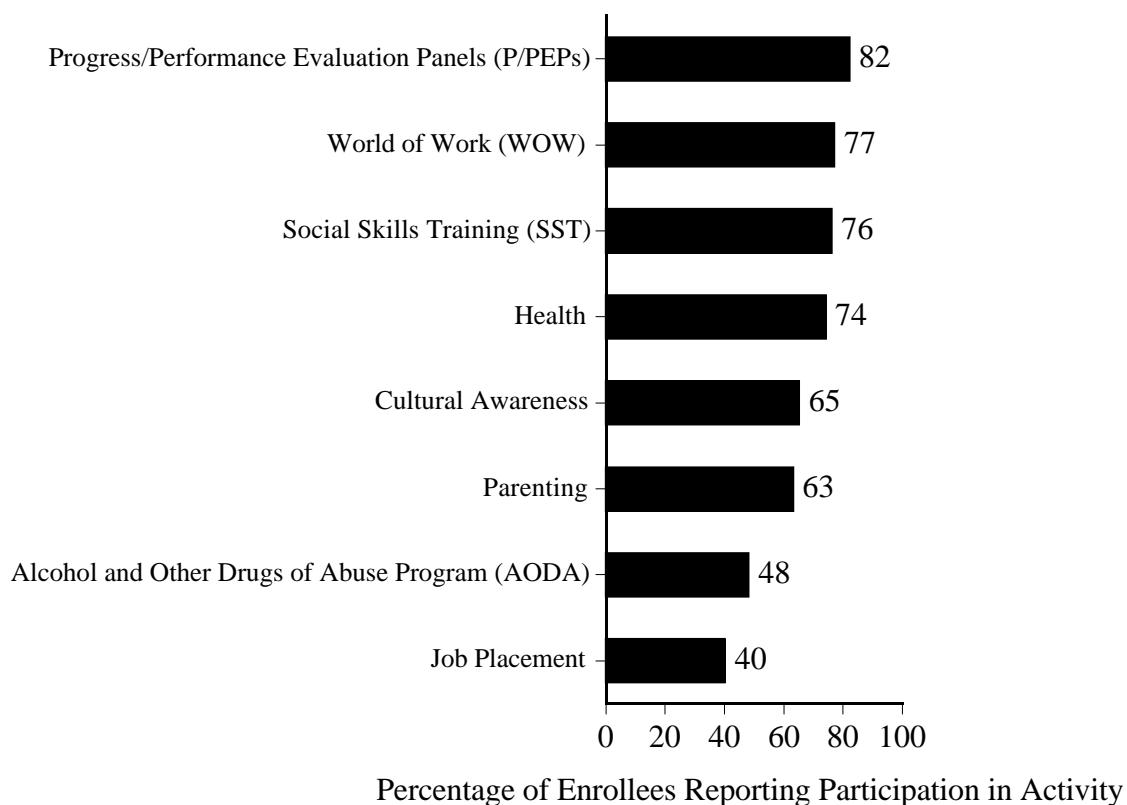
TABLE IV.5

## DESCRIPTION OF SELECTED JOB CORPS ACTIVITIES

Activity	Department Providing the Activity	Activity or Topics Covered
Progress/Performance Evaluation Panels (P/PEP)	Led by the student's counselor, each panel includes a residential living adviser, an education instructor, a vocational instructor, and the student	Meets 30 to 45 days after a student enrolls, and then every 60 days thereafter to review student progress and performance, based on ratings from staff who work with the student
World of Work (WOW)	Offered through the academic program	Introductory phase, taught shortly after entry, covers general skills for getting and keeping a job. Exit readiness phase, taught shortly before a student leaves, consists of three units: (1) preparation of a resume, cover letter, and job application; (2) job sources and interviewing; and (3) transition issues
Health Education	Offered through the academic department	Units on emotional and social well-being, human sexuality, sexually transmitted diseases, HIV/AIDS, nutrition, fitness, dental hygiene, consumer health, and safety
Alcohol and Other Drugs of Abuse Program (AODA)	A unit within Health Education, with specialized counselors	Covers the Job Corps ZT policy, anger control, building self-esteem, and other topics to teach students about decision making. Counselors work with students who test positive for drugs or alcohol upon entry and with others who request help
Cultural Awareness	Part of the Intergroup Relations Program offered through the academic department	Topics include living among different cultural groups, acceptance of differences, and discussion of languages, music, food, and art of specific cultural groups
Parenting	Offered through the academic department and required for all students	Covers essential parenting skills
Social Skills Training (SST)	Offered through the residential living department through small discussion groups led by a residential adviser	Curriculum has 50 lessons, addressing topics like being left out, honesty and accusation, giving and accepting criticism
Placement Assistance	Provided by placement assistance contractors	Assist student in finding a job or further education after returning home

FIGURE IV.2

OTHER ACTIVITIES IN JOB CORPS



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

Note: Questions on these activities in Job Corps were not asked in the 48-month interview. Thus, these figures pertain to those who completed 30-month interviews.

enrollees reported getting a job as a result of placement assistance. This information also appears to be broadly consistent with the administrative data information presented in the process report, which indicates that about half of reported “placements” are “self-placements.” (Students who found jobs on their own would be recorded as “placed” in the administrative data, although they might not have received help.)

In addition to measuring whether enrollees participated in the selected activities shown in Table IV.5, we asked students for their opinions about the usefulness of each activity (Table B.4). Specifically, the interview asked whether each activity helped “a lot,” “a little,” or “not at all.” While subjective, the measure does show whether students thought the activities were useful.

Of those who participated in each of the socialization activities, most stated that the activity was helpful. Each program activity was reported to have helped “a lot” by 56 to 61 percent of participants and “not at all” by only about 8 to 15 percent of participants. The remaining 26 to 34 percent (depending on the activity) said the program activity helped “a little.” Thus, for each activity, between 85 and 92 percent of students said the activity helped a little or a lot.

#### **D. CHILD CARE UTILIZATION**

About 30 percent of female program group members had children where they enrolled in Job Corps. Furthermore, most of these children were very young (about 85 percent were younger than three years old). Consequently, these mothers had to make child care arrangements to enroll in Job Corps. In fact, an eligibility requirement for Job Corps is that program applicants with children must demonstrate that they have an adequate child care plan for the proposed period of enrollment.

It is often difficult for young disadvantaged mothers to find appropriate child care, and child care is often found to be a significant barrier to attaining economic self-sufficiency for young mothers (Ross 1998). Finding suitable child care is especially challenging for residential females,

because they need to find a place where their children can live for a substantial period while they participate in the program. Not surprisingly, then, more than one-half of females with children are nonresidents who live at home. Because the recruitment of young mothers for Job Corps hinges on the ability of these mothers to obtain adequate child care, it is of policy interest to examine the child care arrangements used by those who enroll in the program.

In this section, we briefly discuss the child care arrangements used by mothers who enroll in Job Corps. We focus on mothers only, because although 11 percent of males in our sample had children at program application, only about 20 percent of these fathers lived with their children. Thus, only about 2.5 percent of males needed to find child care. The analysis uses information from the 12- and 30-month interviews on the main child care arrangement used by mothers for their youngest child. We present figures separately for the 374 nonresidential designees and the 242 residential designees because the child care needs differed for these two groups.

Not surprisingly, the most common child care arrangement for both residential and nonresidential designees was care by relatives (including the child's father, grandparents, or other relatives; Table IV.6). However, the child care arrangements for nonresidential designees were much more diverse than for residential designees. Among nonresidential designees, nearly one-half of children were cared for by relatives, about 35 percent were cared for in day care centers, and 12 percent were cared for by nonrelatives (about 60 percent of whom were paid). Among residential designees, however, virtually all (more than 85 percent) were cared for by relatives, most of whom were grandparents. Only about 5 percent of residential designees and 3 percent of nonresidential designees used Job Corps care, because child care programs were available only at 19 centers at the time that our sample was enrolled in Job Corps (Johnson et al. 1999).

TABLE IV.6

CHILD CARE ARRANGEMENTS USED BY FEMALES WITH CHILDREN  
WHILE THEY WERE ENROLLED IN JOB CORPS  
(Percentages)

Type of Child Care Arrangement	Nonresidential Designees	Residential Designees	Total
Relative	48.4	86.9	67.1
Child's father or stepfather	7.5	14.1	10.6
Child's grandparent	29.4	64.1	46.1
Other relative	11.5	8.7	10.4
Nonrelative	11.8	0.8	6.3
Paid	7.2	0.4	3.8
Unpaid	4.6	0.4	2.5
Day Care Center, Preschool, or Before- or After-School Program	34.8	4.6	19.9
Job Corps Child Care	3.2	5.4	4.5
Other	1.9	2.5	2.1
<b>Sample Size</b>	<b>374</b>	<b>242</b>	<b>616</b>

SOURCE: 12- and 30-month follow-up interview data for females in the program group who completed 30- and 48-month interviews and who had children while enrolled in Job Corps.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. The child care questions were not asked at the 48-month interview. Thus, the figures pertain to female participants in the analysis sample who completed 30-month interviews and who reported using child care while enrolled at Job Corps at the 12- or 30-month interviews.

## V. EDUCATION AND TRAINING

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

This chapter describes the education and training experiences of program and control group members and provides estimates of the impact of Job Corps on key education and training outcomes during the 48 months after random assignment. We examine education and training experiences of the *program group*, both in Job Corps and elsewhere, to provide a complete picture of the services they received. The education and training experiences of the *control group* are the “counterfactual” for the study. Although control group members were not permitted to enroll in Job Corps for three years after random assignment, they could enroll in all other programs available in their communities. The control group’s experiences are a benchmark that shows what education and training the program group would have engaged in had Job Corps not been available. The net increase in education and training due to Job Corps depends critically on what education and training the control group received and what education and training the program group received from other sources, as well as on the education and training the program group received in Job Corps.

This chapter addresses three primary questions:

1. What amount and types of education and training would Job Corps participants receive if they did not participate in Job Corps?
2. Do Job Corps participants receive more education and training than they would have received if they had not participated in Job Corps?
3. Does Job Corps influence educational attainment as measured by the receipt of a GED, vocational certificate, or college degree?

We addressed these questions using survey data on the education and training experiences of sample members during the 48-month follow-up period. For the analysis, we used information on dates of enrollment in education and training programs, the types of programs attended, time spent in academic classes and vocational training, degrees received, and the highest grade completed at the interview date. To compare education and training experiences of members of both the program and control groups, we considered Job Corps along with all other programs, such as English as a Second Language (ESL) and Adult Basic Education (ABE) programs, high school, GED programs, vocational and technical schools, and two-year and four-year colleges. The bulk of education and training for program group members who enrolled in Job Corps came from Job Corps itself, but some enrollees and many program group members who did not enroll in the program (that is, the no-shows) received other types of education and training.

Our analysis distinguishes between academic classroom instruction and vocational training. Academic instruction included classes at regular school or college, as well as classes taken in some other setting for the purpose of improving reading, writing, or mathematics skills; obtaining a GED or high school diploma; or learning English as a second language. Vocational training was for a specific job or occupation and might have been taken in any setting.

We analyzed academic classroom instruction and specific vocational training separately, because provision of both components is one hallmark of Job Corps. Thus, fully understanding Job Corps and the counterfactual against which Job Corps is measured requires describing not only the overall time spent in education and training, but also the time spent in its component parts: academic classes and vocational training.

Many control group members received substantial amounts of education and training. Nearly 72 percent participated in an education or training program during the 48 months after random assignment. On average, they received 853 hours of education and training, roughly equivalent to three-quarters of a year of high school. Participation rates were highest in programs that substitute for Job Corps: GED programs (37 percent), high school (32 percent), and vocational, technical, or trade schools (29 percent).

Job Corps substantially increased the education and training that program participants received, despite the activity of the control group. Nearly 93 percent of the program group engaged in some education or training, compared to about 72 percent of the control group (an impact of 21 percentage points per eligible applicant). The average program group member spent nearly twice as many hours in education and training as the average control group member (7.6 hours per week, compared to 4.1 hours per week). In total, the typical program group member received 1,581 hours of education and training, compared to 853 hours for the typical control group member. Over the 48-month period, Job Corps *participants* spent an average of 4.8 hours per week (998 hours in total) more in programs than they would have if they had not enrolled in the program. This impact per participant corresponds to roughly one school year.

The program group also spent significantly more time in academic classes, and even more in vocational training. Program group members spent an average of 3.1 hours per week (645 hours in



total) in academic classes, compared to 2.5 hours per week (520 hours) for the control group (an impact of 0.6 hours per week, or 125 hours in total). The program group typically received about three times more vocational training than the control group (3.1 hours per week, compared to 0.9 hours per week).

Job Corps increased the receipt of GED and vocational certificates but had small negative impacts on the attainment of a high school diploma. Among those without a high school credential at random assignment, about 42 percent of program group members (and 46 percent of program group participants) obtained a GED during the 48-month period, as compared to only 27 percent of control group members (an impact of 15 percentage points per eligible applicant). Similarly, about 38 percent of program group members (and 45 percent of Job Corps participants) reported receiving a vocational certificate, compared to about 15 percent of control group members (an impact of 22 percentage points). Among those without a credential at baseline, a slightly higher percentage of control group members obtained a high school diploma (7.5 percent, compared to 5.3 percent of program group members). Although many of the younger control group members attended high school, most of those in high school did not graduate, because they attended for an average of only about nine months.

At 48 months after random assignment, college attendance and completion had not been affected. About 12 percent of each research group attended a two-year college, and about 3 percent attended a four-year college. Less than 2 percent obtained a two- or four-year college degree.

Finally, impacts on education and training were large across all subgroups defined by youth characteristics. However, the pattern of impacts across age groups exhibited some differences. We find no impacts on hours in academic classes for those 16 and 17 at application to Job Corps, because nearly half of all control group members who were 16 and 17 attended academic classes in

high school. However, impacts on hours spent in academic classes were large for the older youths, and hours spent in vocational training were large across all age groups.

The rest of the chapter provides details on our findings. The first section presents impact estimates on participation and time spent in education and training programs, and on types of programs attended. This section also discusses impact findings on time spent in academic classes and vocational training. In the second section, we present impacts on educational attainment. Finally, we present impacts for key subgroups. Supplementary tables are included in Appendix C.

## **A. IMPACTS ON PARTICIPATION AND TIME SPENT IN EDUCATION AND TRAINING PROGRAMS**

This section compares the participation in education and training programs of the full program and control groups during the 48 months after random assignment. We expected that these impacts would be large during the period soon after random assignment, because many program group members were enrolled in Job Corps then. Job Corps might also increase participation during the postprogram period, because Job Corps encourages students to pursue additional training after finishing Job Corps and helps place them in such programs.

### **1. Impacts on Participation in Education and Training Programs**

Many control group members participated in education and training programs (Table V.1). Nearly 72 percent of the control group participated in a program at some point during the 48-month follow-up period. More than one-third (and about 47 percent of those in programs) attended more than one program. Interestingly, the control group participation rate declined only slightly over time. It was about 30 percent per quarter during the first five quarters (that is, 15 months) after random assignment and decreased to about 20 percent between quarters 8 and 16. These high participation rates are not surprising, because control group members demonstrated motivation to

TABLE V.1

## IMPACTS ON PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Program Group Job Corps Participants	Estimated Impact per Participant <sup>b</sup>	Percentage Gain from Participation <sup>c</sup>
Percentage Ever Enrolled in a Program During the 48 Months After Random Assignment	92.5	71.7	20.8***	100.0	28.9***	40.5
Number of Programs Ever Enrolled in (Percentages)						
0	7.6	27.9	-20.4*** <sup>d</sup>	-0.3	-28.3*** <sup>d</sup>	-101.0
1	42.0	38.3	3.7	41.6	5.1	13.9
2	33.4	24.8	8.6	37.7	11.9	46.1
3 or more	17.1	9.0	8.2	21.0	11.3	117.1
Average Number of Programs Ever Enrolled in	1.6	1.2	0.5***	1.8	0.7***	55.4
Percentage Enrolled in a Program, by Quarter After Random Assignment						
1	76.4	29.4	47.0***	95.0	65.3***	219.8
2	64.7	32.3	32.5***	79.5	45.1***	131.0
3	54.0	32.2	21.8***	64.4	30.2***	88.7
4	45.8	32.4	13.4***	52.4	18.6***	54.9
5	39.6	29.6	9.9***	44.0	13.8***	45.6
6	31.4	25.9	5.5***	33.6	7.6***	29.4
7	26.6	23.4	3.2***	27.9	4.5***	19.1
8	23.9	22.0	1.8**	24.3	2.5**	11.7
9	22.5	21.5	1.1	22.4	1.5	7.0
10	20.7	21.3	-0.6	20.3	-0.9	-4.0
11	20.9	20.6	0.4	20.5	0.5	2.5
12	18.8	19.2	-0.5	18.1	-0.7	-3.6
13	17.3	18.4	-1.1	16.4	-1.5	-8.4
14	16.4	17.8	-1.4*	15.6	-1.9*	-11.0
15	16.5	17.8	-1.3*	15.9	-1.9*	-10.5
16	17.2	17.1	0.1	16.5	0.2	1.0
Percentage Enrolled in a Program at 48 Months	13.0	12.9	0.1	12.6	0.1	1.0
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>4,925</b>		

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

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

<sup>a</sup> Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

<sup>b</sup> Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

<sup>c</sup> The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

<sup>d</sup> The significance levels pertain to statistical tests for differences in the distribution of the outcome measure 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.

obtain training by persisting with their Job Corps application to the point of being determined eligible. Thus, it is not surprising that they had the motivation to find other programs.<sup>1,2</sup>

Despite high control group participation rates, Job Corps substantially increased participation rates in education and training programs (Table V.1). Nearly 93 percent of program group members (and all program group members who enrolled in Job Corps) received some education or training during the four-year observation period, compared to 72 percent of control group members--an impact per eligible applicant of 21 percentage points. The impact per participant was 29 percentage points.

Consistent with this finding is that the typical program group member participated in more programs than the typical control group member (1.6 programs as compared to 1.2 programs). Even among those who participated in education and training programs, the program group participated in more programs. For example, among those who attended programs, about 55 percent of program group members enrolled in at least two programs, as compared to 47 percent of control group members. As discussed below, this is because about 60 percent of Job Corps participants enrolled in another education or training program during the 48-month period (including programs attended before and after they enrolled in Job Corps).

Figure V.1 plots quarterly participation rates in education and training programs by research status. The figure shows the percentage of program and control group members who ever

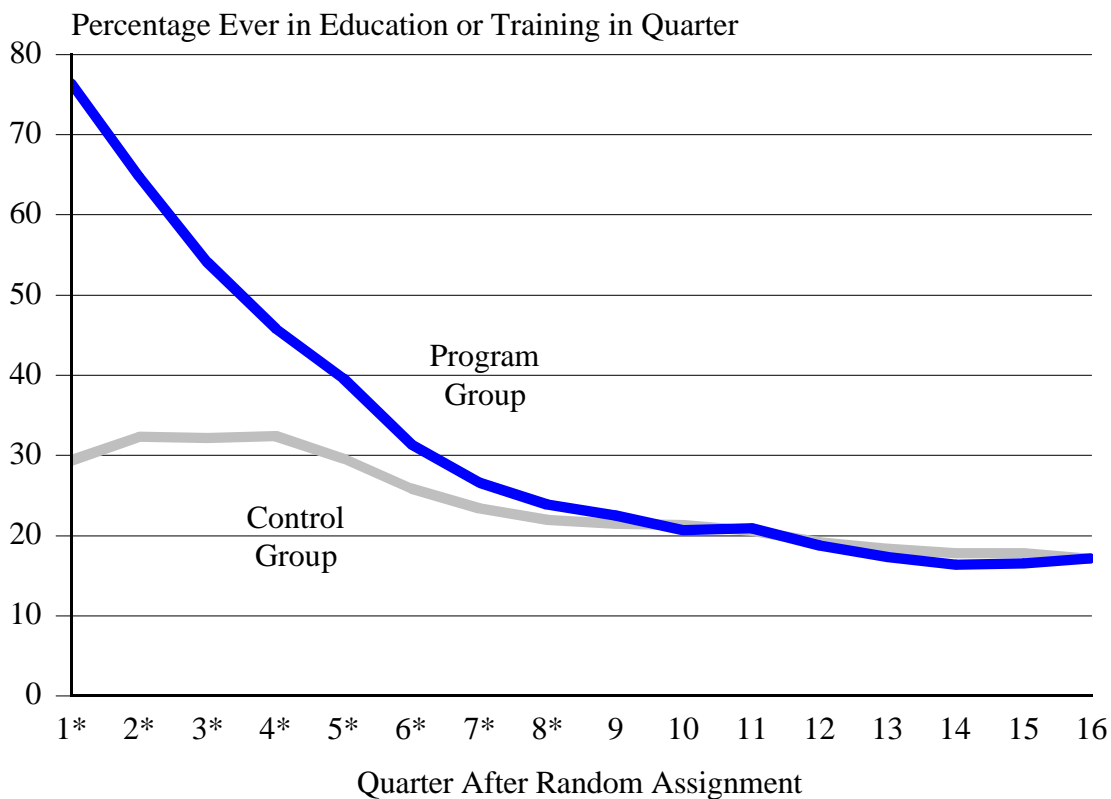
---

<sup>1</sup>This high rate of attending education and training programs, however, was not due to their exposure to Job Corps. Less than 2 percent of control group members who attended programs before the 12-month interview reported that their most important source of information about the program was the Job Corps OA counselor. Thus, most learned about these programs from other sources (the most common of which were friends, parents, school, and the media).

<sup>2</sup>These educational experiences pertain to eligible program *applicants*, and do not necessarily pertain to the broader population of youths who were eligible for Job Corps but who did not apply to the program.

FIGURE V.1

PARTICIPATION RATES IN EDUCATION AND TRAINING PROGRAMS,  
BY QUARTER



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

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

participated in an education or training program (including Job Corps) during each of the 16 quarters after random assignment. Differences in the program and control group participation rates are estimated impacts per eligible applicant. The statistical significance of these quarterly impacts is denoted by asterisks along the horizontal axis.

The impacts on participation in education-related programs were concentrated in the first six quarters (that is, 18 months) after random assignment. Impacts were large during this period, because many program group members were enrolled in Job Corps then. The quarterly impacts, however, decreased as program group members started leaving Job Corps, and these impacts were not statistically significant at the 5 percent significance level after quarter 8 (that is, after year 2). The impact per eligible applicant was 47 percentage points in quarter 1 and decreased to 22 percentage points in quarter 3 and 10 percentage points in quarter 5. The impact was about 3 percentage points in quarter 7 and near zero in quarters 9 to 16, although enrollment rates were slightly higher for control group members during this period. About 13 percent of both research groups were enrolled in a program during the last week of the 48-month follow-up period.

The finding that similar percentages of program and control group members were enrolled in programs during the postprogram period is important, because it suggests that impacts on employment and earnings during the last two years of the 48-month period were not affected by differences in school enrollment rates by research status.

## **2. Impacts on Time Spent in Education and Training Programs**

We report two period-specific measures of time spent in education and training programs: (1) proportion of weeks spent in programs, and (2) hours per week spent in programs. The measures were constructed by dividing the total weeks (or hours) spent in programs during the period by the

number of weeks in the period. The measures were set to zero for those who did not participate in education or training programs during the period.

Consistent with the participation findings, impacts on time spent in education and training were positive and large (Table V.2). Program group members spent an average of 24 percent of weeks in programs, compared to 18 percent of weeks for control group members (an impact of 6 percentage points per eligible applicant). Similarly, program group members spent nearly twice as many hours in programs (an average of 7.6 hours per week, as compared to an average of 4.1 hours per week for the control group). Over the entire 48-month (208-week) period, program group members received an average of 1,581 hours of education and training, whereas control group members received an average of 853 hours. Job Corps *participants* spent about 4.8 hours per week (998 hours in total) more in programs than they would have if they had not enrolled in Job Corps. This impact per participant corresponds to roughly one school year. The impact on hours was larger proportionately than the impact on weeks, because Job Corps involves more hours per week than most alternative education and training programs.

Not surprisingly, the time profile of the quarterly impacts on hours per week in programs closely resembles that of the impacts on program participation rates (Table V.2 and Figure V.2). Impacts were largest during the period when many program group members were enrolled in Job Corps, and these impacts decreased as they left the program. Impacts were not statistically significant after quarter 10.

### **3. Impacts on the Types of Programs Attended**

Control group members were not permitted to enroll in Job Corps for three years after random assignment. However, many did enroll in other education and training programs in their communities. Therefore, Job Corps opportunities offered to eligible applicants probably reduce their

TABLE V.2

## IMPACTS ON TIME SPENT IN EDUCATION AND TRAINING PROGRAMS

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Program Group Job Corps Participants	Estimated Impact per Participant <sup>b</sup>	Percentage Gain from Participation <sup>c</sup>
Percentage of Weeks in Education or Training During the 48 Months After Random Assignment (Percentage)						
0	8.5	30.2	-21.7*** <sup>d</sup>	0.1	-30.2*** <sup>d</sup>	-99.5
0 to 25	52.8	42.1	10.7	55.8	14.8	36.1
25 to 50	26.1	18.3	7.8	30.4	10.8	55.1
50 to 75	9.4	6.4	3.0	10.3	4.1	67.8
75 to 100	3.3	3.0	0.3	3.4	0.4	14.6
Average Percentage of Weeks Ever in Education or Training	24.4	18.2	6.3***	27.1	8.7***	47.4
Hours per Week Ever in Education or Training (Percentage)						
0	8.6	30.4	-21.8*** <sup>d</sup>	0.2	-30.3*** <sup>d</sup>	-99.4
0 to 5	35.8	41.1	-5.3	32.4	-7.4	-18.5
5 to 10	26.7	15.0	11.7	32.0	16.2	103.1
10 to 15	15.5	7.7	7.9	19.3	10.9	130.9
More than 15	13.4	5.9	7.5	16.2	10.5	184.9
Average Hours per Week Ever in Education or Training	7.6	4.1	3.5***	8.9	4.8***	117.0
Average Hours per Week in Education or Training, by Quarter						
1	20.9	5.5	15.4***	26.9	21.4***	392.7
2	20.4	6.3	14.1***	26.3	19.6***	291.4
3	16.2	6.4	9.9***	20.4	13.7***	204
4	12.1	5.9	6.2***	14.7	8.6***	138.9
5	9.6	5.4	4.2***	11.3	5.8***	104.9
6	7.4	4.8	2.6***	8.5	3.7***	76.4
7	5.8	4.3	1.6***	6.5	2.2***	50.6
8	5.0	3.9	1.2***	5.4	1.6***	42.3
9	4.3	3.6	0.7***	4.4	0.9***	26.9
10	3.7	3.3	0.5***	3.8	0.6***	19.9
11	3.6	3.3	0.3	3.6	0.4	11.4
12	3.2	3.2	0.0	3.2	0.0	1.4
13	2.9	3.0	-0.2	2.8	-0.2	-7.1
14	2.6	2.8	-0.2	2.6	-0.3	-10.6
15	2.5	2.7	-0.2	2.4	-0.3	-10.2
16	2.5	2.6	-0.1	2.5	-0.1	-4.2
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>4,925</b>		

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

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

<sup>a</sup> Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

<sup>b</sup> Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

<sup>c</sup> The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

<sup>d</sup> The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

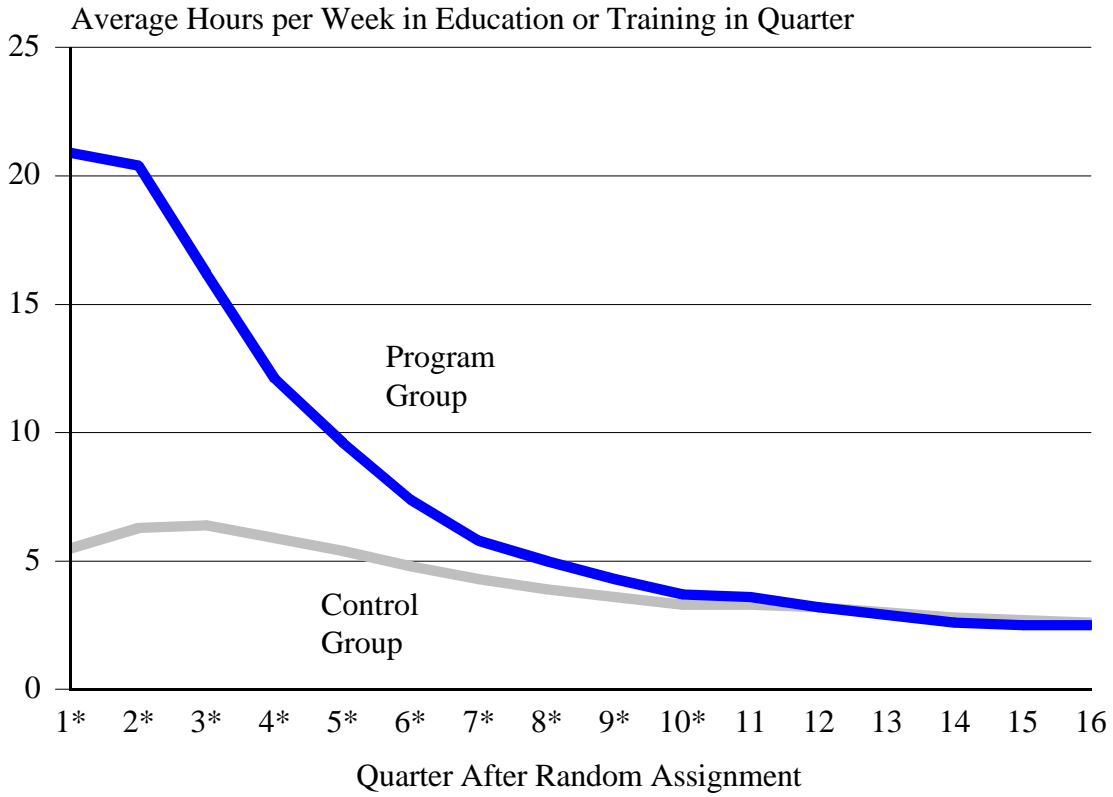
\*Significantly different from zero at the .10 level, two-tailed test.

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

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



FIGURE V.2  
 AVERAGE HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS,  
 BY QUARTER



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

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

participation in other programs that may substitute for Job Corps, such as high school, GED programs, and vocational and technical schools. It is very important to examine impacts on the time spent in these alternative programs, because the net costs of participation in these programs offset the costs of participation in Job Corps in the benefit-cost analysis (McConnell et al. 2001).

Figure V.3 displays data on participation of the program and control groups in several types of education and training programs. Table V.3 provides more details on the calculations.

As noted above, about 71 percent of the control group attended programs other than Job Corps.<sup>3</sup> Participation rates among the control group were highest for programs that could be considered close substitutes for Job Corps: GED programs (42 percent); high school (32 percent); vocational, technical, or trade schools (29 percent); and ESL or ABE classes (9 percent). Only small percentages of the control group attended two-year colleges (12 percent) or four-year colleges (3 percent). Most of those who enrolled in high school or GED programs did so early in the follow-up period (that is, within the first two years after random assignment). However, enrollment in vocational, technical, or trade schools and two-year and four-year colleges continued throughout the follow-up period.

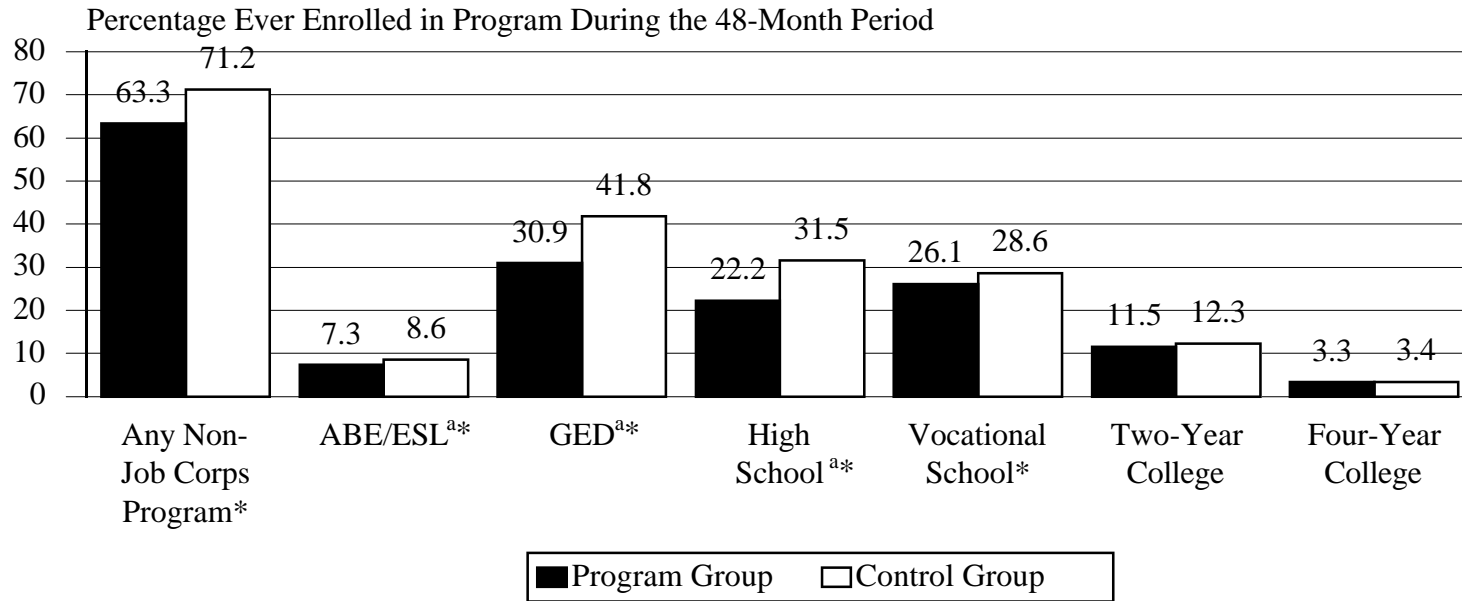
As expected, control group members were more likely than program group members to enroll in a program other than Job Corps during the 48-month period (71 percent as compared to 63 percent). The differences in participation rates in high school, GED programs, vocational schools, and ABE and ESL programs are statistically significant. There were no differences in enrollment rates in two- or four-year colleges.

---

<sup>3</sup>About 4.4 percent enrolled in Job Corps (1.2 percent before their three-year restriction period ended and the remainder afterwards).

FIGURE V.3

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS,  
BY TYPE OF PROGRAM



06

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

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

<sup>a</sup>Figures pertain to those who did not have a high school diploma or GED at random assignment.

TABLE V.3

IMPACTS ON PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS,  
BY TYPE OF PROGRAM

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Program Group Job Corps Participants	Estimated Impact per Participant <sup>b</sup>	Percentage Gain from Participation <sup>c</sup>
Types of Programs Ever Attended During the 48 Months After Random Assignment (Percentage)						
Job Corps	73.2	4.3	68.9***	100.0	95.8***	
Any program other than Job Corps	63.3	71.2	-7.9***	60.2	-11.0***	-15.5
ABE or ESL <sup>d</sup>	7.3	8.6	-1.3**	6.3	-1.8**	-21.9
GED <sup>d</sup>	30.9	41.8	-10.9***	26.5	-15.2***	-36.4
High school <sup>d</sup>	22.2	31.5	-9.3***	21.6	-12.9***	-37.3
Vocational, technical, or trade school	26.1	28.6	-2.5***	24.1	-3.5***	-12.7
Two-year college	11.5	12.3	-0.8	11.3	-1.1	-9.1
Four-year college	3.3	3.4	-0.1	3.1	-0.1	-4.0
Other	2.8	4.0	-1.2***	2.7	-1.7***	-38.9
Types of Program Attended During the 24 Months After Random Assignment (Percentage)						
Job Corps	72.7	1.2	71.5***	99.3	99.3***	
Any program other than Job Corps	48.9	59.7	-10.8***	45.7	-15.0***	-24.8
ABE or ESL <sup>d</sup>	5.1	6.3	-1.2***	4.2	-1.7***	-29.2
GED <sup>d</sup>	18.0	26.6	-8.6***	15.0	-11.9***	-44.3
High school <sup>d</sup>	18.5	26.7	-8.2***	17.9	-11.4***	-39.0
Vocational, technical, or trade school	15.0	17.5	-2.5***	13.5	-3.5***	-20.4
Two-year college	7.1	7.9	-0.8	6.7	-1.1	-14.6
Four-year college	1.6	1.4	0.1	1.3	0.1	13.1
Other	1.4	2.0	-0.6**	1.3	-0.8**	-38.4
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>4,925</b>		

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

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

<sup>a</sup>Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

<sup>c</sup>The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

<sup>d</sup>Figures pertain to sample members who did not have a high school credential at random assignment.

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

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

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

Impacts on time spent in alternative education and training programs follow similar patterns (Table C.1). However, the impact on time spent in alternative programs is proportionately larger than the impact on participation rates, because control group members who attended alternative programs did so for longer periods than their program group counterparts (Table C.2). For example, among those who attended high school, control group members were enrolled for an average of 40 weeks (approximately nine months) as compared to an average of 28 weeks for program group members.<sup>4</sup> Among those who enrolled in two-year colleges, the corresponding periods of enrollment were nearly 51 weeks for the control group and 46 weeks for the program group.

While impacts on participation in alternative programs are statistically significant, we were surprised at how small they were. Program group members made considerable use of these same programs, which increased impacts on education and training and reduced the offset to Job Corps program costs. To understand more fully the education and training experiences of the program group outside Job Corps, we tabulated enrollment rates in these programs for Job Corps participants before and after they enrolled in Job Corps, and for the no-shows (Table V.4).

About 15 percent of Job Corps participants attended an education program during the follow-up period *before* they enrolled in Job Corps (that is, between their random assignment and Job Corps enrollment dates). Not surprisingly, most of this activity was high school attendance. This finding is consistent with the fact that about one-quarter of eligible applicants in our sample were in school in the month prior to application to Job Corps (Schochet 1998a), and thus some were still enrolled at random assignment (that is, when they were determined to be eligible for the program).

---

<sup>4</sup>These figures were calculated using the results that control group attendees were enrolled for 19.4 percent of weeks during the 208-week period, compared to 13.5 percent of weeks for program group attendees.

TABLE V.4

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS OTHER THAN  
JOB CORPS FOR JOB CORPS PARTICIPANTS AND NO-SHOWS  
(Percentages)

Programs Ever Attended Other than Job Corps	Job Corps Participants		No-Shows
	Pre- enrollment	Post- enrollment	
Any Program	15.1	49.0	71.9
ABE/ESL <sup>a</sup>	1.7	4.6	8.5
GED <sup>a</sup>	2.5	23.1	37.3
High School <sup>a</sup>	12.7	9.1	20.9
Vocational, Technical, or Trade School	1.7	20.6	31.5
Two-Year College	0.3	10.1	12.1
Four-Year College	0.0	2.8	3.7
Other	0.2	2.4	3.0

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

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

<sup>a</sup>Figures pertain to sample members who did not have a high school credential at random assignment.

About one-half of Job Corps participants enrolled in an education or training program *after* leaving Job Corps.<sup>5</sup> Over 30 percent of Job Corps trainees attended GED programs (23 percent) or returned to high school (9 percent). This group is composed of students who went to Job Corps but did not obtain a high school credential and decided to go back to school in their home community. More than one-third enrolled in vocational or trade schools (21 percent), two-year colleges (10 percent), or four-year colleges (3 percent). While some of these students did not complete Job Corps, this pattern of participation is more consistent with first completing Job Corps and then seeking advanced training after termination.

Finally, many of the 27 percent of program group members who never participated in Job Corps (the no-shows) enrolled in other programs. About 72 percent enrolled in a program during the 48-month period. Interestingly, the pattern of participation in non-Job Corps programs for this group closely follows the pattern for control group members, although high school attendance was somewhat lower.

#### **4. Impacts on Participation in Academic Classes and Vocational Training**

On the basis of results discussed thus far, we might expect large impacts on time spent in academic classes and vocational training. Job Corps substantially increased time spent in education and training programs during the 48-month period, and most program group Job Corps enrollees participated extensively in the academic and vocational program components.

We also expect larger impacts on the amount of vocational training than on the amount of academic classroom instruction. A large percentage of control group members who attended education and training programs enrolled in high school and GED programs, which are academic

---

<sup>5</sup>Some youths reported being enrolled in programs outside Job Corps while also enrolled in Job Corps. These programs were excluded from Table V.4.

programs.<sup>6</sup> A smaller percentage enrolled in vocational programs. Thus, control group members were more likely to receive academic classroom instruction than vocational training, whereas program group members received significant amounts of both. Analysis of impacts on participation in academic instruction and vocational training confirmed these expectations.<sup>7</sup>

Program group members received substantially more academic classroom instruction than did control group members (Figure V.4 and Table V.5). About 81 percent of program group members (and 91 percent of Job Corps participants) ever took academic classes during the 48 months after random assignment, as compared to 57 percent of control group members (an impact of 24 percentage points per eligible applicant). Similarly, the impact per eligible applicant on hours per week in academic classes was 0.6 hours (an average of 3.1 hours for the program group and 2.5 hours for the control group). These figures translate to about 645 hours of academic classroom training for the typical program group member over the 48-month period and 520 hours for the typical control group member. Not surprisingly, impacts occurred primarily during the first 12 months after random assignment (the in-program period). Most of the academic instruction received by the program group took place in Job Corps, whereas most of the academic instruction received by the control group took place in high school, GED, and ABE programs (Table C.3).

---

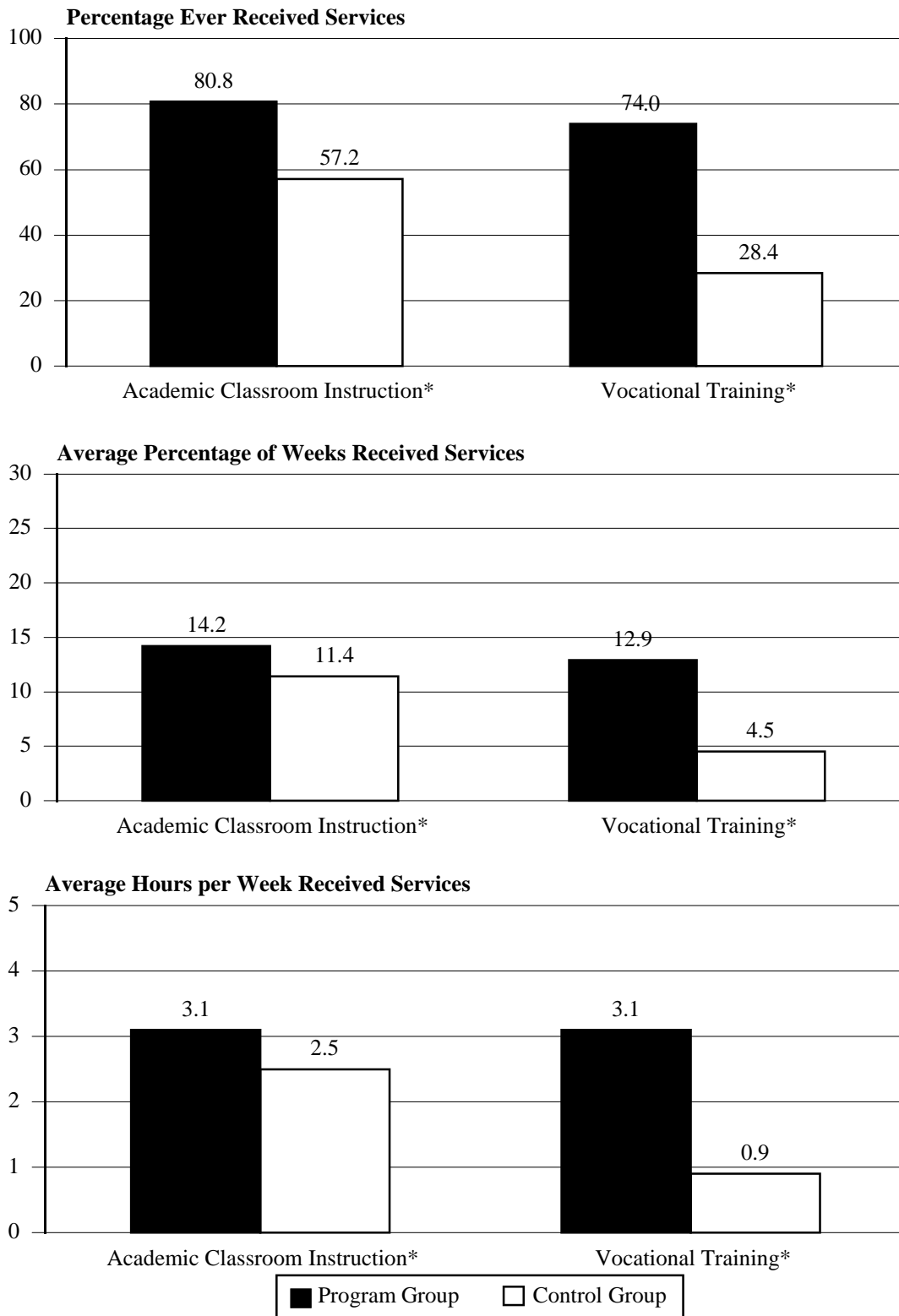
<sup>6</sup>Students who said they were attending a GED course were assumed to be in an academic program. Students who said they were attending high school were asked separately about academic and vocational instruction.

<sup>7</sup>The part of the 30-month follow-up questionnaire that collected information on academic and vocational training was changed in the middle of data collection to correct an error in the instrument's skip logic. Therefore, among those in the 48-month sample who completed 30-month interviews, results on vocational and academic training are based on a restricted sample consisting of those whose 30-month interview took place after April 1998, or about 45 percent of the full 30-month sample. Any differences between those interviewed early and later in the cycle are likely to be equally present, on average, in both program and control groups. The sample for this analysis also includes all those who completed a 48-month interview but not a 30-month interview. Thus, the impact estimates, though probably unbiased, may not be representative of the full sample.



FIGURE V.4

PARTICIPATION IN ACADEMIC CLASSES AND VOCATIONAL TRAINING DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT



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

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

TABLE V.5

## IMPACTS ON PARTICIPATION IN ACADEMIC CLASSES

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Program Group Job Corps Participants	Estimated Impact per Participant <sup>b</sup>	Percentage Gain from Participation <sup>c</sup>
Percentage Ever Took Academic Classes During the 48 Months After Random Assignment	80.8	57.2	23.7***	90.6	32.9***	57.0
Percentage Took Academic Classes, by Quarter After Random Assignment						
1	64.7	26.0	38.7***	81.0	53.7***	197.2
2	55.6	27.7	27.9***	69.1	38.7***	127.4
3	46.5	27.8	18.7***	56.1	26.0***	86.4
4	39.6	27.3	12.2***	45.7	17.0***	59.1
5	34.6	25.9	8.7***	39.1	12.1***	44.7
6	26.5	20.8	5.7***	29.0	7.9***	37.6
7	21.5	18.7	2.8***	23.0	3.9***	20.7
8	18.5	17.1	1.4*	19.2	1.9*	11.0
9	17.0	16.8	0.2	17.3	0.3	1.7
10	15.4	16.0	-0.6	15.3	-0.9	-5.5
11	13.1	12.4	0.7	13.0	1.0	8.0
12	7.1	6.5	0.6	7.2	0.8	12.4
13	5.6	5.2	0.3	5.8	0.4	8.0
14	4.7	4.8	-0.2	4.5	-0.2	-5.0
15	4.7	4.6	0.1	4.7	0.2	3.4
16	4.3	4.0	0.3	4.5	0.4	10.0
Average Percentage of Weeks in Academic Classes, by Year						
All years	14.2	11.4	2.7***	15.6	3.8***	32.6
1	30.3	19.4	11.0***	35.4	15.3***	75.6
2	16.5	16.0	0.5	17.2	0.7	4.1
3	8.7	8.7	0.0	8.4	0.0	-0.2
4	3.2	3.5	-0.3	3.0	-0.4	-12.1
Average Hours per Week in Academic Classes, by Year						
All years	3.1	2.5	0.6***	3.4	0.8***	31.2
1	6.8	4.9	1.9***	7.9	2.7***	51.2
2	3.4	3.2	0.2	3.6	0.3	9.8
3	1.6	1.6	0.1	1.6	0.1	6.5
4	0.5	0.6	-0.1	0.5	-0.1	-14.5
<b>Sample Size</b>	<b>3,378</b>	<b>2,346</b>	<b>5,724</b>	<b>2,410</b>		

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

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

<sup>a</sup>Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job

TABLE V.5 (continued)

---

Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

<sup>c</sup> The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

\*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.

Impacts on the amount of vocational training were larger (Figure V.4 and Table V.6). The percentage of program group members who received vocational training was nearly three times that for the control group (74 percent as compared to 28 percent). Furthermore, average hours per week in vocational training was more than three times higher for the program group (3.1 hours per week, compared to 0.9 hours per week for the control group). Program group members had an average of 645 hours of vocational training over the 48-month period, compared to 187 hours per control group member. Impacts were largest during the first year after random assignment, when many program group members were enrolled in Job Corps, although they were still positive and statistically significant during the second year and even the third year.

## **B. IMPACTS ON EDUCATIONAL ATTAINMENT**

Job Corps substantially increased the overall time youths devoted to education and training programs, as well as time devoted to academic instruction and vocational training. Did these increases in effort lead to gains in the attainment of GED certificates, vocational certificates, and college degrees or to gains in years of school completed?

Job Corps could affect attainment of a high school credential and a vocational certificate, because of both the additional time devoted to training and the emphasis placed on reaching these milestones. In all Job Corps centers, the academic department emphasizes helping students who do not have a high school credential at program entry to obtain a GED. About one-quarter of centers are also accredited to grant a high school diploma. Reflecting the importance that program managers attach to these goals, the Job Corps performance measurement system incorporates strong incentives promoting it. At the time program group members were enrolled, performance ratings of center operators depended directly on how many students earned a GED or diploma.

TABLE V.6

## IMPACTS ON PARTICIPATION IN VOCATIONAL TRAINING

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Program Group Job Corps Participants	Estimated Impact per Participant <sup>b</sup>	Percentage Gain from Participation <sup>c</sup>
Percentage Ever Received Vocational Training During the 48 Months After Random Assignment	74.0	28.4	45.6***	91.1	63.4***	229.0
Percentage Received Vocational Training, by Quarter After Random Assignment						
1	62.2	5.5	56.7***	82.9	78.8***	1,944.2
2	53.3	6.0	47.3***	71.0	65.7***	1,246.9
3	41.3	5.9	35.4***	54.6	49.2***	903.3
4	31.2	6.6	24.6***	40.6	34.1***	528.5
5	26.5	7.0	19.5***	33.4	27.1***	429.9
6	18.8	6.1	12.7***	23.1	17.6***	324.4
7	14.2	5.4	8.7***	16.8	12.1***	255.0
8	11.4	5.4	6.0***	13.2	8.3***	170.4
9	9.9	5.5	4.4***	11.1	6.2***	125.4
10	8.7	5.9	2.9***	9.4	4.0***	73.3
11	8.5	6.0	2.5***	9.0	3.4***	62.4
12	7.2	5.8	1.4***	7.6	1.9***	34.4
13	6.5	5.9	0.5	6.6	0.7	12.6
14	6.5	6.1	0.4	6.2	0.5	8.6
15	6.4	6.0	0.5	6.2	0.7	12.5
16	6.4	6.2	0.2	6.0	0.3	5.3
Average Percentage of Weeks Received Vocational Training, by Year						
All years	12.9	4.5	8.5***	16.1	11.8***	273.5
1	30.1	5.1	25.0***	39.6	34.7***	712.4
2	11.8	4.6	7.2***	14.4	10.1***	230.4
3	5.8	4.1	1.7***	6.3	2.3***	58.5
4	4.5	4.0	0.5	4.4	0.7	17.8
Average Hours per Week Received Vocational Training, by Year						
All years	3.1	0.9	2.2***	3.9	3.1***	355.4
1	7.3	1.0	6.4***	9.7	8.8***	1,019.1
2	2.9	1.0	1.8***	3.5	2.5***	265.4
3	1.3	0.9	0.4***	1.5	0.6***	67.3
4	1.0	0.8	0.2	1.0	0.2	32.7
<b>Sample Size</b>	<b>3,378</b>	<b>2,346</b>	<b>5,724</b>	<b>2,410</b>		

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

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

<sup>a</sup> Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

<sup>b</sup> Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

TABLE V.6 (continued)

---

<sup>c</sup>The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

\*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 defining feature of the Job Corps vocational education program is its emphasis on competency-based instruction. Each trade follows a prescribed plan of activities and has criterion-referenced measurements that are used to verify student competencies in each of the skills required of an entry-level position in an occupation. Students receive vocational certificates at various step-off levels. Currently, performance ratings depend on ensuring that students complete Job Corps and secure jobs or postprogram training. Obtaining a GED or completing vocational training are requisites for defining a student as a Job Corps completer.

It is unclear whether Job Corps is likely to affect attainment of a high school diploma. On the one hand, as noted, about one-quarter of Job Corps centers can grant state-recognized high school diplomas. On the other hand, the alternative to Job Corps includes a substantial amount of attendance in high school. Which effect is stronger is an empirical question.

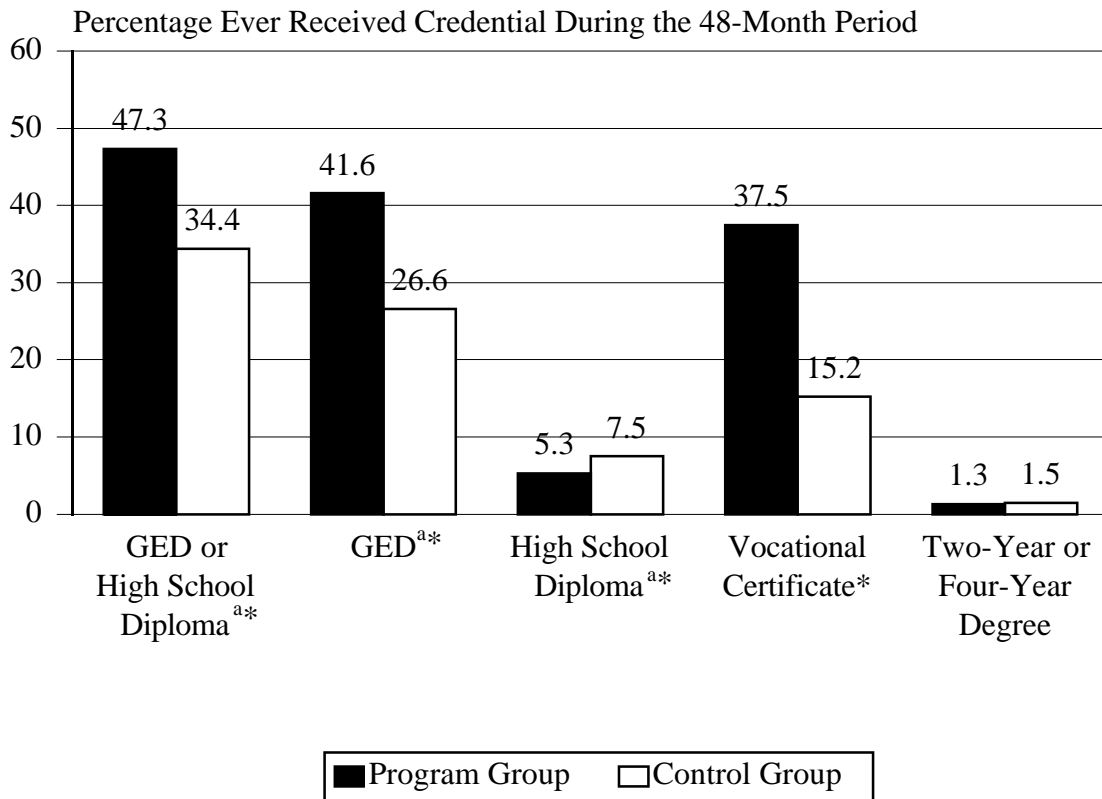
### **1. Impacts on the Attainment of a High School Credential**

Job Corps had a large positive impact on GED completion for the 80 percent of youths without a high school credential at random assignment (Figure V.5 and Table V.7). Of those who did not already have a high school credential, 42 percent of the program group and 27 percent of the control group received a GED, an impact of 15 percentage points per eligible applicant. About 46 percent of program group members who enrolled in Job Corps without a credential received a GED.

Few youths without a high school credential at random assignment obtained a high school diploma, although slightly more control group members did so (Figure V.5 and Table V.7). Among those without a credential at baseline, 7.5 percent of control group members obtained a high school diploma, as compared to 5.3 percent of program group members (a statistically significant impact of -2.2 percentage points per eligible applicant). As discussed, about 32 percent of dropouts in the

FIGURE V.5

DEGREES, DIPLOMAS, AND CERTIFICATES RECEIVED



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

<sup>a</sup>Figures pertain to those who did not have a high school credential at random assignment.

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



TABLE V.7  
IMPACTS ON EDUCATIONAL ATTAINMENT

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Program Group Job Corps Participants	Estimated Impact per Participant <sup>b</sup>	Percentage Gain from Participation <sup>c</sup>
Degrees, Diplomas, and Certificates Received During the 48 Months After Random Assignment (Percentage)						
GED certificate or high school diploma <sup>d</sup>	47.3	34.4	12.9***	51.4	18.0***	53.8
GED certificate <sup>d</sup>	41.6	26.6	15.0***	46.3	20.9***	82.3
High school diploma <sup>d</sup>	5.3	7.5	-2.2***	4.7	-3.1***	-40.1
Vocational, technical, or trade certificate	37.5	15.2	22.3***	45.1	30.9***	218.7
College degree (two-year or four-year)	1.3	1.5	-0.2	1.2	-0.3	-19.1
Highest Grade Completed at the 48-Month Interview						
Less than 9	6.7	5.9	0.8	7.0	1.1	18.9
9 to 11	58.9	59.5	-0.5	60.2	-0.7	-1.2
12	27.5	27.6	0.0	26.7	0.0	-0.2
Greater than 12	6.8	7.1	-0.2	6.1	-0.3	-4.9
Average Highest Grade Completed	10.7	10.8	0.0	10.7	0.0	-0.2
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>4,925</b>		

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

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

<sup>a</sup>Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

<sup>c</sup>The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

<sup>d</sup>Figures pertain to sample members who did not have a high school credential at random assignment.

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

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

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

control group enrolled in high school. Thus, just 23 percent of those who attended high school obtained a high school diploma. This low completion rate was due to the fact that students in high school attended for an average of only about nine months, while the average dropout had completed less than the 10th grade at the time of Job Corps enrollment.

Overall, program group dropouts were much more likely than control group dropouts to obtain a high school credential (either a GED certificate or a high school diploma) during the 48-month period (47 percent, compared to 34 percent). These impacts were large, because Job Corps slightly reduced the high school diploma completion rate but substantially increased the GED completion rate.

The rate of high school completion for the control group was similar to the rate for low-income dropouts based on data from the 1988 National Education Longitudinal Study (NELS). Among low-income 1988 eighth-graders who dropped out of high school at least once between 1988 and 1992, about 20 percent received a GED by 1994 (as compared to 27 percent of the control group), and about 13 percent obtained a high school diploma by 1994 (as compared to about 8 percent of the control group).<sup>8</sup>

The high school diploma and the GED are both meant to certify completion of a secondary school education. However, some have argued that a GED is worth less than a diploma in the labor market (Heckman and Cameron 1993; and Boesel et al. 1998), although the empirical evidence is mixed. Furthermore, it may be that a GED earned through a special program such as Job Corps is more valuable than one earned, for example, as a result of a narrowly focused test-preparation course. We examine the extent to which earnings impacts differed for those who completed a GED and those who did not in a separate report (Gritz et al. 2001).

---

<sup>8</sup>See Berkthold et al. 1998.

## **2. Impacts on the Attainment of a Vocational Certificate**

Job Corps had very large impacts on the attainment of a vocational certificate (Figure V.5 and Table V.7). The estimated impact was 22 percentage points (38 percent of the program group received a vocational certificate, compared to 15 percent of the control group), and is even larger than the GED impact.

The emphasis given to documenting progress and certifying vocational completion in Job Corps creates a need for caution in interpreting these large impacts. The unique structure of Job Corps may have made program group members more likely to receive a vocational certificate than control group members who achieved similar levels of competency in alternative vocational programs. Still, the impacts on vocational certification are in line with impacts on receipt of vocational training, which lends credence to the findings.

## **3. Impacts on the Attainment of a College Degree**

As discussed, only a small percentage of either the control group or the program group attended two-year or four-year colleges during the 48 months after random assignment. Thus, less than 2 percent of youth in both groups earned a two- or four-year college degree (Figure V.5 and Table V.7).

## **4. Impacts on Highest Grade Completed**

Because we find few differences by research status in the attainment of high school diplomas or college degrees, it is not surprising that we find no impact on years of formal schooling completed at the 48-month interview (Table V.7). The average highest grade completed was about 10.7 for both groups (as compared to 10.1 for both groups at random assignment), and the distributions of highest grade completed were nearly identical for the two groups. These results reflect the fact that youth who attended formal school did not remain there for very long.

These results suggest that Job Corps does not affect the educational attainment as measured by self-reported grade completion, which presumably includes only formal schooling and thus captures only one dimension of education. Those who participated in GED programs or other academic courses outside a regular high school were not likely to have reported a change in their highest grade completed, nor were those whose training activities were vocational.

Self-reports of highest grade completed are somewhat unreliable. This is evident in the many inconsistent responses given by the same person from one interview to the next, such as “highest” grade levels that went down over time. Indeed, researchers who study educational attainment have noted the presence of measurement error in this kind of report (Ashenfelter and Krueger 1994). We estimated impacts using a number of alternative measures of highest grade completed, including the maximum report and an “edited” version based on alternative rules for eliminating or recoding certain suspicious or inconsistent cases. The particular correction did affect the final attainment levels, but it had no effect on the finding that program and control group differences were negligible.

### **C. FINDINGS FOR SUBGROUPS**

This section presents data on the education and training experiences of key subgroups defined by youth characteristics at baseline. We focus our discussion on subgroups defined by age at application to Job Corps and high school credential status at random assignment. These subgroups are of particular interest because of substantial differences in their skill levels and educational needs at baseline.

In the rest of this section, we present evidence that for broad groups of youths served by Job Corps, the program had a very large effect on time spent in education and training and on the attainment of a GED (for those without a high school credential at baseline) and vocational certificate. First, we present findings for subgroups defined by age and high school credential status.

We examine the experiences of (1) those 16 and 17, (2) those 18 to 24 who did not have a high school credential, and (3) those 18 to 24 who had a high school credential. Nearly all those in our sample who were 16 and 17 years old did not have a high school credential, compared to 73 percent of those 18 and 19 and 50 percent of those 20 to 24. We combined the 18- and 19-year-old dropouts with the 20- to 24-year-old dropouts, because the education and training experiences and impact findings were very similar for these groups. For similar reasons, we also combined the two older groups with a high school credential. Then, we briefly present findings on key outcomes for other youth subgroups defined by gender, residential designation status, arrest history, race, and ethnicity, and date of application to Job Corps. We present findings using a series of figures and charts. Tables C.4 to C.6 present more details.

## **1. Impacts by Age and High School Credential Status**

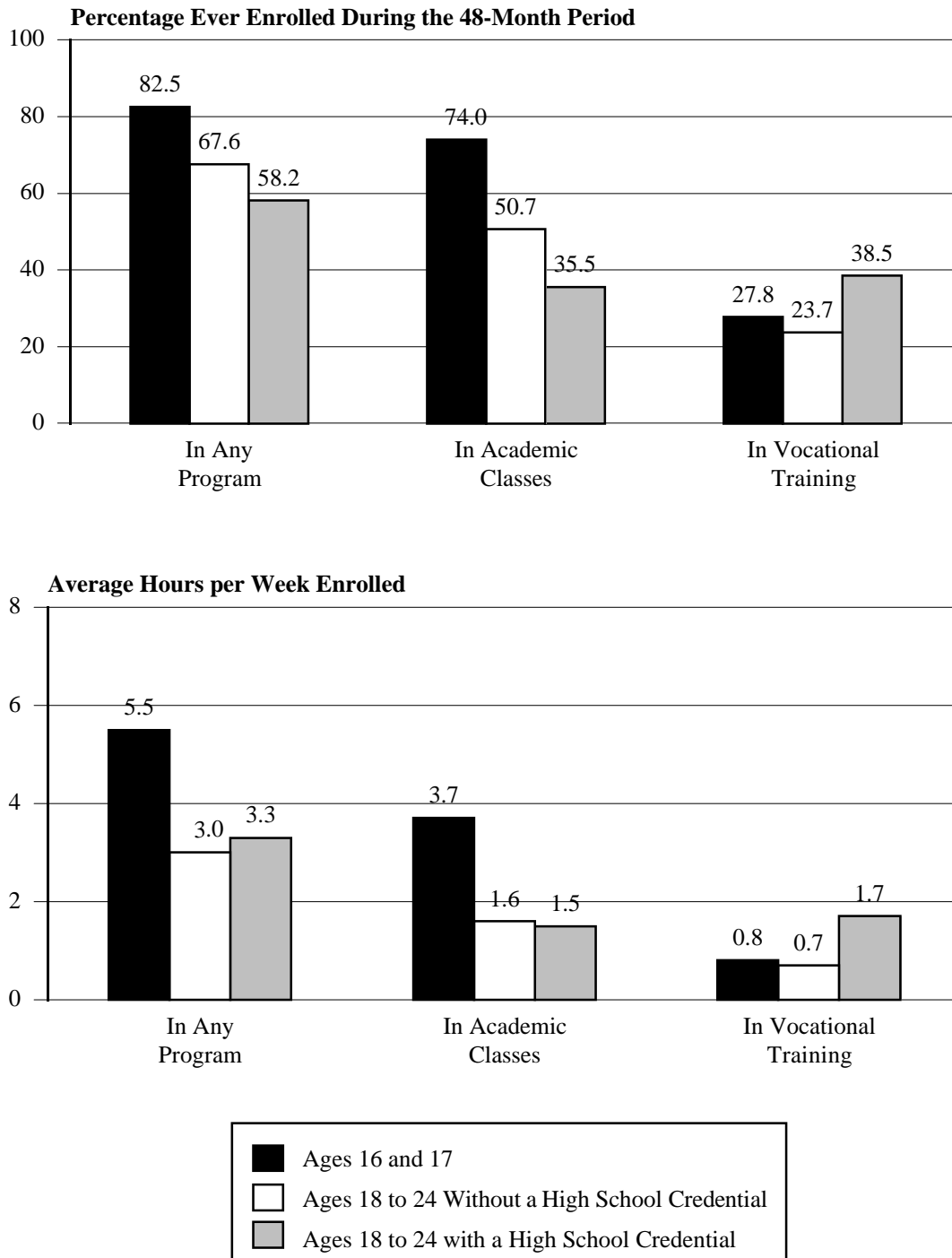
Our impact findings for subgroups defined by age and educational level at baseline were largely due to subgroup differences in the experiences of control group members. Program group experiences varied less because, as discussed in Chapter IV, all subgroups of participants received substantial amounts of education and training in Job Corps. We first discuss the control group experiences, then the impact findings.

### **a. Control Group Experiences**

Among the control group, levels of participation in education and training programs were higher for those 16 and 17 than for the older youth (Figure V.6). About 83 percent of those 16 and 17 ever enrolled in a program during the 48-month period, compared to 68 percent of the older youth without a high school credential at baseline and 58 percent of the older graduates. Similarly, the youngest control group members spent an average of 5.5 hours per week (1,144 hours during the 48-month

FIGURE V.6

PARTICIPATION AND HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS FOR CONTROL GROUP MEMBERS, BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE



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

period) in programs, whereas the older groups spent only about 3.2 hours per week in programs (about 666 hours in total).

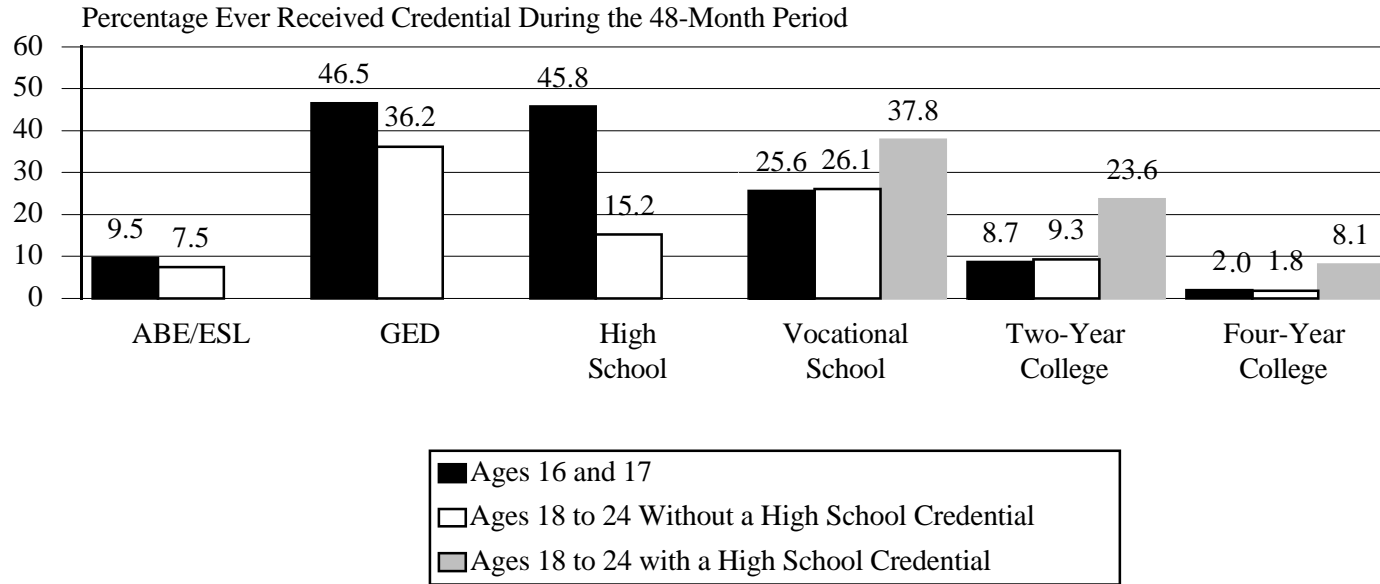
The time profile of participation in programs also differed for the younger and older control group members, although similar percentages were in programs late in the observation period (Tables C.4 to C.6). About 45 percent of the 16- and 17-year-olds were enrolled in programs during each of the first five quarters after random assignment, but the participation rate dipped to about 30 percent in quarter 7 and about 20 percent after quarter 10. The participation rate for the older groups, however, remained constant at about 20 percent per quarter throughout the follow-up period. Importantly, the control group participation rates were about 20 percent for all age groups during the postprogram period, so the earnings impacts by age were not differentially affected by differences in school enrollment rates.

The younger control group members spent more time in programs than the older ones, because they spent much more time in academic classes--but not in vocational training (Figure V.6). The typical 16- and 17-year-old control group member spent 3.7 hours per week in academic classes but only 0.8 hours per week in vocational training (so that more than 80 percent of total hours spent in programs were spent in academic classes). On the other hand, the older high school completers spent more than double the hours in vocational training than the younger group, but spent substantially fewer hours in academic classes.

These findings reflect the types of programs that control group members attended (Figure V.7). Many 16- and 17-year-olds attended academic programs, but fewer went to vocational programs. About half of these youth attended high school, and about half attended GED programs. About one-

FIGURE V.7

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS FOR CONTROL GROUP MEMBERS,  
BY TYPE OF PROGRAM, AGE, AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE



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



quarter attended vocational and technical schools, and about 9 percent enrolled in two-year colleges. Because most of the schooling for this group took place in high school and GED programs, it is not surprising that the youngest control group members received large amounts of academic classroom instruction and smaller amounts of vocational training.

In contrast, the older graduates tended to enroll in programs that offer vocational training: nearly 40 percent enrolled in vocational schools, and nearly one-quarter enrolled in two-year colleges. Thus, these youth received more vocational training than their counterparts. Participation rates among the older dropouts were largest in GED programs (about 36 percent) and vocational programs (about 26 percent); only about 15 percent enrolled in high school.

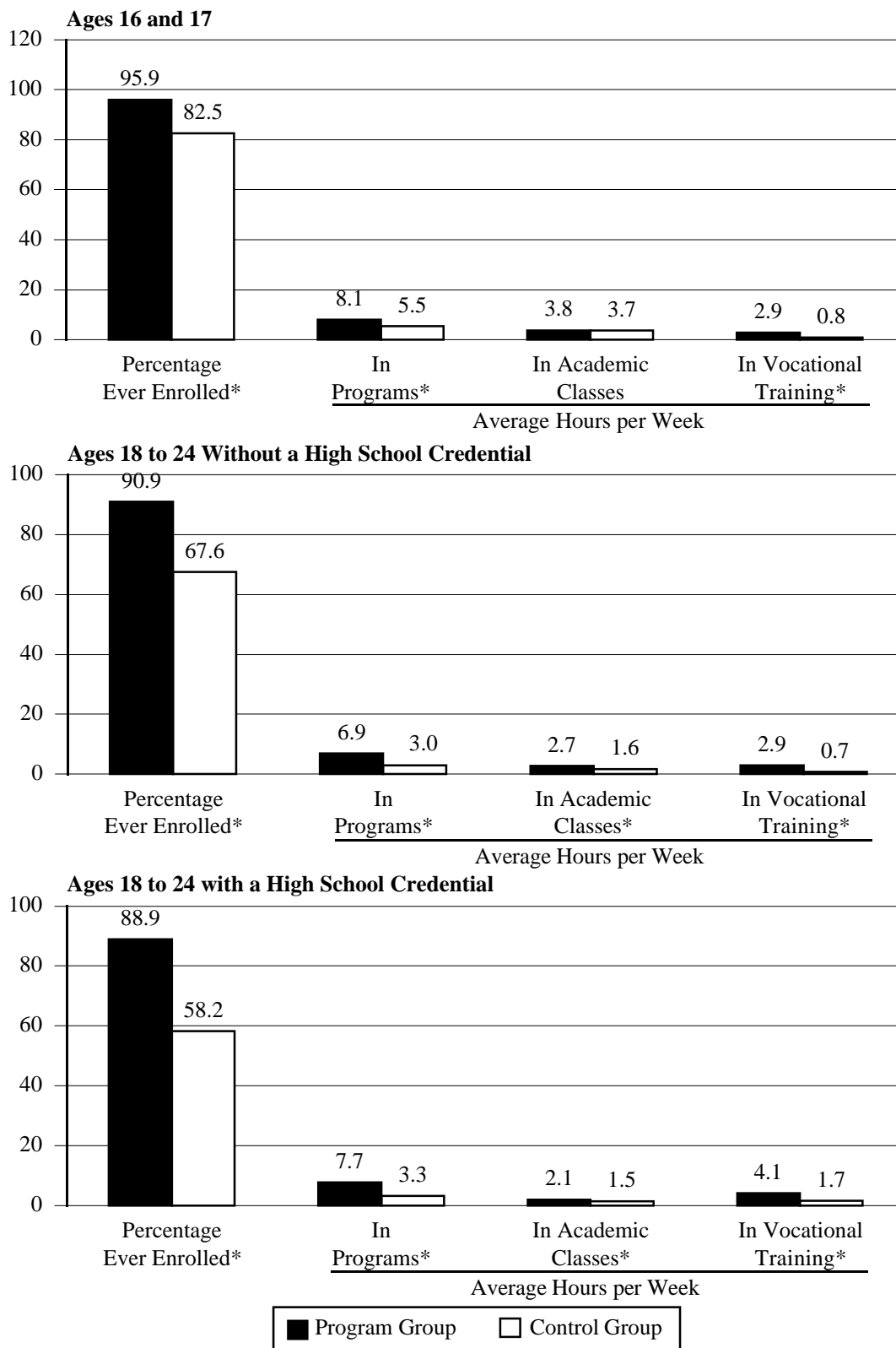
#### **b. Impact Findings**

The impacts on overall measures of participation in education and training programs were very large for each subgroup (Figure V.8). However, they were somewhat smaller for the 16- and 17-year-olds because of high control group participation rates for this group. The impact per eligible applicant on hours per week spent in programs was about 2.6 hours per week (541 hours in total) for the youngest group and about 4 hours per week (832 hours in total) for the two older groups.

Impacts on time spent in academic classroom training were large and statistically significant for the older youth, but not for those 16 and 17 (Figure V.8). We find no impacts on time spent in academic classes for those 16 and 17, because many control group members in this group received intensive academic classroom instruction in high school and in GED programs. However, we find large positive impacts on the receipt of academic services for the two older groups, because the older control group members were less likely to participate in academic-intensive programs, whereas the older Job Corps participants in the program group received some academic instruction in Job Corps.

FIGURE V.8

PARTICIPATION AND HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS,  
BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE



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

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

Impacts on time spent in vocational training, however, were very large and positive for each subgroup. Program group members typically received about three times more hours of vocational training than control group members.

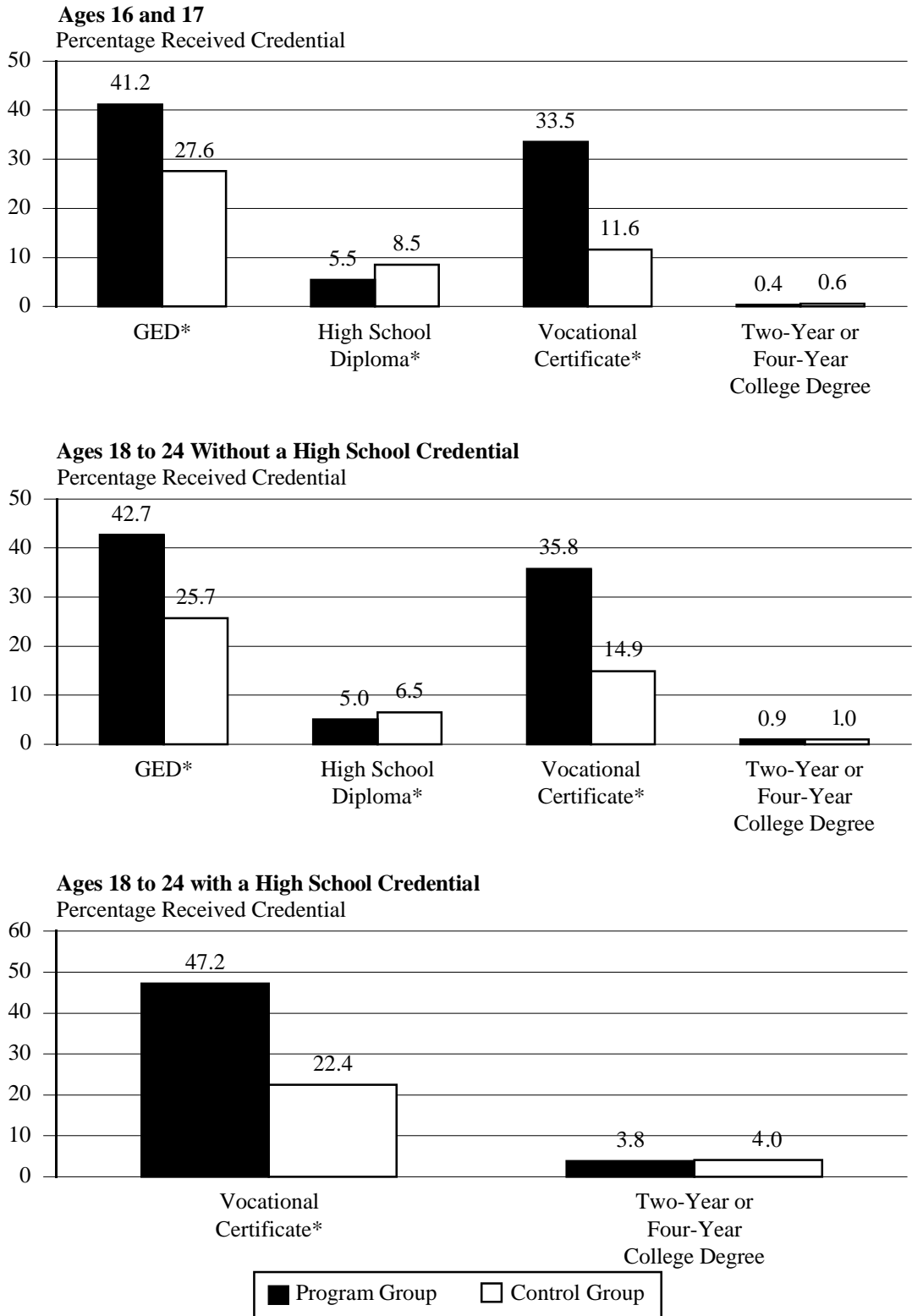
Finally, for all age groups, we find large impacts on the receipt of certificates emphasized by Job Corps, but small differences by research status on the attainment of a high school diploma or college degree (Figure V.9). Impacts on the receipt of a GED were similarly large for both the younger and older dropouts. Although there were no impacts on time spent in academics for those 16 and 17, we find large impacts on the attainment of a GED for this group, because of the emphasis that Job Corps places on it. Impacts on the receipt of a high school diploma were negative, but small, for both dropout groups, because of the low rates of high school completion among the control group (only about 7.5 percent of all control group dropouts attained a diploma). Impacts on the receipt of a vocational certificate were also very large for all groups. Finally, at 48 months, Job Corps had no effect on the receipt of a two-year or four-year college degree for those who had a high school credential at baseline.

## **2. Impacts for Other Key Subgroups**

Table C.7 presents impact results on selected education-related outcomes for each of the following subgroups: gender, residential designation status by gender, arrest history, race and ethnicity, and application date (whether before or after ZT policies took effect). Average control group measures and impacts on these outcome measures were remarkably similar across the subgroups. Thus, Job Corps leads to large increases in participation in education and training programs and in educational attainment across diverse groups of youths served by the program.

FIGURE V.9

EDUCATIONAL ATTAINMENT, BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE



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

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

Of particular note, we find similar impacts for those assigned to the residential and nonresidential component. This is consistent with our finding from the process analysis that nonresidential students are fully integrated into the academic and vocational components of Job Corps.

## VI. EMPLOYMENT AND EARNINGS

Chapter V showed that Job Corps participation leads to large impacts on time spent in academic classes and vocational training and on the attainment of GED and vocational certificates. In addition, Job Corps leads to increases in participants' functional literacy skills (Glazerman et al. 2000). Thus, Job Corps could increase participants' labor market productivity, which may in turn enhance their time spent employed, earnings, wage rates, and fringe benefits.

We expect negative impacts on participants' employment and earnings during the period of enrollment, because some participants would have held jobs if they had not gone to Job Corps. However, because of improvements in participants' skills, we expect positive impacts on employment and earnings after participants leave the program and after a period of readjustment. In light of the variation in the duration of program participation and the period of readjustment, it is difficult to predict when positive impacts are likely to emerge. Thus, we cannot predict in which month after random assignment the earnings of the program group were likely to have exceeded those of the control group.

This chapter presents program impacts on employment and earnings. It presents impacts for the full sample and for key subgroups during the 48 months after each youth was found eligible for Job Corps.

We find that Job Corps generated positive employment and earnings impacts beginning in the third year after random assignment, and that the impacts persisted through the end of the 48-month follow-up period. The employment and earnings of the control group were larger than those of the program group early in the follow-up period, because many program group members were enrolled in Job Corps then. It took about two years from random assignment for the earnings of the program

group to overtake those of the control group. The impacts grew between quarters 8 and 12, and then remained fairly constant from quarters 13 to 16 (that is, they *persisted* in year 4). In year 4, average weekly earnings for program group members were \$16 higher than for control group members (\$211, compared to \$195). The estimated impact per Job Corps *participant* was \$22 per week (or \$1,150 in total during year 4), which translates into a 12 percent gain in average weekly earnings due to program participation. These year 4 impacts are statistically significant at the 1 percent level.

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

Job Corps also had positive effects on the employment rate and time spent employed beginning in year 3. As expected, the impacts on the employment measures were negative during the in-program period. They became positive in quarter 8, increased sharply between quarters 8 and 12, and remained fairly constant afterwards. In year 4, the average quarterly impact on the employment rate was about 3 percentage points per eligible applicant (69 percent for the program group, compared to 66 percent for the control group). The year 4 impact on hours employed per week was 1.4 hours per eligible applicant (27.4 hours for the program group, compared to 26 hours for the control group). This translates to an impact of nearly 2 hours per participant, or an 8 percent gain due to program participation. The year 4 impact per eligible applicant on the percentage of weeks employed was about 3 percentage points (60 percent, compared to 57 percent). These impact estimates are all statistically significant at the 1 percent level.

The earnings gains late in the period were due to a combination of greater hours of work and higher earnings per hour. We estimate that program group members earned about \$11 more per week in year 4 than control group members because they worked more hours, and that they earned

about \$5 more per week because they had higher earnings per hour. These gains sum to the \$16 impact on earnings per week in year 4.

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

Positive impacts in the postprogram period were found broadly across most key subgroups of students. Beneficial program impacts were found for males and females, younger and older students, those with and without a high school credential at random assignment, and whites and African Americans (but not Hispanics).

Both the residential and the nonresidential program components were effective for the students they served. Earnings and employment impacts in years 3 and 4 were positive overall for those assigned to each component. Furthermore, employment and earnings gains were found for males, females with children, and females without children in each component, except for nonresidential females without children. Thus, the residential and nonresidential program components were effective for broad groups of students.



In the rest of this chapter, we present details of our findings on impacts on labor market outcomes. The next section discusses the impacts on employment rates, time employed, and earnings for all students. To provide insight on the nature and quality of the jobs held, we next compare the characteristics of jobs held by program and control group members. The third section presents impacts on the likelihood of being employed or engaging in educational activities (that is, engaging in an activity that improves a youth's long-run employment prospects). Finally, in the fourth section, we present impact findings for key subgroups. Appendix D contains supplementary tables.

## **A. IMPACTS ON EMPLOYMENT RATES, TIME EMPLOYED, AND EARNINGS**

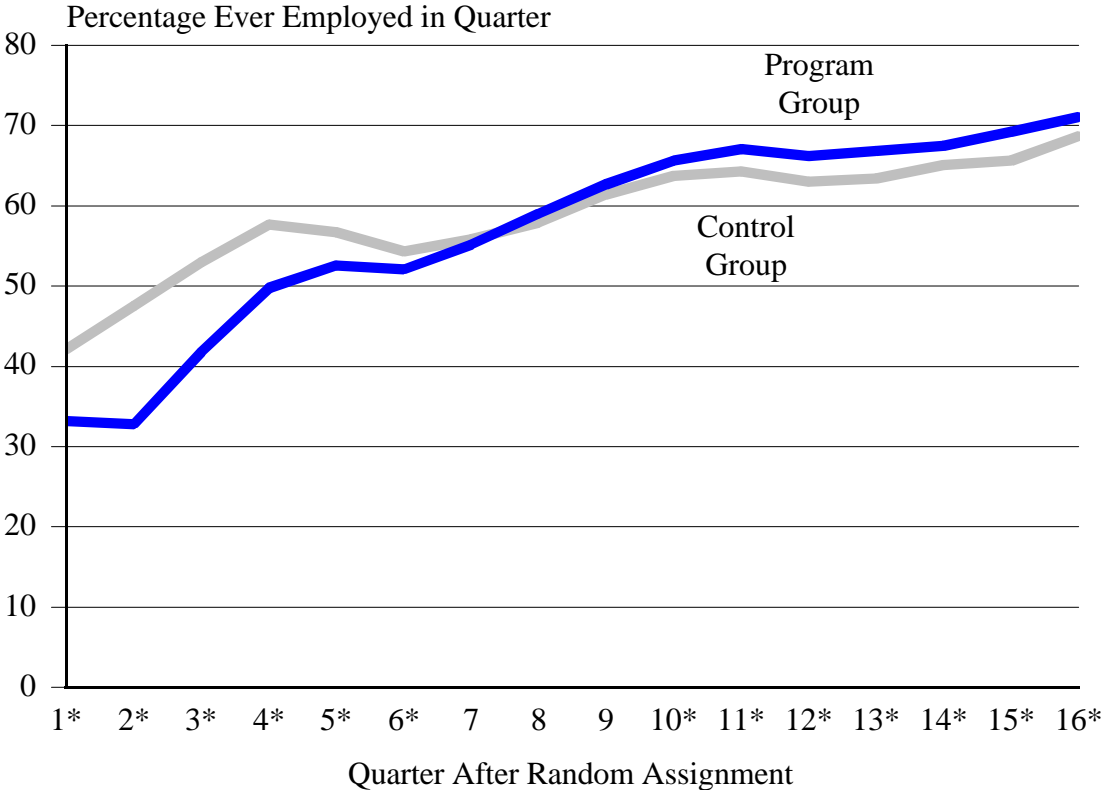
This section compares employment experiences of all control and program group members during the first 48 months after each applicant was determined eligible for Job Corps. We focus on the last two years of the observation period, because most enrollees in the program group had left Job Corps by then.

### **1. Impacts on Employment Rates**

Figure VI.1 displays the proportion of all program and control group members who were ever employed during each quarter (3-month period) over the 48-month period after random assignment. The quarterly employment rates of the control group show what program group members would have experienced if they had not had the opportunity to enroll in Job Corps. The differences between the quarterly employment rates of the program and the control group are estimated impacts per eligible applicant. Asterisks along the x-axis indicate the statistical significance of the impact estimates. Table VI.1 displays the calculations and also shows impacts per participant.

FIGURE VI.1

EMPLOYMENT RATES, BY QUARTER



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

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

TABLE VI.1

## IMPACTS ON EMPLOYMENT RATES AND THE NUMBER OF JOBS

Outcome Measure	Program Group	Control Group	Estimated Impact per Eligible Applicant <sup>a</sup>	Program Group Job Corps Participants	Estimated Impact per Participant <sup>b</sup>	Percentage Gain from Participation <sup>c</sup>
Percentage Employed, by Quarter After Random Assignment						
1	33.2	42.1	-8.9***	28.1	-12.4***	-30.6
2	32.8	47.5	-14.7***	25.8	-20.4***	-44.2
3	41.8	53.0	-11.1***	36.6	-15.4***	-29.6
4	49.8	57.7	-7.9***	46.3	-10.9***	-19.1
5	52.6	56.7	-4.1***	50.8	-5.7***	-10.1
6	52.1	54.3	-2.2**	51.1	-3.0**	-5.6
7	55.2	55.8	-0.6	54.5	-0.8	-1.5
8	59.0	57.9	1.2	59.0	1.6	2.8
9	62.7	61.4	1.2	63.3	1.7	2.7
10	65.6	63.7	1.9**	66.5	2.7**	4.2
11	67.1	64.3	2.9***	67.7	4.0***	6.2
12	66.2	63.0	3.2***	66.3	4.4***	7.1
13	66.8	63.4	3.4***	67.3	4.8***	7.6
14	67.5	65.1	2.4***	67.9	3.3***	5.1
15	69.2	65.6	3.6***	70.1	5.0***	7.7
16	71.1	68.7	2.4***	71.6	3.3***	4.9
Percentage Employed at 48 Months	62.1	59.1	3.0***	62.5	4.2***	7.1
Percentage Ever Employed	95.8	95.0	0.7*	96.0	1.0*	1.1
Number of Jobs (Percentages)						
0	4.7	5.3	-0.7	4.4	-1.0	-17.8
1	11.6	11.7	-0.1	11.6	-0.1	-1.2
2	18.1	17.3	0.8	18.4	1.1	6.4
3	18.4	18.8	-0.4	18.6	-0.5	-2.7
4 or more	47.3	46.9	0.4	47.0	0.5	1.1
Average Number of Jobs	3.6	3.6	0.0	3.6	0.0	-1.2
<b>Sample Size</b>	<b>6,828</b>	<b>4,485</b>	<b>11,313</b>	<b>4,925</b>		

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

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

<sup>a</sup>Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

<sup>b</sup>Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

<sup>c</sup>The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

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

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

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

The employment rate of the control group increased over time. It was 42 percent in quarter 1, 58 percent in quarter 8, 63 percent in quarter 12, and 69 percent in quarter 16. Employment increased as the youths left school and gained work experience.<sup>1</sup>

The employment rate of the control group was significantly higher than that of the program group (impacts were negative) during the period when many program group members were enrolled in Job Corps. The differences narrowed over time as some program group enrollees started to leave Job Corps and take jobs. Impacts became positive by quarter 8 (that is, two years after random assignment). For example, the employment rate was about 9 percentage points lower for the program group than for the control group in quarter 1 (33 percent, compared to 42 percent), about 4 percentage points lower in quarter 5, and about 1 percentage point higher in quarter 8.

The impact per eligible applicant on the employment rate nearly tripled, from 1.2 percentage points in quarter 8 to 3.2 percentage points in quarter 12, and remained fairly constant at about 3 percentage points between quarters 12 and 16. The impact *per participant* was about 4 percentage points during the fourth year after random assignment (that is, during year 4). The quarterly impacts were statistically significant at the 5 percent level starting in quarter 10.

Nearly all sample members in both the program and the control groups (about 95 percent) worked at some point during the 48-month period (Table VI.1). The distribution of the number of jobs held by the two groups is very similar. Nearly half of each group had four or more jobs during the 48-month period, and only 12 percent had only one job. Thus, job turnover was common for both groups.

---

<sup>1</sup>The employment rate was 43 percent in the quarter prior to random assignment and 43.5 percent in the quarter before that.

## **2. Impacts on Time Employed**

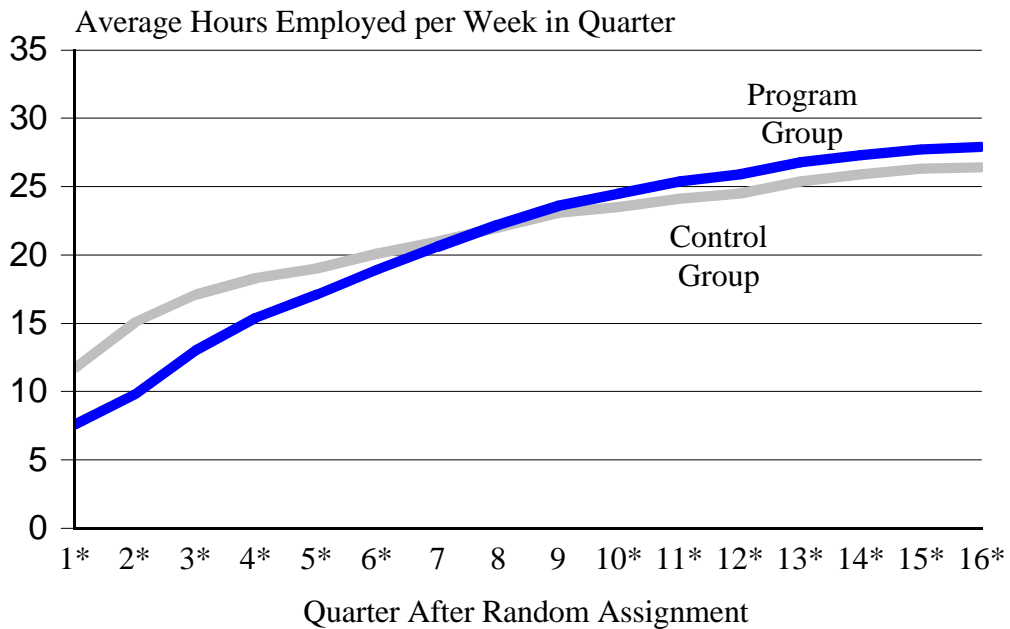
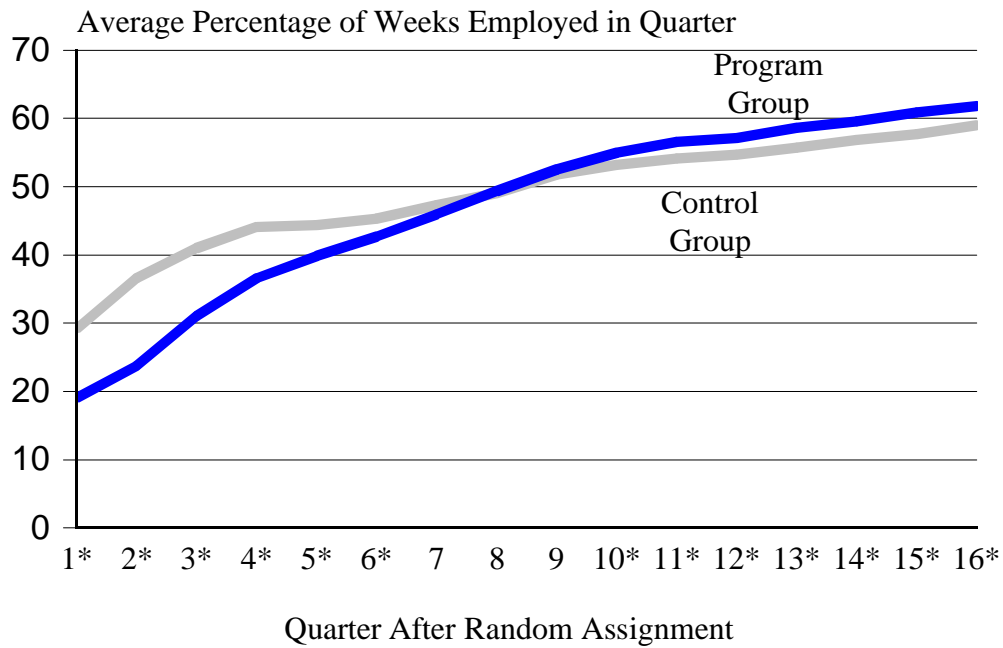
We used two measures of the time that sample members were employed during a given period: (1) the proportion of weeks employed, and (2) the number of hours worked per week. We calculated the proportion of weeks employed by dividing the total number of weeks that each youth was employed during the period by the number of weeks in the period (for example, 13 weeks for a quarter and 208 weeks for the entire 48-month period). Similarly, we calculated hours worked per week by dividing the total number of hours that the youth worked during the period by the number of weeks in the period. The measures were set to 0 for those who were not employed during the period.

Not surprisingly, the profile of the quarterly-time-employed measures follows a pattern similar to that of the quarterly employment rates (Figure VI.2 and Tables VI.2 and VI.3). Impacts were negative and statistically significant during quarters 1 to 6 and became positive in quarter 8 (about two years after random assignment). For example, the average hours worked per week during quarter 1 was about 12 hours for control group members and 8 hours for program group members (an impact of -4 hours per week). The impact on hours worked per week was -1.9 hours in quarter 5 and 0.2 hours in quarter 8.

The positive impacts on weeks and hours employed increased sharply between quarters 8 and 12, and then remained fairly constant through quarter 16. The impacts were statistically significant at the 5 percent level starting in quarter 10 (that is, after two and a half years after random assignment). Program group members were employed for an average of about 60 percent of weeks

FIGURE VI.2

TIME EMPLOYED, BY QUARTER



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

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







in year 4, compared to 57 percent of weeks for control group members. Similarly, the average weekly hours worked per eligible applicant increased from 26 to 27.4 hours during this period. These differences translate to increases of about 7.5 percent in the weeks and hours worked by Job Corps participants.

Over the entire 48-month period, control group members worked slightly more than program group members, who spent more time in education and training programs and whose employment rate did not “overtake” that of the control group until quarter 8. Control group members spent an average of about 47 percent of weeks employed, compared to about 45 percent for program group members (a statistically significant impact of about -2 percentage points, or about 4 weeks over 48 months). Similarly, the average control group member worked 0.5 hours per week more than the average program group member, or about 100 hours more over the entire 48-month period.

### **3. Impacts on Earnings**

Earnings are the most comprehensive employment-related measure, because they reflect both work effort and earnings per hour. To examine earnings impacts, we calculated period-specific earnings per week from all jobs for each sample member. We calculated earnings per week by dividing total period earnings by the number of weeks in the period. Thus, the measure represents the earnings (in 1995 dollars) of a youth in a typical week during the period.<sup>2</sup>

Earnings per week increased over time for the control group (Figure VI.3 and Table VI.4). For example, control group members earned an average of \$66 per week in quarter 1, \$147 per week in quarter 8, \$179 per week in quarter 12, and \$199 per week in quarter 16. Earnings increased because

---

<sup>2</sup>We measure earnings in 1995 dollars to be consistent with our measure of program costs used in the benefit-cost analysis (McConnell et al. 2001). We use primarily program costs in PY 1995 because that was the period when most program group participants entered Job Corps.

FIGURE VI.3 AVERAGE EARNINGS PER WEEK BY QUARTER

TABLE VI.4 IMPACTS ON EARNINGS

both hours worked and hourly wage rates increased as the youths left school and gained work experience.

Interestingly, control group earnings decreased in the recent period prior to random assignment (not shown). Average earnings per week were \$49 in the quarter prior to random assignment and \$62 in the quarter before that. This preprogram dip in earnings could have come about because youths worked less in anticipation of enrolling in Job Corps, or because they had particularly poor labor market experiences (which could have induced them to apply to Job Corps).<sup>3</sup>

The general pattern of the earnings impacts over time is similar to that of the employment impacts. However, positive impacts on earnings emerged earlier, and the earnings impacts were larger in years 3 and 4. Average weekly earnings were significantly higher for control group members than for program group members during the first five quarters after random assignment. The impacts were most negative in quarters 1 to 3 and became smaller in quarters 4 to 6, as participants started leaving Job Corps. Control group members earned an average of about \$22 more per week during quarter 1, \$14 more per week during quarter 4, and less than \$9 more per week during quarter 5.

Earnings impacts became positive in quarter 7 and continued to grow in quarters 8 to 12. They remained fairly constant from quarters 12 to 16 (that is, they *persisted* in year 4). The impacts were statistically significant at the 5 percent level after quarter 8. In year 4, program group members earned an average of about \$211 per week, compared to \$195 per week for control group members. This \$16 impact per eligible applicant translates to a \$22 impact per program participant. In year

---

<sup>3</sup>The earnings dip occurred for all age groups, although the dip was larger for the older youths. Average earnings per week decreased from \$33 to \$28 for those 16 and 17, and from \$97 to \$72 for those 20 to 24.

4, participants earned an average of about \$1,150 (or 12 percent) more than they would have if they had not enrolled in the program.

The estimated impact per participant on earnings over the whole 48-month period was about \$3 per week (\$624 overall). This impact is not statistically significant.

It is noteworthy that, as discussed in Chapter V, similar percentages of program and control group members were in education and training programs in years 3 and 4, and only 13 percent of both groups were in programs in the last week in month 48. Consequently, it is unlikely that the postprogram earnings and employment impact estimates were greatly affected by differences across the research groups in school enrollment rates.

#### **4. Decomposition of Impacts on Earnings in Year 4 into Its Components**

Earnings over a given period are the product of hours worked during the period and earnings per hour. As discussed, we find positive impacts on both earnings and hours worked in year 4. We also find a positive impact of \$0.20 on earnings per hour in year 4 (\$7.72 for the program group and \$7.52 for the control group).<sup>4</sup>

To assess the extent to which the earnings impact was due to the impact on hours worked and how much was due to the impact on hourly earnings, we express average earnings per week for program group members as follows:

$$(1) \quad \bar{E}_P = \frac{\bar{E}_P \bar{H}_P}{\bar{H}_P} = \bar{W}_P \bar{H}_P,$$

---

<sup>4</sup>We calculated the \$0.20 impact using Tables VI.3 and VI.4 and noting that average hourly earnings in year 4 were \$7.72 (\$211.4 earned/27.4 hours worked) for the program group and \$7.52 (\$195.4 earned/26.0 hours worked) for the control group.

where  $\bar{E}_p$  is average earnings per week for the program group,  $\bar{H}_p$  is average hours worked per week, and  $\bar{W}_p$  is hourly earnings (that is, average earnings divided by average hours).<sup>5</sup> Average earnings for the control group can be written in the same way, and thus impacts on earnings per week can be expressed as follows:

$$(2) \quad (\bar{E}_p - \bar{E}_c) = \bar{W}_p \bar{H}_p - \bar{W}_c \bar{H}_c.$$

If we add and subtract the term  $\bar{W}_p \bar{H}_c$  in equation (2) and rearrange terms, then equation (2) becomes:

$$(3) \quad (\bar{E}_p - \bar{E}_c) = \bar{W}_p(\bar{H}_p - \bar{H}_c) + \bar{H}_c(\bar{W}_p - \bar{W}_c).$$

Equation (3) decomposes the impact on earnings into a weighted average of the impact on hours employed per week and the impact on hourly earnings, where the weights are average hourly earnings for the program group and average hours worked per week for the control group, respectively.<sup>6</sup>

Using equation (3), we find that about two-thirds of the earnings impact in year 4 was due to the impact on hours worked and that one-third was due to the impact on earnings per hour. Stated another way, program group members earned about \$11 more per week because they worked more hours, and earned about \$5 more per week because they had higher earnings per hour.

---

<sup>5</sup>This expression is only an *approximation* to the average wage received by the program group, because to calculate the average wage, it would be necessary to divide earnings by hours worked for *each* youth, and then take the average of these individual values. This procedure is difficult to implement for those who did not work (because we would be dividing by zero hours worked). In Section B below, we discuss hourly wages for those employed in quarter 16.

<sup>6</sup>One can instead add and subtract the term  $\bar{W}_c \bar{H}_p$  from equation (2) to derive a slightly different set of weights in equation (3). We obtained the same conclusions using either approach.

## 5. The Overtaking Point

Average program group earnings overtook average control group earnings in quarter 7, and the overtaking point for the employment rate and hours worked was in quarter 8. Thus, it took nearly two years until positive employment-related impacts emerged.

The average program group participant enrolled in Job Corps about 1.4 months after random assignment and remained in the program for eight months. Thus, by quarter 4, the typical program member had left Job Corps. Why did a full year elapse between the time an average participant left Job Corps and the overtaking point?

Many factors could have influenced the timing of the “overtaking point” (the point at which program impacts became positive) for the employment and earnings outcomes. The timing of the overtaking point was due in part to (1) the length of time that each participant spent in the program, (2) the length of time until the potential gains from participation were realized in the form of more work and better jobs, (3) the size of the gain for each student, and (4) the interaction among these three factors. However, these same factors also affected the outcomes of the control group, because, as discussed, many of these youth also enrolled in education programs. Furthermore, sample members participated in programs at different points during the follow-up period because they entered their programs at different points and had different durations of stay. Thus, it is very difficult to disentangle the factors that can explain the timing of the overtaking point.

However, we offer several possible reasons that positive program impacts on the employment and earnings outcomes did not occur until about two years after random assignment. First, impacts on participation in education programs were relatively large until quarter 7, primarily because of intensive program group participation in Job Corps. For example, in quarter 6, the impact per participant on the enrollment rate in education programs was about 8 percentage points, and about 14 percent of program group participants were still in Job Corps. Second, it took time for some

participants to find jobs after they left the program. For example, in the year after leaving the program, about 21 percent of participants did not work, and 16 percent first worked more than six months after leaving.<sup>7</sup> In addition, about 30 percent of program trainees enrolled in another education program during the one-year period. To be sure, control group members may have also had a period of readjustment after they left their programs. However, for Job Corps participants, this period may have been longer, because most were residential students and had been away from home for a relatively long time.

## **6. Effects of the Strong Economy**

The 48-month follow-up data cover the period from November 1994 to February 2000, a period of strong economic growth. The unemployment rate for the civilian population of those 16 and older was 5.5 percent in late 1994, which was low by recent historical standards. The rate decreased to about 4.5 percent in mid-1998 and to about 4 percent in early 2000. Similarly, the unemployment rate for those 16 to 19 decreased from about 17 percent to under 14 percent during the same period. In addition, inflation was low.

It is impossible to know whether employment and earnings impacts would have differed in a weaker economy. Employment rates and earnings were probably higher in the strong economy than they would have been in a weaker one. However, they were likely to have been higher for *both* program and control group members.

There is some evidence that the strong economy increased average earnings more for the control group than the program group. This is because the control group typically had less training and lower skills, and the literature suggests that those with lower skills benefit more from a tight labor

---

<sup>7</sup>These figures were calculated using only program group members who enrolled in Job Corps and who left the program at least a year before month 48 (that is, those who left before month 36).



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 control group because more of them had lower skills. This would suggest that our employment and earnings impacts may be smaller than those that would have been obtained in a weaker economy.

We believe, however, that our impact estimates are probably representative of program effects generally. Unemployment rates are high for disadvantaged youth even in good economic times. In addition, the differences in skill levels between the program and control groups are small relative to the differences between high-skilled and low-skilled workers economywide. Consequently, it seems likely any advantage for the control group was small.

## **B. DIFFERENCES IN HOURLY WAGES AND OTHER JOB CHARACTERISTICS**

In this section, we examine the hourly wage and other characteristics of jobs held by program and control group members during quarters 10 and 16, including job tenure, usual hours worked per week, weekly earnings, occupations, types of employers, and available fringe benefits. We examine job characteristics at two time points to assess changes over time.

The analysis uses information on the most recent jobs held by sample members during the 10th and 16th quarters after random assignment. Youth who were not employed in quarter 10 were excluded from the quarter 10 analysis, and similarly for the quarter 16 analysis. Because we included only employed sample members in this analysis, and because Job Corps participation affected employment rates, and hence, which people were employed, differences in job characteristics should not be interpreted as impacts of the program.

To clarify this limitation, suppose that employment gains due to participation in Job Corps were concentrated among students who had lesser skills and ability and received lower wages. In this case, the employed program group would include a higher proportion of lower-skill/lower-wage

workers than the employed control group. Consequently, differences in the average hourly wage rates of employed program and employed control group members would be a downwardly biased estimate of the true impact of Job Corps on the hourly wage rate of a particular participant.

To investigate whether the offer of Job Corps participation might have resulted in differences in the characteristics of employed sample members, we compared baseline characteristics and pre-program experiences of program and control group members who worked in quarters 10 and 16. The observable characteristics of workers in the program and control groups were similar on average (not shown). To be sure, some *unmeasured* differences between the two groups may have been correlated with the types of jobs held by the youths. In our judgment, however, simple program and control group comparisons are suggestive of program impacts on the characteristics of jobs held by participants, although these estimates should be interpreted with caution. To reinforce this distinction, we do not refer to these differences as impacts. In addition, we present differences per eligible applicant but not per program participant, because the assumptions needed to obtain estimates for participants are less tenable for these outcomes, which are conditional on other outcomes.

The comparisons lead to several conclusions:

- The average hourly wage rate in both quarters was about \$0.23 higher for the employed program group than for the employed control group.
- Job Corps did not alter the distribution of workers across broad occupational categories, and the wage gains were similar across these broad occupations.
- Employed program group members in both quarters were more likely to hold jobs that offered fringe benefits.

Thus, the evidence suggests that program group members secured higher-paying jobs with more benefits, and that the effects persisted during the postprogram period. These findings are consistent

with our finding that average functional literacy and numeracy levels were higher for the program group than the control group at 30 months (Glazerman et al. 2000), which suggests that labor market productivity was typically higher for program group members.

### **1. Differences in Job Tenure, Hours Worked, Hourly Wages, and Weekly Earnings**

A higher percentage of program group than control group members were employed in quarter 10 (66 percent, compared to 64 percent) and in quarter 16 (71 percent, compared to 69 percent) (Table VI.5).<sup>8</sup> Only these workers were used in the analysis.

Most employed youths in both quarters had held their jobs for a short time, although, as expected, job tenure was typically longer in quarter 16. In quarter 10, average job tenure was 8.7 months for the employed control group, compared to 7.9 months for the employed program group. This difference reflects the longer time program group members spent in training. In quarter 16, average job tenure was 12 months for employed youths in both groups, and about 45 percent had been on their jobs for less than 6 months. The finding that many youths had short job tenure is consistent with our finding that many of them held several jobs during the 48-month period, which suggests that job turnover was common.

Most employed youths in both research groups were employed full-time. On average, program and control group members worked more than 40 hours per week in both quarters, and about 85 percent worked at least 30 hours. The small difference in hours worked by research status suggests that program impacts on hours worked (including workers *and* nonworkers) were due to program impacts on the employment rate and not to differences in work effort for those employed.

Employed control group members earned an average of \$6.53 per hour in quarter 10 and \$7.33 per hour in quarter 16. Hourly wages were low for most employed control group members, although

---

<sup>8</sup>About three-quarters of those employed in quarter 16 were also employed in quarter 10.

TABLE VI.5 EMPLOYMENT TENURE, HOURS, AND HOURLY WAGES IN THE MOST  
RECENT JOB IN QUARTERS 10 AND 16

TABLE VI.5 (p2.)

they differed somewhat across workers. For example, in quarter 16, about one-third earned less than \$6.00 per hour, while nearly 20 percent earned more than \$9.00 per hour.

Differences in average hourly wage rates between the employed program and control groups were small, but they were positive and statistically significant in *both* periods (that is, the wage differences persisted). Employed program group members earned an average of \$0.24 more per hour than employed control group members in their most recent job in quarter 10 (\$6.77, compared to \$6.53). In quarter 16, the difference in the average wage rate was \$0.22 (\$7.55, compared to \$7.33). Furthermore, a higher percentage of the program group earned higher wages (27 percent earned \$7.50 or more per hour in quarter 16, compared to 23 percent of the control group), and a smaller percentage of the program group earned lower wages (31 percent earned less than \$6.00 in quarter 16, compared to 34 percent of the control group).<sup>9</sup>

The wage rate gains could be due to several factors. First, as discussed in Glazerman et al. (2000), Job Corps participation leads to statistically significant gains in functional literacy skills. Job Corps raised the average test scores of program group participants at 30 months by about 4 points on the prose literacy scale, 2 points on the document literacy scale, and 5 points on the quantitative literacy scale. In addition, Job Corps moved some participants out of the lowest proficiency level. Thus, increases in the skill level of program participants probably led to increases in labor market productivity and, hence, to higher wages.

The impacts on hourly wages and earnings, however, are larger than can be explained by the impacts on literacy skills alone (Glazerman et al. 2000). Thus, the wage and earnings gains were likely to have also been due to other factors that are influenced by Job Corps but not captured in the

---

<sup>9</sup>We also estimated multivariate models (such as tobit models) to obtain program effects on hourly wage rates. These models controlled for both observable and unobservable differences between the two groups of workers. These results were very similar to the simple program and control group differences.

test scores. These factors might include impacts on vocational skills for a specific job that are not captured in the literacy test, improvements in social skills and attitudes about work, and credentialing effects from obtaining a GED or vocational certificate. It is also possible that the higher wages of the program group were due to placement assistance they received, which increased their chances of finding a job that matched their skills. However, as reported in Chapter IV, few program participants reported that they received significant placement assistance. Thus, the hourly wage gains were probably due only in small part to the Job Corps placement component.

## **2. Differences in Occupations**

The follow-up interviews collected information on the nature of the work performed on each job during the 48-month follow-up period, and the responses were assigned two-digit Standard Occupational Classification (SOC) codes.<sup>10</sup> Occupations were then aggregated into eight broad categories according to two main criteria: (1) each category should correspond to major vocational areas offered in Job Corps, and (2) sample sizes in each category should be large enough to support reasonably precise comparisons between the program and control groups.

Job Corps did not shift workers among the broad occupations in which sample members worked (Table VI.6). Furthermore, the distribution of occupations in which sample members worked changed only slightly over time. About 22 percent of both groups worked in service occupations (such as food and health service) in both quarters. An additional 20 percent worked in construction occupations. About 13 percent worked in sales in quarter 10, compared to about 11 percent in quarter 16. About 11.5 percent in quarter 10 and 13.5 percent in quarter 16 were mechanics, repairers, or machinists. Less than 10 percent were in clerical occupations in quarter 10, but this figure increased to 12.5 percent in quarter 16. Less than 8 percent were in private household

---

<sup>10</sup>The responses did not usually contain enough detail to be assigned three-digit SOC codes.

TABLE VI.6 OCCUPATIONS AND TYPE OF EMPLOYER ON THE MOST RECENT JOB IN  
QUARTERS 10 AND 16



occupations (such as building and apartment maintenance, babysitting, and child care), or agricultural or forestry trades.

The types of employers that the employed program and control group members worked for were nearly identical. Most youths worked for a private company (84 percent in quarter 10 and 80 percent in quarter 16). Only a small percentage worked for the government (8 percent in quarter 10 and 10 percent in quarter 16), were self-employed (5 percent in both quarters), or were in the military (2 percent in both quarters).

### **3. Differences in Hourly Wages Within Occupations**

Similar percentages of the employed program and control group members were in each occupational area. However, the average hourly wage was higher for the employed program group. Thus, there must have been differences between the wages of program and control group members *within* occupations. An important issue is whether these wage gains were concentrated in selected occupations or occurred uniformly across occupations.

In general, the wage gains occurred in most occupation groups (Table VI.7). Employed program members had higher wages in six of the eight occupational areas in quarter 10 and in five of the eight areas in quarter 16, including higher-paying occupations (such as construction) and lower-paying occupations (such as service). Thus, participants probably obtained jobs requiring higher skill levels in most occupational areas.

### **4. Differences in the Availability of Job Benefits**

The availability of job benefits is another indicator of job quality. Many, though by no means all, employed control group members were receiving the major fringe benefits in the jobs they held in quarter 10, and benefit receipt rates increased between quarters 10 and 16 as the sample members gained work experience and obtained better jobs (Table VI.8). About 48 percent in quarter 10 and

TABLE VI.7 HOURLY WAGES BY OCCUPATION FOR THOSE EMPLOYED IN QUARTER

10

TABLE VI.8 BENEFITS AVAILABLE ON THE MOST RECENT JOB IN QUARTERS 10 AND  
16 FOR THOSE EMPLOYED

54 percent in quarter 16 received health insurance, about 54 percent in quarter 10 and 61 percent in quarter 16 had paid vacation, and 38 percent in quarter 10 and about 44 percent in quarter 16 had retirement or pension benefits.

Job Corps appears to have had small positive effects on the availability of benefits on the job. Employed program group members were more likely to have each type of benefit available than were employed control group members, and the differences were similar in quarters 10 and 16. The differences were small, though many are statistically significant. For example, in quarter 16, about 57 percent of the program group received health insurance compared to 54 percent of the control group (a statistically significant increase of 3 percentage points, or nearly 6 percent). These findings provide additional evidence that Job Corps participants obtained better jobs as a result of their gains in skill level.

As described more fully in McConnell et al. (2001), the impacts on total compensation were somewhat larger than the impacts on earnings, because employed program group members were more likely to receive fringe benefits than employed control group members.

### **C. IMPACTS ON PARTICIPATION IN ANY ACTIVITY**

Both current employment and current education and training are likely to improve youths' long-run employment prospects. Each of these activities provides skills and experiences that employers value. In this section, we examine the extent to which eligible Job Corps applicants engaged in either or both of these activities.

Chapter V showed that program group members were more likely than control group members to participate in education and training programs during the first two years after random assignment. The impacts were largest in the early part of the follow-up period, when most program group members were enrolled in Job Corps, decreased as participants left Job Corps, and were very small

after quarter 8. Conversely, control group members worked more than program group members during the early part of the follow-up period, and impacts on employment did not become positive until quarter 8. To assess the extent to which these opposing impact trends offset each other, we calculated program impacts on being either employed or in an education or training program, by quarter and over the entire 48-month period.

Many control group members worked or engaged in education or training during each quarter of the follow-up period (Figure VI.4 and Table VI.9). The percentage of the control group in an activity increased during the first year after random assignment (from 60 percent in quarter 1 to 74 percent in quarter 4) because both employment and school enrollment rates increased. The percentage remained relatively constant after the first year (it was 72 percent in quarter 10 and 75 percent in quarter 16), because increases in the employment rate offset declines in enrollment in school. Nearly all control group members either worked or undertook education or training at some point during the 48-month period. Since all these youths had made the decision to apply to Job Corps, this high level of productive activity is not surprising.

Estimated impacts on working or being in school were positive and statistically significant in each quarter of the follow-up period. The impacts were largest during the first year after random assignment, because most program group members were enrolled in Job Corps then. The program group's higher rates of participation in education or training during this period more than offset the higher employment rates of the control group.

The impacts were positive, but they were much smaller between quarters 4 and 7, because impacts on participation in education and training programs decreased as more program group members left Job Corps and because the declines in education were not fully offset by increases in employment. Impacts in the second half of the follow-up period (quarters 8 to 16) remained positive

FIGURE VI.4 PERCENTAGE EMPLOYED OR IN SCHOOL, BY QUARTER

TABLE VI.9 IMPACTS ON BEING EMPLOYED OR IN AN EDUCATION OR TRAINING PROGRAM

(though small), because employment rates of the program group were higher. The impact per participant in quarter 16 was 2.7 percentage points, a 3.6 percent gain due to Job Corps participation.

Impacts on the proportion of weeks and hours per week spent working or in an education or training program follow the same pattern (Tables D.1 and D.2). They were positive and statistically significant in all quarters, but largest early in the follow-up period, when most program group members were enrolled in the program. In sum, Job Corps had a sustained positive effect on promoting activities aimed at improving participants' long-run employment prospects.

#### **D. FINDINGS FOR SUBGROUPS**

Overall, Job Corps produced modest gains in employment and earnings starting about two years after youths applied for the program and were determined eligible. Positive impacts for the full sample, however, could mask important differences in program impacts across subgroups of students. An important question is whether these positive impacts were similar for most subgroups of students or were concentrated among certain groups. This section provides evidence on this question.

After briefly summarizing the subgroup findings, we present detailed findings for the most important subgroups--those defined by age, gender, and residential or nonresidential assignment. We present the full detail on employment and earnings impacts for these groups. In the third section, we discuss findings for other subgroups of interest--whether the youth had a high school diploma or GED at baseline, whether the youth was ever arrested before application, race and ethnicity, and whether the youth applied to Job Corps before or after the new ZT policies became effective. For these subgroups, the discussion focuses on employment and earnings in year 4.

For each subgroup, we present impacts per eligible applicant and impacts per program participant. However, it is especially important to focus on the impacts per participant in the



subgroup analysis. Rates of Job Corps enrollment among the program group differed somewhat across the subgroups (as discussed in Chapter IV). Consequently, the impacts per eligible applicant were inflated by different participation rates in calculating the impacts per participant. Because of these differing participation rates across subgroups, impacts per participant provide the most accurate picture of relative program impacts across the different groups.

### **1. Impacts by Age**

As one would expect, employment rates and average earnings of older applicants were higher than those of younger applicants during each quarter of the 48-month follow-up period (Figure VI.5 and Tables D.3 to D.5). Among the control group, employment and earnings increased over time for all age groups but increased proportionately more for those 16 and 17 years old. For example, average earnings per week of 16- and 17-year-old control group members nearly tripled, from \$61 in year 1 to \$175 in year 4, whereas those of control group members 20 and older less than doubled during the same period (from \$123 to \$214).

The impacts on employment and earnings were large for those who were 20 or older at application to Job Corps (Figures VI.5 and VI.6 and Tables D.3 to D.5). Impacts on their earnings per week became positive in quarter 7 and were statistically significant by quarter 9. The impacts increased throughout the postprogram period; the impact per eligible applicant more than doubled from \$15 in quarter 9 to \$37 in quarter 16. In year 4, the impact on earnings per participant was about \$50 per week (or \$2,600 in total)--a 25 percent gain. Impacts per participant on the quarterly employment rates and the percentage of weeks employed in year 4 were about 8 percentage points each and are statistically significant. Over the entire 48-month period, participants earned about \$11

FIGURE VI.5 AVERAGE EARNINGS PER WEEK, BY QUARTER AND AGE

FIGURE VI.6 IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE  
PERCENTAGE OF WEEKS EMPLOYED IN QUARTER 10, BY AGE

more per week (about \$2,300 in total) more than they would have if they had not enrolled in Job Corps.<sup>11</sup>

The program also produced meaningful earnings gains for 16- and 17-year-olds. Impacts on earnings per week were positive beginning in quarter 6, and were statistically significant beginning in quarter 7. The earnings impacts remained relatively constant between quarters 7 and 16. In year 4, the impact per participant on earnings was \$17 per week (nearly \$900 in total)--a 10 percent gain. Job Corps participation also increased the percentage of weeks employed and average hours per week employed in year 4 for this group by about 7 percent, and these impacts are statistically significant. The impact per participant on earnings over the entire 48-month period was about \$1,800.

The employment and earnings impacts were small for 18- and 19-year-old participants. In year 4, the impact per participant on earnings per week was about \$6 and the impact on the percentage of weeks employed was about 2 percentage points. These small positive impacts, however, are not statistically significant. Furthermore, the small impacts for those 18 and 19 were found across other subgroups (such as gender and education level at baseline).

The results for the 18- and 19-year-olds are puzzling in light of the positive impacts found for the other age groups. The baseline characteristics of those 18 and 19 are not unusual (Schochet 1998a). In addition, the Job Corps experiences of 18- and 19-year-old participants appear to have been similar to those of participants in other age groups (as discussed in Chapter IV). Furthermore, the estimated impacts on education-related outcomes were large for all age groups (as discussed in Chapter V). Finally, the small impacts for those 18 and 19 appear to be due to high employment and

---

<sup>11</sup>We also estimated impacts for each age group separately (that is, for those 20, 21, 22, 23, and 24) and found very similar results for each age group.

earnings levels for the control group and not to low levels for the program group.<sup>12</sup> Thus, it is difficult to determine whether impact findings for this group are anomalous.

It is noteworthy that the differences in earnings impacts by age were not due to differences in school enrollment rates by age. About 17 percent of program and control group members in each age group were enrolled in an education program per quarter in year 4.

## **2. Impacts by Gender**

Impacts on employment and earnings were very similar for males and females (Figures VI.7 and VI.8 and Tables D.6 and D.7). Indeed, the timing of the overtaking points and the size of the impacts were similar. For example, the impact per participant on year 4 earnings per week was \$24 for males (an 11 percent increase) and \$21 for females (a 14 percent increase). Impacts on hours worked and hourly earnings were also very similar for males and females. The differences between the year 4 impact estimates by gender are not statistically significant. The gender findings are similar across most other subgroups.

The finding that Job Corps improved employment-related outcomes for both males and females is of policy importance because of differences in the characteristics and programmatic needs of these groups. Female students tend to be older, to have completed high school, to have children, and to be nonresidential students. Thus, the program effectively serves these two groups of students with different training needs and barriers to successful employment.

---

<sup>12</sup>For example, among the control group, average weekly earnings in year 4 of those 18 and 19 were 18 percent higher than the average weekly earnings of those 16 and 17, but were only 4 percent less than the average weekly earnings of those 20 to 24. The corresponding figures for the program group were 12 percent and 15 percent, respectively. Thus, the average earnings differences between those 18 and 19 and those 20 to 24 in the control group were much less than one would have anticipated.

FIGURE VI.7 AVERAGE EARNINGS PER WEEK, BY QUARTER AND GENDER

FIGURE VI.8 IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE  
PERCENTAGE OF WEEKS EMPLOYED IN QUARTER 10, BY GENDER

### **3. Impacts for Residential and Nonresidential Students**

Most students reside at their center while attending Job Corps. Indeed, one eligibility criterion is that the student must live in a home or community environment so debilitating that the youth cannot benefit from education and job training while living at home. Yet up to 20 percent of Job Corps slots can be used to serve nonresidential students--those who live at home while attending Job Corps. About 12 percent of students were nonresidential during the period of the study. Nonresidential students must live within commuting distance of their center, and they must be judged able to benefit from Job Corps without leaving their community.

Impacts of the residential component were estimated by comparing the outcomes of program group members designated for a residential slot before random assignment with the outcomes of control group members designated for a residential slot. Similarly, the impacts of the nonresidential component were estimated by comparing the experiences of program and control group members designated for nonresidential slots. Accordingly, the analysis examines (1) the effectiveness of the residential program for youths who are typically assigned to residential slots, and (2) the effectiveness of the nonresidential program for youths who are typically assigned to nonresidential slots. Differences in the students assigned to each component require that we interpret the findings cautiously: they do *not* tell us about the effectiveness of each component for the average Job Corps student or how students assigned to one component would have fared in the other.

These important qualifications can be understood further by noting that the characteristics of residential and nonresidential designees differ in important ways. As described in Chapter III, for



both males and females, nonresidential designees are much more likely than residential designees to be older, to have children, and to have a high school credential, and are less likely to ever have been arrested. Thus, the residential and nonresidential program components serve very different students, and our design can only address the extent to which each component effectively serves students suited for it.

For each component, we present separate impact estimates for (1) males, (2) females without children, and (3) females with children. Samples for some of these subgroups are small (for example, the control group contains only about 200 female residential designees with children, 200 female nonresidential designees without children, and 200 male nonresidential designees). Accordingly, some of the subgroup impact estimates are imprecise. Still, the differences in students served in each component made it important to present separate estimates for these groups.

#### **a. Impacts for Residential Students**

Job Corps was effective for students assigned to the residential program, and similarly effective for broad groups of students (Figures VI.9 and VI.10 and Tables D.8 to D.10). The estimated impacts on employment and earnings in years 3 and 4 were very similar for male residents, female residents with children, and female residents without children. The impact per participant on year 4 earnings per week was about \$21 for males and for females without children, and it was \$31 for females with children. These impacts translate into percentage increases in earnings of 10 percent for males, 15 percent for females without children, and 21 percent for females with children. These results suggest that disadvantaged youths who are suitable for the residential component can benefit from being removed from their home environments and given intensive services in a residential setting for a significant period of time.

FIGURE VI.9 AVERAGE EARNINGS PER WEEK FOR RESIDENTS, BY QUARTER AND  
GENDER

FIGURE VI.10      IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE  
PERCENTAGE OF WEEKS EMPLOYED IN QUARTER 10 FOR  
RESIDENTS, BY GENDER

## **b. Impacts for Nonresidential Students**

The nonresidential component was also effective overall and for most students that it served. The nonresidential component substantially improved the employment-related outcomes of females with children and males, but it did not improve these outcomes for females without children (Figures VI.11 and VI.12 and Tables D.11 to D.13). Participation in the nonresidential component improved earnings per week in year 4 by more than \$35 for females with children (an increase of 24 percent), and by more than \$55 for males (an increase of 26 percent).<sup>13</sup> The estimated impacts on earnings for females without children are not statistically significant.

The finding that estimated program impacts were large for females with children is important because, as discussed, their barriers to successful employment are particularly acute. For example, these women (who represent about 30 percent of all female students and about half of all nonresidential students) tend to be highly dependent on public assistance, and many lack adequate support systems. Thus, the fact that Job Corps can increase employment and earnings for this group is an important policy finding.

## **c. Interpretation of Findings**

The impact findings by residential status should be interpreted with caution. As discussed, our estimates provide information about the effectiveness of each component for the populations it serves. The estimates cannot be used to assess how a youth in one component would fare in the other one, or how effective each component would be for the average Job Corps student. This is because the characteristics of residents differ from those of nonresidents in ways that can affect outcomes.

---

<sup>13</sup>The large earnings impact for males was due in part to an anomalous dip in the average earnings of control group members in this group during year 4. Thus, while we believe that the impact for this group is positive, our estimated impact may be overstated.

FIGURE VI.11      AVERAGE EARNINGS PER WEEK FOR NONRESIDENTS, BY  
QUARTER AND GENDER

FIGURE VI.12      IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE  
PERCENTAGE OF WEEKS EMPLOYED IN QUARTER 10 FOR  
NONRESIDENTS, BY GENDER

For example, we find positive impact for males in the residential component and for males in the nonresidential component. It is tempting, then, to conclude that all males should receive training in the slightly less expensive nonresidential component.<sup>14</sup> However, our results *cannot* be used to support this conjecture, because there are known differences in the characteristics of male residents and male nonresidents. While it is possible to control for some of these differences (such as age, education level, and the presence of children), others (such as family commitments and support, and motivation) are probably correlated with outcomes and cannot be measured. These unmeasured differences could lead to erroneous conclusions about how residential males would fare in the nonresidential component (and vice versa).

Furthermore, most centers with nonresidential slots also have residential slots. Thus, nearly all nonresidential students train with residential students and may benefit from this interaction. It would be impossible from our results to determine the effectiveness of the nonresidential component if nonresidential and residential students enrolled in separate centers.

In sum, our results shed light on how well the residential program serves youths who are suitable for the residential component, and how well the nonresidential program serves youths who are suitable for the nonresidential component, given the interaction of students in the two components.

#### **4. Impacts for Other Key Subgroups**

Positive impacts on postprogram employment and earnings were found for most other key subgroups defined by youth characteristics. Beneficial impacts were found both for those who lacked a high school credential at application and for those with a high school credential, although impacts were particularly large for those 20 and older with a high school credential. Whites and

---

<sup>14</sup>As discussed in McConnell et al. (2001), the cost per participant is about 16 percent less for nonresidential students than for residential students.

African Americans experienced earnings gains, although no gains were found for Hispanics. Although some evidence suggests that earnings impacts were smaller for those with serious arrest charges, impacts were similar for those who had and had not been arrested. Impacts were the same for those who applied before and after the new Job Corps ZT policies took effect.

**a. Educational Attainment**

Impacts on employment and earnings were positive and statistically significant for those with a high school credential (GED or high school diploma) *and* for those who lacked a high school credential at random assignment (Figure VI.13 and Table D.14). Across all ages, participants with a high school credential earned an average of about \$33 more per week in year 4 than they would have if they had not enrolled in Job Corps, and their percentage of weeks worked in year 4 was about 5 percentage points higher. Similarly, the impact per participant on year 4 earnings per week for those without a high school credential at baseline was about \$19, and the impact on the percentage of weeks worked was 4 percentage points. The differences between the impacts for those with and without a high school credential are not statistically significant.<sup>15</sup>

The estimates for students without a high school credential are heavily influenced by the 16- and 17-year-old students, nearly all of whom had no credential. In contrast, about half the students 20

---

<sup>15</sup>We also estimated separate impacts for those with a GED and those with a high school diploma at random assignment. The employment and earnings *levels* for those with a GED and those with a high school diploma were similar, although the *impacts* for those with a GED and those who lacked a high school credential were similar. The estimated impacts for those with a GED are not statistically significant. Furthermore, sample sizes are small for the GED group (see Table A.1). Thus, we are not confident that the GED results represent true effects; hence, we do not highlight them.



FIGURE VI.13      IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE  
PERCENTAGE OF WEEKS EMPLOYED IN QUARTER 10, BY HIGH  
SCHOOL CREDENTIAL STATUS AND AGE

or older had no credential. To disentangle the effects of age and educational attainment, we also estimated impacts by high school credential status for the older age groups separately (Figure VI.13).

Among those 20 to 24, impacts were positive for those both with and without a high school credential, although they were much larger for those with one. The impact per participant on earnings per week in year 4 was more than \$72 for those with a credential, which translates to a 36 percent increase due to program participation. The corresponding impact for 20- to 24-year-olds with a GED or high school diploma was about \$29. The estimated impacts for the 18- and 19-year-olds are not statistically significant for those either with or without a high school credential, although the estimates were larger for those without one.

#### **b. Arrest Experience**

To be eligible for Job Corps, applicants must be free of behavioral problems that would prevent them from adjusting to Job Corps' standards of conduct or that would pose risks to other students. While prior involvement with the criminal justice system does not disqualify an applicant, youths with such involvement are carefully screened by the OA agency and often by the regional office. An important policy question is whether Job Corps can effectively serve those who have had problems with the law.

Job Corps impacts on employment and earnings were similar for those who were never arrested and those who were arrested for nonserious crimes (Figure VI.14 and Table D.14). The impact estimate on earnings per week in year 4 was about \$22 for program participants in both groups.

The estimated impacts for those who were ever arrested for serious crimes (murder, aggravated assault, robbery, and burglary), however, were smaller. These results suggest that those who have had serious encounters with the law do not benefit significantly from participation in Job Corps.

FIGURE VI.14      IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE  
PERCENTAGE OF WEEKS EMPLOYED IN QUARTER 10, BY ARREST  
HISTORY, RACE, AND APPLICATION DATE

However, the group with serious arrests is very small (less than 5 percent of the sample). Thus, conclusions for this group should be treated with caution.

**c. Race and Ethnicity**

Job Corps was more effective for whites and African Americans than for Hispanics and other racial and ethnic groups (which includes American Indians, Alaskan Natives, Asians, and Pacific Islanders). As shown in Figure VI.14 and Table D.14, the estimated impact on year 4 earnings per week was \$46 for white students and \$23 for African American students, and both are statistically significant. The percentage increase in earnings was 21 percent for whites and 14 percent for African Americans. We find no program impacts for Hispanics. In addition, the impact estimates were small and not statistically significant for the remaining racial and ethnic group. The differences between the year 4 earnings impacts across the four racial and ethnic groups are statistically significant.

The finding of no program effects for Hispanics (who are about 18 percent of all youths served by Job Corps) is puzzling because they cannot be explained by differences in program group participation in education and training programs by race and ethnicity. The Job Corps enrollment rate among the program group was similar for Hispanics and other racial and ethnic groups, and the average duration of stay in Job Corps was actually *longer* for Hispanics (9.4 months, compared to 7.7 months). Furthermore, our process analysis site visits to Job Corps centers revealed no differences in the quality of Job Corps services provided to Hispanics and other youths. Finally, the impact on hours spent in all education and training programs during the four-year follow-up period was *larger* for Hispanics than for the other racial and ethnic groups (about 1,200 hours, compared to about 975 hours).

We conducted several additional analyses to help explain the impact findings for Hispanics. First, we estimated program impacts by race and ethnicity across other key subgroups defined by gender, age, and educational level, and found that the impacts for Hispanics were small across each of these subgroups (Table D.15). For example, estimated impacts for earnings in year 4 were not statistically significant for Hispanic males, females, 16- and 17-year-olds, or 20- to 24-year-olds, whereas earnings impacts were positive for whites and African Americans in each of the gender and age groups.

Second, we compared key baseline characteristics of Hispanics, whites, and African Americans in our sample (Table VI.10). Potential differences in the characteristics of Hispanics and other youths could account for the impact findings if Hispanics are more likely to have characteristics associated with smaller impacts.

The main observable differences between Hispanics and other racial and ethnic groups are their geographic locations and primary languages (Table VI.10). Hispanics are heavily concentrated in regions 2, 6, and 9; more than 60 percent of Hispanics live in these three regions, as compared to about 20 percent of whites and African Americans. English is the primary language for less than one-half of Hispanics but for nearly all whites and African Americans. Furthermore, OA counselors deemed that about 12 percent of Hispanics needed a bilingual program in Job Corps, as compared to less than 1 percent of whites and African Americans. Interestingly, however, the age and gender distributions, education levels, and employment, welfare, and arrest histories prior to application are very similar for Hispanics and African Americans.

On the basis of these findings, we estimated impacts for Hispanics, whites, and African Americans by (1) region, (2) whether English was the youth's primary language, and (3) whether

TABLE VI.10 KEY BASELINE CHARACTERISTICS, BY RACE AND ETHNICITY

PAGE 2

the youth needed a bilingual program in Job Corps. In addition, we estimated impacts by race/ethnicity and by whether the youth was designated for one of 23 centers where at least 25 percent of students were Hispanic.<sup>16,17</sup> We conducted this analysis to test the hypothesis that impacts for Hispanics were small because impacts for subgroups in which Hispanics were heavily concentrated were small.

We strongly rejected this hypothesis, however, because estimated impacts for Hispanics were small across *all* levels of the tested subgroups (Table D.15). For example, the impacts for Hispanics were not statistically significant for those in regions and centers in which Hispanics were heavily concentrated *or* for those in other regions and centers with lower concentrations of Hispanic students. Furthermore, impact estimates for whites and African Americans were mostly positive in areas with large concentrations of Hispanic students (although they were larger in other areas). Similarly, impacts did not differ for Hispanics whose primary language was English or for those whose primary language was Spanish.

These findings support our conclusion that Job Corps did not appear to improve the postprogram employment-related outcomes of Hispanic students. Although Hispanic students in the program group were successful in Job Corps, their in-program success did not translate into postprogram earnings gains. This finding, pervasive among Hispanic students, is due neither to their personal characteristics (such as age, gender, or English language status) nor to the centers or regions of the country in which they typically enroll.

---

<sup>16</sup>As part of the application process, OA counselors provided information on the center to which a youth was likely to be assigned on the Supplemental ETA-652 form. This information was collected prior to random assignment, and thus, is available for both the program and control groups. Impacts for groups of centers were obtained by comparing the outcomes of program group and control group members who were designated for those centers.

<sup>17</sup>Of the 23 largely Hispanic centers, 8 were in region 9; 5 were in region 6; 5 were in region 2; 2 were in region 1; and 1 each was in regions 4, 7/8, and 10.



#### **d. Job Corps Application Date and the New Job Corps Policies**

Job Corps instituted strict ZT policies for violence and drugs in March 1995 (early in the sample intake period for the study) in response to congressional concerns about safety at centers. Students suspected of specific acts of violence or of possession or sale of illegal drugs are now removed from the center immediately and, if fact-finding establishes that they committed the alleged offenses, they are terminated from the program. To assess the extent to which these new policies might have affected the impact estimates, we calculated impacts separately for those who applied before and after March 1, 1995.

Postprogram employment and earnings impacts were similar for the cohorts enrolled before and after the ZT policies took effect (Figure VI.14 and Table D.14). The impact estimate on earnings per week in year 4 was about \$24 for the post-ZT group, compared to \$16 for the pre- ZT group, and the difference in the impact estimates is not statistically significant. In addition, Job Corps enrollment rates among the program group, the distribution of the duration of stay in the program, and impacts on education-related outcomes were similar for the two groups. Thus, it does not appear that the new policies had much effect on earnings impacts.

The impact estimates for the pre-ZT group should be interpreted with caution, because program group members in the pre-ZT group who were in Job Corps after March 1, 1995, became subject to the new rules. About 91 percent of program group enrollees in the pre-ZT group participated in Job Corps after March 1, 1995, and the pre-ZT group spent an average of 78 percent of their total time in Job Corps after the ZT policies took effect. Thus, impact estimates pertaining to the pre-ZT period are contaminated. Furthermore, program experiences could differ by season, and because of the limited sample intake period, the data are not available to compare impacts for those in pre-ZT and post-ZT groups who were recruited during the same time of year. Thus, while we find no effect of the new policies, the evidence is fairly weak.