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ABSTRACT

This report presents data and findings on the economic impact of Job Corps on its participants and analyses of the program's benefits in relation to its costs. Findings are based on postprogram experience of individuals enrolled in the Job Corps in 1977, compared to a group of disadvantaged youth not in the program. The first three chapters introduce the report, review the Job Corps program and the evaluation, and discuss the estimation procedures used in the data analysis underlying subsequent chapters. Chapters 4 through 7 present the empirical findings on whether Job Corps is successful in (1) increasing employment and earnings; (2) improving future labor market opportunities through higher education, military training, and other human capital activities; (3) reducing dependence on welfare assistance and other public transfers; and (4) reducing criminality among former Corps members. The comparative evaluation of Job Corps benefits and costs is summarized in chapter 8. Chapter 9 examines issues pertaining to drawing general inferences about Job Corps from the existing data and discusses the generalizability of the findings. Chapter 10 offers some concluding remarks. It confirms that Job Corps had a positive and sizable impact on participants and that its economic benefits for society are greater than its costs. (YLB)

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PROJECT REPORT

EVALUATION OF THE ECONOMIC IMPACT OF THE JOB CORPS PROGRAM

THIRD FOLLOW-UP REPORT
SEPTEMBER 1982

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EXECUTIVE SUMMARY

THIRD FOLLOW-UP REPORT OF THE EVALUATION OF THE ECONOMIC IMPACT OF THE JOB CORPS PROGRAM

This is the third follow-up report of a study designed to provide the Department of Labor with a comprehensive evaluation of the economic impact of the Job Corps program, a program that provides economically disadvantaged youths between 16 and 21 years of age with basic education, vocational training, and related services in a residential setting in an attempt to help the youths become more productive workers, improve their lifetime earnings prospects, and help them become more responsible citizens. The report addresses three major issues: (1) the short-term economic impact of Job Corps on program participants, (2) the longevity of these program effects, and (3) whether the benefits of Job Corps outweigh the costs of the program.

The information provided in this report is based on the most comprehensive data yet available to conduct a study on Corpsmembers. The first detailed personal interviews were conducted in the spring of 1977 with a sample of Corpsmembers then participating in the program and with a comparable group of disadvantaged youths who had not attempted to enroll in Job Corps. At periods 9, 24, and, most recently, 54 months after the baseline survey, reinterviews were conducted with all of the youths in the comparison group and with Corpsmembers who had been out of the program for a sufficient length of time to provide the needed postprogram information.

The baseline survey obtained detailed information on the demographic characteristics of the youths, their socio-economic backgrounds, and their work histories and related activities beginning 6 months before the Corpsmembers enrolled in the program and continuing up to the date of

the interview, which, on average, represented approximately 6 months of program experience. The three follow-up surveys continued to collect detailed information on work histories and related activities during the postprogram period after Corpsmembers had been out of the program from 42 to 54 months altogether.

The fourth survey (the third follow-up interview) provides the new information for this report. Occurring nearly two and one-half years after the second follow-up survey, this third follow-up greatly extends the postprogram observation period, thereby yielding the most comprehensive data set yet available to conduct a study on the postprogram behavior of Corpsmembers. The third follow-up survey increases the average length of observed postprogram experiences for Corpsmembers from just over 18 months to nearly 48 months--more than doubling the length of the postprogram observation period. Over 3,900 youths were interviewed for the third follow-up, which represents nearly 70 percent of the baseline observations and just over 90 percent of the second follow-up observations. Altogether, the data base for this evaluation contains both baseline and some follow-up data on approximately 5,200 youths.

On average, from the information obtained in the baseline survey through the second follow-up, Job Corps was found to be successful in the short term at achieving its primary objective--to improve the economic prospects of Corpsmembers. However, the usefulness of this finding was severely constrained by the short length of the postprogram observation period--between 12 and 24 months, with an average per Corpsmember of approximately 18 months. The estimated time trend in participant impacts over the first two postprogram years showed a rapid increase in employment and related benefits for Corpsmembers during the first few months after

leaving Job Corps and then relatively constant effects throughout the remainder of the two-year period.

Little, if anything, was known about the economic impact of Job Corps on participants after the second postprogram year. On the one hand, it was thought that the effects might decline, as had previously been found for some adult employment and training programs. On the other hand, it was thought that the effects might grow further over time--especially for a youth program such as Job Corps, which, by causing participants to increase postprogram investments in human capital (e.g., there were observed increases in postsecondary education and military service), could lead to future increases in earnings. With the additional follow-up data, we now have a unique opportunity (i.e., a sufficient sample over four postprogram years) to obtain empirical evidence on the duration of Job Corps benefits beyond the second postprogram year.

The findings covered in this report are based on estimates of the differences between the postprogram behavior of Corpsmembers and what their behavior would have been had they not participated in Job Corps (which includes some amount of alternative education, training, and work experience that they forego in favor of participating in Job Corps). The most important of these findings can be highlighted as follows:

1. The estimated effects of Job Corps on former participants' postprogram behavior are generally consistent with hypothesized economic impacts and the important program goal of improving Corpsmembers' economic prospects. During the first four postprogram years, we find that Job Corps is at least moderately successful overall in achieving its desired effects: (1) increasing employment and earnings, (2) improving future labor-market opportunities through work experience, military service, higher education and training, better health, and geographic mobility, (3) reducing dependence on welfare assistance and other public transfers, and (4) reducing criminality.

2. In terms of size and statistical significance, some of the most noteworthy effects of Job Corps on the behavior of former participants are (on a per-Corpsmember basis, including military jobs, and averaged over the four-year observation period, see further in Table 1):

   - o an increase in employment of over three weeks per year
   - o an increase in earnings of approximately $655 per year and over a 15 percent increase (controlling for nonlinear time trends)
   - o a very substantial increase in the probability of having a high school diploma or equivalent degree (a fivefold increase)
   - o higher college attendance
   - o a decrease in high school attendance associated with the effects of high school degrees obtained in Job Corps
   - o better health, with a reduction in serious health problems of an average of over one week per year
   - o a reduction in the receipt of financial welfare assistance, amounting to an average of over two weeks per year, and
   - o a reduction in the receipt of Unemployment Insurance of nearly one week per year.

   The crime effects (not shown in Table 1) are erratic over the postprogram period and, in aggregate over the entire period, show no effect on arrests; however, there is a significant shift from more to less serious crimes.

3. The positive, overall impacts generally persist throughout the four years of postprogram observation. The trend over the four-year postprogram observation period (see Figures 1 and 2, and Table 1) appears to be an increase in program benefits during the first few months (especially for employment and earnings during the transition from center life to re-entering the regular labor market), and then relatively stable effects throughout the rest of the four-year period.

4. We believe that the most prudent conclusion about the longevity of the Job Corps effects is that the effects persist at a relatively stable rate from approximately three months after termination until the end of the four-year observation period; beyond that point, our ability to extrapolate is very limited. The evidence is

mixed as to whether the program effects were growing or shrinking toward the end of the observation period. When only civilian jobs are considered and a simple linear time trend is assumed for the economy, the empirical estimates show a substantial shrinkage of the employment and earnings effects for the fourth postprogram year. However, when military jobs are included (as we believe they should be) and better account is taken of the time trends in the economy, the employment effects are relatively constant for the fourth postprogram year (see row 4 in Table 1, and Figure 2), and the earnings effects show a sizable (21 percent) growth (see row 8 in Table 1). Furthermore, for males--the largest group of Corpsmembers (70 percent of all Corpsmembers)--we have adequate observations to extend our analysis further, from 48 to 51 postprogram months (as in Figures 1 and 2); we find a significant upturn in the estimated effects for males for both employment and earnings during that time period. (Through month 48, the estimated effects for females without children are similar on average to males, but are more erratic; the estimated effects for females with children, the smallest group, are much lower and more erratic than for males.)

5. Differential impacts among Corpsmembers are found to be associated with sex and child responsibility. We find relatively larger impacts on males for the probability of being in military service (more than doubled by the end of the observation period) and the receipt of Unemployment Insurance, while relatively larger impacts on females who have no children present are found for education, health, and the receipt of welfare. The estimated Job Corps impacts on females who have children living with them are generally much less positive than for either males or females without children. This latter finding can possibly be attributed to delays in childbirth among Corpswomen, such that those who have children living with them some time during the observation period are more likely to have been temporarily faced with labor-market constraints from pregnancy (which also increases their reporting of health problems) and from having very young children during the period of postprogram observation.

6. We also find differential program impacts among categories of program completion. A substantial, positive correlation exists between the estimated Job Corps impacts and the proportion of the Job Corps program completed. Program completers consistently benefit the most, particularly in terms of employment, earnings, and welfare dependence. The impact estimates for youths who complete the program are generally more than twice the overall program average. In contrast, early program dropouts appear to benefit little or not at all. Furthermore,

these differential impacts by completion category seem to be at least partially attributable to the effect of staying in the program longer and completing the program, which indicates the potential for additional benefits to the program from increased lengths of stay and greater numbers of completions.

7. In examining the sensitivity of our findings to alternative econometric specifications, we find that (1) adding controls for differences in marital status (even pre-enrollment values) makes the estimates consistently more favorable for Job Corps among all three major sex and family-responsibility groups (males, females without children, and females with children), (2) adding controls for differences in the age composition of children makes the findings much more positive for females with children, (3) accounting for Job Corps effects on fertility and childbearing increases the estimated benefits to females, and (4) not controlling for any differences between the Job Corps and comparison groups makes the findings much less favorable for males and much more favorable for females with children, and changes the findings only slightly for females without children.

8. The findings summarized in Table 2 from a comprehensive evaluation of the social benefits and costs of the Job Corps program suggest that the program is a worthwhile public investment. Our benchmark estimate is that in 1977 social benefits exceed costs by over $2,300 per Corps-member (nearly $3,500 in 1982 dollars), or, equivalently, by approximately 45 percent of costs. Thus, Job Corps is an economically efficient use of public resources in the sense that the program provides greater value to society than the value of the resources it uses up. Because over 40,000 youths enrolled in Job Corps during fiscal 1977, our benchmark estimate of the net social benefit for the entire program exceeds $90-million for that year.

9. The information obtained from organizing all of the estimated program effects into a systematic comparison of the benefits and costs is much more powerful than can be summarized by a few aggregate numbers on the economic efficiency to society. Therefore, we have been careful to provide adequate detail in the report to ensure that informed policymakers can form their own judgments and value the estimated effects in alternative ways. However, under a wide range of alternative assumptions, estimated effects, and values, we find that the program is economically efficient without including the unmeasured benefits shown in Table 1.

10. Over 50 percent of the estimated social benefits are derived from increases in the value of output produced by Corpsmembers. Another 40 percent of the social benefits are attributable to reductions in criminal activities among Corpsmembers, particularly murder, robbery, and larceny (including very large reductions during the program).

11. In assessing the distribution of benefits and costs, we find a net transfer from non-Corpsmembers as a group (everyone in society other than Corpsmembers) to Corpsmembers. The primary economic benefits to Corpsmembers are derived from increased earnings (approximately 70 percent of the benefits) and transfers they receive while in Job Corps. The primary economic benefits to non-Corpsmembers are derived from reductions in Corpsmembers' criminal activities, Corpsmembers' reduced use of transfer programs, and increased tax payments.

12. The estimated timing of benefits suggests that the average social investment per Corpsmember is paid back in approximately three years. Counting only the estimated benefits for the observation period (the first four postprogram years), Job Corps has an internal rate of return of approximately 18 percent (i.e., under the assumption that no further benefits occur after that point).

13. Alternative benefit-cost estimates are made for a wide range of assumptions. A sensitivity analysis based on this range of alternatives generally confirms that Job Corps is an economically efficient program. As long as displacement in the labor markets that Corpsmembers enter is not severe and the observed crime reductions are minimally valued, Job Corps is estimated to be an economically efficient investment.

14. The benefit-cost findings based on the additional data from the third follow-up survey are very close to those estimated in the Second Follow-Up Report: the social net present value estimates are now higher but by less than 3 percent. However, because benefits are now estimated to exceed costs without extrapolating benefits into the future, we feel that more confidence can now be placed in the overall finding that Job Corps is an economically efficient investment.

While the estimates presented above are not exact, and while any single number will not generalize very well, after a careful analysis we are relatively confident about the broad implications of our findings on

disadvantaged youths in general and the Job Corps program in particular. In the report, we present much more supportive evidence and more detailed discussions of all of the findings summarized above.

Chapter I introduces the report and briefly indicates the availability of other reports from our evaluation of the Job Corps program. Chapter II provides an overview of the Job Corps program and our evaluation. Chapter III discusses the estimation procedures used in the data analysis underlying subsequent chapters.

Chapters IV through VII present the empirical findings on whether Job Corps is successful in, respectively, (1) increasing employment and earnings, (2) improving future labor-market opportunities through higher education, military training, and other human-capital activities, (3) reducing dependence on welfare assistance and other public transfers, and (4) reducing criminality among former Corpsmembers. In these chapters we present findings that include youths who were in the military (as well as findings for civilians), and we consider the differential effects for major subgroups, the time duration of effects, and the sensitivity of findings to alternative econometric specifications.

Chapter VIII summarizes the comparative evaluation of Job Corps benefits and costs. Chapter IX examines issues that pertain to drawing general inferences about Job Corps from the existing data, and discusses the generalizability of the findings presented in previous chapters. Finally, Chapter X offers some concluding remarks and thoughts.

TABLE

SUMMARY OF MAIN FINDINGS FOR OVERALL IMPACTS PER CORPSMEMBER ON AN ANNUALIZED BASIS

Variable	Estimated Job Corps Effects For First Year After Termination	Estimated Job Corps Effects For Second Year After Termination	Estimated Job Corps Effects For Third Year After Termination	Estimated Job Corps Effects For Fourth Year After Termination
1. Weeks employed, civilians	1.56	4.84	3.12	1.35
2. Weeks employed, controlling for nonlinear time trends, civilians	1.75	4.20	3.60	2.04
3. Weeks employed, civilians & military	1.35	5.04	4.06	2.81
4. Weeks employed, controlling for nonlinear time trends, civilians & military	1.53	4.37	3.69	3.51
5. Earnings, civilians	$406	$626	$386	$ 77
6. Earnings, controlling for nonlinear time trends, civilians	$571	$608	$442	$370
7. Earnings, civilians & military	$376	$706	$626	$533
8. Earnings, controlling for nonlinear time trends, civilians & military	$515	$667	$652	$787
9. Probability of high school diploma or GED	0.239	0.265	N.A.	0.275
10. Weeks in college, civilians	0.39	0.95	0.65	1.04
11. Weeks in college, civilians & military	0.16	0.29	0.34	0.65
12. Weeks in high school, civilians	-1.51	-1.20	-0.68	-0.60
13. Weeks in high school, civilians & military	-1.22	-0.99	-0.55	-0.26
14. Weeks of serious health problems, civilians	-1.01	-1.07	-1.22	-1.09
15. Weeks of serious health problems, civilians & military	-0.96	-1.07	-1.22	-1.12
16. Weeks received any cash welfare, civilians	-2.99	-1.87	-2.31	-1.46
17. Weeks received any cash welfare, civilians & military	-2.96	-1.87	-2.37	-1.51
18. Weeks received Unemployment Insurance, civilians	-1.07	-0.65	-0.75	-0.70
19. Weeks received Unemployment Insurance, civilians & military	-1.04	-0.65	-0.75	-0.75

TABLE 2

ESTIMATED NET PRESENT VALUES PER CORPSMEMBER UNDER THE BENCHMARK ASSUMPTIONS
(1977 DOLLARS)

	Perspective[a]		
	Social	Non-Corpsmember	Corpsmember
BENEFITS			
1. Output Produced by CorPsmembers			
o In-Program output	$ 757	$ 673	$ 83
o Increased postprogram employment output	3,276	0	3,276
o Increased postprogram tax payments	0	596	-596
2. Reduced Dependence on Transfer Programs			
o Reduced public transfers	0	791	-791
o Reduced administrative costs	172	172	0
o Increased utility from reduced welfare dependence	+	+	+
3. Reduced Criminal Activity			
o Reduced criminal justice system costs	1,253	1,253	0
o Reduced personal injury and property damage	1,366	1,366	0
o Reduced stolen property	300	462	-162
o Reduced psychological costs	+	+	+
4. Reduced Drug/Alcohol Abuse			
o Reduced drug/alcohol treatment costs	31	31	0
o Increased utility from reduced drug/alcohol dependence	+	+	+
5. Reduced Utilization of Alternative Services			
o Reduced costs of training and education programs other than Job Corps	244	244	0
o Reduced training allowances	0	33	-33
6. Other Benefits			
o Increased utility from redistribution	+	+	+
o Increased utility from improved well-being of CorPsmembers	+	+	+
Total Benefits	$7,399	$5,621	$1,777
COSTS			
1. Program Operating Expenditures			
o Center operating expenditures, excluding transfers to Corpsmembers	$2,796	$2,796	$ 0
o Transfers to Corpsmembers	0	1,208	-1,208
o Central administrative costs	1,347	1,347	0
2. Opportunity Cost of CorPsmember Labor During the Program			
o Foregone output	881	0	881
o Foregone tax payments	0	153	-153
3. Unbudgeted Expenditures Other than Corpsmember Labor			
o Resource costs	46	46	0
o Transfers to CorPsmembers	0	185	-185
Total Costs	$5,070	$5,735	-$665
Net Present Value (Benefits minus Costs)	$2,327	-$115	$2,442
Benefit-Cost Ratio[b]	1.46	0.98	1.99

NOTE: Details may not sum exactly to totals because of rounding.

[a] In addition to the value to society as a whole, the estimates are calculated from the non-CorPsmember and CorPsmember perspectives in order to indicate redistributional effects. In doing so, CorPsmembers are treated as nontaxpayers (except for their own taxes) to simplify the exposition, and non-CorPsmembers encompass everyone in society other than Corpsmembers.

[b] The numerators for the benefit-cost ratios include all of the benefits listed in this table as either positive benefits or negative costs, and the denominator includes all of the costs listed in this table as either positive costs or negative benefits.

FIGURE 1

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED, INCLUDING MILITARY SECTOR: MALES

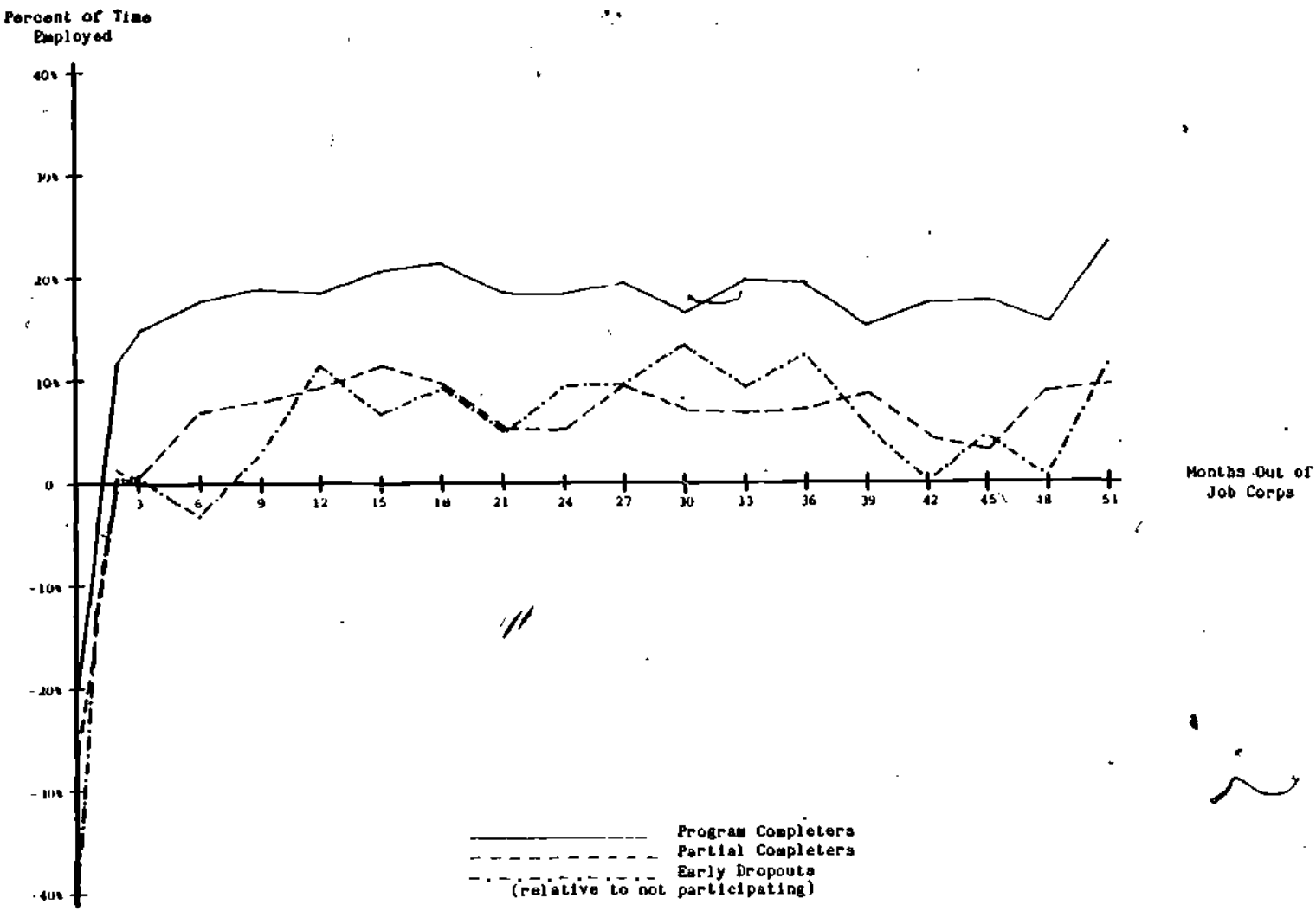
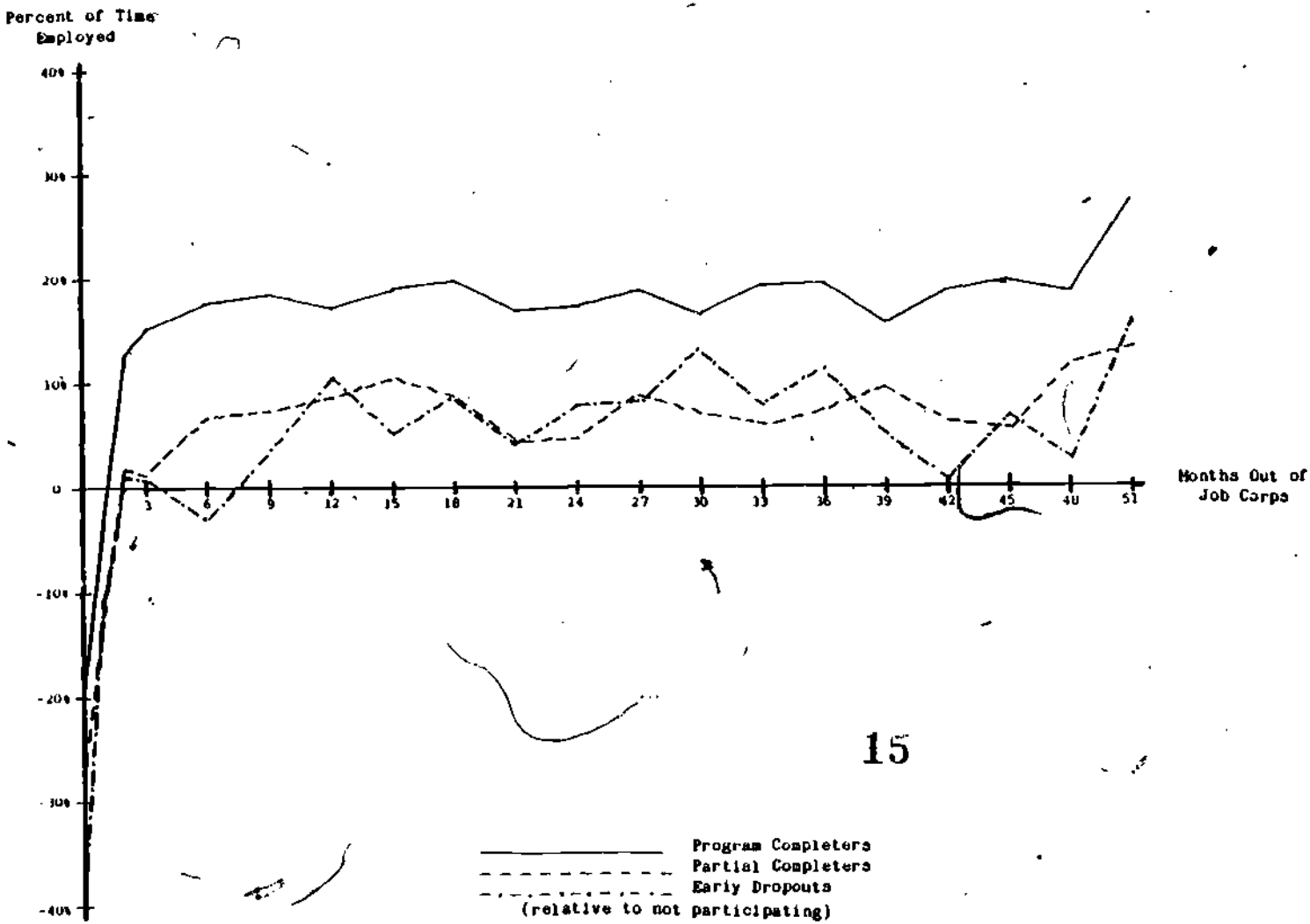


FIGURE 2

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED, INCLUDING MILITARY SECTOR AND CONTROLLING FOR NONLINEAR TIME TRENDS: MALES



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Numerous people made extraordinary contributions to the four nationwide surveys of Corpsmembers and comparison youths that were conducted as part of this evaluation project. At MPR Julie Moran and Audrey McDonald directed and managed the four surveys. The large interviewing staff was supervised on a day-to-day basis by Joanna Cerf, Michael Floyd, John Homrighausen, and David Keuter (Keuter on the baseline survey, Floyd and Homrighausen on the first and second follow-up surveys, and Cerf on the third follow-up survey). The efforts of these key staff and the numerous people who worked under them were essential to our success in maintaining the longitudinal samples and in obtaining the high quality of data underlying the empirical analysis presented in this report.

In addition to the above-mentioned staff, David Kershaw, Charles Metcalf, and David Zimmerman made valuable contributions to the design of this project at MPR, and Patricia Lapszynski and Judy Wayno contributed importantly to the implementation of that design. Assistance in project management was provided by Anne Freeman, Jerilyn Weber, Louise Shaw, and

Gay Rowe over the course of the project. Finally, my administrative assistant, Carol Szul, helped schedule this report and ensured that the project ran smoothly.

The contributions from the staff at the Department of Labor (DOL) were equally significant. Our project officer, John Elliott, deserves special recognition for all of his efforts in providing us with timely feedback and comments, in overseeing the federal clearance process for all four surveys, and in making necessary arrangements with countless federal, state, and local officials. Seymour Brandwein also merits special recognition for providing us with his valuable comments, advice, encouragement, and general support on this project. Other past and present members of DOL staff who provided assistance include John Amos, Burt Barnow, Harris Foster, Stanley Leibner, Charles Nalley, Richard Pervis, Ernst Stromsdorfer, Robert Taggart, and Frances Wattenberg. Finally, we would like to thank all of the regional directors of ETA, all of the directors of the Job Corps centers, the several officials from Job Corps subcontractors who provided assistance, and the directors of the local employment-service offices in the local education agencies in the comparison-group sites.

Charles Mallar
Project Director

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I. INTRODUCTION

This is the Third Follow-Up Report of a study designed to provide the Employment and Training Administration of the U.S. Department of Labor (DOL) with a comprehensive evaluation of the economic impact of the Job Corps program, a program that provides economically disadvantaged youths between 16 and 21 years of age with basic education, vocational training, and related services in a residential setting.[1/] The evaluation was designed to examine whether Job Corps was successful at achieving its objectives of helping youths become more productive workers, improving their lifetime earnings prospects, and helping them become more responsible citizens. This report addresses three major issues: (1) the short-term economic impact of Job Corps on program participants, (2) the longevity of these program effects, and (3) whether the benefits of Job Corps outweigh the costs of the program.

The information used to prepare this study is drawn primarily from four surveys that collected relevant data from both Corpsmembers and a comparison sample of youths who did not participate in the program, but who were similar to Corpsmembers in terms of other characteristics.[2/] The first survey (the baseline interview) was administered during April-June

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[1/] This study focuses on the residential Job Corps program and does not include the relatively small segment (approximately 5 percent in 1977, the base year for this evaluation) of Corpsmembers who receive Job Corps services but do not reside at a center.

[2/] "Corpsmembers" is used throughout this report to refer to participants or former participants in Job Corps.

1977 to a cross-section of Corpsmembers (who were then in Job Corps and residing at centers) and to the comparison sample. The second survey (the first follow-up interview) was administered approximstely 9 months later to the same comparison sample and to youths in the Job Corps sample who had been out of the program for at least 5 months. The third survey (the second follow-up interview) was administered approximately 15 months after the first follow-up survey, again to the same comparison sample, but this time to all youths in the Job Corps sample who had been out of the program for at least 12 months. For the second follow-up, sampled Corpsmembers had been out of the program between 12 and 24 months, with an average per Corpsmember of slightly over 18 months.

The fourth survey (the third follow-up interview) followed the same youths as the previous one and provides the new information for this report. Occurring nearly two and one-half years after the second follow-up survey, this third follow-up greatly extends the postprogram observation period, thereby yielding the most comprehensive data set yet available to conduct a study on the behavior of Corpsmembers. The prior survey data covered approximately one-and-one-half years of postprogram experience, which the third follow-up extends to nearly four years. Over 3,900 youths were interviewed for the third follow-up, which represents nearly 70 percent of the baseline observations and just over 90 percent of the second follow-up observations. Altogether, the data base for this evaluation contains both baseline and some follow-up data on approximately 5,200 youths.

The questions in the four surveys were designed to obtain detailed longitudinal information on the following topics:

- o General demographic characteristics
- o Socioeconomic background
- o Employment and earnings
- o Military service
- o Education and training
- o Geographic mobility
- o Health status
- o Receipt of public assistance
- o Receipt of Unemployment Insurance and Workers' Compensation
- o Antisocial behavior (drug use and criminal activities)

In addition, information was obtained from Corpsmembers on how they rated the program (see Mallar et al., 1978), on job placements by the program (ibid.), and on the extent to which Job Corps education and training helped them obtain employment (ibid.). Finally, information was obtained on where all respondents could be reached for future interviews.

On average, from the data obtained from the first survey through the second follow-up, Job Corps was found to be successful in the short term at achieving its primary objective of improving the economic prospects of Corpsmembers. However, this finding was limited by the short length of the postprogram observation period--an average of approximately 18 months. The estimated time trend in participant impacts over the first two postprogram years showed a rapid increase in employment and related benefits for Corpsmembers during the first few months after leaving Job Corps and then relatively constant effects throughout the remainder of the two-year period.

Little, if anything, was known about the economic impact of Job Corps on participants after the second postprogram year. On the one hand, it was thought that the effects might decline, as had previously been found for some adult employment and training programs. On the other hand, it was thought that the effects might grow further over time for a youth program such as Job Corps, which was also found to cause increases in postprogram investments in human capital (e.g., increases in postsecondary education and military service), which, in turn, should lead to future increases in earnings. With the additional follow-up data, we now have a unique opportunity (i.e., a sufficient sample over four postprogram years) to obtain empirical evidence on the duration of Job Corps benefits beyond the second postprogram year.

Chapter II provides an overview of the Job Corps program and our evaluation. The first part of Chapter II describes the program setting in which the evaluation takes place, including descriptions of the goals of the program, the main Job Corps institutions, the clientele served, the types of individuals who are served by the program, the types of services provided at centers, the size of the program at the time of our study, the changes in the program since our study began, and the current direction of changes in Job Corps. In particular, this discussion focuses on the program's goal of increasing the employability of youths who began the program with severe employment problems, and on what approaches are used in Job Corps to achieve that goal.

The second part of Chapter II summarizes our evaluation design in the context of the three main analytical components: the impact on participants, the time duration of effects, and the benefit-cost

comparison. This discussion in Chapter II focuses on the policy and research issues underlying the evaluation, our conceptual approach and the theoretical underpinnings, and the sample design and survey implementation.

Chapter III discusses the estimation procedures used in the data analysis underlying subsequent chapters. The discussion begins with background information on how the data were arrayed; next, it considers the specific procedures that were used to control for both observed and unobserved differences between the Job Corps and comparison groups; it then focuses attention on the disaggregations necessary to obtain adequate overall estimates and to understand the findings; and, finally, it presents samples of the details of the actual regression estimates.

Chapters IV through VII present the empirical findings on whether Job Corps is successful in, respectively, (1) increasing employment and earnings, (2) improving future labor-market opportunities through higher education, military training, and other human-capital activities, (3) reducing dependence on welfare assistance and other public transfers, and (4) reducing criminality among former Corpsmembers. The findings in these chapters encompass both civilian youths and youths who were in the military; we also consider the differential effects for major subgroups, the time duration of effects, and the sensitivity of findings to alternative econometric specifications.

Chapter VIII summarizes the comparative evaluation of the benefits and costs of Job Corps. This chapter develops estimates of the value of Job Corps effects by combining the estimates of postprogram impacts from Chapters IV through VII with secondary data on the values, or prices, of these effects. Program costs are then estimated with financial data from

the Job Corps financial reporting system, from supplemental information we collected at individual centers, and from information provided by the U.S. Office of Management and Budget. An overall estimate of the program's net present value is computed under a benchmark set of estimated values and assumptions, including the rate of discount for future benefits and the pattern of impacts projected beyond the observation period. The robustness of this overall estimate is then examined by testing its sensitivity to varying the most speculative of the underlying assumptions. Finally, the chapter also presents estimates of the distributional impact of Job Corps, the payback period for the program, and its internal rate of return.

Chapter IX examines issues pertaining to our ability to draw inferences about Job Corps from the existing data, and discusses the generalizability of the findings presented in previous chapters. Specifically, we discuss the interpretation of our findings in the context of a changing program and changing labor market, the accuracy and reliability of our overall approach (from sampling to data collection to econometrics), and the limited length of postprogram observation. Finally, Chapter X offers some concluding remarks and thoughts.

Currently available are several other reports that were generated from this evaluation of the economic impact of the Job Corps program; the interested reader should consult all appropriate volumes.[1/] These additional reports are listed by title after the contents page, and many are

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[1/] All reports are available from MPR's Office of Publications for the cost of reproduction.

referred to in the text of this report. Those available include three other primary reports and seventeen supplemental technical reports.

The four additional primary data reports include (1) the Interim Report (which covers baseline data and assesses the adequacy of the comparison sample), (2) An Examination of Job Corps Participation (which describes Corpsmembers and examines their ratings of the program), (3) the First Follow-Up Report (which covers the first postprogram findings based on the first follow-up survey), and (4) the Second Follow-Up Report (which covers postprogram findings through the second follow-up survey).

The seventeen supplemental technical reports cover a wide range of topics that can be grouped into three broad areas: (1) sampling and survey procedures (five reports: Technical Reports A, B, H, L, and P); (2) the details and derivations of evaluation findings and econometric procedures from the main follow-up reports (seven reports: Technical Reports C, D, E, F, J, K, and Q); and (3) secondary topics that are outside the main focus of the evaluation, but which are of important policy interest and can be addressed with the data collected as part of our evaluation of Job Corps (five reports: Technical Reports G, I, M, N, and O).

The five reports on sampling and survey procedures include one on sample design and implementation, three on survey methods and results, and one on nonresponse to the interviews. The seven technical reports that provide additional detail include one on econometric methodology, three on comparisons of benefits and costs, one on the value of output in work projects, one on resource usage at centers, and one on program operating costs. The five reports on secondary topics include two on an experiment that provided incentive payments to survey respondents, one on comparisons

between Job Corps and other youth programs, one on Job Corps MIS data, and one on the distribution of Job Corps effects.

II. OVERVIEW OF JOB CORPS AND THE EVALUATION

Job Corps is a major public program that attempts to alleviate the severe employment problems faced by disadvantaged youths in the United States--especially those who live in poverty areas.[1] Youth employment problems, while always a serious concern, have been more severe in recent years because of the growth in the teenage population and the persistent downturn in the demand for labor in youth labor markets. During the time period covered by our analysis (1977 through 1981), an average of four out of every ten black youths between the ages of 16 and 21 who were in the labor market were unemployed. Moreover, recent surveys and other data sources have shown that fewer than two out of every ten black youths in the poverty areas of central cities hold jobs.[2]

A. THE JOB CORPS PROGRAM IN 1977 AND SINCE[3]

The Job Corps approach is to provide a comprehensive set of services that include "vocational skills training, basic education, health

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[1] The term disadvantaged is used throughout this report to refer to the set of youths who have employability problems caused by their socio-economic background. Thus, it embodies several factors related to age, educational level, income status, race-ethnicity, employment history, previous social behavior, etc., that limit the ability of young men and women to obtain and hold productive jobs.

[2] For example, see the various recent bulletins on the general employment situation and on youth employment from the Bureau of Labor Statistics, U.S. Department of Labor, as well as Freeman and Wise (1982).

[3] This chapter draws very heavily on three sets of documents prepared by the national Job Corps staff of DOL: (1) Job Corps in Brief, from various fiscal years; (2) A Planning Charter for the Job Corps, 1978; and (3) The Expansion and Enrichment of the Job Corps, 1978. The interested reader should refer to these papers, as well as to Kerachsky and Mallar (1978) and Mallar et al. (1978), for further details. Also, Levitan and Johnston (1975) have summarized the first ten years (1964-74) of Job Corps operations.

care, and residential support for young people who are poor, out of school and out of work. Its aim is to break the cycle of poverty permanently by improving lifetime earnings prospects.[1/] Job Corps is designed to serve youths who currently live in such debilitating environments that, for the most part, they must be relocated to residential centers and provided with residential support in order to benefit from basic education, vocational training, and ancillary services.[2/] Education and training conducted in a supportive environment are the key elements of the program's effort to improve the employability of disadvantaged youths, which, in turn, is expected to help the youths become more productive and responsible citizens.

1. Institutional Setting

The Job Corps program was originally established by the Economic Opportunity Act of 1964. In 1969, control of the program was transferred from the Office of Economic Opportunity to DOL, and Job Corps was eventually inconporated without changes as Title IV in the Comprehensive

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1/ Quoted from The Expansion and Enrichment of the Job Corps. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1978, p. 1.

2/ Some of the Job Corps centers in urban locations added a few nonresidential slots (i.e., enrollee positions) during the 1970s. However, the nonresidential components of Job Corps were not included in our evaluation and, hence, will not be considered in this report. The nonresidential components were excluded because of an earlier judgment that the limited funds available for this project would be more productively allocated to the residential slots. Nonresidential Corpsmembers accounted only for approximately 5 percent of all Corpsmembers. In order produotively to study nonresidential Corpsmembers, most of them who were enrolled during the spring of 1977 would have to have been sampled, at great cost in terms of both the dispersion of the sample and the number of residential observations foregone.

Employment and Training Act (CETA) as enacted in 1973 and as amended in 1978. While a general decentralization and decategorization of the other employment and training programs under CETA has taken place, Job Corps is still administered primarily at the federal level, at least in part because of the widespread nature of youth employment problems and the specific target population for the program. Job Corps' incorporation into CETA, however, has resulted in the transfer of direct responsibility for program operations and center contracting to DOL's regional employment and training offices.

Even though Job Corps is one of the most centralized of the DOL programs administered under CETA, it has a complex operational structure. Job Corps encompasses multiple levels of administrative responsibility, several distinct program components, and numerous contractors and subcontractors. Some Job Corps centers are administered under interagency agreements with the departments of Agriculture and Interior, while others are administered under local contracts through DOL's regional offices. Within the confines of the basic program treatment, different combinations of program services are provided to different groups of youths at different Job Corps centers. Finally, multiple contracts and subcontracts have been enacted at both the national and local levels to recruit eligible youths, to provide vocational training, basic education, and ancillary services at Job Corps centers, and to provide former Corpsmembers with placement and other postprogram services.

Job Corps centers fall under two main types of administration: those operated by private contractors who are selected in a competitive bidding process conducted by the regional offices, and those located on public lands (predominantly in national parks and forests) and operated by

the Department of Agriculture or the Department of the Interior. The former centers are usually referred to as "contract centers," and the latter as "civilian conservation centers" (CCCs). In fiscal 1977, sixty-one centers were in operation, located in thirty-two states and Puerto Rico: twenty-seven CCCs; two CCC-type centers operated by the Commonwealth of Puerto Rico; thirty centers operated under contracts with private business firms, nonprofit organizations, and state and local government agencies; and two extension centers for advanced vocational training operated by unions.[1/] Two contract centers had just opened during the year--a new center in Mississippi and a relocated center in New York.[2/]

Recruitment and placement activities are carried out under contracts with employment service offices, various unions, local schools, volunteer agencies such as Women in Community Service, Inc. (WICS) and Joint Action in Community Service, Inc. (JACS), and special private agencies,[3/] in addition to the efforts of individual centers and the

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[1/] One of these extension centers is operated by the Brotherhood of Railway Airline and Steamship Clerks (BRAC) of the AFL/CIO; the other is operated by Stewards Training and Recreation, Inc., of the Marine Cooks and Stewards Union of the AFL/CIO. In addition, several unions (particularly in construction trades) have contracts to provide training at the other centers (at all CCCs and some of the contract centers).

[2/] Other centers have since opened in an effort to enable Job Corps to achieve an expansion goal of doubling the number of slots compared to fiscal 1976 (see further in Section A.5 below). The MPR evaluation of Job Corps and this report focus on all centers operating in the continental United States during year 1977. The last part of this chapter (Section B.4) and Technical Reports A and B present more detailed discussions of the sample used in this evaluation.

[3/] Private contracting agencies, such as the separate GATE-house (Graduate Aid to Employment for Ex-Corpsmembers) contractors, were operating in six densely populated metropolitan areas--Atlanta, Baltimore, Los Angeles, New York, Philadelphia, and Washington, D.C.--where large numbers of ex-Corpsmembers reside after they leave the centers.

regional offices. The placement groups (especially the volunteer agencies and special private agencies) often provide other support services to youths who have recently left Job Corps, to facilitate their transition from center living to a job and regular living arrangements.

2. Enrollees in Job Corps[1/]

Data gathered as part of this evaluation show that the youths served by Job Corps are severely disadvantaged. In addition to being members of predominantly low-income and minority families, the Corpsmembers generally have low levels of educational attainment and weak employment histories prior to enrolling in the program. Relatedly, they generally have had high incidences of welfare dependence, and many have had brushes with the law prior to enrolling. The combined effect of these characteristics limits their ability to obtain and hold productive jobs.

A review of the socioeconomic characteristics of youths in Job Corps during the spring of 1977 shows that--

- Approximately one-half of the Corpsmembers were under age 18 at the time they enrolled, and nearly one-quarter were age 16 (the statutory age limits were and continue to be 14 to 21, but very few youths under age 16 were then or are now admitted).
- Approximately 70 percent of the Corpsmembers were male (this continues to be the case despite efforts during the 1970s to increase female participation to 50 percent of the total enrollment).
- Over 75 percent came from minority backgrounds--59 percent black, 11 percent Hispanic, 5 percent American Indian, and less than 1 percent Asian or Pacific Islander.

[1/] For a more detailed description of Job Corps participants in 1977, see Kerachsky and Mallar (1978).

- Between 85 and 90 percent of the Corpsmembers had not completed high school at the time they enrolled.

- Almost all Corpsmembers had experienced difficulties in obtaining and holding jobs; moreover, when they did find work, the jobs usually did not pay well. Over one-third of the enrollees had never held a job at which they worked at least twenty hours per week and which lasted for at least one month. In the six months before enrolling in Job Corps, the typical Corpsmember was employed less than one-third of the time and averaged fewer than 12.5 hours of work per week at an average wage rate of $2.81, which was only slightly above the federal minimum wage rate.

- Almost all Corpsmembers had experienced poverty, welfare dependence, or both; in the six months before enrolling in Job Corps, over 90 percent either had incomes that were below the poverty line or were receiving welfare assistance.

- While many (28 percent) had attempted to enlist in the military service, most of them failed to qualify (85 percent of those who attempted).

- Many Corpsmembers had experienced a brush with the law--at least 38 percent had been arrested at some time before enrolling, and 19 percent had been convicted (i.e., approximately one-half of the 38 percent of Corpsmembers who had been arrested had also been convicted).

Also, despite the fluctuations in the scale of the program, the "characteristics of Job Corps enrollees have remained relatively constant over the years."[1/]

3. Types of Services Provided at Centers

To help Corpsmembers overcome the problems highlighted above, Job Corps attempts to provide a comprehensive program that is tailored and

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[1/] Quoted from the Employment and Training Report of the President. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1980, p. 37. For further documentation, see Mallar et al. (1980), Chapter VIII.

flexible enough to meet the individual needs and problems of each disadvantaged youth. The components of the Job Corps program include remedial education, high school equivalency classes, vocational training, health care and education, residential living, counseling, and other ancillary services. Specific services from each of these components are supposed to be incorporated into a unified framework tailored separately to meet the individual needs of each youth.

Education. The Job Corps education program has evolved with the purpose of correcting the varied deficiencies in the backgrounds of Corpsmembers and enabling them to proceed at the maximum pace commensurate with their abilities. The education program includes remedial education (emphasizing reading and mathematics), World of Work (including consumer education, driver education, home and family living, health education, and programs designed for individuals whose primary language is not English), and a General Educational Development (GED) program of high school equivalency for Corpsmembers who are academically qualified. State educational agencies recognize the GED certificate as the equivalent of a high school diploma. The Job Corps encourages and emphasizes the GED program "for those who are academically qualified. In fiscal year 1977, over 4,000 enrollees were awarded the General Education Development Certificate."[1/]

Vocational Skills Training. Like the education program, the training program at Job Corps centers is designed to (1) meet individual

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[1/] Job Corps in Brief, FY-77, p. 3.

needs and problems[1] and (2) enable Corpsmembers to advance at the maximum pace commensurate with their abilities. Therefore, all training programs provide for an open entrance and exit capability and are continually being reviewed and revised in order to keep pace with both the changing needs of Corpsmembers and the changing labor market.

Some notable differences exist between vocational training programs at CCCs and those at contract centers. The training programs at CCCs are often operated by unions and tend to be of a "hands-on" work-project nature, involving actual construction and production and emphasizing the construction and building trades.[2] In contrast, the training programs at contract centers are more often operated by the centers themselves or by individual private subcontractors, and the training tends more often to be of a classroom-instruction, shop-type, or "mock-up" nature, with some work-experience positions available upon the successful completion of the training.

Health Care and Education. Comprehensive health services are provided to all enrollees, including medical examinations (with follow-up treatments, if necessary), immunization, dental examinations (for all Corpsmembers who stay at least ninety days) and dental treatment,

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[1] For example, because approximately 20 percent of all new Job Corps enrollees are functionally illiterate, there must be a corresponding number of vocational training slots that do not have reading as a pre-requisite.

[2] Most of the union instructors use curricula approved for the first two years of the union's apprenticeship program.

professional help for emotional and other mental-health problems, and instruction in basic hygiene, preventive medicine, and self-care. Job Corps also places great emphasis on health education in an attempt to prepare Corpsmembers "to make responsible decisions regarding health and health-related matters by providing them with relevant, factual information."[1/]

Residential Living. Residential living is a key component of the Job Corps program and distinguishes it from most other public employment and training programs, in terms of both a programmatic difference and higher costs for Job Corps. The concept behind residential living is that the target population comes from such debilitating environments that they need a new and more supportive environment to derive the intended benefits of the vocational training and education courses.

The residential-living program includes meals, health services, dormitory life, entertainment, sports and recreation, center government, center maintenance, and other related activities. This program is "planned to help new Corpsmembers adapt to center life, motivate and support constructive attitudes and lifestyles and prepare them to function effectively in the outside world. . . . It involves such complex areas as relationships among racial and ethnic groups, motivation of alienated or discouraged young people, adaptation to unfamiliar group living situations, adult-youth cooperation in an institutional setting, and the role of peer groups in influencing conduct and attitudes."[2/]

1/ Job Corps in Brief, FY-77, p. 3.

2/ Job Corps in Brief, FY-77, pp. 4 and 5.

Counseling and Other Ancillary Services. The centers provide counseling services and residential advisors both to help Corpsmembers plan their educational and vocational curricula and to offer motivation and create a supportive environment. Some of the other support services provided by Job Corps (for example, during recruitment, placement, and the transition to regular life and jobs) were discussed above.

4. Size of Job Corps

At the start of fiscal 1970 the Job Corps program was cut back drastically in terms of both financial expenditures and the number of youths served, with the center capacity cut nearly in half, from slightly under 40,000 slots, or enrollee positions, to slightly over 20,000. From then until fiscal 1977 the budget was held roughly constant in nominal amounts, and the number of youths served stabilized at approximately 21,000 to 22,000 slots and 45,000 new enrollees annually (Corpsmembers stay in the program approximately six months on average). Over the same time period (1970 to 1977), however, inflation greatly eroded the real purchasing power of that budget, which was being held fixed in nominal amounts. Consequently, capital equipment was allowed to deteriorate in order to serve the same number of youths within the more restrictive purchasing power.

With the decision in fiscal 1977 to renovate and expand Job Corps (see the next section), the budget and number of slots in the program were increased greatly. In fiscal 1977 the budget was increased by 58 percent in nominal terms, to $274-million, while the applied funding (i.e., the costs actually incurred rather than budgeted) increased by 23 percent, to $231-million. Some of the additional expenditures began to be allocated for planned expansion, actual expansion, improvements in services,

increases in staffing, and the repair and replacement of capital equipment that had been allowed to deteriorate during the previous seven years. Similarly, the number of slots in the program rose 7 percent in fiscal 1977, to 22,225 slots, with the addition of one new center and a small amount of expansion at some existing centers. However, the number of new youths enrolled declined slightly in fiscal 1977 because the turnover rate fell; hence, the average length of stay in Job Corps and the proportion of program completers increased during that year (conceivably as a result of improvements made possible by the additional resources).

5. Current Trends

The recent trends in Job Corps (i.e., since fiscal 1977) have been dominated by a large expansion of the program under the Carter Administration and considerations of whether and how to scale back the program under the general budget reductions of the Reagan Administration. Job Corps began increasing its capacity in fiscal 1977 in response to a congressional authorization to double the size of the program, as recommended by DOL under Secretary Marshall--from its fiscal 1977 level of 22,000 slots to 44,000 slots by the end of fiscal 1978. The actual expansion proceeded more slowly, but the program capacity had been increased to 41,000 slots in fiscal 1981, where it remained for fiscal 1982. Major funding considerations for fiscal 1983 include whether and how to cut the number of slots and the average cost per slot. These potential cuts in funding are being considered in the context of the Administration's broad curtailments of employment and training programs.

Several factors were taken into account in deciding how best to provide the additional program slots for the expansion between 1977 and

1981. First, positions were allocated across the country according to the relative needs of the various regions; need was determined from regional data on the incidence of poverty and unemployment among youths. Second, in allocating the new slots, consideration was given to the two existing types of center administration, as well as to other potential new types of administration. As a result, the contract centers received the bulk of the new slots; the CCCs received only about 5 percent of the new growth. Thus, the proportion of CCCs declined.

Another 5 percent of the new slots were to be devoted to industry work-experience programs, and approximately 15 percent of the new slots were to be in the Advanced Career Training program, which allows qualified Corpsmembers to attend junior college or technical school under Job Corps sponsorship. For the most part, Corpsmembers in those programs were assigned, at least for administrative purposes, to a regular contract center or CCC.

During the general expansion of Job Corps, DOL emphasized nine "improvement" areas for the program, as follows (quoted from the Employment and Training Report of the President, 1979, p. 170):

1. Arrangements have been made with prime sponsors and with the Armed Forces for referrals to Job Corps. Increased outreach is needed to recruit more young women. Screening procedures should be simplified wherever possible while they should also assure that youth who can most benefit from Job Corps are identified.

2. Only a minority of Corpsmembers complete training and are placed directly into training-related jobs. Better linkages are needed with the labor market. The Industry Work Experience Program and a variety of newly developed advanced career training programs should assist in this effort.

3. New reading and GED programs have been developed and are being introduced, and an experimental college program has been implemented. An Educational Improvement Effort will experiment with alternative education approaches.

4. The world-of-work program to provide Corpsmembers with jobseeking and jobholding skills needs to be strengthened. Alternative systems are now being tested.

5. A formula for living and readjustment allowances has been derived in response to a congressionally authorized increase while attempting to balance the effects on recruiting, retention, performance reward, readjustment, and equity. This formula is now under review by Job Corps and the Department.

6. Comprehensive health services are provided at Job Corps centers, and for 1 in 7 enrollees a previously undetected health condition is identified.

7. Food in Job Corps centers is nutritionally sound but apparently not as appealing to most Corpsmembers as could be desired. The elimination of the statutory ceiling on center operating costs will permit modest increases in the amounts spent on food.

8. Needed and long-delayed improvements have been made in center facilities to enhance the quality of life in Job Corps.

9. The placement system must be reexamined in the coming year with the aim of more closely linking jobs and training as well as shortening the readjustment period.

Under active consideration for fiscal 1983 is a plan to scale back the program to 22,000 slots (the approximate size of Job Corps in fiscal 1977), including the closing of the 30 federally operated CCCs. The closing of CCCs is consistent with our previous findings that (1) the beneficial effects of the program on Corpsmembers' subsequent employment are lower for CCCs, although the difference is not statistically significant (see Mallar et al., 1978, Chapter V) and (2) the cost of operating CCCs is higher because of their smaller size and emphasis on training in the construction industry, which is relatively expensive (see Mallar et

al., 1980, Chapter VII). In addition to the CCCs, however, several contract centers would have to be closed or scaled back in order to accomplish a reduction in the number of positions from the current 41,000 slots to the prior number of 22,000 slots.

B. THE EVALUATION DESIGN

The Job Corps program has survived over fifteen years of changing attitudes toward social problems and has emerged as an important component of the current effort to train and employ disadvantaged youths. However, given the relatively large investment of nearly $6,000 in federal financial costs per enrollee in 1977 (or $12,000 per year of service for participants who stay an average of six months), surprisingly little was known about the magnitude of most of its economic impacts.[1/] Of important concern were the following issues: Does the program provide economic benefits to participants and society? What are the magnitudes of the primary benefits? Do some Corpsmembers benefit more than others? What are the durations of Job Corps effects? Do some variants of the program work better than others? Does the total dollar value of benefits outweigh the costs?

In order to design an evaluation to answer the above questions, we constructed a detailed list of policy and research issues from the hypothesized effects of Job Corps. (These issues are described in the next

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[1/] Aside from program data, only the survey conducted by Louis Harris and Associates between 1966 and 1969 has provided economic data on a reasonable-size sample of former Corpsmembers (i.e., with any reasonable degree of statistical precision). For a comparison group, however, both program data and the Harris (1969) survey are limited to either early dropouts from the program or "no shows" (i.e., youths who signed up for Job Corps and were admitted, but who never attended). Furthermore, the Harris data are obviously outdated given the subsequent changes in both Job Corps and youth labor markets. (For further details, see Louis Harris and Associates, 1969.)

section of this chapter.) We then used the policy and research issues as a guide to develop an evaluation design (see further below).

1. Policy and Research Issues

The objective of our evaluation is to provide DOL with a comprehensive assessment of the economic impacts of the Job Corps program. To meet this objective, we must focus on concrete policy and research issues. The issues addressed are as follows:

1. The extent to which the Job Corps program provides early economic benefits to its participants in terms of gains in employment, earnings, and other related measures of economic well-being

2. The extent to which participation in Job Corps influences subsequent decisions and the ability to enter school, training or work-experience programs, or the military service

3. The extent to which the Job Corps program affects participants' receipt of transfer payments

4. The extent to which participation in Job Corps reduces various forms of antisocial behavior, particularly criminal activities and drug abuse

5. The length of time for which Job Corps effects can be expected to last

6. The existence of differential program impacts by participant characteristics (age, race, sex, prior educational level), by duration of participation in the program, by center type (size, location, operator), and by program component (education received, vocational training, etc.)

7. The extent to which program benefits (both during and after program participation) outweigh program costs,

8. The satisfaction of Job Corps participants with their program experience, and their assessment of the strengths and weaknesses of the program (see Mallar et al., 1978)

The first six items on this research agenda show the range of potential benefits to participants based on a human-capital approach which suggests that training will improve subsequent earnings potential. The magnitudes of economic impact will be measured by comparing the postprogram behavior and economic status of Corpsmembers with what they would have been had the youths not participated in Job Corps.[1] Item 7 requires valuing program benefits and comparing them to the costs. This benefit-cost comparison (or set of comparisons, as we describe in Chapter VIII) is made by aggregating estimates of the dollar values of postprogram benefits with similar measures of in-program benefits, and comparing the total dollar value of program benefits to the total dollar value of program costs. Thus, the benefit-cost research builds upon the impact analysis by assigning dollar values to the estimated program benefits and costs.[2] Item 8 on the research agenda, which focuses on Corpsmembers' perceptions of the program impacts and their assessments of program-related experiences, was completed as part of the First Follow-up Report (see Mallar et al., 1978).

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[1] Note that this is not a Job Corps versus zero program-treatment comparison. Rather, it is a comparison of Job Corps effects to the effect resulting from the average constellation of alternative programs--education, training, work experience, etc.--that the Corpsmembers would have obtained in the absence of Job Corps.

[2] A benefit-cost analysis has the advantage of providing summary measures that can be used to judge the worth of the program in terms of economic efficiency. However, it is difficult to value and aggregate all of the benefits from programs. In addition to providing inputs into the benefit-cost calculations, the impact analysis has the advantage of being able to show program effects that cannot readily be valued in dollar amounts, and allows readers to make their own judgments about the value of various program benefits.

2. Analysis of Participant Impacts

The theory of economic choice underlies many studies of employment and training programs. The theory suggests that individuals choose among competing demands on their time according to the wage rates they can receive, other prices, and sources of nonemployment income that are available. A person's wage rate is hypothesized to depend on his or her productivity, which increases with education and vocational training. By providing education and vocational training, Job Corps should increase participants' productivity, wage rates, employment opportunities, and economic incentives to work.[1] However, institutional labor-market factors such as the minimum wage might cause an excess supply of labor in the markets for disadvantaged youths, so that another effect of Job Corps might be to increase the employment of Corpsmembers (because they have increased productivity) without affecting their short-term wage rates.[2] Also, the self-selection of some of the more productive Corpsmembers into college and military service will reduce the observed impacts on wages in the short run.

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[1] The effect of an increase in wage rates on economic incentives to work is not completely unambiguous, because higher wages might afford some individuals the opportunity to spend more time in activities other than work. However, most studies of youth labor supply have found that work effort is positively associated with wage rates.

[2] Under this example, the minimum wage bolsters the average wage rate received by disadvantaged youths who are employed, while increasing their overall unemployment rate. In such a labor market, Job Corps training could help former Corpsmembers reach the front of the queue for employment when an excess supply of labor exists, in which case they displace other disadvantaged youths in the short run. However, Job Corps training is geared toward moving participants from occupations, industries, and geographic areas in which an excess supply of labor exists to those in which an excess of demand for labor exists.

Chapters IV through VII discuss the effects of Job Corps on several important postprogram activities. These activities can be categorized into four broad areas. The first includes labor-market activities, such as labor-force status, employment, hours worked, wage rates, and earnings. Improvement in this area is considered to be the primary objective of Job Corps. The second area includes additional training and education. Improvement in this area is an important short-term objective because it is expected to increase employment and earnings in the long-run. The third area is dependence on welfare and other public transfers. The final area is antisocial behavior. The anticipated changes in these last two areas relate to the changes in employment and earnings (and in training and educational activities). As better opportunities arise in the labor market (and scholastically), we expect a decline in welfare dependence and antisocial behavior.

One complicating factor in terms of hypothesizing Job Corps impacts is that our base of comparison is not Job Corps versus a zero program treatment. Rather, it is a comparison of Job Corps effects to the effects resulting from the average configuration of alternative programs--education, training, work experience, etc.--that the Corpsmembers would have obtained in the absence of Job Corps. The hypothesized effects of Job Corps in each of the four areas are discussed briefly below and are summarized in Table II.1.

Employment and Earnings. The primary hypothesis is that, other things being equal, young adults who obtain Job Corps training will become more productive and, hence, will gain more employment and receive higher

earnings than those who do not.[1] The increased productivity is expected to lead to improved employability (as measured by increases in labor-force participation, employment, hours worked per week, and the proportion of weeks worked), as well as to higher wage rates and higher earnings. This hypothesis is based on previous research on the effects of training and education on labor-market activities.

In addition to the short-term impacts after leaving Job Corps, subsequent reinforcing effects or a depreciation of the short-term findings might occur. For example, regular employment often provides on-the-job training and a record of worker reliability that, in turn, is rewarded with even higher wage rates and earnings in the future. In contrast, the impacts of Job Corps could fade out over time as the influence of the program becomes less significant the farther removed former Corpsmembers become from the program in time. The time duration of Job Corps effects is one of the primary concerns of our analysis.

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[1] Each of the hypotheses developed in this section is based on the difference between the postprogram behavior of Corpsmembers and what their behavior would have been had they not participated in Job Corps. For ease of presentation, the discussion is sometimes presented as if there were no underlying differences between the Corpsmember and comparison groups, so that the impacts of Job Corps can be characterized by direct contrasts between the behavior of Corpsmembers and that of comparison-group members. Of course, the statistical techniques used (see Chapter III) attempt to compensate for any underlying differences between the Corpsmember and comparison groups. In addition, all the hypotheses discussed herein are weakened when allowances are made for the alternative training and education programs available to youths. In most of the empirical sections of this report we measure Job Corps impacts relative to what Corpsmembers' activities would have been had they not participated in Job Corps. In the absence of Job Corps, many Corpsmembers would not have obtained zero training and related services but, instead, would have received some amount of alternative education, training, and work experience that they forego to participate in Job Corps.

TABLE II.1

SUMMARY OF HYPOTHESES FOR PROGRAM IMPACTS ON PARTICIPANTS

Relative to their nonparticipation in the program, participants will--

1. Employment and Earnings

   - o Have more employment
   - o Have more stable employment
   - o Have higher earnings
   - o Have higher wage rates

2. Investments in Human Capital

   - o Be more likely to have productive work experiences
   - o Be more likely to return to school or to continue their education in other ways, especially at higher levels of education
   - o Be more likely to participate in training programs
   - o Be healthier
   - o Be more geographically mobile
   - o Be more likely to qualify for military service

3. Dependence on Welfare and Other Public Transfers

   - o Have reduced receipt of cash transfer payments
   - o Have reduced receipt of in-kind transfer payments

4. Antisocial Behavior

   - o Be less likely to engage in criminal activities
   - o Be less likely to abuse drugs and alcohol

Investments in Human Capital. Economists define "investments in human capital" as current activities that lead to future increases in productivity and, hence, earnings potential (indirect program effects on productivity and earnings). In this evaluation we will consider six types of investments in human capital: (1) work experience (see above), (2) education, (3) training, (4) improved health, (5) geographic mobility, and (6) military service.

Work-experience, education, and training programs are important placement alternatives to regular employment for Job Corps terminees, especially for younger Corpsmembers. Many of the younger terminees could still profit from additional work experience, schooling, and training after they leave Job Corps; moreover, job placements are often difficult for them to obtain. Therefore, both the impact and benefit-cost analyses must take into account any postprogram increases in such investments in human capital. While increased employment and higher earnings continue to be the long-run goals of both the program and participant, work-experience, education, and training programs are important short-term, intervening factors that might lead to increased employment and higher earnings in the future.

We hypothesize that former Corpsmembers have greater probabilities of participating in higher levels of work-experience, education, and training programs than comparison-group members. However, to the extent that Job Corps succeeds in improving immediate postprogram labor-market opportunities (thereby increasing the opportunity cost of time spent in human-capital programs), this hypothesis is weakened. In any case, it is expected that former Corpsmembers will be more likely to participate in higher-level programs than would youths in the comparison group and will be

more likely to complete any given level (i.e., more likely to obtain advanced degrees or certificates).

An additional hypothesis that falls into the category of human-capital investments is that participation in Job Corps increases geographic mobility. This is supported by the fact that the Job Corps program provides services that help terminees relocate to areas in which employment opportunities exist. We also expect that the additional income from earnings, as well as the health education and treatments provided by Job Corps, will lead to the improved health status of former Corpsmembers relative to youths in the comparison group.

The expected effect of Job Corps on enrollment in the military is somewhat ambiguous; it is unclear whether former Corpsmembers should be more or less likely to enlist in the military. They might be more likely to enlist for the human-capital investments associated with military service (e.g., for the vocational-training aspects and broadened experiences), or they might be less likely to enlist because of the increased opportunity cost of their time (i.e., better job opportunities in the civilian labor force). However, we do hypothesize that Job Corps terminees who take the Armed Forces Qualifying Test are more likely than comparison-group members to pass the test. In addition, military service is an explicit placement target for some Corpsmembers, and GED training in Job Corps should increase both the opportunity to enlist and the rewards for doing so.[1/]

[1/] The working relationship between Job Corps and the military, which was initiated by the signing of a memorandum of understanding by the Department of Labor and the Department of Defense on January 13, 1978, should facilitate the enlistment of Job Corps terminees into the military service.

Dependence on Welfare and Other Public Transfers. A set of hypotheses that are closely related to labor-market activities concerns the effects of Job Corps participation on welfare dependence. Of course, Corpsmembers experience a reduction in welfare receipt while they are at the centers. In addition, because of increased earnings after leaving Job Corps, former Corpsmembers are expected to receive fewer transfers--including AFDC, General Assistance, Food Stamps, public housing, Unemployment Insurance, and Workers' Compensation--than they would have received otherwise during the postprogram period.

These transfer-payment effects of Job Corps might be attenuated (or possibly reversed) if participants become more knowledgeable about the nuances of transfer programs and, consequently, increase their participation in them. In addition, those Corpsmembers who seek additional training or education in the postprogram period might obtain a temporary increase in their transfer payments. Nevertheless, on balance, the amount of transfer payments received by Corpsmembers is expected to be lower than that received by the comparison group both during the program and in the postprogram period.

Antisocial Behavior. Corpsmembers are expected to reduce drug and alcohol abuse and have lower probabilities of engaging in criminal behavior as a result of the program. While the Corpsmembers are at the centers, both of these responses should be very large because their activities are greatly restricted, their behavior is closely monitored, and their material needs are met; consequently, they have few opportunities and less incentive to engage in drug abuse or crime. After Corpsmembers leave the program, these reductions in antisocial behavior are expected to continue, but probably at a smaller rate. The postprogram reductions in antisocial

behavior stem from the entire Job Corps effort to increase employability in order to promote more regular life-styles--from vocational training and educational services to general counseling and center living. Training and education are important because, to the extent that Job Corps increases the employability and the educational abilities of Corpsmembers, legitimate activities become increasingly more attractive than illegitimate activities.

3. Comparative Evaluation of Benefits and Costs

The purpose of the comparative evaluation of benefits and costs is to determine whether program benefits outweigh costs: Does society have more goods and services at its disposal because of the investment in Job Corps? The benefit-cost analysis, which is presented in Chapter VIII, builds upon the results for participant benefits and compares the dollar values of benefits and costs. Implementing a benefit-cost assessment is especially difficult for a program such as Job Corps, which has a wide range of potential effects that could occur over several years. The key elements of our benefit-cost analysis are summarized in Chapter VIII and presented in more detail in Technical Report Q.

4. Evaluation Design

The previous sections summarized the objectives in an evaluation of the economic impact of Job Corps. It should be clear from the discussion that the study design must be comprehensive in order to address all of the relevant policy and research issues. This section summarizes the evaluation design we developed to meet the objectives of the study.

Comparison-Group Methodology. During the design phase of this study, much effort was devoted to selecting an appropriate comparison

group. Program operational considerations and the need for quick turnaround of findings prohibited the random assignment of potential Job Corps enrollees to nonparticipant status. Therefore, it was necessary to devote considerable effort in developing a suitable group of nonparticipants and appropriate statistical procedures with which to compare Corpsmember behavior, so that the hypothesized impacts of the program could be tested and the magnitude of the effects of the program estimated, and so as to increase the reliability of estimates (i.e., reduce the sensitivity of estimates to reasonable changes in the underlying assumptions).

Within the constraint against randomization and the budget and time limitations for the evaluation, we had to develop a sample design that would both minimize bias and maximize efficiency in estimating the effects of Job Corps.[1/] We had to take into account two important factors--(1) that Job Corps was geographically clustered in terms of both the home areas in which Corpsmembers lived and where the centers were located, and (2) that in order to obtain findings quickly enough for DOL we would have to sample from Corpsmembers already enrolled in the program. The comparison-group procedure we judged most efficient called for sequential matching--first obtaining appropriate comparison sites and then finding

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[1/] The fundamental objective of our comparison-group design, survey data collection, and econometric procedures has been to obtain consistent and precise estimates of program effects in the absence of an experimental design. However, only with an experimental design is it possible to ensure a knowledgeable degree of statistical confidence without relying on some modeling assumptions. Our attempt has been to design the evaluation so that the necessary modeling assumptions are minimal and plausible (for more details, see Chapter III).

appropriate youths within those sites. Finally, we included in the baseline questionnaire detailed information about the Corpsmembers' socioeconomic backgrounds, so that the comparability of the Corpsmember and comparison groups could be tested and any important differences could be controlled with statistical techniques.

The first step was to eliminate a few program sites in order to reduce the probability of self-selection biases (e.g., more highly motivated youths, or less employable youths, enrolling in Job Corps). These few eliminated sites were defined as geographical areas that are saturated by Job Corps participation, where youths not entering Job Corps could not be presumed to be similar to Corpsmembers (e.g., very high proportions of eligible youths entering the program from the site of or a location near a Job Corps center, so that those not entering are likely to be systematically different from Corpsmembers). In practice, this meant that three-digit ZIP code areas in which Job Corps centers were located were excluded as potential comparison-group sites.[1/] The nonsaturated areas were then assigned selection probabilities in proportion to their similarities to the home areas of Corpsmembers, based primarily on the poverty and racial composition of the areas as determined from Census data.[2/]

[1/] Of course, there was some (varying) amount of Job Corps participation within the comparison-group sites and some overlap in the residential locations of our Corpsmember and comparison samples.

[2/] Socioeconomic characteristics of the home areas of recent Job Corps participants were used to select the locations of the comparison-group sites. The Primary Sampling Units (PSUs) were five-digit ZIP code areas in urban locations (Standard Metropolitan Statistical Areas) and three-digit ZIP code areas in rural locations. Data from the 1970 Census on population density, geographic location, percent of poverty families, mean family income, housing quality, percent of young (16- to 21-year-old)

Once the control sites were chosen, youths living in those areas were assigned selection probabilities in proportion to their similarity to Job Corps participants (i.e., actual participants and not just Job Corps eligibles), based on their poverty status, age, race, and educational status. Names of youths, their personal characteristics, and addresses were obtained from school dropout lists and from local employment-service offices. Together, these two list sources provided an adequate sampling frame from the universe of youths who participate in Job Corps. School dropout lists identified young recent dropouts who were similar to approximately 70 percent of the Corpsmembers, and the active files at local employment services provided older youths who had been out of school for a longer time and were similar to the other 30 percent of Corpsmembers.

A stratified random sample of youths was chosen from the lists for inclusion in the comparison group, with proportional sampling within strata, except that females were oversampled relative to their proportion

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adults, percent of Hispanic youths, percent of black youths, and youth unemployment rates in the PSUs were used to assign selection probabilities. Regression analysis was used to determine which of these variables would best predict the home regions of Corpsmembers. For both three-digit and five-digit ZIP codes, the best predictor was the percent of families in the region whose income was below the poverty level and which were headed by someone younger than 54 years of age. The second best predictor was the percent of minority youths in the region. The percent of poverty families by itself explained nearly 30 percent of the variances in the proportion of Job Corps enrollments by ZIP code regions. Probabilities of selection were then assigned to all of the nonsaturated ZIP code areas in the United States, proportional to their similarity to the home areas of Job Corps participants, as measured by the percent of poverty families. Proportional stratifications by race and region of the country were also maintained (see Technical Reports A and B for more details).

to increase the efficiency of estimates computed separately by sex.[1/] This quasi-experimental design seems appropriate for our evaluation and, together with the data we obtained on individuals and our econometric procedures (see further in Chapter III), should lead to reasonably accurate estimates of the economic impact of the program. The assumptions necessary to calculate unbiased and relatively efficient estimates of the program treatment effects seem plausible provided that appropriate statistical techniques are used (see further in Chapter III).[2/] No overlap was allowed between the Job Corps and comparison-group samples, and after statistical controls (see further in Chapter III), the Corpsmember sample should differ from the comparison group primarily in terms of access to Job Corps centers and knowledge of the program.[3/]

Sample Size and Selection. The sample-selection procedures were based on the necessity of balancing the evaluation, operational, and cost

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[1/] The target for the male:female ratio was 50:50 in the comparison group, as opposed to the 70:30 split for the Job Corps sample, in order to obtain increased precision in separate estimates for females.

[2/] As used here, "unbiased" means that, on average, the estimator should yield a value close to the "true" one. In other words, any biases are both likely to be small and unlikely to affect the substantive findings of our evaluation. Of course, all estimates are biased to some extent because all statistical models only approximate reality. "Efficiency" means that the estimator has a smaller variance than any other with the same (or smaller) amount of bias and using the same data.

[3/] The comparison-group methodology is further explained and assessed in Technical Reports A and C. Also, see Kerachsky and Mallar (1978).

considerations.[1] For the Job Corps sample, the strategy we chose was to select a random sample of participants in the program at a point in time. For analytical purposes, an enrollment-based sample would have had more appeal, but would have been much more expensive, would have yielded many early dropouts, and would have greatly delayed the research findings.

The sample size was targeted to be large enough to ensure a high probability of observing statistically significant impacts if the "true" Job Corps effects are large enough to be policy-relevant. Specifically, the sample size was chosen to be large enough to have a 90 percent chance of obtaining statistically significant Job Corps effects for employment and related activities if the "true" net present value of Job Corps training is positive (i.e., if the "true" benefit-cost ratio is greater than one). If the Job Corps program is economically efficient, we should, and do (see Chapters IV through VIII), observe many statistically significant effects of Job Corps on employment and related activities. The sample size for Corpsmembers was targeted to be larger than for the comparison group because of interest in estimating differential program impacts among subgroups of Corpsmembers.[2]

To obtain an area probability sample, we used standard procedures to randomly select approximately one-third of the Corpsmembers in the

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[1] The sample design was chosen to minimize the cost of obtaining the desired level of statistical precision for estimates of Job Corps effects (see Technical Report A).

[2] For further details and justification, see Technical Report A.

program during April 1977. Each Corpsmember then at a center had an equal probability of being selected (approximately one-third).[1] For the baseline survey, 5,297 Corpsmembers were selected, and 5,133 of those were interviewed (completed interviews) during April and May of 1977. The first follow-up sample of Corpsmembers included everyone from the original sample who had left Job Corps before October 15, 1977 and, hence, who had been out of Job Corps for at least five months (2,887 youths), and 2,417 Corpsmember interviews were completed.

The second follow-up sample of Corpsmembers included all those who had left Job Corps before April 15, 1978 (4,347 youths), and 3,042 Corpsmember interviews were completed.[2] The third follow-up sample of Corpsmembers was the same as the second, excluding 13 Corpsmembers who died (thus yielding a total of 4,334 youths), and 2,791 Corpsmember interviews were completed. Thus, the survey response rates for Corpsmembers between the three follow-ups were, respectively, 84, 79, and 92 percent, and the

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[1] There were only two exclusions from the sampling frame--youths or centers outside the continental United States, and nonresident Corpsmembers. Justifications for these sample exclusions are presented below.

[2] By the second follow-up survey, 85 percent of the baseline Job Corps sample had been included in the follow-up sample. The 15 percent who had not been included were composed of partial and full completers with long stays in the program. Their exclusion is cost-efficient for three reasons: (1) they have a short postprogram experience, (2) our sample sizes are adequate for these groups without them, and (3) leaving them out approximately offsets any biases from a point-in-time survey that overrepresents long-term stayers within the completion categories.

cumulative response rates from baseline were, respectively, 84, 70, and 65 percent.[1]

For the comparison sample, 1,496 youths were interviewed at baseline, but 39 were lost over time due to deaths and attempts to enter Job Corps. All of the comparison-group observations were attempted at each follow-up, and the numbers of completions were, respectively, 1,306; 1,267, and 1,118. Thus, the survey response rates between the three follow-up surveys for the comparison samples were, respectively, 87, 97, and 89 percent, and the cumulative response rates from baseline were, respectively, 87, 85, and 75 percent.

Note that Corpsmembers who drop out of the program early are less likely than program completers to be at a center at any point in time; hence, they will be underrepresented by point-in-time sampling such as ours.[2] With our point-in-time sampling, there are proportionally more

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[1] Over time, the base for these completion rates includes more and more cases that cannot be interviewed (e.g., deceased youths), and the real base shrinks over time. The completion rates are lower for the Job Corps sample at the second follow-up, because one-half of the sample was contacted solely by telephone (for further details, see below, as well as Technical Reports B, H, and P).

[2] For our purpose, the fundamental difference between "enrollees" and "participants" is that Corpsmembers who stay in the program a long time (i.e., program completers) will be overrepresented in participant samples compared to all enrollees. Among Job Corps enrollees, a high proportion (approximately 40 percent) leave the program within ninety days. These early dropouts are replaced continuously by new Corpsmembers, so that a sample of participants at a point in time has a higher proportion of completers than found among enrollees. For the MPR evaluation of Job Corps, a high proportion of program completers is desirable because the impact of the program on early dropouts is probably negligible and differential impacts for different programs and Corpsmembers could occur among program completers. In Chapter III we explain how the observations are reweighted to obtain unbiased estimates applicable to all enrollees.

program completers in the sample of Corpsmembers than would generally be obtained from a sampling frame based on all enrollees. For all enrollees in fiscal 1977, approximately 40 percent were classified as early dropouts (terminated during their first ninety days), 30 percent were classified as having completed only a portion of the program, and 30 percent were classified as having completed a full program; the corresponding percentages for our second follow-up sample are, respectively, 9, 35, and 56 percent. To obtain estimates that are applicable to an average enrollee will necessitate reweighting the data (see further in Chapter III).

Two exclusions were made from the Job Corps sampling frame--Corpsmembers in centers or from regions outside of the continental United States, and those not residing at centers. This was done for two reasons: (1) those two groups represent only a small proportion of Corpsmembers (less than 1 percent and approximately 5 percent, respectively), and (2) their backgrounds and program treatment seem systematically different from the main group, which would probably require separate analyses (which would necessarily be imprecise for these two additional groups) and would reduce the precision of estimates for the main group.

Data Collection. All three primary research topics (short-term impacts, duration of effects, and benefit-cost estimates) require in-depth data on each sample member that must be obtained from personal interviews. Alternative interviewing strategies were examined to identify the method that would best minimize response errors, cost, and analytical difficulties. We adopted a strategy that consisted of administering four sets of interviews. The first set was administered in person to Corpsmembers at centers and to the comparison sample in their homes. The purpose of the

first interview was to collect baseline data on the pre-enrollment period for the Job Corps sample and the same data over a similar period for the comparison sample. The timing of these interviews represents a compromise between minimizing the length of the recall period and maximizing the length of the observation period.

The first follow-up interviews were administered in person approximately nine months after the baseline. The entire comparison sample and a subset of the Job Corps sample were reinterviewed. The subset of the Job Corps group included all members of the original sample who had terminated from the program at least five months prior to the first follow-up interview (an effective cut-off date of leaving Job Corps by October 15, 1977). This criterion ensured an adequate period of postprogram observation within the constraints of the overall budget and the time permitted for the First Follow-Up Report (Mallar et al., 1978). The first follow-up sample of Corpsmembers had been out of the program for a time ranging from five to nine months, an average of seven months at the time of the survey.

For the second follow-up survey the Job Corps sample size was increased by extending the cut-off date of Job Corps termination from October 15, 1977 to April 15, 1978 (yielding a larger sample size by including 1,462 additional Corpsmembers compared to the first follow-up, and excluding only very long-term stayers who had been overrepresented at baseline). The second follow-up sample of Corpsmembers had been out of the program for up to two years and at least one full year, an average of eighteen months at the time of the survey. The third follow-up sample

was essentially the same as the second,[1] except that Corpsmembers had been out of the program between three-and-one-half and four-and-one-half years, an average of nearly four years.[2]

The only major change in survey procedures between the first and second follow-up surveys involved two decisions--(1) to use a mixed-mode interviewing strategy and (2) to switch the primary interviewing mode from in-person interviewing to telephone interviewing.[3] However, in order to

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[1] The same identical sample was pursued in the third follow-up survey as in the second, except for sample members who had died by the time of the second follow-up and comparison youths who had attempted to enroll in Job Corps. A reduced sample size could have seriously jeopardized the number of observations and the precision of estimates, because the response rate was already expected to be substantially lower for the third follow-up than for the second because of the length of the intervening time period. Furthermore, the only way to increase the sample size would have been to add more of the Corpsmembers who had been in the program too long to be included at the second follow-up. However, it would have been very expensive to find them (they had not been interviewed since baseline), and they were all very long-term stayers in the program who were already overrepresented in our sample.

[2] The increased time period between the second and third follow-up surveys is advantageous because it greatly extends the length of the post-program observation period. However, it places greater burden on the survey procedures, because of the added risks of reduced response rates and increased recall errors. Therefore, the questionnaire and related survey procedures from the second follow-up were carefully reviewed and modified based both on our experience from the previous Job Corps surveys and on recent evidence from other survey research. The same basic questions were asked in the interview, with only slight modifications in order to (1) allow for the longer time period (e.g., more activity grids, multiple changes in marital status, changes in wage rates within jobs, etc.), (2) encourage better recall (e.g., basing the questions on a major event, such as January 1, rather than on previous interview dates), and (3) clarify a few wordings that had caused minor problems in the second follow-up.

[3] This survey process is described further in Technical Reports B, H, and P. The desirability of telephone interviewing at the second and third follow-up surveys is documented in Technical Reports H and P and is supported by the analysis of nonresponse (see Chapter IX and Technical Report L).

reduce the nonresponse problems associated with conducting a telephone survey of a sample that contains a large proportion of disadvantaged youths, in-person interviews were also attempted for sample members who either could not be located by telephone or were located but did not respond to a telephone attempt. The cost-efficient design for the second follow-up led to the targeting of all the comparison-group sites and one-half of the Corpsmember sites for in-person interviews if telephone attempts were unsuccessful. At the third follow-up, we attempted to interview everyone in person (except for a few youths living in remote areas) if they could not be interviewed by telephone.

The comparative evaluation of benefits and costs required additional data. Data on program costs were provided by the national Job Corps office. These cost data were supplemented with information from Job Corps centers on center expenditures that were not included in Job Corps financial data and from the U.S. Office of Management and Budget on federal administrative costs that were not included in the Job Corps financial data. In addition, special studies were made of a random selection of Job Corps work projects in order to value the products and services produced in the projects.[1] Finally, dollar values for many of the benefits had to be imputed from secondary data sources.[2]

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[1] This work is described further in Technical Reports D, E, F, K, and Q.

[2] See Chapter VIII and Technical Reports D, K, and Q for more details.

III. ESTIMATION PROCEDURES

Before presenting our findings on the economic impact of Job Corps, we first discuss the underlying procedures used to estimate the effects of Job Corps on participants' behavior. The purpose of this chapter is to document, explain, and justify the quantitative techniques that were used. Thus, by necessity, this chapter deviates somewhat from the objective of making this report readily accessible and comprehensible to a wide audience of informed policymakers who are interested in education and training programs for disadvantaged youths. Readers who have only a limited understanding of or interest in econometrics might wish to skim this chapter or proceed instead to the substantive findings in Chapters IV through VIII.

We begin this chapter by providing background information on how the data were arrayed. Next, we discuss the regression techniques used to adjust for both observed and unobserved differences between the Job Corps and comparison groups, and then discuss the disaggregations necessary to obtain adequate overall estimates and to understand the overall findings. We conclude by presenting samples of the details on the actual regression estimates.

A. DATA ARRAYS

The additional data from the third follow-up survey enable us to obtain more accurate estimates of Job Corps effects than were previously possible, due to the substantial increase in the length of the postprogram observation period. By the time of the third follow-up survey, the sampled Corpsmembers had been out of the program from 42 to 54 months, with an

average per Corpsmember of nearly 48 months. This represents a 167 percent increase in the length of the postprogram time period compared to the data set at the end of the second follow-up survey, at which time the sampled Corpsmembers had been out of the program from 12 to 24 months, with an average per Corpsmember of approximately 18 months.

The sample for the third follow-up survey was the same as for the second, and, as with the previous follow-up surveys, we attempted to obtain detailed between-interview histories of youths' employment and earnings, education and training, receipt of public assistance, criminal behavior, and related activities.[1/] The full panel of postProgram observations--incorporating all relevant data from the first, second, and third follow-up surveys--was organized into quarterly aggregates for each youth who was ever in the evaluation sample. Quarterly aggregates were used because they are straightforward, preserve most of the variation in the data over time, allow estimates of the several potential Job Corps effects to be computed at feasible cost, and allow the time trends of interest to be examined.

The data were arrayed into quarterly aggregates by calendar quarters according to seasons--summer (June, July, and August), fall (September, October, and November), winter (December, January, and February), and spring (March, April, and May); this delineation of quarters

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[1/] In fact, the time frames for the follow-up interviews overlap. In order to encourage better recall, the beginning dates for the questions were always an earlier, more salient calendar date (e.g., January 1 of the relevant year), rather than the date of the previous interview, which would have been unlikely to have had much intrinsic meaning to respondents. In setting up data arrays and constructing analysis files, we always used the data that embodied the least amount of recall. Examination of the overlap data indicates that there are substantial recall errors, but they do not appear to result in any systematic biases (i.e., they appear to add only "white noise").

differs from the usual fiscal quarters but enables us to take better account of the influences of seasonality. The first quarter for which we obtained any postprogram data for any youth was spring 1977; the last quarter was fall 1981. Therefore, for each youth, we have up to nineteen quarterly observations in the postprogram period that can be pooled in the statistical analysis. The average for someone who was interviewed in each of the follow-ups is approximately seventeen quarters of observations, including partial quarters at the beginning and end of the overall observation period.

Many of the quarterly outcome variables were defined as the fraction of time in the quarter during which the youth was in an activity (for example, the fraction of time employed). The fraction of time provides a very flexible specification that can easily be translated into any desired units of time per time period. For example, to obtain the number of weeks on an annualized basis, we would simply multiply the fraction of time by 52, the number of weeks in a year.

In addition to the fraction of time in important activities, outcome variables were defined as the average or total over the quarter for commonly used units--for example, averages of hours worked per week or earnings per week, and total numbers of arrests per quarter (on a six-month basis, since we disaggregated impact estimates on that basis). For explanatory variables that are not constant over a quarterly time period (i.e., age, calendar time, and length of time out of Job Corps), the quarterly arrays used the value of the variable at the midpoint of the quarter.

B. ECONOMETRIC METHODOLOGY

The objective of our comparison-group and our other econometric procedures is to obtain unbiased estimates of the impact of Job Corps on participant behavior[1] and as precise estimates as possible within the constraints of the project. Since an experimental design was not feasible for this evaluation study,[2] some degree of modeling and assumptions will be necessary in order to draw statistical inferences. Our overall study design has been developed to minimize the amount and maximize the plausibility of the modeling and assumptions underlying the estimates.

In this section we first present the justification for using a comparison-group strategy and establish the necessity of controlling for observed and unobserved differences between the Job Corps and comparison groups. We then outline the actual procedures we used to select the comparison group, to control for observed differences, and to control for unobserved differences, respectively.

1. Justifications for Using a Comparison Group Strategy and for Controlling for Observed and Unobserved Differences Between Corpsmembers and Comparison Youths

An experimental design was not feasible for this evaluation study because of operational considerations (exacerbated by an on-going program)

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[1] We are using "unbiased" here to mean asymptotically unbiased; under usual assumptions, the estimators we develop have the large sample property of statistical consistency.

[2] An experimental design was not feasible due to the difficulties associated with an experimental intervention into the selection process of an on-going national program like Job Corps, and due to the necessity of obtaining postprogram findings within a very short time frame (nine months after baseline), such that any program completers would still be in Job Corps with a new cohort of enrollees. For further details, see Kershaw, Mallar, and Metcalf (1976).

and because the sponsoring agency wanted findings more quickly than could be obtained by following a new cohort of enrollees. As discussed in our previous reports, participants could not serve as their own controls. Before-after differences for participants are inadequate measures of program impacts for youths whose behavior will change naturally with aging and for other groups whose pre-enrollment behavior reflects a transient state and, hence, for whom substantial changes in behavior can be expected to take place over time in the absence of any program impact. Therefore, if an experimental design is infeasible, an effective comparison-group strategy must be devised in order to draw reliable statistical inferences about program impacts.

Even with an efficient comparison-group design and implementation, however, simple program/comparison-group differences in sample means are unreliable estimators of program effects because observed and unobserved differences can be present. In general, with the absence of random assignment, it is not possible to ensure the elimination of unobserved differences with any measurable degree of certainty. Some modeling and assumptions are necessary in nonexperimental contexts in order to draw statistical inferences, and the plausibility of the modeling and assumptions is somewhat judgmental.

Generally, before-after differences would greatly overstate the beneficial economic impact of youth training programs because youths are just beginning to enter labor markets, and their economic prospects would improve substantially with age even in the absence of any training program. In contrast, both the direction and magnitude of bias with simple program/comparison-group differences would be unknown a priori, even if there are

no observed differences between the groups. Youths with the greatest likelihood of participating in Job Corps could be dominated either by those who, for unobserved reasons, perform inordinately well in training programs (i.e., enter Job Corps because they will receive relatively large benefits) or by those who, for unobserved reasons, perform especially poorly outside the program (i.e., enter Job Corps because of poor labor-market performance, causing a low opportunity cost to reducing their effort in the regular labor market and entering the program). Thus, given these competing factors in self-selection and recruitment into Job Corps, youths who choose to participate in the program could be more or less productive in unobserved dimensions than observationally similar nonparticipants.[1/]

Given the above considerations, it seems clear that a multiple-variable statistical approach is necessary--one that controls for both observed and unobserved differences between the Job Corps and comparison groups. In our First Follow-Up Report we relied heavily on a relatively simple regression adjustment that was based on the change over time in program-comparison differences in sample means (or, equivalently, on the program-comparison differences in changes in sample means over time). In addition, we were able to show that our basic findings were not changed substantially for a wide range of more and less complicated econometric techniques based on very different assumptions, although the estimated dollar value of the benefits did vary considerably (see Mallar, 1979).

However, the change-in-differences methodology assumes that if Corpsmembers had not entered Job Corps, the growth rates in the outcome

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[1/] Furthermore, we cannot infer that the biases are small in magnitude simply because the direction is ambiguous.

variables of interest would be the same over time on average for the Corpsmember and comparison groups--and such an assumption becomes less tenable as the period of postprogram observation lengthens. Further, with added observations (i.e., more degrees of freedom) and recent developments in computational procedures, less restrictive techniques (which are more complex computationally) become practicable. Therefore, in the analysis for both the Second Follow-Up Report and the present report, we used regression approaches that control for both observed and unobserved differences between the Job Corps and comparison groups, but which have less restrictive underlying assumptions than the change-in-differences technique.

2. The Comparison Group Methodology

Our comparison-group strategy was designed to yield a sample of youths who were similar to Corpsmembers, but who did not enter the program largely because they had little or no knowledge about the program. The basic comparison-group procedure entailed the random selection of eligible nonparticipants who were similar to Corpsmembers in observed characteristics and who lived in geographic areas similar to the neighborhoods where Corpsmembers lived before entering Job Corps centers.[1/] In addition, sites that were very proximate to centers--neighborhoods within the three-digit ZIP code areas where centers were located--were eliminated from consideration as comparison sites. The rationale for eliminating these sites was based on the fact that participation in and knowledge of the Job Corps program was likely to be prevalent in these sites, so that youths who lived there but who did not participate in Job

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[1/] For more details, see Section B of Chapter II.

Corps were suspected of being dissimilar to Corpsmembers. This comparison strategy was feasible for four reasons: (1) there were large numbers of eligible youths who were similar in observed characteristics to participating Corpsmembers but who did not attempt to enter Job Corps,[1/] (2) program participation was geographically clustered because of historical locations and limited recruitment, (3) there were many areas of the country where Job Corps did not recruit heavily but which were otherwise similar to the recruitment areas, and (4) most Corpsmembers first learned about the program from friends or relatives (see Kerachsky and Mallar, 1978).

A comparison-group procedure yielding a sample of youths who are similar in observed characteristics to Corpsmembers but who do not participate largely because they do not know about the program should yield a relatively efficient comparison group (see further below). Members of the comparison sample should be similar to Corpsmembers, and, as a result, estimates of program effects should be less sensitive to varying the assumptions about observed and unobserved differences between the program and comparison groups than under other potential comparison-group methodologies (see Mallar, 1979). However, the comparison sample could still differ substantially from Corpsmembers either in observed characteristics, by chance, or in unobserved dimensions, by chance, or because Corpsmembers come from an extreme tail of the distribution of important unobserved factors, while the comparison sample is drawn more randomly across the spectrum.

[1/] For example, the National Commission for Manpower Policy (1978) estimated that 132 eligible youths were available for every Job Corps slot.

3. Controlling for Observed Differences Between Corpsmembers and Comparison Youths

Our basic econometric model of behavior suggests that the behavior of interest (e.g., employment and related activities) is affected by Job Corps treatments, other observed explanatory variables, and unobserved factors. This can be approximated by a single-equation, linear regression:

$$Y_{it} = \underset{\sim}{\beta}'\underset{\sim}{X}_{it} + \underset{\sim}{\gamma}'\underset{\sim}{T}_{it} + \varepsilon_{it}, \qquad (1)$$

where Y_{it} is the economic behavior of interest for the i^{th} individual during the t^{th} time period; the β's are coefficients for explanatory variables; the X's are explanatory variables (exogenous) and lagged values of dependent variables from pre-enrollment that explain the behavior of interest; the γ's are program effects on the behavior of interest; the T's are program-treatment variables; and ε is an error term.[1/]

Table III.1 documents the explanatory variables that are included in our regressions as direct controls for observed differences between Corpsmembers and comparison youths. These variables are based on factors that have previously been found to affect productivity, employment, and related behavior. They control for age (5 variables); pre-enrollment education (3 variables); race/ethnicity (4 variables); pre-enrollment health (1 variable); seasonality (3 variables) and time trends (1 variable);

[1/] The types of behaviors of interest in this study are related. Therefore, some gains in statistical efficiency could be achieved by using a multiple-equation technique that accounts for correlations in error terms across equations, such as a seemingly unrelated regression procedure. However, the gains in statistical efficiency with a seemingly unrelated regression approach would be small at best, because the explanatory variables are nearly identical for all of the types of behavior that we examine.

and pre-enrollment experiences with employment (1 variable), welfare (1 variable), illegal activities (1 variable), and drug use (2 variables). The explanatory variables will adjust for any observed differences in these dimensions and will also yield smaller estimated standard errors for the coefficients of the Job Corps treatment variables by reducing the estimated standard error of the regression. (These are good arguments for using the regression approach, even when an experimental design is implemented.) Even though our earlier findings (see Kerachsky and Mallar, 1977) suggested that the Job Corps and comparison groups were reasonably well matched on observable variables, it is still prudent to control for any remaining differences.

In developing the explanatory variables, we used current values only for factors that absolutely could not be affected by Job Corps participation. If Job Corps participation could conceivably affect the explanatory variable, then a pre-enrollment value was used. This ensures that the coefficients for the Job Corps variables measure the total impact of program treatments and are not diluted by other variables that indirectly measure part of the impact (although at some cost in terms of less explanatory power and larger standard errors for the regressions).

The correlation of error terms for the same individual over time was accounted for in an error-components (or variance-components) model. This model should yield greater efficiency for coefficient estimates and more accurate estimates of standard errors than ordinary least squares (OLS) estimates (for more details, see Maddala, 1971; Nerlove, 1971a and

1971b; and Wallace and Hussain, 1969).[1] A two-stage procedure was used with the variance component for individual youths, estimated from a first-stage OLS regression and then substituted into a second-stage generalized least squares framework.[2]

The computational routine that we used enabled us to include individuals with varying lengths of postprogram observation, and allowed individuals to have missing periods of data (early, late, or intervening quarters).[3] Allowing for varying lengths of time and missing quarters is an essential feature of our application, since Corpsmembers left Job Corps at different times, and, hence, their postprogram data start at varying points in time (as much as one year apart). Also, different youths missed various interviews or could not recall an intervening date or other piece of information that affects some quarters but not others, and we want to use all available information. Seasonality and time trends across

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[1] The point-in-time probability models with binary dependent variables (e.g., the probabilities of military service during the survey week, of having a high school diploma or GED by the survey week, and of being in jail during the survey week) were estimated with probit maximum likelihood techniques, with one observation per individual youth.

[2] The lagged values of dependent variables from pre-enrollment cannot reasonably be assumed to be strictly predetermined when we pool observations for individual youths over time. These lagged dependent variables are generally endogenous if we assume (as we must) that equation error terms are correlated over time for individuals. However, using estimators of error variances and covariances from ordinary least squares residuals will still yield consistent estimators of coefficients in a second-stage generalized least squares technique when the error-components model is appropriate, as we assume in our estimation procedure (for more details and proofs, see Wallace and Hussain, 1969, and Nerlove, 1971a and 1971b).

[3] For documentation of the computational routine, see Avery (1975).

individuals are specified explicitly in the regression equations (the error term for our econometric model includes only an individual component, and no time component).

4. Controlling for Unobserved Differences Between Corpsmembers and Comparison Youths

In addition to controlling for observed factors that affect the behavior of interest, we should use an econometric methodology that controls for unobserved differences between samples--specifically, in this application, to control for unobserved differences between youths in the Job Corps and comparison groups (for example, unobserved differences in employability, trainability, and motivation). With the technique we use to control for unobserved differences, the basic procedure entails modeling and estimating program participation and then including in the regression equations a control variable that is a function of the estimated probability of program participation.

With a nonrandomized control group (a comparison group) the program-treatment variables, T's, in equation (1) are potentially correlated with the error term, ε. Many important unobserved variables that affect the economic behavior of interest are also likely to affect individual decisions about whether to participate in the program and, hence, the T's. Therefore, the T's are potentially endogenous with respect to the behavior of interest, in which case least squares (LS) regression estimators will generally be biased.

As an example, variables such as innate ability and motivation are not observed directly; however, they undoubtedly affect both employment-related behavior and the decision about whether to participate in Job Corps (or in other similar programs). If youths are motivated to maximize their

incomes, then any variable that affects employment and income will also affect decisions about whether or not to attempt to participate in Job Corps. Thus, the same unobserved variables can be important elements in the error terms for both Y and the T's with a comparison group. This is in contrast to the situation with a randomized control group, in which unobserved (as well as observed) variables should tend to be orthogonal to (or uncorrelated with) the T's. When a comparison group is used, the T's will generally be correlated with ϵ, and the program variables should be treated as endogenous. Furthermore (as is well known), LS estimators will generally be biased when endogenous variables are included in regression equations, unless the correlation between the endogenous variables and the error term (ϵ) can be netted out.

Different procedures for selecting comparison groups in the absence of randomization will lead to varying levels of (1) statistical inefficiency from correlation between the X's and T's, because the program and comparison groups do not match well with respect to observed variables, and (2) LS bias from correlation between the T's and ϵ when the groups do not match well with respect to unobserved variables and when program effects, the γ's, are estimated via LS. Some procedures that have been used to obtain comparison samples yield very poor matches and, hence, lead to very inaccurate estimates for the γ's with either difference-in-sample-means or LS estimation procedures (e.g., evaluations that use the pre-enrollment experience of participants as a comparison and rely on before-after comparisons for youths or other new entrants or re-entrants to the labor force, those that use individuals who enrolled but did not show up for the program, and those that use individuals who dropped out of the program very soon after entering).

However, our comparison-group procedure outlined above should be relatively efficient and have relatively little LS bias. If, as designed, comparison-group members differ from Corpsmembers primarily in terms of random access to information about the program and random proximity to the program, then the X's and T's and the T's and ε should be weakly correlated. In this case, estimates of program effects will not differ too much, depending on whether we control for observed differences between the Job Corps and comparison groups, unobserved differences, both (observed and unobserved differences), or neither.[1/]

Even with the rigorous comparison-group procedures that we developed, interview data and any available secondary data should be used to control for both observed and unobserved differences that remain between the comparison and program-participant groups. Only in that way can we ensure against bias in the estimates of program effects. In principle, consistent estimates of program effects can be obtained by controlling for observed differences directly and by controlling for unobserved differences indirectly through a model of the participation decision.[2/]

[1/] Some of the discussion is phrased as if there were only one (binary) program variable, which might or might not be the case. However, the results generalize to multiple T's, as is the case for our application (see further below), as long as the selectivity being modeled is only between the Job Corps and comparison groups. It becomes much more difficult, in practice, to control for selectivity into various types of program treatments. Often, the same factors affect the decisions about whether different program services are received; hence, the statistical identification of models of selectivity into multiple components of programs is almost always difficult and often impossible.

[2/] For more details, see Heckman (1979), Barnow, Cain, and Goldberger (1978), and Mallar (1979).

If a normal distribution is assumed for the error term in equation (1), that error term can then be decomposed into two components--an estimable component that is correlated with the T's, and another component that is uncorrelated with the T's and has the properties of an LS error term. Thus, equation (1) can be rewritten as:

$$Y_{it} = \underset{\sim}{\beta}'\underset{\sim}{X}_{it} + \underset{\sim}{\gamma}'\underset{\sim}{T}_{it} + \delta\lambda_i + \omega_{it} , \qquad (2)$$

and

$$\lambda_i = P_i \frac{f(\underset{\sim}{\alpha}'\underset{\sim}{Z}_i)}{F(\underset{\sim}{\alpha}'\underset{\sim}{Z}_i)} - (1 - P_i) \frac{f(\underset{\sim}{\alpha}'\underset{\sim}{Z}_i)}{1 - F(\underset{\sim}{\alpha}'\underset{\sim}{Z}_i)} , \qquad (3)$$

where P_i is a binary program-participation variable that equals one for Corpsmembers (zero otherwise); $f(\cdot)$ denotes the standard normal density function for program participation; $F(\cdot)$ denotes the standard normal distribution function for program participation; the Z's are explanatory variables that affect program participation; and δ and the α's are coefficients.

We can estimate λ for each individual youth (Job Corps or comparison group) by estimating the α's with a probit equation for participation in Job Corps. The estimated values of λ will be close to zero for Corpsmembers who have a high estimated probability of participation in Job Corps and for comparison youths who have a low estimated probability of participation. In other words, λ will be close to zero for cases that are correctly classified by the participation model and, thus, that have small errors in the probit equation estimated for P.

The estimated values of λ will become progressively larger positive numbers for Corpsmembers who have lower estimated probabilities of

participation (i.e., youths who participate in Job Corps for unobserved reasons) and, hence, who have larger positive errors in the probit equation estimated for P. Similarly, the estimated values of λ will become progressively more negative for comparison youths who have higher estimated probabilities of participation (i.e., youths who do not participate in Job Corps for unobserved reasons) and, hence, who have more negative errors in the probit equation estimated for P.

The sign of δ is determined by the correlation of the errors (e.g., from omitted variables) in the underlying equations for Y and P. If the estimated value for δ is positive, it indicates that individuals who have higher values of λ and are more likely to participate in Job Corps for unobserved reasons will also have higher values of Y, on average, for unobserved reasons. Therefore, when δ is positive, the failure to adjust for sample selectivity (i.e., unobserved differences) will bias program effects in a positive direction for Y, because youths with high Y's for unobserved reasons will tend to be program participants for unobserved reasons.[1/] Similarly, if the estimated value for δ is negative, it indicates that individuals who are more likely to participate for unobserved reasons will have lower values of Y, on average, for unobserved reasons, in which case the failure to adjust for sample selectivity will bias program effects in a negative direction for Y.

[1/] Another way to see the direction of LS bias from not controlling for unobserved differences between Corpsmembers and the comparison group is to note that a positive correlation exists between the T's and λ. Therefore, if δ is positive (negative) and we do not control for unobserved differences--the $\delta\lambda i$ term is omitted from equation (2)--then part of the positive (negative) effect of the unobserved differences--the omitted term, $\delta\lambda i$--will incorrectly be attributed to the T's.

As noted previously, when estimating Job Corps effects on employment and related activities, the bias stemming from the failure to adjust for sample selection could be in either direction. A positive δ and positive (i.e., upward) bias for the γ's from omitting λ will occur for employment and earnings if there is a predominance of youths with higher unmeasured abilities who are more likely to participate in Job Corps (e.g., because they benefit more from the training opportunities). A negative δ and negative bias for the γ's from omitting λ will occur for employment and earnings if there is a predominance of youths with lower unmeasured abilities who are more likely to participate in Job Corps because it costs them less (i.e., fewer opportunities outside of Job Corps).

If a consistent estimate of $F(\cdot)$ is obtained through probit procedures, then consistent estimates for all of the coeff ents in equation (2) can be obtained from LS by substituting the resulting predicted values of $\hat{\lambda}$ into equation (2). However, the standard errors and t-statistics for the estimated coefficients might be biased slightly if the predicted $\hat{\lambda}$'s are used in a typical regression package. The typical regression programs will not account for the implicit heteroscedasticity involved in controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach of using predicted $\hat{\lambda}$'s. In practice, however, the standard errors and t-statistics from the typical regressi packages are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are small and/or statistically insignificant (which is often the case with our estimates).

Therefore, the standard errors and t-statistics from typical regression packages are approximately accurate and are indicative of the true values of these statistics. Maximum likelihood estimates could yield some gains in terms of (1) the statistical efficiency for coefficient estimates and (2) unbiasedness for estimates of the standard errors. With our large sample sizes, however, maximum likelihood estimation would be prohibitively expensive for gains that are likely to be only very small.

Another issue that arises when using the above procedures to control for unobserved differences between program and comparison groups is whether equation (2) is statistically identified when predicted values of λ are used, as they must be because λ is inherently unobservable. Conceptually, as suggested by Barnow, Cain, and Goldberger 1978), equation (2) is statisticaly identified by the inherent nonlinearities in the model for λ and Y, even if the X's and Z's are identical. In practice, however, these nonlinearities by themselves often turn out to be ineffective for statistically identifying behavioral models, and they can lead to multicollinearity if used alone.[1/] Parameter restrictions are necessary for ensuring identification in models like those represented by equation (2). The parameter restrictions amount to obtaining Z variables that can reasonably be modeled as affecting the decision to participate in Job Corps, P, but that do not directly affect the behavior of interest (i.e., are not among the X variables in equations (1) and (2)).

Variables associated with youths' knowledge about Job Corps (information, perceptions, etc.) are potential candidates for identifying

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[1/] The λ function is nearly linear across a broad range of probabilities, P (from approximately 0.2 to 0.8).

the employment and related behavior of interest to this study. Differential knowledge about Job Corps will undoubtedly affect youths' decisions about whether or not to participate in the program, and, by itself, such knowledge should not have a direct effect on employment and related behavior. Based on our earlier findings that recruitment for Job Corps differs substantially across geographic areas (see Kershaw, Mallar, and Metcalf, 1976) and that friends and relatives are by far the most important sources of information about Job Corps (see Kerachsky and Mallar, 1978), we developed two proxy variables for knowledge of Job Corps from the pre-enrollment addresses of Corpsmembers and comparison youths. These two proxy variables (#JCMEM-75 and %JCMEM-75) indicate, respectively, the number and fraction of youths from pre-enrollment neighborhoods who participated in Job Corps during the period just prior to when our sample was deciding whether or not to attempt to enroll in Job Corps. More specifically, these two knowledge proxies are obtained from data on Job Corps enrollments by three-digit ZIP code areas during fiscal 1975.

The greater the number of previous Corpsmembers from a youth's neighborhood, ceteris paribus, the more likely the youth is to know about Job Corps and, hence, to participate in the program. Therefore, these knowledge proxies should be important variables for explaining observations about whether or not youths participate in Job Corps.[1/] Furthermore, the amount of Job Corps participation in youths' pre-enrollment neighborhoods,

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[1/] The high correlation between the knowledge proxies and Job Corps participation for our sample is partly an artifact of the sample design (i.e., Job Corps saturation areas proximate to centers were not allowed to be comparison sites). However, that statistical artifact has only a positive effect on the suitability of the knowledge proxies as identifying variables.

per se, should not have a direct effect upon their postprogram employment and related behavior,[1/] so it is plausible to exclude #JCMEM-75 and %JCMEM-75 from the list of X variables. Therefore, #JCMEM-75 and %JCMEM-75 appear to satisfy the properties of variables to identify equation (2)--they belong in the set of Z variables but not in the X variables.

Even if these knowledge proxies or other characteristics of re-enrollment neighborhoods are added to the estimating equations for employment and related behavior, they generally will not affect the estimates of Job Corps impacts.[2/] By design, the pre-enrollment neighborhoods of Job Corps and comparison youths should be similar in terms of the labor-market characteristics and other relevant factors for the employment and related behavior of youths.[3/] Our previous findings (see Kerachsky and Mallar, 1977) substantiate these expectations of labor-market similarities. First, the earlier findings show that the pre-enrollment neighborhoods of the Corpsmember and comparison samples are, on average, very similar with respect to important dimensions such as population density, local youth unemployment, numbers of youths, income, non-aged welfare dependence, education, race/ethnic composition, etc. Second, the earlier findings indicate that the omission of characteristics of

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[1/] The postprogram period is two to six years later, and most of the Corpsmembers no longer even lived in these neighborhoods.

[2/] However, the standard errors of impact estimates would be much larger if the knowledge proxies were added to the equations for employment and related behavior, because only slight nonlinearities in the probability equation would be statistically identifying the model.

[3/] The local comparison sites were drawn randomly but with systematic procedures that helped ensure comparability to the neighborhoods of Corpsmembers in terms of population density, geographic dispersion, non-aged welfare dependence, and race/ethnicity.

pre-enrollment neighborhoods from the equations for employment and related behavior will not affect the estimates of Job Corps impacts.

For estimating Job Corps impacts, the pre-enrollment labor markets can be viewed as similar across observations, and the lack of explanatory variables to control for differences in pre-enrollment labor markets cannot be used as an argument to include in the employment and related equations local variables--such as #JCMEM-75 and %JCMEM-75--that do not otherwise belong there. Because the local proxies for knowledge of Job Corps are not expected to affect the employment and related behavior of youths directly, they can be omitted from the employment and related equations. Thus, by providing a comparison sample with pre-enrollment labor-market backgrounds that are similar to those of Corpsmembers, our comparison-group design helped ensure the plausibility of the one critical assumption used in our empirical model to control for unobserved differences between Corpsmember and comparison youths[1/]--namely, that #JCMEM-75 and %JCMEM-75 can be omitted from the employment and related equations.

The other explanatory variables used in the Job Corps participation equations are very similar to those used in the equations for employment and related behavior (identical, except for slight differences in functional form; see Table III.1). The primary difference between the Job Corps participation equations and the equations for employment and related behavior is that the proxy variables for knowledge of Job Corps are

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[1/] The other assumptions or maintained hypotheses of our model are more commonly used in econometric analysis--the explanatory variables included in equation (2) other than λ, the normality of error terms, the error-components model of the correlation of individuals' error terms across time periods, etc.

included as explanatory variables in the Job Corps participation equations but are omitted from the equations for employment and related behavior. Thus, the proxies for knowledge of Job Corps help statistically identify the employment and related equations, in addition to the statistical identification provided by the inherent nonlinearities in the model.

With the econometric procedures outlined above we should obtain consistent estimates of the impact of Job Corps on participant behavior. Thus, in principle, the estimates of Job Corps effects presented in subsequent chapters are based on differences between groups of Corpsmembers and comparison youths that have similar compositions in terms of both observed and unobserved characteristics. These procedures should also enable us to obtain unbiased estimates of what Corpsmembers' activities would have been had they not participated in Job Corps, by netting out (i.e., subtracting) estimated Job Corps effects from the observed sample means for Corpsmembers.

C. DISAGGREGATIONS USED IN THE ESTIMATION

While this report focuses on the <u>overall</u> effects of Job Corps on participants' behavior, some disaggregations are necessary in order to obtain accurate overall estimates, as well as to better understand the overall estimates. Disaggregations and reweighting are essential to ensure accuracy when the unweighted sample overrepresents some segments of Corpsmembers and underrepresents others and when the behavior of interest differs among subgroups. Two general types of disaggregations were undertaken in our analysis: (1) separate estimates of regression equations for subgroups of the population that have completely different behavioral relationships for the activities of interest, and (2) decompositions of the

program-treatment specification to capture hypothesized differentials in observed Job Corps impacts within subgroups whose behavior is otherwise similar.

Subgroups of the Population. In general, we have pooled observations across individuals and time to take full advantage of the panel nature of the data (discussed further below). However, separate estimates are computed for three subgroups of youths, based on their personal characteristics with respect to sex and child responsibilities, as follows: (1) males, (2) females who have no children present for whom they are responsible, and (3) females who have children living with them for whom they are responsible.[1/] Our previous research (see Mallar et al., 1978) found that the behavioral relationships of interest were substantially different for these three subgroups, based on statistical tests (Chow tests) for any differences in parameters among subgroups. With an appropriate specification, however, we found that observations on youths could be pooled together across other demographic classifications, such as child responsibilities for males, age, race/ethnicity, and marital status.[2/]

Males represent the largest of these Corpsmember subgroups--approximately 70 percent of all Corpsmembers. Females, who represent only 30 percent of all Corpsmembers, were intentionally overrepresented in the comparison group (approximately a 50/50 split between males and females in

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[1/] In addition to at least one child living with the female, these subgroup definitions for females depend on whether or not the female and/or her spouse are the parent or legal guardian of the child(ren).

[2/] We found that the primary differences in behavior for these latter subgroups can be captured with simple specifications (e.g., dummy variables for age, race/ethnicity, and marital status).

the comparison group) in order to increase the precision of separate estimates for females. However, the sample sizes for the female subgroups are relatively small, in part because the necessity of disaggregating the female subgroup by presence of children was not completely anticipated. Unexpectedly, the comparison group, relative to the Corpsmember sample, overrepresents females who have children living with them, which exacerbates the need for separate estimation but provides some added precision for this smallest subgroup of Corpsmembers (see further below).

For the overall postprogram observation period, the Job Corps sample consists of approximately 70 percent males, 21 percent females without children, and 9 percent females with children present (the corresponding average numbers of observations per time period are, respectively, 2,700, 800, and 350). However, the female Corpsmember sample begins with almost no children present and works its way up to approximately one-half having children present (15 percent of the overall sample) by the end of the four-year observation period. The corresponding overall percentages for the comparison group are 48, 25, and 27 percent for, respectively, males, females without children, and females with children (the corresponding average numbers of observations per time period are, respectively, 650, 340, and 360). Only the male subgroup has adequate numbers of observations to achieve very precise estimates, especially when the Job Corps samples and estimates are broken down by completion categories--program completers, partial completers, and early dropouts (see further below).

The appropriate Job Corps proportions, by time period for females, are used for weighting separate estimates to obtain the overall estimates of Job Corps effects. However, the female subgroups pose additional

problems for analysis. First, we have undertaken only a limited exploration of the impacts of Job Corps on marriage and on the fertility of Corpswomen (e.g., timing and number of births), and further observation and research on these topics are necessary. Potentially, some of the largest impacts of Job Corps on females' behavior could come from decreases in fertility (delayed timing of births and reduced number of births), which could lead to increases in employability and reductions in welfare dependence. Separate estimates based on the presence and absence of children completely miss the impacts of Job Corps that are caused by changes in the child-responsibility status under which former Corpswomen are observed. Also, Corpswomen with children are more likely to exhibit delayed marriage and childbearing and, hence, to be just starting families. Consequently, Corpswomen with children will tend to have very young children during our observation period, which, in the short run, greatly limits their ability to work and will cause the estimates to be biased downward (in a negative direction) for females with children.

The second problem in the analysis of impacts on females is the extremely small sample sizes and the concomitant instability of estimates for Corpswomen, especially for those who have children living with them. For example, during the postprogram period we observe only fifteen females who were early dropouts from Job Corps and who had children living with them by the time of the second follow-up survey. As a result, it is not surprising to find that the estimates of Job Corps impacts on employment and earnings fluctuate erratically for the subgroup of females with children, and, for them, the estimates are very sensitive to changes in the specifications of the control variables in the regression equations.

Program-Treatment Specifications. Two disaggregations of Job Corps statuses among participants are used to improve the overall accuracy of estimates and to explicate the findings: (1) separate estimates by completion category and (2) interactions with the length of time since leaving the program. The program effects are expected to vary both across completion categories and by length of time out of the program. In addition, the postprogram sample contains distributions that are unrepresentative of all Corpsmembers in both of the above dimensions (and, hence, which will require some reweighting), and for Corpsmembers, on average, the postprogram observation period is shorter the greater the length of stay in the program.

As discussed briefly in Chapter II, our sample design overrepresents program completers because youths who remain in Job Corps for a long period of time have a higher probability of being at centers at any point in time and, specifically, when the sample was drawn. The three program-completion categories used by Job Corps--program completers, partial completers, and early dropouts--are convenient to use for reweighting because data are readily available on their actual proportions among all Job Corps enrollees. However, a perfect correlation does not exist between length of stay in Job Corps and these completion categories, since, given the individualized and self-paced nature of Job Corps instruction, some youths can complete the program faster than others.

A program completer is, as the designation indicates, a Corpsmember who completes an entire vocational and/or education program in Job Corps. A partial completer is defined as a Corpsmember who remains in the program for at least ninety days and who completes at least one specific segment of a vocational or education program, but not an entire program. Early

dropouts are defined as youths who terminate from Job Corps before the end of their first ninety days at a center and who do not complete any specific component of the program.

For fiscal 1977 the proportions of all Job Corps enrollees who become program completers, partial completers, and early dropouts are, respectively, approximately 30, 30, and 40 percent. In contrast, the proportions of program completers, partial completers, and early dropouts in our postprogram sample are, respectively, approximately 56, 35, and 9 percent. Therefore, in order to obtain impact estimates that are applicable to the average for all Job Corps enrollees, we must use our knowledge of the "correct" proportions by completion statuses to reweight the observations. Estimates are computed separately for program completers, partial completers, and early dropouts, and are then added together with weights of, respectively, 0.30, 0.30, and 0.40.[1/] The

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[1/] To obtain estimates that are representative of all Corpsmembers, we reweighted the separate estimates by completion statuses as follows:

Estimated effect for all enrollees =
0.30*(estimated effect for program completers)
+ 0.30*(estimated effect for partial completers)
+ 0.40*(estimated effect for early dropouts).

In addition, note that the relationship between the unweighted estimate for the sample and the separate estimates by completion categories is as follows:

Unweighted estimate for sample =
0.56*(estimated effect for program completers)
+ 0.35*(estimated effect for partial completers)
+ 0.09*(estimated effect for early dropouts).

This clearly shows how the unweighted estimate overrepresents Corpsmembers who are completers. Because the estimated impacts of Job Corps are usually much larger for completers, the effect of the reweighting to obtain estimates that are representative of all enrollees (and, hence, giving completers less weight than in the sample) is to lower the overall estimates compared to unweighted estimates.

unweighted estimates would generally overestimate Job Corps benefits, because the overrepresented group of program completers is usually observed to do better in the labor market than the other groups.

The small sample of early dropouts, together with their relatively large weight, causes some instability and erratic fluctuations in the estimates. Essentially, our sample design assumed a zero effect for early dropouts, and they were included primarily as a check on our econometric techniques to test whether we obtain a zero effect for them. Generally, our estimates for early dropouts are insignificantly different from zero but are erratic because of the small underlying sample. We would obtain more stable and more precise estimates by imposing the assumption of zero effects for early dropouts, which would also yield more accurate estimates (increased precision with no added bias) if both that assumption and our model were "correct."

With the data available, we have not been able thus far to obtain reliable estimates that control for unobserved differences among Corpsmembers by completion category, due to identification problems in modeling multiple completion statuses simultaneously with employment or other related behavior. However, even though some biases might exist among completion categories, the weighted estimates for overall impacts should be unbiased. (We know the "true" proportion necessary for reweighting each category.) Furthermore, the evidence supports the conclusion that the observed differences by completion category are at least in part

attributable to a program effect from staying longer and completing more of the program.[1/]

With quarterly data for an average of nearly four years of postprogram observation, we are able to learn a great deal about the timing of impacts after Corpsmembers leave the program. Our examination of the timing of effects has already been fruitful both in identifying transition problems as Corpsmembers leave the centers and re-enter the regular labor market and in noting little change over the first two postprogram years after this transition. Furthermore, with the third follow-up data we are better able to test for the alleged quick "fadeout," or "decay," of Job Corps effects that has been suggested previously with less rigorous techniques and less accurate data. Estimates of the interactions between completion categories and length of time out of Job Corps are also important, because we have fewer observations as the length of the postprogram period increases, and because the observations that we do have for the longest postprogram periods are for youths who had shorter stays in Job Corps than the average.

We pool all of the quarterly observations for each individual youth and estimate two types of specifications by length of time out of the program--(1) six-month averages (nine variables for each completion category, although only eight are typically tabulated because we have so

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[1/] Briefly, we obtain statistically significant and moderate-size effects for the program as a whole; the estimated effects for the group with near zero treatment (early dropouts) are close to zero; we control for a wide range of important variables that are observed; and the potential sources of remaining bias work in opposite directions.

few observations for the ninth time period), and (2) a more flexible, continuous time pattern (a piecewise linear specification that allows the slope to change periodically--at first monthly and then quarterly--at twenty points in time and that is based on twenty-one variables for each completion category.[1] The six-month averages are presented in tables, discussed extensively in the text, and form the basis for the benefit-cost estimates in Chapter VIII. The more continuous time patterns are presented in figures and provide the most comprehensive evidence both on the general timing of effects and, specifically, on the duration of Job Corps impacts (how long they are maintained, how quickly they fade out, or how much further they grow).

Underlying the estimates presented in tables throughout this report, Job Corps effects by program-completion status--program completers, partial completers, and early dropouts--were obtained (see below in Tables III.5 through III.16) separately for each six-month postprogram time period (eight or nine six-month time intervals altogether for the four postprogram years), and separately for each of the three subgroups of sex and child responsibility--males, females who have no children present, and females who have children living with them--whose behavior is so different as to necessitate completely separate estimations. For each six-month

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[1] Correlations of individual errors over time are adjusted in a two-stage error-components (or variance-components) model that should yield greater efficiency than ordinary least squares (for more details, see Maddala, 1971; Nerlove, 1971a and 1971b; and Wallace and Hussain, 1969). The computational program used enables us to include individuals with varying lengths of time (essential for our application) and allows individuals to have missing periods of data (early, late, or intervening quarters). For documentation of the computer routine, see Avery (1975). As noted above, seasonality and time trends across individuals are specified explicitly in the regression equations.

postprogram period, we then obtained estimates of Job Corps effects and the corresponding levels of statistical significance for each of the three subgroups of sex and child responsibility by computing the weighted average of the estimates by completion status, using the Job Corps population weights--0.30, 0.30, and 0.40--for, respectively, program completers, partial completers, and early dropouts.

Next, for each six-month postprogram period, we obtained overall Job Corps estimates (representative of all Corpsmembers as a whole) and the corresponding levels of statistical significance by computing the weighted average of the estimates by subgroup, using the Job Corps population weights for males, females without children, and females with children.[1/] Finally, simple averages over the six-month time periods were then computed to obtain annual estimates.

D. DETAILS OF REPRESENTATIVE ESTIMATES

Tables III.2, III.3, and III.4 show the probit estimates for the probability of being in Job Corps for, respectively, males, females without children, and females with children. The two most imporant points to note are that (1) the identifying variables (#JCMEM-75 and %JCMEM-75) are highly significant and, hence, adequately identify the equations for employment and related behavior, and (2) these probability estimates are exceptionally

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[1/] The appropriate weight for males is 0.70 for all nine of the six-month time periods. The appropriate weights for females without children decline over time as more of the Corpswomen have children; these weights are 0.28, 0.26, 0.22, 0.21, 0.20, 0.18, 0.15, 0.15, and 0.15 across the nine respective six-month time periods. Correspondingly, the appropriate weights for females with children are 0.02, 0.04, 0.08, 0.09, 0.10, 0.12, 0.15, 0.15, and 0.15 across the nine respective six-month time periods.

good predictors of who in our sample is in the Job Corps program versus the comparison group, as evidenced by the exceptionally high chi-square statistic for the equation. As expected, youths who lived in neighborhoods with a greater number of previous Job Corps participants were more likely to participate themselves (expected as a result of their greater awareness and knowledge of Job Corps and because of our sample selection procedures). The other coefficients are difficult to interpret and vary from one subgroup to another, but they do show some other statistically significant differences between the Job Corps and comparison groups (which is not surprising given our sample sizes and the explanatory power of the knowledge variables.

Tables III.5 through III.16 show representative estimates for the employment and related behavior of interest to this study; Tables III.5, III.6, and III.7 show the details of regression estimates for employment; Tables III.8, III.9, and III.10 show the details of regression estimates for college attendance; Tables III.11, III.12, and III.13 show the details of regression estimates for any cash welfare; and Tables III.14, III.15, and III.16 show the details of regression estimates for number of arrests.[1/] The coefficients on the Job Corps variables in these representative regression equations are those used to construct the tables of impact estimates in subsequent chapters.

[1/] The standard errors and t-statistics given in Tables III.5 through III.16 might be biased slightly because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables

The coefficient estimates for the lambda variables (i.e., δ) are often small and statistically insignificant. However, it is important to consider them because in some instances (e.g., male employment and earnings) they can have a substantial effect on the estimates of Job Corps impacts. For employment and college attendance, the coefficient estimates for the lambda variables are negative for males and females without children. This indicates that these groups of Corpsmembers would tend to be less employable (especially males) and to have lower college attendance than the comparison sample in the absence of Job Corps, and that smaller estimates of the positive Job Corps effects would have been obtained had we not controlled for unobserved differences between the Corpsmember and comparison groups. The opposite was true of females with children (a relatively large and significantly positive δ, which indicates greater employability and higher college attendance for unobserved reasons, and more positive estimates had we omitted λ and not controlled for unobserv. differences between the Corpsmember and comparison groups).

The coefficient estimates for the lambda variables in the welfare and arrest equations are positive for all three groups, although generally small. This indicates that Corpsmembers would tend to be slightly more welfare dependent and criminally inclined than the comparison sample in the

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are small or statistically insignificant (which is often the case with our estimates). Therefore, the standard errors and t-statistics shown are approximately accurate and are indicative of the true values of these statistics. Maximum likelihood estimates could yield slight gains in terms of statistical efficiency for coefficient estimates and of unbiasedness for estimates of the standard errors. With our large sample sizes, however, maximum likelihood estimation would be prohibitively expensive, with little gain in terms of the statistical properties.

absence of Job Corps, and that slightly smaller estimates of reductions in welfare and crime for Corpsmembers would have been obtained had we not controlled for unobserved differences between the Job Corps and comparison groups.

Overall, a consistent pattern for males and females without children exists: (1) for unobserved reasons, Corpsmembers would do less well than the comparison group in the absence of Job Corps and (2) controlling for these unobserved differences will make the estimates of Job Corps impacts more positive (i.e., higher estimated benefits). This indicates that for males and females without children those enrolling in Job Corps were predominantly youths who would have had trouble obtaining gainful employment and, hence, for whom the opportunity cost of enrolling in Job Corps was low. For females with children, the evidence is mixed; it appears that these Corpsmembers would fare much better in employment for unobserved reasons (lower estimated beneficial impacts) and would perform slightly better in college, but would do slightly worse in terms of welfare dependence and criminality.

There are also interesting aspects to the other control variables, which will not be fully developed here because they are not of primary interest. Some of these other effects for disadvantaged youths can be highlighted briefly as follows: (1) youths generally perform better in the labor market and commit fewer crimes as they become older; (2) youths with higher pre-enrollment educations generally perform better in the labor market and also have higher college attendance than those with lower pre-enrollment educations; (3) minority youths generally perform worse than whites in the labor market; (4) for youths, employment is highest in the

summer and fall, and college attendance is lowest in the summer; (5) the employability of youths was improving and welfare dependence was declining over the short postprogram time period (but, as discussed later, employability increased at a slower rate as time passed); (6) youths who had better pre-enrollment work and related histories generally performed much better than other youths in those activities during subsequent time periods; and (7) youths with greater welfare dependence and criminality at pre-enrollment generally exhibited similar tendencies over time.

TABLE III.1

GLOSSARY OF EXPLANATORY VARIABLES[a]

Variable Label[b]	Definition[c]
AGE[d] =	the youth's age in years.
AGE18[d] =	1 if the youth is at least eighteen years old; 0 otherwise (allows for an intercept change at eighteen).
OVER18[d] =	AGE minus 18 if AGE is greater than eighteen; 0 otherwise (allows for a slope change at eighteen).
AGE21[d] =	1 if the youth is at least twenty-one years old; 0 otherwise (allows for an intercept change at twenty-one).
OVER21[d] =	AGE minus 21 if AGE is greater than twenty-one; 0 otherwise (allows for a slope change at twenty-one).
EDUCATION-PRE[d] =	the youth's highest grade of formal education in years completed before the Job Corps enrollment date (pre-enrollment—defined as six months before the baseline interview for the comparison sample).
DIPLOMA-PRE[d] =	1 if the youth had a high school diploma or equivalency (allows for an intercept change with high school diploma or equivalency).
EDUCATION12-PRE[d] =	1 if the youth had completed at least twelve years of formal education at pre-enrollment (allows for an additional intercept change at high school diploma).
EDOVER12-PRE[e] =	EDUCATION-PRE minus 12 if EDUCATION-PRE is greater than 12; 0 otherwise (allows for an intercept change at twelve).
BLACK[d] =	1 if the youth is black and not of Hispanic origin; 0 otherwise.
HISPANIC[d] =	1 if the youth is a person of Mexican, Puerto Rican, Cuban, Central or South American, or other Hispanic culture or origin, regardless of race; 0 otherwise.
AMERICAN INDIAN[d] =	1 if the youth is an American Indian or Alaskan native; 0 otherwise.
OTHER RACE/ETH[f] =	1 if the youth is from a race/ethnicity other than WHITE, BLACK, HISPANIC, or AMERICAN INDIAN (mostly Asian or Pacific Islander); 0 otherwise.
HEALTHPROB-BASE[d] =	1 if the youth reported a serious health problem in the baseline interview that both limited the kind or amount of work that (s)he could do and had lasted for at least one year (this misses health problems present at pre-enrollment that were cured before the baseline interview and includes some health problems that developed during the program period); 0 otherwise.
FALL[f] =	1 if the quarter is during the fall season (September, October, and November); 0 otherwise.

Table III.1 (continued)

Variable Label[b]	Definition[c]
WINTER[f] =	1 if the quarter is during the winter season (December, January, and February); 0 otherwise.
SPRING[f] =	1 if the quarter is during the spring season (March, April, and May); 0 otherwise.
MONTHS[f] =	the number of months since April 1977 (allows for time trends).
ANYEMPLOY-PRE[e] =	1 if the youth was employed any time during the six months before enrollment; 0 otherwise.
%EMPLOY-PRE[f] =	the proportion of weeks that the youth was employed during the six months before enrollment.
ANYWELF-PRE[e] =	1 if the youth received any welfare payments during the six months before enrollment; 0 otherwise.
%WELF-PRE[f] =	the proportion of months that the youth received welfare payments during the six months before enrollment.
ANYARRESTS-PRE[d] =	1 if the youth was ever arrested (had one or more arrests) during the six months before enrollment; 0 otherwise.
MJ/ALCOHOL-PRE[d] =	1 if the youth ever used marijuana or alcohol before enrollment; 0 otherwise.
COKE/HEROIN-PRE[d] =	1 if the youth ever used cocaine or heroin/methadone before enrollment; 0 otherwise.
#JOMEM-75[e] =	the total number of youths from the pre-enrollment neighborhood who enrolled in Job Corps during fiscal 1975 (based on the three-digit ZIP code of the youth's home address before enrollment).
%JOMEM-75[e] =	the fraction of youths from the pre-enrollment neighborhood who enrolled in Job Corps during fiscal 1975 (based on the three-digit ZIP code of the youth's home address before enrollment).
LAMBDA[f] =	a function of the probability of being in the Job Corps sample that controls for unobserved differences between Corpsmembers and the comparison sample (the density divided by the distribution function for Corpsmembers and the negative of the density divided by one minus the distribution function for the comparison sample—see the text for more details).
CAT1 0 TO 6[f] =	1 if the youth is a program completer and the quarter is zero to six months after (s)he terminated from Job Corps; 0 otherwise.
CAT1 6 TO 12[f] =	1 if the youth is a program completer and the quarter is six to twelve months after (s)he terminated from Job Corps; 0 otherwise.
CAT1 12 TO 18[f] =	1 if the youth is a program completer and the quarter is twelve to eighteen months after (s)he terminated from Job Corps; 0 otherwise.
CAT1 18 TO 24[f] =	1 if the youth is a program completer and the quarter is eighteen to twenty-four months after (s)he terminated from Job Corps; 0 otherwise.

Table III.1 (continued)

Variable Label[b]	Definition[c]
CAT1 24 TO 30[f] =	1 if the youth is a program completer and the quarter is twenty-four to thirty months after (s)he terminated from Job Corps; 0 otherwise.
CAT1 30 TO 36[f] =	1 if the youth is a program completer and the quarter is thirty to thirty-six months after (s)he terminated from Job Corps; 0 otherwise.
CAT1 36 TO 42[f] =	1 if the youth is a program completer and the quarter is thirty-six to forty-two months after (s)he terminated from Job Corps; 0 otherwise.
CAT1 42 TO 48[f] =	1 if the youth is a program completer and the quarter is forty-two to forty-eight months after (s)he terminated from Job Corps; 0 otherwise.
CAT1 48 TO 54[f] =	1 if the youth is a program completer and the quarter is forty-eight to fifty-four months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 0 TO 6[f] =	1 if the youth is a partial completer and the quarter is zero to six months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 6 TO 12[f] =	1 if the youth is a partial completer and the quarter is six to twelve months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 12 TO 18[f] =	1 if the youth is a partial completer and the quarter is twelve to eighteen months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 18 TO 24[f] =	1 if the youth is a partial completer and the quarter is eighteen to twenty-four months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 24 TO 30[f] =	1 if the youth is a partial completer and the quarter is twenty-four to thirty months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 30 TO 36[f] =	1 if the youth is a partial completer and the quarter is thirty to thirty-six months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 36 TO 42[f] =	1 if the youth is a partial completer and the quarter is thirty-six to forty-two months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 42 TO 48[f] =	1 if the youth is a partial completer and the quarter is forty-two to forty-eight months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 48 TO 54[f] =	1 if the youth is a partial completer and the quarter is forty-eight to fifty-four months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 0 TO 6[f] =	1 if the youth is an early dropout and the quarter is zero to six months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 6 TO 12[f] =	1 if the youth is an early dropout and the quarter is six to twelve months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 12 TO 18[f] =	1 if the youth is an early dropout and the quarter is twelve to eighteen months after (s)he terminated from Job Corps; 0 otherwise.

Table III.1 (continued)

Variable Label[b]	Definition[c]
CAT3 18 TO 24[f] =	1 if the youth is an early dropout and the quarter is eighteen to twenty-four months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 24 TO 30[f] =	1 if the youth is an early dropout and the quarter is twenty-four to thirty months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 30 TO 36[f] =	1 if the youth is an early dropout and the quarter is thirty to thirty-six months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 36 TO 42[f] =	1 if the youth is an early dropout and the quarter is thirty-six to forty-two months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 42 TO 48[f] =	1 if the youth is an early dropout and the quarter is forty-two to forty-eight months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 48 TO 54[f] =	1 if the youth is an early dropout and the quarter is forty-eight to fifty-four months after (s)he terminated from Job Corps; 0 otherwise.

[a] The lagged values of dependent variables from pre-enrollment cannot reasonably be assumed to be strictly predetermined when we pool observations for individual youths over time. These lagged dependent variables are generally endogenous if we assume (as we must) that equation error terms are correlated over time for individuals. However, the use of estimators of error variances and covariances from ordinary least squares residuals will still yield consistent estimators of coefficients in a second-stage generalized least squares technique when the error-components model is appropriate, as we assume in our estimation procedures (for more details and proofs, see Wallace and Hussain, 1969, and Nerlove, 1971a and 1971b).

[b] The explanatory variables are arranged in this table in approximately the same order as the findings presented in subsequent tables.

[c] The pre-enrollment period is defined as six months before the baseline interview for the comparison sample, because Corpsmembers had been in the program approximately six months on average when the comparison youths were interviewed. The data are arrayed into quarterly aggregates by calendar quarters according to the seasons—summer (June, July, and August), fall (September, October, and November), winter (December, January, and February), and spring (March, April, and May)—which differ from the usual fiscal quarters but provide better controls for seasonality. Time-dependent variables—such as those related to age, calendar time, and length of time out of Job Corps—are defined for the midpoint of each quarter.

[d] These explanatory variables were included <u>both</u> in the Job Corps probability (Tables III.2 through III.4) <u>and</u> in the regressions for impact estimates (Tables III.5 through III.16).

[e] These explanatory variables were included <u>only</u> in the Job Corps probability equations (Tables III.2 through III.4) and <u>not</u> in the regressions for impact estimates (Tables III.5 through III.16).

[f] These explanatory variables were included <u>only</u> in the regressions for impact estimates (Tables III.5 through III.16) and <u>not</u> in the Job Corps probability equations (Tables III.2 through III.4).

TABLE III.2

PROBIT ESTIMATES FOR THE PROBABILITY OF BEING IN THE JOB CORPS SAMPLE: MALES[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic[c]	Partial Derivative at Point of Means[d]
CONSTANT	-2.297	1.047	-2.193	-0.375
AGE	0.214	0.064	3.329	0.035
AGE18	-0.006	0.083	-0.070	-0.001
OVER18	-0.521	0.117	-4.464	-0.085
AGE21	-0.681	0.281	-2.425	-0.111
OVER21	-0.344	0.171	-0.201	-0.056
EDUCATION-PRE	-0.133	0.025	-5.420	-0.022
DIPLOMA-PRE	-0.278	0.144	-1.932	-0.045
EDUCATION12-PRE	2.588	4.623	0.560	0.423
EDOVER12-PRE	1.021	0.172	5.945	0.167
BLACK	-0.014	0.061	-0.225	-0.002
HISPANIC	-0.067	0.097	-0.689	-0.011
AMERICAN INDIAN	0.342	0.147	2.319	0.056
HEALTHPROB-BASE	0.133	0.130	1.020	0.022
ANYEMPLOY-PRE	0.137	0.055	2.507	0.022
ANYWELF-PRE	0.060	0.104	0.575	0.010
ANYARRESTS-PRE	0.180	0.069	2.593	0.029
MJ/ALCOHOL-PRE	0.055	0.064	0.872	0.009
COKE/HEROIN-PRE	0.516	0.100	5.149	0.084
#JOMEM-75	0.002	0.0004	3.989	0.0003
%JOMEM-75	0.006	0.0004	13.663	0.001

Number of observations = 4,155

Mean of dependent variable = 0.829

Chi-Square statistic for equation = 698.134

- Degrees of freedom = 20
- Significance level = > 99% statistical confidence

[a] Maximum likelihood estimates are computed by an iterative Newton-Raphson procedure.

[b] For definitions of explanatory variables, see Table III.1.

[c] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

[d] The change in probability associated with a marginal change in the relevant explanatory variable equals the coefficient estimate times the value of the density function, which is obtained here with the mean values for all explanatory variables (i.e., the point of means). For binary explanatory variables it is more appropriate to use the difference between the distribution function values with and without the relevant coefficient, with all other explanatory variables at their mean values. This latter approach is used in this report to obtain impact estimates for probability variables.

TABLE III.3

PROBIT ESTIMATES FOR THE PROBABILITY OF BEING IN THE JOB CORPS SAMPLE: FEMALES WITHOUT CHILDREN[a/]

Explanatory Variable[b/]	Coefficient Estimate	Standard Error	T-Statistic[c/]	Partial Derivative at Point of Means[d/]
CONSTANT	-3.525	1.312	-2.687	-0.624
AGE	0.191	0.080	2.386	0.034
AGE18	-0.204	0.111	-1.838	-0.036
OVER18	-0.153	0.156	-0.983	-0.027
AGE21	1.641	6.995	0.235	0.291
OVER21	0.302	0.280	1.079	0.054
EDUCATION-PRE	-0.085	0.043	-1.969	-0.015
DIPLOMA-PRE	-0.065	0.229	-0.285	-0.012
EDUCATION12-PRE	0.021	0.353	0.060	0.004
EDOVER12-PRE	1.064	0.253	4.207	0.188
BLACK	0.592	0.092	6.419	0.105
HISPANIC	0.719	0.148	4.857	0.127
AMERICAN INDIAN	1.151	0.243	4.738	0.204
HEALTHPROB-BASE	0.060	0.193	0.313	0.011
ANYEMPLOY-PRE	0.235	0.084	2.809	0.042
ANYWELF-PRE	-0.223	0.149	-1.494	-0.040
ANYARRESTS-PRE	-0.085	0.198	-0.430	-0.015
MJ/ALCOHOL-PRE	0.218	0.087	2.512	0.039
COKE/HEROIN-PRE	0.381	0.156	2.442	0.067
#JOMEM-75	0.003	0.001	4.498	0.001
%JOMEM-75	0.007	0.001	8.886	0.001

Number of observations = 1,710

Mean of dependent variable = 0.760

Chi-Square statistic for equation = 555.672

- Degrees of freedom = 20
- Significance level = > 99% statistical confidence

[a/] Maximum likelihood estimates are computed by an iterative Newton-Rapheson procedure.

[b/] For definitions of explanatory variables, see Table III.1.

[c/] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

[d/] The change in probability associated with a marginal change in the relevant explanatory variable equals the coefficient estimate times the value of the density function, which is obtained here with the mean values for all explanatory variables (i.e., the point of means). For binary explanatory variables it is more appropriate to use the difference between the distribution function values with and without the relevant coefficient, with all other explanatory variables at their mean values. This latter approach is used in this report to obtain impact estimates for probability variables.

TABLE III.4

PROBIT ESTIMATES FOR THE PROBABILITY OF BEING IN THE JCB CORPS SAMPLE: FEMALES WITH CHILDREN[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic[c]	Partial Derivative at Point of Means[d]
CONSTANT	-4.732	3.549	-1.333	-1.862
AGE	0.260	0.215	1.211	0.102
AGE18	-0.097	0.242	-0.402	-0.038
OVER18	-0.602	0.341	-1.766	-0.237
AGE21	-0.074	0.585	-0.126	-0.029
OVER21	-0.176	0.331	-0.532	-0.069
EDUCATION-PRE	-0.213	0.062	-3.444	-0.084
DIPLOMA-PRE	-0.019	0.317	-0.059	-0.007
EDUCATION12-PRE	0.270	0.768	0.352	0.106
EDOVER12-PRE	0.822	0.363	2.267	0.324
BLACK	0.609	0.200	3.045	0.240
HISPANIC	0.352	0.305	1.155	0.138
AMERICAN INDIAN	0.116	0.422	2.754	0.458
HEALTHPROB-BASE	-0.211	0.375	-0.564	-0.083
ANYEMPLOY-PRE	0.427	0.140	3.058	0.168
ANYWELF-PRE	0.211	0.144	1.467	0.083
ANYARRESTS-PRE	0.406	0.276	1.472	0.160
MJ/ALCOHOL-PRE	0.154	0.141	0.061	0.061
COKE/HEROIN-PRE	0.983	0.293	0.392	0.387
#JCMEM-75	.0.002	0.001	2.507	0.001
%JCMEM-75	10.439	1.335	7.817	4.107

Number of observations = 516

Mean of dependent variable = 0.421

Chi-Square statistic for equation = 216.043

- Degrees of freedom = 20
- Significance level = > 99% statistical confidence

[a] Maximum likelihood estimates are computed by an iterative Newton-Rapheson procedure.

[b] For definitions of explanatory variables, see Table III.1.

[c] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

[d] The change in probability associated with a marginal change in the relevant explanatory variable equals the coefficient estimate times the value of the density function, which is obtained here with the mean values for all explanatory variables (i.e., the point of means). For binary explanatory variables it is more appropriate to use the difference between the distribution function values with and without the relevant coefficient, with all other explanatory variables at their mean values. This latter approach is used in this report to obtain impact estimates for probability variables.

TABLE III.5

REGRESSION ESTIMATES FOR IMPACT ON FRACTION OF TIME EMPLOYED, INCLUDING MILITARY: MALES[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	-0.943	0.342	-2.756
AGE	0.074	0.020	3.692
AGE18	0.014	0.024	0.582
OVER18	-0.085	0.020	-4.199
AGE21	-0.003	-0.008	-0.451
OVER21	0.008	0.004	1.786
EDUCATION-PRE	0.011	0.005	2.328
DIPLOMA-PRE	0.070	0.029	2.369
EDUCATION12-PRE	-0.014	0.034	-0.409
BLACK	-0.101	0.012	-8.135
HISPANIC	-0.006	0.018	-0.348
AMERICAN INDIAN	-0.130	0.029	-4.534
OTHER RACE/ETH	-0.097	0.049	-1.989
HEALTHPROB-BASE	-0.011	0.024	-0.467
FALL	0.008	0.005	1.706
WINTER	-0.019	0.005	-3.875
SPRING	-0.006	0.005	-1.217
MONTHS	0.003	0.0003	7.653
%EMPLOY-PRE	0.134	0.014	9.519
%WELF-PRE	-0.052	0.027	-1.940
ANYARRESTS-PRE	-0.036	0.014	-2.634
MJ/ALCOHOL-PRE	0.003	0.012	0.280
COKE/HEROIN-PRE	-0.019	0.017	-1.107
LAMBDA	-0.048	0.022	-2.139
CAT1 0 TO 6	0.067	0.037	1.812
CAT1 6 TO 12	0.188	0.037	5.126
CAT1 12 TO 18	0.201	0.037	5.454
CAT1 18 TO 24	0.184	0.037	4.985
CAT1 24 TO 30	0.180	0.037	4.865
CAT1 30 TO 36	0.183	0.037	4.909
CAT1 36 TO 42	0.157	0.038	4.199
CAT1 42 TO 48	0.160	0.038	4.217
CAT1 48 TO 54	0.170	0.042	4.065
CAT2 0 TO 6	-0.031	0.037	-0.818
CAT2 6 TO 12	0.082	0.038	2.198
CAT2 12 TO 18	0.103	0.038	2.736

Table III.5 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	0.064	0.038	1.702
CAT2 24 TO 30	0.076	0.038	2.005
CAT2 30 TO 36	0.065	0.038	1.692
CAT2 36 TO 42	0.063	0.039	1.633
CAT2 42 TO 48	0.030	0.039	0.773
CAT2 48 TO 54	0.089	0.043	2.035
CAT3 0 TO 6	-0.063	0.044	-1.457
CAT3 6 TO 12	0.035	0.044	0.788
CAT3 12 TO 18	0.083	0.044	1.871
CAT3 18 TO 24	0.074	0.044	1.668
CAT3 24 TO 30	0.107	0.046	2.354
CAT3 30 TO 36	0.097	0.047	2.056
CAT3 36 TO 42	0.055	0.047	1.173
CAT3 42 TO 48	0.008	0.047	0.185
CAT3 48 TO 54	0.057	0.051	1.117

Number of observations = 38,578

- Number of individuals = 3,245
- Average number of time periods = 11.888

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.357

Mean of dependent variable = 0.611

F-Statistic for equation = 39.571

- Degrees of freedom = 50; 38,527
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.6

REGRESSION ESTIMATES FOR IMPACT ON FRACTION OF TIME EMPLOYED, INCLUDING MILITARY: FEMALES WITHOUT CHILDREN[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	-1.133	0.459	-2.469
AGE	0.069	0.027	2.565
AGE18	-0.053	0.035	-1.533
OVER18	-0.063	0.028	-2.272
AGE21	-0.003	0.013	-0.222
OVER21	0.016	0.008	1.978
EDUCATION-PRE	0.024	0.008	2.975
DIPLOMA-PPE	0.073	0.043	1.684
EDUCATION12-PPE	-0.003	0.049	-0.052
BLACK	-0.120	0.022	-5.566
HISPANIC	-0.045	0.031	-1.436
AMERICAN INDIAN	-0.097	0.051	-1.918
OTHER RACE/ETH	0.009	0.085	0.108
HEALTHPROB-BASE	-0.079	0.037	-2.119
FALL	0.030	0.008	3.693
WINTER	0.005	0.008	0.638
SPRING	-0.008	0.008	-0.955
MONTHS	0.001	0.0005	1.852
%EMPLOY-PRE	0.122	0.024	5.078
%WELF-PRE	-0.125	0.031	-4.040
ANYARRESTS-PRE	-0.085	0.040	-2.129
MJ/ALCOHOL-PRE	0.021	0.017	1.270
COKE/HEROIN-PRE	0.021	0.031	0.694
LAMBDA	-0.003	0.025	-0.124
CAT1 0 TO 6	0.060	0.041	1.471
CAT1 6 TO 12	0.169	0.041	4.142
CAT1 12 TO 18	0.210	0.041	5.081
CAT1 18 TO 24	0.189	0.042	4.526
CAT1 24 TO 30	0.158	0.042	3.749
CAT1 30 TO 36	0.189	0.043	4.355
CAT1 36 TO 42	0.223	0.045	4.991
CAT1 42 TO 48	0.236	0.046	5.099
CAT1 48 TO 54	0.244	0.061	3.975
CAT2 0 TO 6	-0.030	0.043	-0.698
CAT2 6 TO 12	0.046	0.044	1.066
CAT2 12 TO 18	0.081	0.044	1.811

Table III.6 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	0.071	0.045	1.562
CAT2 24 TO 30	0.042	0.046	0.909
CAT2 30 TO 36	0.080	0.048	1 690
CAT2 36 TO 42	0.052	0.050	1.144
CAT2 42 TO 48	0.100	0.052	1.927
CAT2 48 TO 54	-0.026	0.070	-0.368
CAT3 0 TO 6	-0.064	0.059	-1.081
CAT3 6 TO 12	-0.074	0.061	-1.204
CAT3 12 TO 18	0.093	0.064	1.459
CAT3 18 TO 24	-0.009	0.067	-0.139
CAT3 24 TO 30	-0.035	0.067	-0.518
CAT3 30 TO 36	-0.039	0.069	-0.565
CAT3 36 TO 42	0.213	0.078	2.718
CAT3 42 TO 48	0.114	0.079	1.454
CAT3 48 TO 54	0.044	0.091	0.478

Number of observations = 13,553

- Number of individuals = 1,489
- Average number of time periods = 9.100

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.398

Mean of dependent variable = 0.435

F-Statistic for equation = 17.145

- Degrees of freedom = 50; 13,502
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.7

REGRESSION-ESTIMATES FOR IMPACT ON FRACTION OF TIME EMPLOYED:
FEMALES WITH CHILDREN

Explanatory Variable[a]	Coefficient Estimate	Standard Error[b]	T-Statistic[c]
CONSTANT	0.468	0.984	0.476
AGE	-0.023	0.058	-0.397
AGE18	0.006	0.068	0.085
OVER18	0.027	0.058	0.459
AGE21	0.013	0.015	0.895
OVER21	0.003	0.009	0.379
EDUCATION-PRE	0.005	0.009	0.582
DIPLOMA-PRE	0.059	0.056	1.049
EDUCATION12-PRE	0.048	0.062	0.764
BLACK	0.042	0.025	1.640
HISPANIC	0.026	0.038	0.702
AMERICAN INDIAN	-0.043	0.056	-0.765
OTHER RACE/ETH	0.116	0.176	0.656
HEALTHPROB-BASE	-0.011	0.047	-0.238
FALL	0.006	0.009	0.748
WINTER	0.004	0.009	0.051
SPRING	-0.008	0.009	-0.914
MONTHS	0.003	0.0006	4.724
%EMPLOY-PRE	0.160	0.030	5.283
%WELF-PRE	0.010	0.025	0.393
ANYARRESTS-PRE	-0.041	0.043	-0.938
MJ/ALCOHOL-PRE	0.013	0.020	0.683
COKE/HEROIN-PRE	0.031	0.043	0.727
LAMBDA	0.061	0.026	2.383
CAT1 0 TO 6	-0.105	0.050	-2.100
CAT1 6 TO 12	0.001	0.046	0.010
CAT1 12 TO 18	0.012	0.042	0.284
CAT1 18 TO 24	-0.022	0.041	-0.542
CAT1 24 TO 30	-0.032	0.042	-0.760
CAT1 30 TO 36	-0.016	0.041	-0.385
CAT1 36 TO 42	-0.015	0.041	-0.359
CAT1 42 TO 48	-0.056	0.043	-1.316
CAT1 48 TO 54	-0.078	0.061	-1.277
CAT2 0 TO 6	-0.105	0.056	-1.892
CAT2 6 TO 12	-0.153	0.052	-2.959
CAT2 12 TO 18	-0.060	0.045	-1.335

Table III.7 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	-0.117	0.044	-2.628
CAT2 24 TO 30	-0.108	0.045	-2.402
CAT2 30 TO 36	-0.059	0.045	-1.294
CAT2 36 TO 42	-0.094	0.045	-2.111
CAT2 42 TO 48	-0.133	0.046	-2.868
CAT2 48 TO 54	-0.108	0.059	-1.823
CAT3 0 TO 6	-0.177	0.095	-1.862
CAT3 6 TO 12	-0.254	0.101	-2.525
CAT3 12 TO 18	-0.008	0.082	-0.096
CAT3 18 TO 24	-0.103	0.072	-1.429
CAT3 24 TO 30	-0.209	0.074	-2.819
CAT3 30 TO 36	-0.276	0.077	-3.568
CAT3 36 TO 42	-0.235	0.070	-3.355
CAT3 42 TO 48	-0.264	0.071	-3.715
CAT3 48 TO 54	-0.345	0.081	-4.275

Number of observations = 9,479

- Number of individuals = 1,002
- Average number of time periods = 9.457

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.440

Mean of dependent variable = 0.282

F-Statistic for equation = 5.787

- Degrees of freedom = 50; 9,428
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.8

REGRESSION ESTIMATES FOR IMPACT ON FRACTION OF TIME IN COLLEGE, INCLUDING MILITARY: MALES[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	-0.007	0.081	-0.083
AGE	-0.0009	0.005	-0.194
AGE18	0.004	0.006	0.077
OVER18	0.003	0.005	0.656
AGE21	-0.002	0.002	-1.225
OVER21	-0.003	0.001	-2.841
EDUCATION-PRE	0.002	0.001	1.884
DIPLOMA-PRE	-0.003	0.007	-0.482
EDUCATION12-PRE	0.011	0.008	1.349
BLACK	0.001	0.003	0.215
HISPANIC	0.011	0.004	2.487
AMERICAN INDIAN	0.004	0.007	0.513
OTHER RACE/ETH	0.017	0.012	1.429
HEALTHPROB-BASE	0.005	0.006	0.914
FALL	0.006	0.001	4.893
WINTER	0.006	0.001	5.471
SPRING	0.005	0.001	4.579
MONTHS	0.00008	0.00008	0.905
%EMPLOY-PRE	-0.004	0.003	-1.172
%WELF-PRE	-0.007	0.006	-1.105
ANYARRESTS-PRE	0.004	0.003	1.106
MJ/ALCOHOL-PRE	-0.001	0.003	-0.350
COKE/HEROIN-PRE	0.009	0.004	2.202
LAMBDA	-0.001	0.005	-0.096
CAT1 0 TO 6	0.005	0.009	0.580
CAT1 6 TO 12	0.011	0.009	1.222
CAT1 12 TO 18	0.010	0.009	1.109
CAT1 18 TO 24	0.003	0.009	0.313
CAT1 24 TO 30	0.002	0.009	0.238
CAT1 30 TO 36	0.007	0.009	0.808
CAT1 36 TO 42	0.007	0.009	0.817
CAT1 42 TO 48	0.003	0.009	0.360
CAT1 48 TO 54	-0.007	0.010	-0.686
CAT2 0 TO 6	-0.001	0.009	-0.112
CAT2 6 TO 12	0.002	0.009	0.176
CAT2 12 TO 18	0.003	0.009	0.316

Table III.8 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	-0.001	0.009	-0.055
CAT2 24 TO 30	-0.002	0.009	-0.180
CAT2 30 TO 36	-0.002	0.009	-0.251
CAT2 36 TO 42	0.001	0.009	0.094
CAT2 42 TO 48	0.004	0.009	0.406
CAT2 48 TO 54	0.002	0.011	0.156
CAT3 0 TO 6	-0.003	0.011	-0.281
CAT3 6 TO 12	0.004	0.011	0.342
CAT3 12 TO 18	0.004	0.011	0.292
CAT3 18 TO 24	-0.005	0.011	-0.449
CAT3 24 TO 30	-0.002	0.011	-0.207
CAT3 30 TO 36	-0.0004	0.011	-0.04
CAT3 36 TO 42	0.016	0.011	1.443
CAT3 42 TO 48	0.001	0.011	0.130
CAT3 48 TO 54	0.007	0.012	0.601

Number of observations = 38,578

- Number of individuals = 3,245
- Average number of time periods = 11.888

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.372

Mean of dependent variable = 0.012

F-Statistic for equation = 3.062

- Degrees of freedom = 50; 38,527
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased-counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.9

REGRESSION ESTIMATES FOR IMPACT ON FRACTION OF TIME IN COLLEGE, INCLUDING MILITARY: FEMALES WITHOUT CHILDREN[a/]

Explanatory Variable[b/]	Coefficient Estimate	Standard Error[c/]	T-Statistic[d/]
CONSTANT	-0.091	0.187	-0.489
AGE	0.002	0.011	0.159
AGE18	0.010	0.014	0.712
OVER18	-0.011	0.011	-0.966
AGE21	0.0001	0.005	0.027
OVER21	0.012	0.003	3.542
EDUCATION-PRE	0.005	0.003	1.446
DIPLOMA-PRE	0.026	0.018	1.448
EDUCATION12-PRE	0.010	0.021	0.493
BLACK	-0.001	0.009	-0.142
HISPANIC	0.003	0.013	0.209
AMERICAN INDIAN	-0.010	0.021	-0.468
OTHER RACE/ETH	0.009	0.036	0.241
HEALTHPROB-BASE	0.009	0.016	0.585
FALL	0.022	0.003	6.767
WINTER	0.019	0.003	5.931
SPRING	0.017	0.003	5.063
MONTHS	0.0003	0.0002	1.238
%EMPLOY-PRE	0.020	0.010	1.936
%WELF-PRE	0.020	0.013	1.503
ANYARRESTS-PRE	-0.013	0.017	-0.762
MJ/ALCOHOL-PRE	-0.005	0.007	-0.757
COKE/HEROIN-PRE	0.008	0.013	0.615
LAMBDA	-0.007	0.011	-0.640
CAT1 0 TO 6	0.012	0.017	0.697
CAT1 6 TO 12	0.038	0.017	2.193
CAT1 12 TO 18	0.036	0.017	2.075
CAT1 18 TO 24	0.032	0.018	1.802
CAT1 24 TO 30	0.031	0.018	1.767
CAT1 30 TO 36	0.034	0.018	1.895
CAT1 36 TO 42	0.035	0.019	1.889
CAT1 42 TO 48	0.021	0.019	1.066
CAT1 48 TO 54	-0.036	0.025	-1.409
CAT2 0 TO 6	0.002	0.018	0.124
CAT2 6 TO 12	0.018	0.018	0.988
CAT2 12 TO 18	0.013	0.019	0.712

Table III.9 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	0.008	0.019	0.402
CAT2 24 TO 30	-0.008	0.019	-0.417
CAT2 30 TO 36	-0.015	0.020	-0.779
CAT2 36 TO 42	-0.001	0.021	-0.067
CAT2 42 TO 48	0.029	0.022	1.364
CAT2 48 TO 54	-0.018	0.029	-0.615
CAT3 0 TO 6	-0.014	0.025	-0.544
CAT3 6 TO 12	-0.001	0.026	-0.048
CAT3 12 TO 18	0.004	0.027	0.148
CAT3 18 TO 24	0.032	0.028	1.139
CAT3 24 TO 30	0.057	0.028	2.021
CAT3 30 TO 36	0.111	0.029	3.858
CAT3 36 TO 42	0.139	0.032	4.300
CAT3 42 TO 48	0.141	0.033	4.343
CAT3 48 TO 54	0.036	0.037	0.972

Number of observations = 13,553

- Number of individuals = 1,489
- Average number of time periods = 9,100

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.422

Mean of dependent variable = 0.039

F-Statistic for equation = 5.648

- Degrees of freedom = 50; 13,502
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.10

REGRESSION ESTIMATES FOR IMPACT ON FRACTION OF TIME IN COLLEGE: FEMALES WITH CHILDREN[a/]

Explanatory Variable[b/]	Coefficient Estimate	Standard Error[c/]	T-Statistic[d/]
CONSTANT	0.693	0.304	2.276
AGE	-0.043	0.018	-2.394
AGE18	0.037	0.021	1.781
OVER18	0.045	0.018	2.505
AGE21	-0.009	0.005	-2.017
OVER21	-0.006	0.003	-2.130
EDUCATION-PRE	0.004	0.003	1.254
DIPLOMA-PRE	0.053	0.019	2.830
EDUCATION12-PRE	-0.009	0.021	-0.438
BLACK	0.008	0.009	0.988
HISPANIC	0.008	0.013	0.614
AMERICAN INDIAN	0.010	0.019	0.535
OTHER RACE/ETH	-0.005	0.059	-0.091
HEALTHPROB-BASE	0.006	0.016	0.350
FALL	0.011	0.003	4.194
WINTER	0.006	0.003	2.052
SPRING	0.004	0.003	1.480
MONTHS	0.0002	0.0002	1.085
$EMPLOY-PRE	0.011	0.010	1.114
$WELF-PRE	-0.001	0.008	-0.078
ANYARRESTS-PRE	-0.008	0.015	-0.539
MJ/ALCOHOL-PRE	0.001	0.007	0.101
COKE/HEROIN-PRE	0.010	0.015	0.675
LAMBDA	0.011	0.009	1.224
CAT1 0 TO 6	-0.019	0.016	-1.195
CAT1 6 TO 12	-0.006	0.015	-0.405
CAT1 12 TO 18	0.011	0.014	0.814
CAT1 18 TO 24	-0.009	0.014	-0.634
CAT1 24 TO 30	-0.018	0.014	-1.280
CAT1 30 TO 36	-0.0003	0.014	-0.026
CAT1 36 TO 42	0.001	0.014	0.096
CAT1 42 TO 48	0.011	0.014	0.804
CAT1 48 TO 54	0.053	0.020	2.733
CAT2 0 TO 6	-0.004	0.018	-0.234
CAT2 6 TO 12	-0.016	0.017	-0.974
CAT2 12 TO 18	0.001	0.015	0.085

Table III.10 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	0.001	0.015	0.085
CAT2 24 TO 30	0.013	0.015	0.892
CAT2 30 TO 36	0.007	0.015	0.490
CAT2 36 TO 42	-0.002	0.015	-0.163
CAT2 42 TO 48	-0.005	0.015	-0.317
CAT2 48 TO 54	0.0009	0.019	0.050
CAT3 0 TO 6	-0.034	0.030	-1.118
CAT3 6 TO 12	-0.025	0.032	-0.784
CAT3 12 TO 18	-0.031	0.026	-1.187
CAT3 18 TO 24	-0.004	0.024	-1.187
CAT3 24 TO 30	-0.030	0.024	-1.258
CAT3 30 TO 36	-0.033	0.025	-1.295
CAT3 36 TO 42	-0.034	0.023	-1.478
CAT3 42 TO 48	-0.032	0.023	-1.387
CAT3 48 TO 54	-0.029	0.026	-1.110

Number of observations = 9,479

- Number of individuals = 1,002
- Average number of time periods = 9.457

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.496

Mean of dependent variable = 0.019

F-Statistic for equation = 2.503

- Degrees of freedom = 50; 9,428
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III,1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.11

REGRESSION ESTIMATES FOR IMPACT ON FRACTION OF TIME RECEIVING ANY CASH WELFARE, INCLUDING MILITARY: MALES[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	0.228	0.090	2.533
AGE	-0.009	0.005	-1.788
AGE18	0.013	0.006	2.106
OVER18	0.016	0.005	3.029
AGE21	-0.006	0.002	-3.009
OVER21	-0.006	0.001	-5.088
EDUCATION-PRE	-0.003	0.001	-2.356
DIPLOMA-PRE	-0.003	0.009	-0.369
EDUCATION12-PRE	0.009	0.010	0.840
BLACK	0.008	0.004	0.203
HISPANIC	0.003	0.005	0.486
AMERICAN INDIAN	0.004	0.009	0.509
OTHER RACE/ETH	0.014	0.015	0.993
HEALTHPROB-BASE	0.006	0.007	0.815
FALL	-0.0001	0.001	-0.084
WINTER	0.003	0.001	2.053
SPRING	0.002	0.001	1.509
MONTHS	-0.0006	0.0001	-5.746
%EMPLOY-PRE	-0.013	0.004	-3.098
%WELF-PRE	0.045	0.008	5.625
ANYARRESTS-PRE	-0.0003	0.004	-0.078
MJ/ALCOHOL-PRE	-0.0004	0.004	-0.117
COKE/HEROIN-PRE	-0.004	0.005	-0.737
LAMBDA	0.005	0.007	0.749
CAT1 0 TO 6	-0.033	0.011	-3.016
CAT1 6 TO 12	-0.029	0.011	-2.663
CAT1 12 TO 18	-0.025	0.011	-2.292
CAT1 18 TO 24	-0.025	0.011	-2.282
CAT1 24 TO 30	-0.023	0.011	-2.115
CAT1 30 TO 36	-0.026	0.011	-2.341
CAT1 36 TO 42	-0.018	0.011	-1.644
CAT1 42 TO 48	-0.018	0.011	-1.592
CAT1 48 TO 54	-0.014	0.012	-1.134
CAT2 0 TO 6	-0.034	0.011	-3.014
CAT2 6 TO 12	-0.036	0.011	-3.020
CAT2 12 TO 18	-0.023	0.011	-2.000

Table III.11 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	-0.016	0.011	-1.434
CAT2 24 TO 30	-0.012	0.011	-1.084
CAT2 30 TO 36	-0.016	0.011	-1.386
CAT2 36 TO 42	-0.003	0.011	-0.258
CAT2 42 TO 48	0.001	0.011	0.046
CAT2 48 TO 54	-0.0003	0.013	-0.027
CAT3 0 TO 6	-0.022	0.013	-1.681
CAT3 6 TO 12	-0.025	0.013	-1.952
CAT3 12 TO 18	-0.015	0.013	-1.166
CAT3 18 TO 24	-0.019	0.013	-1.483
CAT3 24 TO 30	-0.033	0.013	-2.474
CAT3 30 TO 36	-0.028	0.014	-2.074
CAT3 36 TO 42	-0.019	0.014	-1.381
CAT3 42 TO 48	-0.022	0.014	-1.581
CAT3 48 TO 54	-0.025	0.015	-1.700

Number of observations = 38,578

- Number of individuals = 3,245
- Average number of time periods = 11.888

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.434

Mean of dependent variable = 0.015

F-Statistic for equation = 4.873

- Degrees of freedom = 50; 38,527
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.12

REGRESSION ESTIMATES FOR IMPACT ON FRACTION OF TIME RECEIVING ANY CASH WELFARE, INCLUDING MILITARY: FEMALES WITHOUT CHILDREN[a/]

Explanatory Variable[b/]	Coefficient Estimate	Standard Error[c/]	T-Statistic[d/]
CONSTANT	0.486	0.215	2.263
AGE	-0.019	0.013	-1.492
AGE18	0.040	0.016	2.518
OVER18	0.033	0.013	2.569
AGE21	-0.003	0.006	-0.455
OVER21	-0.012	0.004	-3.142
EDUCATION-PRE	-0.009	0.005	-1.903
DIPLOMA-PRE	-0.046	0.024	-1.877
EDUCATION12-PRE	0.047	0.028	1.694
BLACK	0.055	0.012	4.461
HISPANIC	0.018	0.018	1.016
AMERICAN INDIAN	0.010	0.029	0.355
OTHER RACE/ETH	0.035	0.048	0.724
HEALTHPROB-BASE	0.056	0.021	2.620
FALL	-0.009	0.004	-2.475
WINTER	-0.001	0.004	-0.283
SPRING	0.008	0.004	2.288
MONTHS	0.0002	0.0003	0.829
%EMPLOY-PRE	-0.016	0.014	-1.181
%WELF-PRE	0.176	0.017	10.115
ANYARRESTS-PRE	-0.031	0.023	-1.385
MJ/ALCOHOL-PRE	0.004	0.010	0.443
COKE/HEROIN-PRE	0.025	0.017	1.463
LAMBDA	0.071	0.014	4.958
CAT1 0 TO 6	-0.148	0.023	-6.506
CAT1 6 TO 12	-0.135	0.023	-5.930
CAT1 12 TO 18	-0.154	0.023	-6.702
CAT1 18 TO 24	-0.154	0.023	-6.661
CAT1 24 TO 30	-0.154	0.023	-6.581
CAT1 30 TO 36	-0.156	0.024	-6.570
CAT1 36 TO 42	-0.158	0.024	-6.518
CAT1 42 TO 48	-0.177	0.025	-7.098
CAT1 48 TO 54	-0.157	0.031	-5.074
CAT2 0 TO 6	-0.154	0.024	-6.428
CAT2 6 TO 12	-0.119	0.024	-4.923
CAT2 12 TO 18	-0.130	0.025	-5.319

Table III.12 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	-0.117	0.025	-4.731
CAT2 24 TO 30	-0.097	0.025	-3.879
CAT2 30 TO 36	-0.125	0.026	-4.845
CAT2 36 TO 42	-0.135	0.027	-5.063
CAT2 42 TO 48	-0.137	0.027	-5.012
CAT2 48 TO 54	-0.154	0.035	-4.408
CAT3 0 TO 6	-0.120	0.032	-3.714
CAT3 6 TO 12	-0.126	0.033	-3.820
CAT3 12 TO 18	-0.115	0.034	-3.363
CAT3 18 TO 24	-0.103	0.035	-2.897
CAT3 24 TO 30	-0.086	0.036	-2.428
CAT3 30 TO 36	-0.101	0.036	-2.774
CAT3 36 TO 42	-0.143	0.040	-3.586
CAT3 42 TO 48	-0.155	0.040	-3.864
CAT3 48 TO 54	-0.148	0.045	-3.277

Number of observations = 13,553

- Number of individuals = 1,489
- Average number of time periods = 9.100

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.525

Mean of dependent variable = 0.054

F-Statistic for equation = 8.841

- Degrees of freedom = 50; 13,502
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.13

REGRESSION ESTIMATES FOR IMPACT ON FRACTION OF TIME RECEIVING ANY CASH WELFARE: FEMALES WITH CHILDREN[a/]

Explanatory Variable[b/]	Coefficient Estimate	Standard Error[c/]	T-Statistic[d/]
CONSTANT	1.968	0.914	2.153
AGE	-0.106	0.054	-1.968
AGE18	0.321	0.063	5.139
OVER18	0.119	0.054	2.204
AGE21	-0.030	0.014	-2.172
OVER21	-0.020	0.008	-2.400
EDUCATION-PRE	-0.001	0.010	-0.077
DIPLOMA-PRE	0.009	0.058	0.159
EDUCATION12-PRE	-0.083	0.065	-1.275
BLACK	0.146	0.027	5.496
HISPANIC	0.025	0.039	0.642
AMERICAN INDIAN	0.024	0.059	0.407
OTHER RACE/ETH	-0.147	0.184	-0.800
HEALTHPROB-BASE	0.048	0.049	0.975
FALL	0.008	0.008	1.048
WINTER	0.048	0.008	5.852
SPRING	0.038	0.008	4.598
MONTHS	-0.007	0.0006	-12.042
%EMPLOY-PRE	-0.083	0.032	-2.623
%WELF-PRE	0.086	0.026	3.291
ANYARRESTS-PRE	-0.017	0.045	-0.375
MJ/ALCOHOL-PRE	-0.002	0.021	-0.108
COKE/HEROIN-PRE	0.080	0.045	1.775
LAMBL	0.024	0.027	0.913
CAT1 0 TO 6	-0.314	0.050	-6.287
CAT1 6 TO 12	-0.140	0.046	-3.035
CAT1 12 TO 18	-0.082	0.043	-1.924
CAT1 18 TO 24	-0.082	0.043	-1.925
CAT1 24 TO 30	-0.109	0.043	-2.532
CAT1 30 TO 36	-0.063	0.043	-1.484
CAT1 36 TO 42	-0.022	0.042	-0.515
CAT1 42 TO 48	0.010	0.044	0.238
CAT1 48 TO 54	-0.055	0.059	-0.917
CAT2 0 TO 6	-0.071	0.055	-1.291
CAT2 6 TO 12	0.019	0.051	0.375
CAT2 12 TO 18	0.046	0.046	1.007

Table III.13 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	0.001	0.045	0.029
CAT2 24 TO 30	-0.043	0.046	-0.930
CAT2 30 TO 36	-0.024	0.046	-0.508
CAT2 36 TO 42	0.024	0.046	0.528
CAT2 42 TO 48	0.033	0.047	0.697
CAT2 48 TO 54	0.034	0.058	0.581
CAT3 0 TO 6	-0.025	0.092	-0.272
CAT3 6 TO 12	0.138	0.097	1.416
CAT3 12 TO 18	0.268	0.081	3.316
CAT3 18 TO 24	0.141	0.073	1.938
CAT3 24 TO 30	-0.065	0.074	-0.880
CAT3 30 TO 36	-0.047	0.077	-0.609
CAT3 36 TO 42	0.036	0.071	0.504
CAT3 42 TO 48	0.044	0.072	0.614
CAT3 48 TO 54	0.058	0.080	0.723

Number of observations = 9,479

- Number of individuals = 1,002
- Average number of time periods = 9.4573

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.516

Mean of dependent variable = 0.255

F-Statistic for equation = 11.076

- Degrees of freedom = 50; 9,428
- Significance level = > 99% statistical confidence

[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.14

REGRESSION ESTIMATES FOR IMPACT ON NUMBER OF ARRESTS PER SIX MONTHS, INCLUDING MILITARY: MALES[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	0.156	0.358	0.436
AGE	-0.002	0.021	-0.093
AGE18	-0.022	0.026	-0.867
OVER18	-0.004	0.022	-0.202
AGE21	-0.001	0.008	-0.130
OVER21	0.004	0.005	0.788
EDUCATION-PRE	-0.004	0.002	-1.760
DIPLOMA-PRE	-0.009	0.014	-0.621
EDUCATION12-PRE	0.003	0.017	0.157
BLACK	-0.013	0.006	-2.116
HISPANIC	-0.010	0.008	-1.170
AMERICAN INDIAN	0.021	0.014	1.481
OTHER RACE/ETH	0.008	0.023	0.341
HEALTHPROB-BASE	-0.009	0.012	-0.758
FALL	-0.002	0.006	-0.440
WINTER	0.008	0.006	1.401
SPRING	-0.013	0.006	-2.347
MONTHS	-0.0002	0.0003	-0.801
%EMPLOY-PRE	-0.001	0.007	-0.128
%WELF-PRE	0.008	0.013	0.650
ANYARRESTS-PRE	0.027	0.007	4.056
MJ/ALCOHOL-PRE	0.023	0.006	3.957
COKE/HEROIN-PRE	0.027	0.008	3.23
LAMBDA	0.006	0.011	0.581
CAT1 0 TO 6	-0.024	0.019	-1.253
CAT1 6 TO 12	-0.011	0.019	-0.581
CAT1 12 TO 18	0.014	0.019	0.728
CAT1 18 TO 24	-0.013	0.019	-0.656
CAT1 24 TO 30	-0.010	0.019	-0.526
CAT1 30 TO 36	-0.009	0.020	-0.465
CAT1 36 TO 42	-0.009	0.021	-0.421
CAT1 42 TO 48	0.020	0.021	0.934
CAT1 48 TO 54	0.072	0.029	2.486
CAT2 0 TO 6	0.013	0.020	0.667
CAT2 6 TO 12	0.009	0.020	0.440
CAT2 12 TO 18	0.031	0.020	1.532

Table III.14 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	0.041	0.020	2.015
CAT2 24 TO 30	0.002	0.020	0.097
CAT2 30 TO 36	0.025	0.022	1.169
CAT2 36 TO 42	-0.005	0.022	-0.244
CAT2 42 TO 48	0.019	0.022	0.832
CAT2 48 TO 54	-0.010	0.032	-0.315
CAT3 0 TO 6	-0.0002	0.027	-0.007
CAT3 6 TO 12	-0.008	0.027	-0.296
CAT3 12 TO 18	-0.011	0.028	0.409
CAT3 18 TO 24	0.019	0.028	0.685
CAT3 24 TO 30	-0.009	0.028	-0.302
CAT3 30 TO 36	0.002	0.030	0.077
CAT3 36 TO 42	0.021	0.033	0.640
CAT3 42 TO 48	0.035	0.033	1.056
CAT3 48 TO 54	-0.014	0.039	-0.365

Number of observations = 38,578

- Number of individuals = 3,245
- Average number of time periods = 11.888

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.031

Mean of dependent variable = 0.057

F-Statistic for equation = 4.401

- Degrees of freedom = 50; 38,527
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.15

REGRESSION ESTIMATES FOR IMPACT ON NUMBER OF ARRESTS PER SIX MONTHS, INCLUDING MILITARY: FEMALES WITHOUT CHILDREN[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	0.336	0.168	2.00
AGE	-0.019	0.010	-1.859
AGE18	0.026	0.013	1.929
OVER18	0.016	0.010	1.597
AGE21	0.0002	0.005	0.045
OVER21	0.001	0.003	0.350
EDUCATION-PRE	-0.001	0.001	-1.018
DIPLOMA-PRE	-0.003	0.007	-0.441
EDUCATION12-PRE	0.009	0.008	1.100
BLACK	-0.003	0.004	-0.894
HISPANIC	-0.004	0.005	-0.732
AMERICAN INDIAN	0.050	0.009	5.340
OTHER RACE/ETH	-0.010	0.015	-0.705
HEALTHPROB-BASE	0.0001	0.006	0.027
FALL	0.002	0.003	0.625
WINTER	0.003	0.003	0.864
SPRING	0.001	0.003	0.161
MONTHS	0.0002	0.0002	1.445
%EMPLOY-PRE	-0.0002	0.004	-0.043
%WELF-PRE	-0.003	0.006	-0.497
ANYARRESTS-PRE	-0.002	0.007	-0.323
MJ/ALCOHOL-PRE	0.004	0.003	1.480
COKE/HEROIN-PRE	0.016	0.005	2.985
LAMBDA	0.007	0.005	1.584
CAT1 0 TO 6	-0.007	0.008	-0.817
CAT1 6 TO 12	-0.014	0.008	-1.707
CAT1 12 TO 18	-0.018	0.009	-2.165
CAT1 18 TO 24	-0.013	0.009	-1.524
CAT1 24 TO 30	-0.020	0.009	-2.170
CAT1 30 TO 36	-0.019	0.010	-1.987
CAT1 36 TO 42	-0.020	0.010	-1.931
CAT1 42 TO 48	-0.020	0.011	-1.771
CAT1 48 TO 54	0.013	0.020	0.644
CAT2 0 TO 6	-0.010	0.009	-1.179
CAT2 6 TO 12	-0.014	0.009	-1.508
CAT2 12 TO 18	0.017	0.010	1.714

Table III.15 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	0.005	0.010	0.456
CAT2 24 TO 30	-0.018	0.011	-1.699
CAT2 30 TO 36	-0.015	0.012	-1.276
CAT2 36 TO 42	-0.023	0.013	-1.787
CAT2 42 TO 48	0.011	0.014	0.807
CAT2 48 TO 54	-0.024	0.024	-1.021
CAT3 0 TO 6	-0.022	0.014	-1.500
CAT3 6 TO 12	-0.024	0.016	-1.548
CAT3 12 TO 18	-0.021	0.017	-1.195
CAT3 18 TO 24	0.011	0.019	0.579
CAT3 24 TO 30	-0.020	0.019	-1.072
CAT3 30 TO 36	-0.021	0.020	-1.044
CAT3 36 TO 42	-0.019	0.025	-0.773
CAT3 42 TO 48	-0.020	0.025	-0.805
CAT3 48 TO 54	-0.019	0.031	-0.609

Number of observations = 13,553

- Number of individuals = 1,489
- Average number of time periods = 9.100

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.021

Mean of dependent variable = 0.009

F-Statistic for equation = 2.314

- Degrees of freedom = 50; 13,502
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

TABLE III.16

REGRESSION ESTIMATES FOR IMPACT ON NUMBER OF ARRESTS PER SIX MONTHS: FEMALES WITH CHILDREN[a/]

Explanatory Variable[b/]	Coefficient Estimate	Standard Error[c/]	T-Statistic[d/]
CONSTANT	-0.296	0.390	-0.757
AGE	0.018	0.023	0.785
AGE18	-0.035	0.028	-1.272
OVER18	-0.019	0.023	-0.830
AGE21	-0.001	0.006	-0.207
OVER21	-0.0002	0.003	-0.084
EDUCATION-PRE	0.0006	0.002	0.410
DIPLOMA-PRE	0.019	0.010	1.912
EDUCATION12-PRE	-0.014	0.011	-1.229
BLACK	0.007	0.004	1.654
HISPANIC	0.003	0.006	0.455
AMERICAN INDIAN	0.006	0.010	0.661
OTHER RACE/ETH	0.004	0.032	0.131
HEALTHPROB-BASE	0.005	0.008	0.626
FALL	-0.003	0.004	-0.754
WINTER	0.002	0.004	0.628
SPRING	0.004	0.004	1.142
MONTHS	0.0002	0.0002	1.146
%EMPLOY-PRE	0.009	0.005	1.851
%WELF-PRE	0.003	0.004	0.690
ANYARRESTS-PRE	-0.007	0.007	-1.002
MJ/ALCOHOL-PRE	0.0002	0.003	0.069
COKE/HEROIN-PRE	0.005	0.008	0.594
LAMBDA	0.010	0.004	2.455
CAT1 0 TO 6	-0.019	0.015	-1.290
CAT1 6 TO 12	0.008	0.012	0.628
CAT1 12 TO 18	-0.011	0.010	-1.146
CAT1 18 TO 24	-0.019	0.010	-1.967
CAT1 24 TO 30	-0.019	0.010	-1.977
CAT1 30 TO 36	-0.019	0.010	-1.950
CAT1 36 TO 42	-0.008	0.009	-0.884
CAT1 42 TO 48	-0.012	0.010	-1.220
CAT1 48 TO 54	-0.018	0.021	-0.842
CAT2 0 TO 6	-0.020	0.017	-1.199
CAT2 6 TO 12	-0.019	0.015	-1.274
CAT2 12 TO 18	-0.017	0.011	-1.533

Table III.16 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 TO 24	-0.009	0.011	-0.810
CAT2 24 TO 30	-0.018	0.011	-1.632
CAT2 30 TO 36	-0.019	0.011	-1.619
CAT2 36 TO 42	-0.018	0.011	-1.725
CAT2 42 TO 48	0.0006	0.012	0.055
CAT2 48 TO 54	-0.018	0.020	-0.912
CAT3 0 TO 6	0.132	0.033	3.999
CAT3 6 TO 12	-0.024	0.036	-0.676
CAT3 12 TO 18	-0.019	0.026	-0.722
CAT3 18 TO 24	0.017	0.020	0.831
CAT3 24 TO 30	-0.020	0.021	-0.943
CAT3 30 TO 36	-0.025	0.024	-1.060
CAT3 36 TO 42	-0.01[8]	0.019	-0.964
CAT3 42 TO 48	-0.023	0.020	-1.189
CAT3 48 TO 54	-0.019	0.026	-0.747

Number of observations = 9,479

- Number of individuals = 1,002
- Average number of time periods = 9.457

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.019

Mean of dependent variable = 0.006

F-Statistic for equation = 1.236

- Degrees of freedom = 50; 9,428
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used, based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.1.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns, because of less rounding error.

IV. IMPACT OF JOB CORPS ON EMPLOYMENT AND EARNINGS

The short-term postprogram impact of Job Corps on the employment and earnings of former Corpsmembers is obviously an important measure of whether or not the Job Corps program is achieving its primary objective--to improve the lifetime economic prospects of its participants. As one of its most prominent and immediate goals, Job Corps is designed to increase the employability of participating youths. The hypothesized economic effects of Job Corps outlined in Chapter II are generally consistent with that goal. In this chapter we present our empirical evidence on the impact of Job Corps on employment and earnings. After providing some background on the computations, we present our overall estimates for civilians and for youths in the military, and, in turn, discuss our findings on (1) the differential impacts among major subgroups, (2) the longevity of the estimated effects, and (3) the sensitivity of the estimates to alternative econometric specifications.

A. BACKGROUND ON THE COMPUTATIONS

Table IV.1 summarizes our overall findings, and, at the end of the chapter, Tables IV.2 through IV.18 provide detailed documentation. The more detailed tables include the following:[1] (1) estimates of what Corpsmembers' postprogram behavior would have been had they not

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[1] Tables IV.2 through IV.7 present the detailed findings on civilians; Tables IV.8 through IV.18 provide detailed findings on civilian and military jobs together.

TABLE IV.1

SUMMARY OF MAIN FINDINGS FOR OVERALL IMPACTS ON EMPLOYMENT AND EARNINGS, PER CORPSMEMBER ON AN ANNUALIZED BASIS

Variable	Estimated Job Corps Effects For First Year After Termination	Estimated Job Corps Effects For Second Year After Termination	Estimated Job Corps Effects For Third Year After Termination	Estimated Job Corps Effects For Fourth Year After Termination
1. Weeks employed, civilians	1.56	4.84	3.12	1.35
2. Weeks employed, controlling for nonlinear time trends, civilians	1.75	4.20	2.80	2.04
3. Weeks employed, civilians and military	1.35	5.04	4.06	2.81
4. Weeks employed, controlling for nonlinear time trends, civilians and military	1.53	4.37	3.69	3.51
5. Earnings, civilians	$406	$626	$386	$ 77
6. Earnings in 1977 dollars, civilians	$277	$565	$342	$ 24
7. Earnings, controlling for nonlinear time trends, civilians	$571	$608	$442	$370
8. Earnings, civilians and military	$376	$706	$626	$533
9. Earnings in 1977 dollars, civilians and military	$262	$652	$567	$405
10. Earnings, controlling for nonlinear time trends, civilian and military	$515	$667	$652	$787
11. Probability in military	0.019	0.039	N.A.	0.040

participated in Job Corps (Tables IV.2, IV.3, IV.8, and IV.9);[1] (2) more refined breakdowns by major subgroup and time period than are presented in the summary table (Tables IV.4, IV.5, IV.10, and IV.11); (3) estimates for additional measures of employment and related effects (Tables IV.6, IV.7, IV.12, and IV.13); (4) alternative estimates derived from varying the underlying econometric specifications (Tables IV.14 through IV.18); and (5) estimates for employment-related activities that are denominated as the fraction of time spent in the various activities, which can readily be converted into any relevant time units (all of the detailed tables--Tables IV.2 through IV.18).[2]

Using the equations presented in Chapter III, we obtained estimates of Job Corps effects by program-completion status (program completers, partial completers, and early dropouts) for each six-month postprogram time period (eight six-month time intervals altogether for the four postprogram years),[3] and separately for each of the three subgroups of sex and child

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[1] Tables IV.2, IV.3, IV.8, and IV.9 provide background on what we estimate would have happened to Corpsmembers' employment and earnings had they not participated in Job Corps (akin to sample means), and, hence, are the backdrop against which Job Corps effects should be considered.

[2] For example, the estimates for "weeks worked per six months" in the various tables are obtained by multiplying the estimates for the fraction of time "employed" in the relevant six-month period by 26. Similarly, to obtain estimates for weeks worked on an annual basis, we would simply multiply the fraction-of-time estimates by 52.

[3] We actually have nine six-month intervals (see Table IV.18) and a postprogram time interval of up to 54 months (four-and-one-half years). However, we have so few observations beyond 48 months that reliable estimates cannot be computed for the postprogram time period of 48 to 54 months. However, we do present such estimates in Table IV.18 as an alternative measure of what was occurring toward the end of our observation period.

responsibility (males, females who have no children present, and females who have children living with them--and whose behavior is so different from each other that they necessitate completely separate estimation). For each six-month postprogram period, we then obtained estimates of the average Job Corps effects and the corresponding levels of statistical significance for each subgroup of sex and child responsibility. These estimates of average subgroup effects were obtained by computing the weighted average of the subgroup estimates by completion status, where the weights used were the Job Corps population fractions for each completion status--program completers, partial completers, and early dropouts (respectively, 0.30, 0.30, and 0.40). The top three panels of Tables IV.2 through IV.18 show the estimates of the average Job Corps effects for each of the three subgroups.

Next, for each six-month postprogram period, we obtained overall estimates (representative of all Corpsmembers as a whole) and the corresponding levels of statistical significance by computing the weighted average of the estimates by the three sex and family status subgroups and using the Job Corps population weights for males, females without children, and females with children.[1/] The bottom panels of Tables IV.2 through IV.18 show the estimates of the overall Job Corps effects. Finally, we

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[1/] The appropriate weight for males is 0.70 for all eight of the six-month time periods. The appropriate weights for females without children decline over time as more of the Corpswomen have children; the weights are 0.28, 0.26, 0.22, 0.21, 0.20, 0.18, 0.15, and 0.15 across the eight respective six-month time periods. Correspondingly, the appropriate weights for females with children are 0.02, 0.04, 0.08, 0.09, 0.10, 0.12, 0.15, and 0.15 across the eight respective six-month time periods.

then computed simple averages over the six-month time periods to obtain the annual estimates, as summarized in Table IV.1.)

Table IV.1 presents summary findings both on civilians only (excluding youths in the military services) and on all Corpsmembers, including both those in the military services and civilians. (Tables IV.2 through IV.7 are restricted to civilians, while Tables IV.8 through IV.18 include observations on youths in the military services, as well as civilians.) When youths with military jobs are included, we assume that--

- They are employed
- They work 40 hours per week (which is probably too low)
- Their earnings and promotions are made at a relatively low rate, similar to the rate observed for recent enlistees from disadvantaged backgrounds (which could be too low for Corpsmembers, compounded by the fact that we do not fully capture the added nonwage benefits from military employment)
- They are not enrolled in any education or training programs (although military service and the education and training that it typically entails for these youths will be considered separately as an investment in their human capital)
- They are not receiving any public transfers outside the military (i.e., no welfare payments, Food Stamps, Unemployment Insurance, etc.), and
- They do not commit any crimes outside the jurisdiction of the military services while they are in the military

While these assumptions are somewhat crude, they enable us to integrate the military effects directly into the regular analysis and to obtain better estimates of Job Corps effects than would otherwise be possible. In addition, this integration yields estimates that are necessary for benefit-cost

purposes, without having to undertake any additional imputations to incorporate military effects.

B. OVERALL FINDINGS ON CIVILIANS AND INCLUDING MILITARY SERVICE

As expected, our estimates of what Corpsmembers' behavior would have been had they not participated in Job Corps indicate that the economic prospects for these youths would have improved somewhat over time as they grew older (see Tables IV.2, V.3, IV.8, and IV.9).[1/] On an absolute scale, however, consistent with our earlier findings on the disadvantaged status of Corpsmembers at pre-enrollment (see Kerachsky and Mallar, 1978), the economic prospects for these youths would have been bleak had they not participated in the program.[2/] The employment rate (as a fraction of all youths) would have risen over the four-year postprogram period, but only approximately from 40 to 50 percent. When employed, most of the youths worked full time (approximately 40 hours per week), so that the time trend in hours worked per week reflects primarily the changes in employment (which is generally the case with our estimates).[3/]

[1/] This natural improvement over time, although at a small annual rate, provides further confirmation of an upward bias in using before-after comparisons to estimate program effects for youths (by the fourth postprogram year, such bias would have been considerable).

[2/] The pre-enrollment and during-program time periods show even worse prospects, since the youths were even younger (many of whom under the age of 18).

[3/] This phenomenon (i.e., that the estimates for hours worked per week reflect primarily the employment rate) is pervasive throughout the findings. Consequently, as a measure of time at work, we will focus primarily on employment.

Earnings would have increased approximately from $2,500 per year in the immediate postprogram time period to $5,500 four years later. However, approximately one-half of this growth in earnings appears to be associated with general inflation, and the bulk of the remaining half is associated with increases in employment.[1] The implicit real growth in hourly wage rates above inflation would have been less than 12 percent--approximately from $3.31 to $3.70 (in 1977 dollars)--and the increase in work time would have been approximately 45 percent. Enlistments in the military would have grown initially, approximately from 3 percent at six months to 5 percent at eighteen months, but would have then declined to under 3 percent by the end of the four-year observation period. When military jobs are aggregated together with civilian jobs, the findings are very similar to those for civilians (which is generally the case with our estimates). All in all, there would have been substantial room for improvement in the employment opportunities for Job Corps youths.

In comparing the employment and earnings of Corpsmembers during the postprogram period to what they would have been had they not participated in Job Corps,[2] we find that the program did lead to increases in employment and earnings for former Corpsmembers. Rows 1 and 2 of Table IV.1 show an estimated gain in civilian employment of approximately three weeks per

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[1] The GNP deflator was used to convert nominal dollar amounts into 1977 dollars in order to obtain the estimates of the proportion of earnings gains related to general inflation over the four-year period.

[2] Again, it should be emphasized that this is not a comparison of Job Corps to "no program." If the Corpsmembers had not participated in Job Corps, they would have obtained some amount of education, training, and work experience through alternative programs during the program period.

year--an average estimated gain of just under three weeks per year for the entire four-year postprogram period, and just over three weeks per year after the first few postprogram months (when Corpsmembers' employment and earnings were low, as they were making the transition from center life to the regular labor market). As indicated by comparing rows 1 and 2 of Table IV.1, these estimated employment gains are, on average, affected very little by whether our estimating equations contain a simple linear time trend or a more flexible nonlinear time trend to control for trends in the underlying (employment) behavior in the absence of the impact of Job Corps.[1/] Across the postprogram time period, however, the estimated employment effects for civilians are much flatter (i.e., more nearly constant) when the nonlinear specification is used to control for underlying time trends (see further in Section IV.D).

The detailed tables (Tables IV.4 and IV.5) indicate that the increases in civilian employment generally are statistically significant (i.e., they are unlikely to be caused by chance).[2/] As with previous

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[1/] For the estimates reported as "controlling for nonlinear time trends," the squared value of MONTHS (the number of months since April 1977) was added as an explanatory variable. We also experimented with adding a cubed term in addition to the squared term; however, the cubic specification yields virtually the same results as the quadratic, but at much greater computational cost because of near multicollinearity. Most of our estimates also excluded the squared term because of added computational costs with the resulting high degree of multicollinearity when the squared term was included for some dependent variables.

[2/] Larger-than-usual confidence levels for two-tailed statistical tests and their one-tailed test equivalents are shown in all of our detailed tables, the purpose of which is to provide additional information on the variances (or standard errors) of estimates in a convenient format, and because it can be argued that one-tailed tests are appropriate, since participation in Job Corps is not expected to have deleterious effects on behavior.

estimates (see Mallar et al., 1978 and 1980), the detailed breakdowns in Table IV.4 show a pattern that exhibits a large growth in estimated Job Corps effects on employment during the first few postprogram months, beginning with some negative effects (as compared to what the youths' employment would have been at that time had they not participated in Job Corps) as Corpsmembers make the transition from center life to re-entering the regular labor market. Some transition problems are expected in light of the fact that many of the Corpsmembers have recently been out of the labor market because of their participation in Job Corps. However, the magnitude of the transition problems (see Mallar et al., 1978 and 1980) suggests that the main impacts of Job Corps do not stem from job placements. In comparing the details of our estimates of the impact of Job Corps on civilian employment (shown in Tables IV.4 and IV.5) to the corresponding details of our estimates of what Corpsmembers' employment would have been in the civilian sector had they not participated in Job Corps (shown in Tables IV.2 and IV.3), we find an average increase in civilian employment rates of approximately 6 percentage points for Corpsmembers--increasing approximately from 45 percent to 51 percent.[1/]

The estimates also show a substantial increase in military jobs for former Corpsmembers (e.g., see row 11 of Table IV.1). The estimated military effects are large and statistically significant. By the time of the third follow-up survey, we estimate that Job Corps more than doubles

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[1/] The observed sample means for Corpsmembers can be obtained by adding the estimated program effects (e.g., as in Tables IV.4 and IV.5) to the estimated values for Corpsmembers had they not participated in Job Corps (e.g., to the values for corresponding variables in Tables IV.2 and IV.3).

the rate of military service--approximately from 2.8 percent to 6.8 percent (a 143 percent increase).

When military jobs are integrated with civilian jobs, the findings on employment and earnings are very similar to those for civilians, except that the effects are somewhat larger. Aside from the lower first year effects, the estimated overall increase in employment for Corpsmembers is nearly four weeks per year when both military and civilian jobs are combined. Also, the estimated effects which include the military sector have greater statistical significance than for civilians alone. The time patterns of estimated Job Corps effects and the influences of controlling for nonlinear time trends are similar to those for civilians--a time path for estimated Job Corps effects that shows substantial increases in the first few postprogram months, and effects that, on average, are approximately the same but that are more nearly constant over time when controlling for nonlinear time trends rather than controlling only for a simple linear time trend.

The average estimated increase in earnings for Corpsmembers is in the neighborhood of $400 per year (or approximately 10 percent) for civilians and $600 per year (or approximately 15 percent) when youths in the military service are included (see rows 5 through 10 in Table IV.1). These estimated Job Corps effects on earnings generally are statistically significant and follow a pattern over time that is similar to the employment effects (see Tables IV.4, IV.5, IV.10, and IV.11). The primary difference between the estimated earnings gains and the employment gains is that the earnings effects are much more sensitive to the controls for underlying time trends. When the earnings time trend in the absence of Job

Corps is allowed to be nonlinear, the estimated Job Corps effects on earnings are not only much more equal over time, but are also approximately $100 per year higher on average and very much higher and much more significant in the fourth postprogram year (see further in Section IV.D).

Using the GNP deflator, we also computed estimated earnings gains based on earnings denominated in constant 1977 dollars. As should be expected, these estimates are somewhat lower than those in current dollars (increases of approximately $300 and $500 for, respectively, civilians and with military jobs included). However, the levels of statistical significance are approximately the same and, if anything, are somewhat improved with the constant dollar estimates.

Which particular deflator should be used to obtain constant dollar estimates depends on one's objectives, but does not affect our basic results. We used the GNP deflator because our primary purpose was to obtain constant dollar estimates to be used in the benefit-cost analysis, which focuses on resource benefits and costs to society as a whole. For resource values to society as a whole, the GNP deflator seemed most appropriate to us.[1/]

Before ending our discussion on the impacts of Job Corps on employment and earnings, we must highlight one potential anomaly lurking

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[1/] The estimates underlying row 9 of Table IV.1 are those used in the benefit-cost analysis. Since these estimates did not control for the nonlinearity of time trends in the absence of Job Corps, it could be argued that they are approximately $100 per year on the low side. However, we relied primarily on the linear time trend because we wanted all of our estimates to be based on the same basic equation, and because, for some cases, the underlying computations became very difficult and costly when trying to control for nonlinear time trends (the underlying data matrices became ill-conditioned because of near multicollinearity).

behind the numbers. From the estimated effects on employment and earnings, we can infer that little if any gains occur with respect to hourly wage rates. However, this could be explained by one of three factors: (1) with increases in employment and military service among Corpsmembers, those who are employed or who are in one of the military services are drawn increasingly from the lower end of the ability spectrum, (2) the gains in earnings among youths in the military service have been undervalued (especially by not taking full account of their extra nonwage benefits and various bonuses), or (3) youth labor markets contain effective constraints (particularly minimum-wage legislation) that tend to equalize the observed hourly wage rate for young age groups.[1/]

Tables IV.6, IV.7, IV.12, and IV.13 present estimates of other employment-related Job Corps effects. These other measures include activity rates (employed, in school, or in training), actively looking for work, participating in the labor force (employed or actively looking for work), keeping house, being employed in a union job, and being employed in a CETA public-service employment (PSE) job that provides publicly subsidized employment. The findings for activity rates mirror those for employment in terms of both size and statistical significance, which indicates that Job Corps leads to increased employment activity but has little effect on the overall amount of time spent in school or training

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[1/] The federal minimum wage was raised from $2.30 per hour to $2.65 in January 1978, to $2.90 in January 1979, to $3.10 in January 1980, and to $3.25 in January 1981. The federal minimum equalled approximately two-thirds of the average comparison wage throughout the observation period, with many of the youths being at or near this minimum.

during the postprogram period (see the further confirmation of this phenomenon in Chapter V).

Estimated reductions are obtained for the amount of time spent actively looking for work; these estimated effects are generally statistically significant. Furthermore, these reductions in job search are of approximately the same magnitude as the estimated increases in employment. As a result of the offsetting findings for employment and looking for work, the estimated Job Corps effects on labor-force participation are virtually nil (i.e., very small, changeable in direction, and statistically insignificant). This observed reduction in job search is perhaps an artifact of the increased employment combined with little change in school or training, so that when interview respondents are forced to report some activity they tend to report looking for work when they are not engaged in employment, school, or training.

The estimated effects of Job Corps on the amount of time spent keeping house shows an interesting time pattern over the postprogram period--initially some statistically significant reductions that increasingly become positive over time and are positive and marginally significant by the end of the fourth postprogram year. This time pattern can be explained by the Job Corps effects on childbirths--they are initially delayed, thereby necessitating fewer family responsibilities, but they eventually become increasingly prevalent, thereby necessitating increased family responsibilities among Corpsmembers (see further in the next section).

Finally, the estimated Job Corps effects on the amount of time spent in union jobs (a positive activity that could lead to future

increases in employment and earnings) and PSE jobs (a publicly subsidized activity which indicates failure in the regular labor market) show virtually zero impact. The estimates for both union jobs and PSE jobs are small in magnitude, changeable in direction, and statistically insignificant.

C. DIFFERENTIAL IMPACTS AMONG SUBGROUPS OF CORPSMEMBERS

In this section we consider the empirical evidence on the differential impacts among subgroups of Corpsmembers by sex and child responsibility and by Job Corps completion statuses. Differential impacts among different program treatments have been explored in earlier reports (see Mallar et al., 1978 and 1980) and will not be pursued further here.[1/] The estimates underlying our discussion are presented in the detailed tables (Tables IV.2 through IV.18) and in graphic form in Figures IV.1 through IV.6.

1. Differential Impacts by Sex and Child Responsibility

Differential impacts among Corpsmembers are found to be associated with sex and child responsibility. The estimates indicate that males and females without children consistently benefit much more in terms of

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[1/] Some of the most important of these earlier findings include larger beneficial impacts for completing a CED program and for residing at centers that provide a more equal coed environment. Differential impacts were also found to be associated with the type of vocational training received, although the cause of the latter correlations could not be inferred directly from the data (e.g., whether due to the programs or to selectivity in assigning participants to training components--for example, assigning youths who cannot read or who have other employability problems to certain components).

employment, earnings, and related activities than do females with children. With respect to estimates of Job Corps effects on employment and earnings, the primary difference between males and females without children is that a much larger impact on the probability of being in a military job occurs for males. However, for females without children, the impacts on employment and earnings are larger than for males on a percentage basis, in part because the females start from a much lower base (i.e., compared to males, females would have had less employment and earnings in the absence of Job Corps).

The estimated Job Corps impacts for females who have children living with them are generally much more negative than for either males or females without children. This can be attributed to delayed childbirths among Corpswomen during the in-program and early postprogram periods, such that those who had children living with them at some time during the postprogram period are more likely to have been faced temporarily with labor-market constraints from pregnancy (which also accounts for their higher reporting of health problems) and from having very young children during the ensuing period of postprogram observation (see Mallar et al., 1980, Chapter IV). The fact that the negative estimated effects for females with children disappear and even become slightly positive when we take into account differences between the Job Corps and comparison groups in terms of the presence of very young children (ibid.) provides empirical evidence of this explanation for the observed negative impacts on females with children.

In previous reports, females with children had little bearing on the overall Job Corps estimates, since they comprised such a small fraction

of Corpsmembers. However, the fraction of Corpswomen with children grew considerably over the course of the postprogram observation period (approximately from 2 percent of all Corpsmembers during the first six months to 15 percent during the last six months). Thus, throughout this report, we have included their negative effects in the overall estimates.

Our estimation procedures probably impart negative bias to the findings on females for at least two reasons. First, the negative effects for females without children might simply be temporary and might disappear if we control for the presence of very young children due to delayed childbirths. Second, we do not include the increased employment and earnings from delayed and reduced childbirths, which leads to the presence of a greater number of female Corpsmembers without child constraints and, as a result, who have higher employment and earnings. If the observed differences in fertility patterns between Corpsmembers and the comparison group would have occurred nonetheless in the absence of Job Corps, then our estimation procedures are appropriate. However, to the extent that Job Corps reduces fertility and delays childbearing (as it appears to do), we have underestimated Job Corps benefits. Unfortunately, our study was not designed in a way to obtain reliable estimates of reduction in fertility and delays in childbearing.

2. Differential Impacts by Category of Job Corps Termination

We also find differential program impacts among Corpsmembers by categories of program completion (see Tables IV.14 and IV.15 and Figures IV.1 through IV.6). A substantial positive correlation exists between the estimated Job Corps effects and the proportion of the Job Corps program completed. Program completers consistently benefit the most in terms of

employment and earnings (much larger estimated effects--more than twice as large as the overall estimated impacts--that are uniformly positive and statistically significant). Early dropouts are found to benefit little or not at all (generally positive but small and statistically insignificant effects). The intermediate group, partial completers, are found to benefit an intermediate amount (uniformly positive, small, and marginally significant effects and approximately one-third the size of impacts for program completers).[1/]

Differences in impacts by completion category are important in this evaluation for two reasons. First, we oversampled program completers and must estimate the impacts for each completion category so that we can reweight them to obtain overall impact estimates that are indicative of the average for all Job Corps enrollees (see Chapter III and Section A of this chapter for more details on this procedure). Second, as measures of the effects of changing the length of stay in Job Corps or the degree of program completion,[2/] these differential impacts are pertinent to improving program operations.

An important issue in drawing inferences for program operations is whether the estimated differential effects are attributable to program completion or to underlying differences among Corpsmembers who self-select

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[1/] The period-to-period fluctuations in estimates for partial completers and early dropouts and their low levels of statistical significance are caused, in part, by their small sample sizes.

[2/] As explained in Chapter III, completion category is not perfectly correlated with length of stay because of the individualized and self-paced nature of Job Corps instruction.

and are selected into different completion statuses (i.e., the sample selectivity problem). As mentioned in Chapter III, we have been unable to obtain reliable estimates that control for unobserved differences among Corpsmembers by completion category. For a number of reasons, however, we believe that the differential effects can plausibly be attributed, at least in part, to program completion.

First, the pattern of effects by completion category is reasonable; in particular, the effects estimated for the group with near-zero treatment (early dropouts) are close to zero. Second, we control for a wide range of observable variables, some of which might also be proxy controls for the effects of unobservable characteristics. Third, any remaining causes of selectivity bias are likely to work in opposite directions and to be at least partially offsetting; for example, the Job Corps completion category includes both youths who are highly motivated and able (i.e., high benefits to staying in the program) and youths who have little initiative and poor labor-market opportunities (i.e., low opportunity costs to staying in the program). Thus, the benefits from Job Corps can be improved by retaining Corpsmembers longer and having more of them complete the program.

D. LONGEVITY OF ESTIMATED EFFECTS

The best evidence on the timing of Job Corps effects is provided graphically in Figures IV.1 through IV.6. These graphs are based on a piecewise linear specification of the timing of effects during the postprogram observation period, which includes 21 variables for each completion status--an intercept term (1 variable), slope changes by month during the first quarter (3 variables), and slope changes by quarter

during the remainder of the postprogram observation periods (17 variables).[1/]

The pattern of findings for the early postprogram period is generally consistent with that reported earlier (see Mallar et al., 1978 and 1980). After some initial downfall during the transition from center life to the regular labor market (and after having been out of the regular labor market from a few days for early dropouts to up to two years for program completers), the Job Corps effects on employment and earnings become increasingly positive over the first few postprogram months. Furthermore, the current findings for the short-term postprogram period are similar in magnitude to those reported previously--overall, approximately a 10 percentage-point increase in employment. The estimates average out to near zero for the first six months of postprogram observation and then become positive thereafter--at approximately a 10 percentage-point increase in employment (see detailed tables also). (The sources of Job Corps impacts still appear to stem from those other than initial job placement.) Any small differences between the current findings for the short-term postprogram period and those reported earlier are attributable primarily to the greater precision in the current estimates--due to added observations

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[1/] The longest we observed anyone was nearly 18 quarters (54 months) for the handful of Corpsmembers who were interviewed early at baseline, left Job Corps soon thereafter, and were interviewed late during the second follow-up (2 partial quarters at the beginning and end, and 17 full quarters in between).

and a longer observation period that help control for spurious events and non-Job Corps influences.[1/]

The most interesting new finding from the extended postprogram observation period is the relatively stable estimates of employment and earnings gains among Corpsmembers, especially program completers, for months 3 to 48 in the postprogram period (and similarly on out to 51 months for males, the group for whom we have adequate data to extend the graphs from 48 to 51 postprogram months).[2/] The positive, overall impacts generally persist throughout the four years of postprogram observation. The trend over the four-year postprogram observation period appears to be an increase in program benefits during the first few months and then relatively stable effects throughout the rest of the four-year period, with little evidence of fadeout (especially when military jobs are included and nonlinear time trends in the economy are taken into account).[3/]

[1/] We find no support for the quick fadeout that was previously inferred (see Goldstein, 1972) from comparisons of Cain's (1968) six-month findings to Woltman and Walton's (1968) eighteen-month findings. This is not particularly surprising, since the program has undoubtedly changed, and because both of these earlier studies were based on much less adequate data, which prevented researchers from undertaking the type of rigorous statistical analysis which underlies our findings.

[2/] The figures end at 48 months of postprogram observations for females and 51 months for males, because we have too few observations to provide reliable estimates beyond that point. The more erratic fluctuations for the early dropout group (and, to a lesser extent, the partial-completer group) can be attributed to the small number of observations.

[3/] In the absence of the nonlinear term, the Job Corps variables pick up part of the downturn in the economy towards the end of the observation period.

The evidence is mixed as to whether the program effects were growing or shrinking toward the end of the observation period. When only civilian jobs are considered and a simple linear time trend is assumed for the economy, the empirical estimates show a substantial shrinkage of the employment and earnings effects for males and overall during the fourth postprogram year. However, when military jobs are included (as we believe they should be) and better account is taken of the time trends in the economy, the employment effects are relatively constant for the fourth postprogram year (see row 4 in Table IV.1 and Figure IV.6), and the earnings effects show a sizable (21 percent) growth (see row 10 in Table IV.1).

Furthermore, for males--the largest group of Corpsmembers (70 percent of all Corpsmembers)--we have adequate observations to extend our analysis further, from 48 to 51 postprogram months (as in Figures IV.1, IV.4, and IV.6); we find a significant upturn in the estimated effects for males for both employment and earnings during that time period. Through month 48, the pattern of estimated effects for females without children (see Figures IV.2 and IV.5) is similar on average to males, but shows no downturn in the fourth postprogram year; the estimated effects for females with children, the smaller group, are much lower and more erratic than males (see Figure IV.3).

Tables IV.14 and IV.15 show that the pattern of overall effects is much flatter and more nearly constant over time when the insignificant but erratic effects are assumed to equal zero for the early drop-out group with few observations. Similarly, when six-month average effects are computed for months 48 to 54 (see Table IV.18), we find that, overall, they are

again positive and statistically significant (and do not show signs of fadeout compared to the previous months). We believe that the most prudent conclusion about the longevity of Job Corps effects is that the effects persist at a relatively stable rate from approximately three months after termination until the end of the four-year observation period; beyond that point, our ability to extrapolate is very limited.

E. SENSITIVITY OF ESTIMATES TO ALTERNATIVE ECONOMETRIC SPECIFICATIONS

In checking the sensitivity of our estimates to alternative econometric specifications, we obtain a number of interesting findings (see Tables IV.1, IV.16, IV.17, and IV.18). First, allowing the time trend for the economy to be nonlinear makes the overall employment effects more nearly equal throughout the postprogram period, changing the level only slightly; however, it does make the earnings effect substantially larger on average (see the earlier discussion). Without the nonlinear term the Job Corps variables tend to pick up the downturn in the economy toward the end of our observation period.

Second, adding controls for differences in pre-enrollment marital status makes the estimates consistently more favorable for Job Corps among all three major sex and child-responsibility groups (males, females without children, and females with children).[1/] Similarly, as reported earlier, adding controls for the presence of very young children completely reverses the negative findings for females with children (see Mallar et al., 1980, Chapter IV). Finally, not controlling for differences between

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[1/] Adding controls for the contemporaneous marital status makes the Job Corps effects even more positive, but has clear endogenity problems.

the Job Corps and comparison groups makes the findings much less favorable for males, much more favorable for females with children, and changes the findings only slightly for females without children (see Tables IV.16, IV.17, and IV.18), so that the overall benefits are reduced by a little more than one-half. However, we believe that the controls are necessary, because we have a comparison group and not a random control group. In our judgment, the most reliable estimates are those based on the estimating equations outlined in Chapter III and which we have emphasized in this chapter and will continue to emphasize throughout the remainder of the report.

TABLE IV.2

ESTIMATES OF CORPSMEMBERS' EMPLOYMENT AND EARNINGS HAD THEY NOT PARTICIPATED IN JOB CORPS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Means 0 to 6 Months After Termination	Job Corps Means 6 to 12 Months After Termination	Job Corps Means 12 to 18 Months After Termination	Job Corps Means 18 to 24 Months After Termination
A. MALES				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.418	0.445	0.474	0.489
o Weeks worked per six months	10.87	11.56	12.33	12.71
o Hours worked per week	15.70	17.19	18.71	19.69
o Earnings per week in current dollars	55.06	66.72	80.32	99.49
o Earnings per week in 1977 dollars	54.92	61.66	69.55	73.87
2. Probability in military during interview week				
B. FEMALES WITHOUT CHILDREN				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.298	0.309	0.331	0.358
o Weeks worked per six months	7.74	8.03	8.61	9.32
o Hours worked per week	11.36	12.30	13.36	14.68
o Earnings per week in current dollars	32.25	38.55	44.58	51.81
o Earnings per week in 1977 dollars	30.96	34.48	38.18	42.69
2. Probability in military during interview week				
C. FEMALES WITH CHILDREN				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.294	0.309	0.300	0.317
o Weeks worked per six months	7.65	8.04	7.79	8.23
o Hours worked per week	11.13	11.40	11.09	11.79
o Earnings per week in current dollars	30.39	34.35	36.78	41.81
o Earnings per week in 1977 dollars	29.11	30.82	31.23	33.76
2. Probability in military during interview week				
D. OVERALL				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.382	0.404	0.429	0.446
o Weeks worked per six months	9.93	10.50	11.15	11.60
o Hours worked per week	14.40	15.69	16.92	17.93
o Earnings per week in current dollars	48.18	58.10	68.97	77.28
o Earnings per week in 1977 dollars	47.69	53.35	59.59	63.72
2. Probability in military during interview week	0.033		0.051	

TABLE IV.3

ESTIMATES OF CORPSMEMBERS' EMPLOYMENT AND EARNINGS HAD THEY NOT PARTICIPATED IN JOB CORPS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Means 24 to 30 Months After Termination	Job Corps Means 30 to 36 Months After Termination	Job Corps Means 36 to 42 Months After Termination	Job Corps Means 42 to 48 Months After Termination
A. MALES				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.520	0.525	0.547	0.554
o Weeks worked per six months	13.52	13.66	14.22	14.39
o Hours worked per week	21.49	21.82	22.75	23.29
o Earnings per week in current dollars	103.28	107.45	117.13	124.21
o Earnings per week in 1977 dollars	81.35	83.45	88.32	91.18
2. Probability in military during interview week				0.036
B. FEMALES WITHOUT CHILDREN				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.369	0.378	0.441	0.440
o Weeks worked per six months	9.59	9.82	11.47	11.43
o Hours worked per week	15.30	15.97	18.37	18.65
o Earnings per week in current dollars	56.52	60.35	72.88	76.07
o Earnings per week in 1977 dollars	44.74	46.95	55.42	56.46
2. Probability in military during interview week				0.014
C. FEMALES WITH CHILDREN				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.346	0.353	0.388	0.399
o Weeks worked per six months	8.96	9.18	9.97	10.37
o Hours worked per week	12.89	13.17	14.39	15.00
o Earnings per week in current dollars	47.86	49.41	55.98	59.46
o Earnings per week in 1977 dollars	37.56	38.37	42.64	44.46
2. Probability in military during interview week				0.0
D. OVERALL				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.472	0.478	0.507	0.513
o Weeks worked per six months	12.28	12.43	13.17	13.34
o Hours worked per week	19.39	19.73	20.84	21.35
o Earnings per week in current dollars	88.39	92.01	101.32	107.28
o Earnings per week in 1977 dollars	69.65	73.47	78.54	78.97
2. Probability in military during interview week				0.028

TABLE IV.4

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT AND EARNINGS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Civilian employment and earnings				
o Employed (fraction of time)	-0.004	0.098****	0.124****	0.097***
o Weeks worked per six months	-0.10	2.55****	3.22****	2.52***
o Hours worked per week	1.86	4.71****	6.01****	4.66****
o Earnings per week in current dollars	5.46	16.97**	19.63**	10.83
o Earnings per week in 1977 dollars	1.58	13.92**	17.33***	9.72
2. Probability in military during interview week	0.024**		0.056***	
B. FEMALES WITHOUT CHILDREN				
1. Civilian employment and earnings				
o Employed (fraction of time)	-0.018	0.032	0.120****	0.061*
o Weeks worked per six months	-0.47	0.83	3.12****	1.74*
o Hours worked per week	-0.81	-0.12	3.66***	1.37
o Earnings per week in current dollars	0.43	2.78	12.77**	7.76
o Earnings per week in 1977 dollars	-0.26	2.68	11.83**	7.02
2. Probability in military during interview week	0.018		0.0003	
C. FEMALES WITH CHILDREN				
1. Civilian employment and earnings				
o Employed (fraction of time)	-0.134****	-0.147****	-0.018	-0.083**
o Weeks worked per six months	-3.48****	-3.82****	-0.47	-2.16**
o Hours worked per week	-5.39****	-5.43****	-0.81	-2.84**
o Earnings per week in current dollars	-14.21**	-15.79****	-6.71	-12.54**
o Earnings per week in 1977 dollars	-13.40****	-13.79****	-4.90	-9.79**
2. Probability in military during interview week	0.0		0.0	
D. OVERALL				
1. Civilian employment and earnings				
o Employed (fraction of time)	-0.011	0.071****	0.112****	0.074****
o Weeks worked per six months	-0.29	1.85****	2.91****	1.92****
o Hours worked per week	0.97	3.05****	4.95****	3.29****
o Earnings per week in current dollars	3.66	11.97*	16.01****	8.08
o Earnings per week in 1977 dollars	0.77	9.09*	14.34****	7.40
2. Probability in military during interview week	0.019****		0.039****	

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.5

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT AND EARNINGS:
THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.103****	0.085***	0.054*	0.018
o Weeks worked per six months	2.68****	2.21***	1.40*	0.47
o Hours worked per week	4.49****	4.05***	3.27**	1.23
o Earnings per week in current dollars	12.76	12.95	8.71	-0.60
o Earnings per week in 1977 dollars	11.04*	11.03*	6.58	-1.55
2. Probability in military during interview week				0.055****
B. FEMALES WITHOUT CHILDREN				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.035	0.051	0.159****	0.135****
o Weeks worked per six months	0.91	1.33	4.13****	3.51****
o Hours worked per week	-0.31	-0.17	2.69*	2.14
o Earnings per week in current dollars	0.88	2.53	11.29*	13.53*
o Earnings per week in 1977 dollars	1.31	2.24	7.82	8.47
2. Probability in military during interview week				0.006
C. FEMALES WITH CHILDREN				
1. Civilian employment and earnings				
o Employed (fraction of time)	-0.125****	-0.133****	-0.127****	-0.162****
o Weeks worked per six months	-3.25****	-3.46****	-3.30****	-4.21****
o Hours worked per week	-4.30***	-4.26***	-4.21***	-6.38****
o Earnings per week in current dollars	-16.62***.	-17.81***	-17.81***	-25.31****
o Earnings per week in 1977 dollars	-12.88***	-13.91***	-13.79***	-19.74****
2. Probability in military during interview week				0.0
D. OVERALL				
1. Civilian employment and earnings				
o Employed (fraction of time)	0.067***	0.053**	0.043*	0.009
o Weeks worked per six months	1.74***	1.38**	1.12*	0.23
o Hours worked per week	2.65***	2.30**	2.06*	0.23
o Earnings per week in current dollars	7.45	7.38	5.17	-2.19
o Earnings per week in 1977 dollars	6.71	6.46	3.71	-2.78
2. Probability in military during interview week				0.040****

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE 7.6

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT-RELATED ACTIVITIES, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Employed, in school, or in training (fraction of time)	-0.037	0.071**	0.085***	0.065**
2. Actively looking for work (fraction of time)	-0.085***	-0.109****	-0.110****	-0.074***
3. Employed or looking for work (fraction of time)	-0.069***	-0.030	-0.028	0.021
4. Keeping house (fraction of time)	-0.005	-0.0002	0.014	0.028
5. Employed in union job (fraction of time)	-0.063***	-0.048**	-0.045**	-0.030
6. Employed in PSE job (fraction of time)	-0.028*	-0.004	-0.002	0.0002
B. FEMALES WITHOUT CHILDREN				
1. Employed, in school, or in training (fraction of time)	-0.054	0.064*	0.136****	0.100***
2. Actively looking for work (fraction of time)	0.062**	0.010	-0.022	0.003
3. Employed or looking for work (fraction of time)	0.042	0.005	0.039	0.051
4. Keeping house (fraction of time)	-0.181****	-0.154****	-0.186****	-0.161****
5. Employed in union job (fraction of time)	0.021	0.030*	0.051***	0.053***
6. Employed in PSE job (fraction of time)	0.004	0.016	0.042***	0.021
C. FEMALES WITH CHILDREN				
1. Employed, in school, or in training (fraction of time)	-0.161****	-0.180****	-0.027	-0.073*
2. Actively looking for work (fraction of time)	0.083**	0.136****	0.091***	0.054*
3. Employed or looking for work (fraction of time)	-0.023	0.026	0.022	-0.041
4. Keeping house (fraction of time)	-0.107**	-0.073	-0.079*	-0.052
5. Employed in union job (fraction of time)	-0.014	-0.007	0.003	0.008
6. Employed in PSE job (fraction of time)	-0.015	0.021	0.047****	0.048****
D. OVERALL				
1. Employed, in school, or in training (fraction of time)	-0.044*	0.060***	0.087****	0.060***
2. Actively looking for work (fraction of time)	-0.040*	-0.068****	-0.075****	-0.046**
3. Employed or looking for work (fraction of time)	-0.054**	-0.014	-0.012	0.011
4. Keeping house (fraction of time)	-0.056****	-0.043****	-0.037**	-0.019
5. Employed in union job (fraction of time)	-0.039***	-0.026*	-0.020	-0.009
6. Employed in PSE job (fraction of time)	-0.019*	0.002	0.012	0.009

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.7

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT-RELATED ACTIVITIES, FOR CIVILIANS: FOURTH POSTPROGRAM YEAR

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Employed, in school, or in training (fraction of time)	0.070**	0.057*	0.040	-0.003
2. Actively looking for work (fraction of time)	-0.102****	-0.091****	-0.067**	-0.022
3. Employed or looking for work (fraction of time)	0.010	-0.006	-0.006	-0.001
4. Keeping house (fraction of time)	0.047***	0.047***	0.058***	0.077****
5. Employed in union job (fraction of time)	-0.033	-0.036**	-0.048**	-0.052**
6. Employed in PSE job (fraction of time)	0.006	-0.004	-0.011	-0.015
B. FEMALES WITHOUT CHILDREN				
1. Employed, in school, or in training (fraction of time)	0.073*	0.108***	0.175****	0.181****
2. Actively looking for work (fraction of time)	-0.036	-0.058*	-0.024	-0.007
3. Employed or looking for work (fraction of time)	-0.020	-0.027	0.109***	0.093**
4. Keeping house (fraction of time)	-0.132****	-0.153****	-0.182****	-0.173****
5. Employed in union job (fraction of time)	0.053***	0.054***	0.069****	0.041**
6. Employed in PSE job (fraction of time)	0.029*	0.026*	-0.002	-0.003
C. FEMALES WITH CHILDREN				
1. Employed, in school, or in training (fraction of time)	-0.105***	-0.073*	-0.121****	-0.140****
2. Actively looking for work (fraction of time)	0.062**	0.055*	0.078***	0.070**
3. Employed or looking for work (fraction of time)	-0.057	-0.070*	-0.058	-0.056**
4. Keeping house (fraction of time)	-0.0004	0.021	0.033	0.053
5. Employed in union job (fraction of time)	-0.001	-0.006	-0.009	-0.012
6. Employed in PSE job (fraction of time)	0.036***	0.020	0.007	-0.008
D. OVERALL				
1. Employed, in school, or in training (fraction of time)	0.053**	0.050**	0.036*	0.004
2. Actively looking for work (fraction of time)	-0.072****	-0.068****	-0.039*	-0.006
3. Employed or looking for work (fraction of time)	-0.006	-0.020*	0.003	-0.001
4. Keeping house (fraction of time)	0.007	0.008	0.018	0.036**
5. Employed in union job (fraction of time)	-0.013	-0.016	-0.025	-0.032*
6. Employed in PSE job (fraction of time)	0.014	0.004	-0.007	-0.012

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.8

ESTIMATES OF CORPSMEMBERS' EMPLOYMENT AND EARNINGS HAD THEY NOT PARTICIPATED IN JOB CORPS, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Means 0 to 6 Months After Termination	Job Corps Means 6 to 12 Months After Termination	Job Corps Means 12 to 18 Months After Termination	Job Corps Means 18 to 24 Months After Termination
	A. MALES			
1. Employed (fraction of time)	0.449	0.477	0.500	0.505
2. Weeks worked per six months	11.68	12.40	13.00	13.13
3. Hours worked per week	16.70	18.23	19.43	20.02
4. Earnings per week in current dollars	60.19	71.39	83.13	91.03
5. Earnings per week in 1977 dollars	59.45	65.81	71.92	74.84
	B. FEMALES WITHOUT CHILDREN			
1. Employed (fraction of time)	0.301	0.312	0.334	0.366
2. Weeks worked per six months	7.83	8.12	8.68	9.50
3. Hours worked per week	11.45	12.43	13.45	14.96
4. Earnings per week in current dollars	32.79	39.50	45.61	54.18
5. Earnings per week in 1977 dollars	31.63	35.37	39.00	44.43
	C. FEMALES WITH CHILDREN			
1. Employed (fraction of time)	0.294	0.3096	0.299	0.3167
2. Weeks worked per six months	7.65	8.05	7.78	8.23
3. Hours worked per week	11.13	11.40	11.08	11.79
4. Earnings per week in current dollars	30.39	34.35	36.78	41.80
5. Earnings per week in 1977 dollars	29.11	30.83	31.23	33.75
	D. OVERALL			
1. Employed (fraction of time)	0.4048	0.427	0.447	0.4588
2. Weeks worked per six months	10.52	11.11	11.63	11.93
3. Hours worked per week	15.12	16.45	17.44	18.22
4. Earnings per week in current dollars	51.93	61.62	71.17	78.86
5. Earnings per week in 1977 dollars	51.05	56.49	61.42	64.76

TABLE IV.9

ESTIMATES OF PARTICIPANTS' EMPLOYMENT AND EARNINGS HAD THEY NOT PARTICIPATED IN JOB CORPS, EXCLUDING MILITARY SECTOR: THIRD AND FOURTH POST-PROGRAM YEARS

Variable	Job Corps Means 24 to 30 Months After Termination	Job Corps Means 30 to 36 Months After Termination	Job Corps Means 36 to 42 Months After Termination	Job Corps Means 42 to 48 Months After Termination
A. MALES				
1. Employed (fraction of time)	0.529	0.530	0.547	0.550
2. Weeks worked per six months	13.74	13.77	14.21	14.29
3. Hours worked per week	21.44	21.64	22.41	22.81
4. Earnings per week in current dollars	$92.91	$96.44	115.23	121.45
5. Earnings per week in '77 dollars	80.61	81.88	85.33	87.83
B. FEMALES WITHOUT CHILDREN				
1. Employed (fraction of time)	0.382	0.392	0.461	0.461
2. Weeks worked per six months	9.92	10.18	11.98	11.99
3. Hours worked per week	15.82	16.59	19.23	19.58
4. Earnings per week in current dollars	60.36	64.89	79.12	82.81
5. Earnings per week in 1977 dollars	47.54	50.18	59.84	61.18
C. FEMALES WITH CHILDREN				
1. Employed (fraction of time)	0.3448	0.3530	0.3831	0.3987
2. Weeks worked per six months	8.9648	9.1780	9.9606	10.3662
3. Hours worked per week	12.90	13.17	14.40	15.00
4. Earnings per week in current dollars	47.85	49.41	55.98	59.46
5. Earnings per week in 1977 dollars	37.57	38.37	42.64	44.45
D. OVERALL				
1. Employed (fraction of time)	0.481	0.488	0.509	0.514
2. Weeks worked per six months	12.50	12.58	13.24	13.36
3. Hours worked per week	19.46	19.72	20.73	21.15
4. Earnings per week in current dollars	88.89	92.12	100.93	106.36
5. Earnings per week in 1977 dollars	69.69	70.95	75.45	77.33

TABLE IV.10

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT AND EARNINGS, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Employed (fraction of time)	-0.015	0.095***	0.124****	0.104****
2. Weeks worked per six months	-0.39	2.47***	3.22****	2.70****
3. Hours worked per week	1.56	4.85****	6.29****	5.23****
4. Earnings per week in current dollars	3.45	17.07**	21.23***	13.41*
5. Earnings per week in 1977 dollars	0.13	14.37**	19.08***	12.41*
B. FEMALES WITHOUT CHILDREN				
1. Employed (fraction of time)	-0.017	0.035	0.124****	0.074**
2. Weeks worked per six months	-0.44	0.91	3.22****	1.92**
3. Hours worked per week	-0.70	0.84	3.87***	1.69
4. Earnings per week in current dollars	0.71	3.11	13.06**	8.18
5. Earnings per week in 1977 dollars	-0.14	2.94	12.18**	7.66
C. FEMALES WITH CHILDREN				
1. Employed (fraction of time)	-0.134****	-0.147****	-0.018	-0.083**
2. Weeks worked per six months	-3.48****	-3.82****	-0.47	-2.16**
3. Hours worked per week	-5.39****	-5.43****	-0.81	-2.84**
4. Earnings per week in current dollars	-14.21**	-15.79***	-6.71	-12.54**
5. Earnings per week in 1977 dollars	-13.40	-13.79***	-4.90	-9.79**
D. OVERALL				
1. Employed (fraction of time)	-0.018	0.070****	0.113****	0.081****
2. Weeks worked per six months	-0.47	1.82****	2.94****	2.11****
3. Hours worked per week	0.79	3.19****	5.19****	3.76****
4. Earnings per week in current dollars	2.33	12.13**	17.20***	9.97*
5. Earnings per week in 1977 dollars	-0.21	10.27**	15.64****	9.42*

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.11

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT AND EARNINGS, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Employed (fraction of time)	0.120****	0.113****	0.088***	0.061*
2. Weeks worked per six months	3.12****	2.94****	2.29***	1.59*
3. Hours worked per week	5.48****	5.41****	4.78****	3.10**
4. Earnings per week in current dollars	17.53**	20.48***	18.62**	12.73
5. Earnings per week in 1977 dollars	15.66**	17.82***	15.13**	9.46
B. FEMALES WITHOUT CHILDREN				
1. Employed (fraction of time)	0.046	0.065*	0.169****	0.147****
2. Weeks worked per six months	1.20	1.69*	4.39****	3.82****
3. Hours worked per week	0.16	0.39	3.17*	2.67*
4. Earnings per week in current dollars	2.02	4.81	14.67**	18.33***
5. Earnings per week in 1977 dollars	2.63	4.37	10.65*	12.04**
C. FEMALES WITH CHILDREN				
1. Employed (fraction of time)	-0.125****	-0.133****	-0.127****	-0.162****
2. Weeks worked per six months	-3.25****	-3.46****	-3.30****	-4.21****
3. Hours worked per week	-4.30***	-4.26***	-4.21***	-6.34****
4. Earnings per week in current dollars	-16.62***	-17.81***	-17.41***	-25.31****
5. Earnings per week in 1977 dollars	-12.84***	-13.91***	-13.79***	-19.71****
D. OVERALL				
1. Employed (fraction of time)	0.081****	0.075****	0.068***	0.040*
2. Weeks worked per six months	2.11****	1.95****	1.77***	1.04*
3. Hours worked per week	3.44****	3.35****	3.20***	1.62
4. Earnings per week in current dollars	11.02*	13.07**	12.63**	7.87
5. Earnings per week in 1977 dollars	10.20**	11.59***	10.12**	5.47

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.12

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT-RELATED ACTIVITIES, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Employed, in school, or in training (fraction of time)	-0.043	0.072***	0.090***	0.073***
2. Actively looking for work (fraction of time)	-0.078***	-0.111***	-0.116***	-0.086***
3. Employed or looking for work (fraction of time)	-0.070***	-0.028	-0.024	0.024
4. Keeping house (fraction of time)	-0.011	-0.009	0.003	0.014
5. Employed in union job (fraction of time)	-0.061***	-0.047**	-0.045**	-0.031
6. Employed in PSE job (fraction of time)	-0.026*	-0.004	-0.002	-0.0009
B. FEMALES WITHOUT CHILDREN				
1. Employed, in school, or in training (fraction of time)	-0.052	0.065*	0.138***	0.105***
2. Actively looking for work (fraction of time)	0.059**	0.006	-0.028	-0.004
3. Employed or looking for work (fraction of time)	0.040	0.003	0.038	0.051
4. Keeping house (fraction of time)	-0.182***	-0.155***	-0.185***	-0.162***
5. Employed in union job (fraction of time)	0.022	0.030*	0.050***	0.052***
6. Employed in PSE job (fraction of time)	0.005	0.016	0.042***	0.020
C. FEMALES WITH CHILDREN				
1. Employed, in school, or in training (fraction of time)	-0.161***	-0.180***	-0.027	-0.073*
2. Actively looking for work (fraction of time)	0.083**	0.136***	0.091***	0.054*
3. Employed or looking for work (fraction of time)	-0.023	0.026	0.022	-0.041
4. Keeping house (fraction of time)	-0.107**	-0.073	-0.079*	-0.052
5. Employed in union job (fraction of time)	-0.014	-0.007	0.003	0.008
6. Employed in PSE job (fraction of time)	-0.015	0.021	0.047***	0.048***
D. OVERALL				
1. Employed, in school, or in training (fraction of time)	-0.048**	0.060***	0.091***	0.067***
2. Actively looking for work (fraction of time)	-0.036*	-0.071***	-0.060	-0.055***
3. Employed or looking for work (fraction of time)	-0.037*	-0.018	-0.007	0.024
4. Keeping house (fraction of time)	-0.061***	-0.057***	-0.045***	-0.029*
5. Employed in union job (fraction of time)	-0.037**	-0.026*	-0.020	-0.010
6. Employed in PSE job (fraction of time)	-0.017*	0.002	0.011	0.008

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.13

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT-RELATED ACTIVITIES, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Employed, in school, or in training (fraction of time)	0.089***	0.085***	0.078***	0.040
2. Actively looking for work (fraction of time)	-0.118*****	-0.114*****	-0.095*****	-0.057**
3. Employed or looking for work (fraction of time)	0.020	0.011	0.013	0.020
4. Keeping house (fraction of time)	0.029*	0.028*	0.037**	0.054***
5. Employed in union job (fraction of time)	-0.035*	-0.039*	-0.050**	-0.054***
6. Employed in PSE job (fraction of time)	0.003	-0.007	-0.014	-0.017
B. FEMALES WITHOUT CHILDREN				
1. Employed, in school, or in training (fraction of time)	0.082**	0.117***	0.182*****	0.188*****
2. Actively looking for work (fraction of time)	-0.043	-0.065**	-0.033	-0.017
3. Employed or looking for work (fraction of time)	-0.015	-0.018	0.112***	0.097**
4. Keeping house (fraction of time)	-0.134*****	-0.155*****	-0.184*	-0.174*****
5. Employed in union job (fraction of time)	0.051***	0.052***	0.065*****	0.038*
6. Employed in PSE job (fraction of time)	0.027*	0.024	-0.003	-0.003*****
C. FEMALES WITH CHILDREN				
1. Employed, in school, or in training (fraction of time)	-0.105***	-0.073*	-0.121*****	-0.140*****
2. Actively looking for work (fraction of time)	0.062**	0.055*	0.078***	0.070**
3. Employed or looking for work (fraction of time)	-0.057	-0.070*	-0.058	-0.096**
4. Keeping house (fraction of time)	-0.0004	0.021	0.033	0.053
5. Employed in union job (fraction of time)	-0.001	-0.006	-0.009	-0.012
6. Employed in PSE job (fraction of time)	0.036***	0.020	0.007	-0.008
D. OVERALL				
1. Employed, in school, or in training (fraction of time)	0.068***	0.072*****	0.061***	0.035
2. Actively looking for work (fraction of time)	-0.085*****	-0.085*****	-0.060*****	-0.032
3. Employed or looking for work (fraction of time)	0.006	-0.004	0.012	0.019
4. Keeping house (fraction of time)	-0.006	-0.006	0.003	0.019
5. Employed in union job (fraction of time)	-0.014	-0.019	-0.027*	-0.034**
6. Employed in PSE job (fraction of time)	0.011	0.002	-0.009	-0.014

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.14

ESTIMATES OF DIFFERENTIAL IMPACT ON FRACTION OF TIME EMPLOYED, BY COMPLETION CATEGORIES and INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Overall benchmark	-0.015	0.095***	0.124****	0.104****
2. Program completers	0.067**	0.188****	0.201****	0.184****
3. Partial completers	-0.031	0.083***	0.103****	0.064**
4. Early dropouts	-0.063*	0.035	0.083**	0.078**.
5. Overall, assuming zero for early dropouts	0.011	0.081****	0.091****	0.074****
6. Overall, excluding early dropouts	0.018	0.135****	0.152****	0.124****
B. FEMALES WITHOUT CHILDREN				
1. Overall benchmark	-0.017	0.035	0.124***	0.074**
2. Program completers	0.060*	0.169****	0.210****	0.189****
3. Partial completers	-0.030	0.046	0.081**	0.071*
4. Early dropouts	-0.064	-0.078	0.093*	-0.009
5. Overall, assuming zero for early dropouts	0.009	0.065****	0.087****	0.078****
6. Overall, excluding early dropouts	0.015	0.108****	0.145****	0.130****
C. FEMALES WITH CHILDREN				
1. Overall benchmark	-0.134***	-0.147****	-0.018	-0.083*
2. Program completers	-0.105***	0.0004	0.012	-0.022
3. Partial completers	-0.105**	-0.153****	-0.060*	-0.117****
4. Early dropouts	-0.177**	-0.254***	-0.008	-0.103*
5. Overall, assuming zero for early dropouts	-0.063***	-0.046**	-0.014	-0.042**
6. Overall, excluding early dropouts	-0.105***	-0.076***	-0.024	-0.070***
D. OVERALL				
1. Overall benchmark	-0.018	0.070***	0.113****	0.081****
2. Program completers	0.061***	0.176****	0.187****	0.166****
3. Partial completers	-0.032	0.064***	0.085****	0.049**
4. Early dropouts	-0.066**	-0.005	0.078***	0.041
5. Overall, assuming zero for early dropouts	0.009	0.072****	0.082****	0.065****
6. Overall, excluding early dropouts	0.015	0.120****	0.136****	0.108****

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.15

ESTIMATES OF DIFFERENTIAL IMPACT ON FRACTION OF TIME EMPLOYED, BY COMPLETION CATEGORIES and INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Overall benchmark	0.120****	0.113****	0.088***	0.061*
2. Program completers	0.180****	0.183****	0.157****	0.160****
3. Partial completers	0.076***	0.065**	0.063*	0.030
4. Early dropouts	0.107***	0.097***	0.055	0.009
5. Overall, assuming zero for early dropouts	0.077****	0.074****	0.066****	0.057***
6. Overall, excluding early dropouts	0.126****	0.124****	0.110****	0.095***
B. FEMALES WITHOUT CHILDREN				
1. Overall benchmark	0.046	0.065*	0.169****	0.147****
2. Program completers	0.158****	0.189****	0.223****	0.236****
3. Partial completers	0.042	0.080**	0.057	0.100**
4. Early dropouts	-0.035	-0.039	0.213****	0.114*
5. Overall, assuming zero for early dropouts	0.060***	0.081****	0.084****	0.101****
6. Overall, excluding early dropouts	0.100****	0.135****	0.1400****	0.168****
C. FEMALES WITH CHILDREN				
1. Overall benchmark	-0.125****	-0.133***	-0.127****	-0.162****
2. Program completers	-0.032	-0.016	-0.015	-0.056*
3. Partial completers	-0.108***	-0.059*	-0.094***	-0.133****
4. Early dropouts	-0.209****	-0.276****	-0.235****	-0.264****
5. Overall, assuming zero for early dropouts	-0.042**	-0.022	-0.033*	-0.057**
6. Overall, excluding early dropouts	-0.070***	-0.037*	-0.054**	-0.094***
D. OVERALL				
1. Overall benchmark	0.081****	0.075****	0.068***	0.040*
2. Program completers	0.154****	0.160****	0.141****	0.139****
3. Partial completers	0.051**	0.053**	0.039*	0.016
4. Early dropouts	0.047*	0.027	0.035	-0.016
5. Overall, assuming zero for early dropouts	0.062****	0.054****	0.054****	0.047****
6. Overall, excluding early dropouts	0.103****	0.107****	0.090****	0.078****

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.16

SENSITIVITY OF EMPLOYMENT ESTIMATES TO ALTERNATIVE SPECIFICATIONS, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Benchmark	-0.015	0.095***	0.124****	0.104****
2. Adding a nonlinear time trend	-0.002	0.091***	0.107****	0.087***
3. Adding pre-enrollment marital status	-0.010	0.100****	0.129****	0.109****
4. Excluding control for unobserved difference	-0.087****	0.022*	0.052****	0.031***
5. Excluding all controls, except for time trend and pre-enrollment employment	-0.092****	0.003	0.031***	0.004
6. Excluding all controls, except for time trend	-0.091****	0.003	0.032***	0.005
B. FEMALES WITHOUT CHILDREN				
1. Benchmark	-0.017	0.035	0.124***	0.074**
2. Adding a nonlinear time trend	-0.011	0.034	0.120****	0.069*
3. Adding pre-enrollment marital status	-0.010	0.042	0.132****	0.081**
4. Excluding control for unobserved difference	-0.021	0.031	0.120****	0.070***
5. Excluding all controls, except for time trend and pre-enrollment employment	-0.018	0.027	0.118****	0.065***
6. Excluding all controls, except for time trend	-0.018	0.029	0.120****	0.067***
C. FEMALES WITH CHILDREN				
1. Benchmark	-0.134***	-0.147****	-0.018	-0.083*
2. Adding a nonlinear time trend	-0.138****	-0.147****	-0.015	-0.080**
3. Adding pre-enrollment marital status	-0.102**	-0.114***	0.015	-0.050
4. Excluding control for unobserved difference	-0.063*	-0.077**	0.053*	-0.012
5. Excluding all controls, except for time trend and pre-enrollment employment	-0.059*	-0.075**	0.058**	-0.011
6. Excluding all controls, except for time trend	-0.057*	-0.073**	0.059**	-0.009
D. OVERALL				
1. Benchmark	-0.018	0.070****	0.113****	0.081****
2. Adding a nonlinear time trend	-0.008	0.067***	0.100****	0.066***
3. Adding pre-enrollment marital status	-0.011	0.077****	0.121****	0.089****
4. Excluding control for unobserved difference	-0.068****	0.021*	0.067****	0.036****
5. Excluding all controls, except for time trend and pre-enrollment employment	-0.071****	0.006	0.052****	0.016
6. Excluding all controls, except for time trend	-0.070****	0.007	0.054****	0.017

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE 7.17

SENSITIVITY OF EMPLOYMENT ESTIMATES TO ALTERNATIVE SPECIFICATIONS, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Benchmark	0.120****	0.113****	0.088***	0.061*
2. Adding a nonlinear time trend	0.109****	0.105****	0.093***	0.090***
3. Adding pre-enrollment marital status	0.125****	0.118****	0.093***	0.066**
4. Excluding control for unobserved difference	0.047****	0.041***	0.016	-0.012
5. Excluding all controls, except for time trend and pre-enrollment employment	0.032**	0.019	0.015	0.003
6. Excluding all controls, except for time trend	0.032**	0.019	0.015	0.003
B. FEMALES WITHOUT CHILDREN				
1. Benchmark	0.046	0.065*	0.169****	0.147****
2. Adding a nonlinear time trend	0.043	0.064a	0.175****	0.161****
3. Adding pre-enrollment marital status	0.053	0.073*	0.177****	0.158****
4. Excluding control for unobserved difference	0.042*	0.061***	0.165****	0.142****
5. Excluding all controls, except for time trend and pre-enrollment employment	0.047a	0.063***	0.187****	0.164****
6. Excluding all controls, except for time trend	0.049**	0.066***	0.189****	0.166****
C. FEMALES WITH CHILDREN				
1. Benchmark	-0.125****	-0.133***	-0.127****	-0.162****
2. Adding a nonlinear time trend	-0.123****	-0.132****	-0.127****	-0.165****
3. Adding pre-enrollment marital status	-0.093***	-0.100***	-0.093***	-0.129****
4. Excluding control for unobserved difference	-0.056**	-0.062**	-0.056**	-0.091****
5. Excluding all controls, except for time trend and pre-enrollment employment	-0.053*	-0.061**	-0.053**	-0.093****
6. Excluding all controls, except for time trend	-0.051*	-0.060**	-0.052**	-0.093****
D. OVERALL				
1. Benchmark	0.081****	0.075****	0.068***	0.040*
2. Adding a nonlinear time trend	0.073****	0.069***	0.073***	0.062***
3. Adding pre-enrollment marital status	0.089****	0.084****	0.078****	0.050**
4. Excluding control for unobserved difference	0.036****	0.032***	0.027**	-0.001
5. Excluding all controls, except for time trend and pre-enrollment employment	0.027***	0.017	0.030***	0.012
6. Excluding all controls, except for time trend	0.027***	0.018	0.031***	0.013

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE IV.18

SENSITIVITY OF EMPLOYMENT ESTIMATES TO ALTERNATIVE SPECIFICATIONS, INCLUDING MILITARY SECTOR: FIFTH POSTPROGRAM YEAR

Variable	Job Corps Effects 48 to 54 Months After Termination
A. MALES	
1. Benchmark	0.101***
2. Adding a nonlinear time trend	0.144*
3. Adding pre-enrollment marital status	0.106****
4. Excluding control for unobserved difference	0.028*
5. Excluding all controls, except for time trend and pre-enrollment employment	0.071****
6. Excluding all controls, except for time trend	0.071****
B. FEMALES WITHOUT CHILDREN	
1. Benchmark	0.083*
2. Adding a nonlinear time trend	0.103**
3. Adding pre-enrollment marital status	0.090*
4. Excluding control for unobserved difference	0.079**
5. Excluding all controls, except for time trend and pre-enrollment employment	0.120****
6. Excluding all controls, except for time trend	0.122****
C. FEMALES WITH CHILDREN	
1. Benchmark	-0.194****
2. Adding a nonlinear time trend	-0.199****
3. Adding pre-enrollment marital status	-0.161****
4. Excluding control for unobserved difference	-0.123****
5. Excluding all controls, except for time trend and pre-enrollment employment	-0.122****
6. Excluding all controls, except for time trend	-0.120****
D. OVERALL	
1. Benchmark	0.054**
2. Adding a nonlinear time trend	0.086****
3. Adding pre-enrollment marital status	0.063***
4. Excluding control for unobserved difference	0.013
5. Excluding all controls, except for time trend and pre-enrollment employment	0.050****
6. Excluding all controls, except for time trend	0.050****

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

FIGURE IV.1

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED:
CIVILIAN MALES

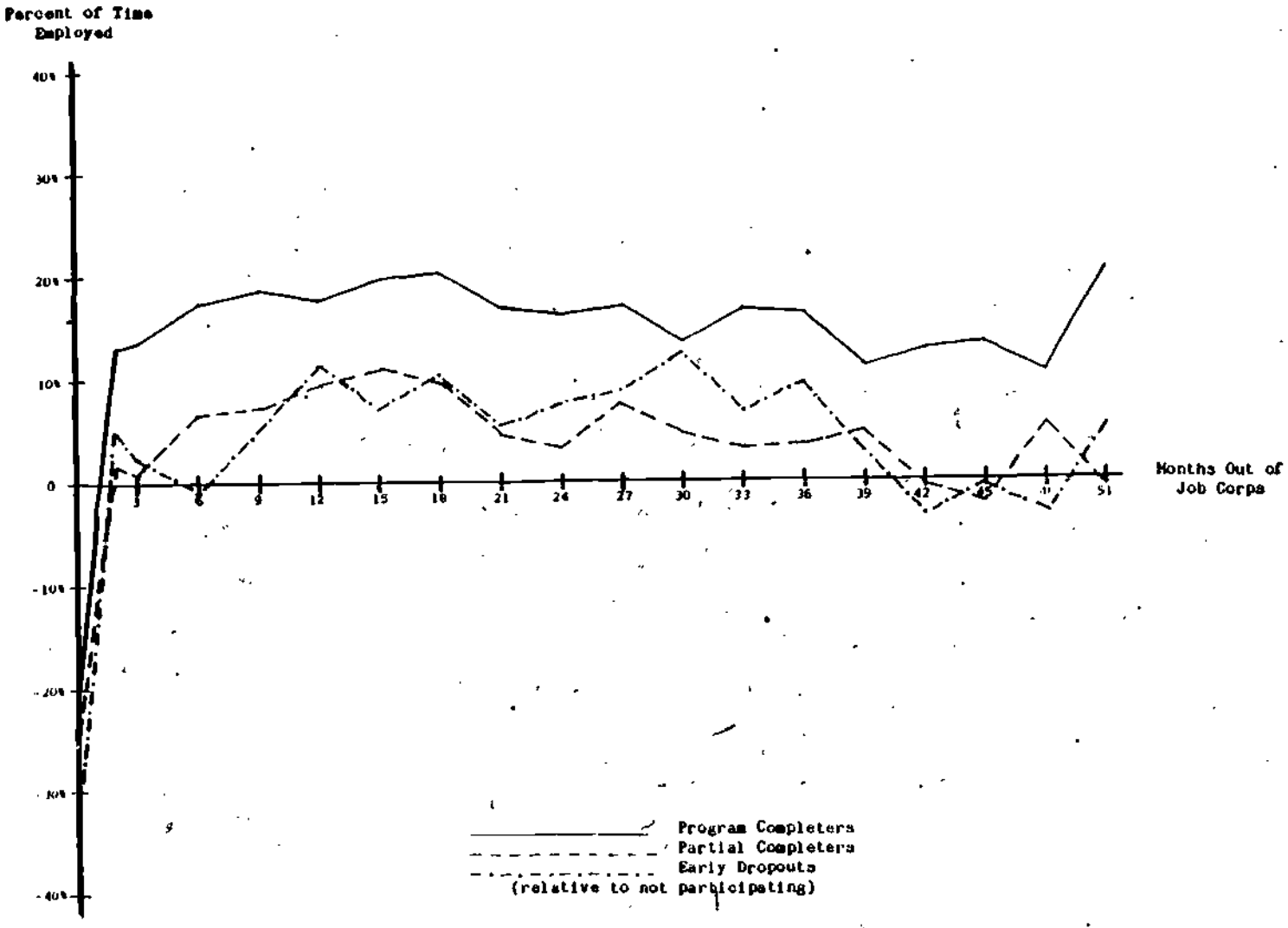


FIGURE IV.2

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED:
CIVILIAN FEMALES WITHOUT CHILDREN

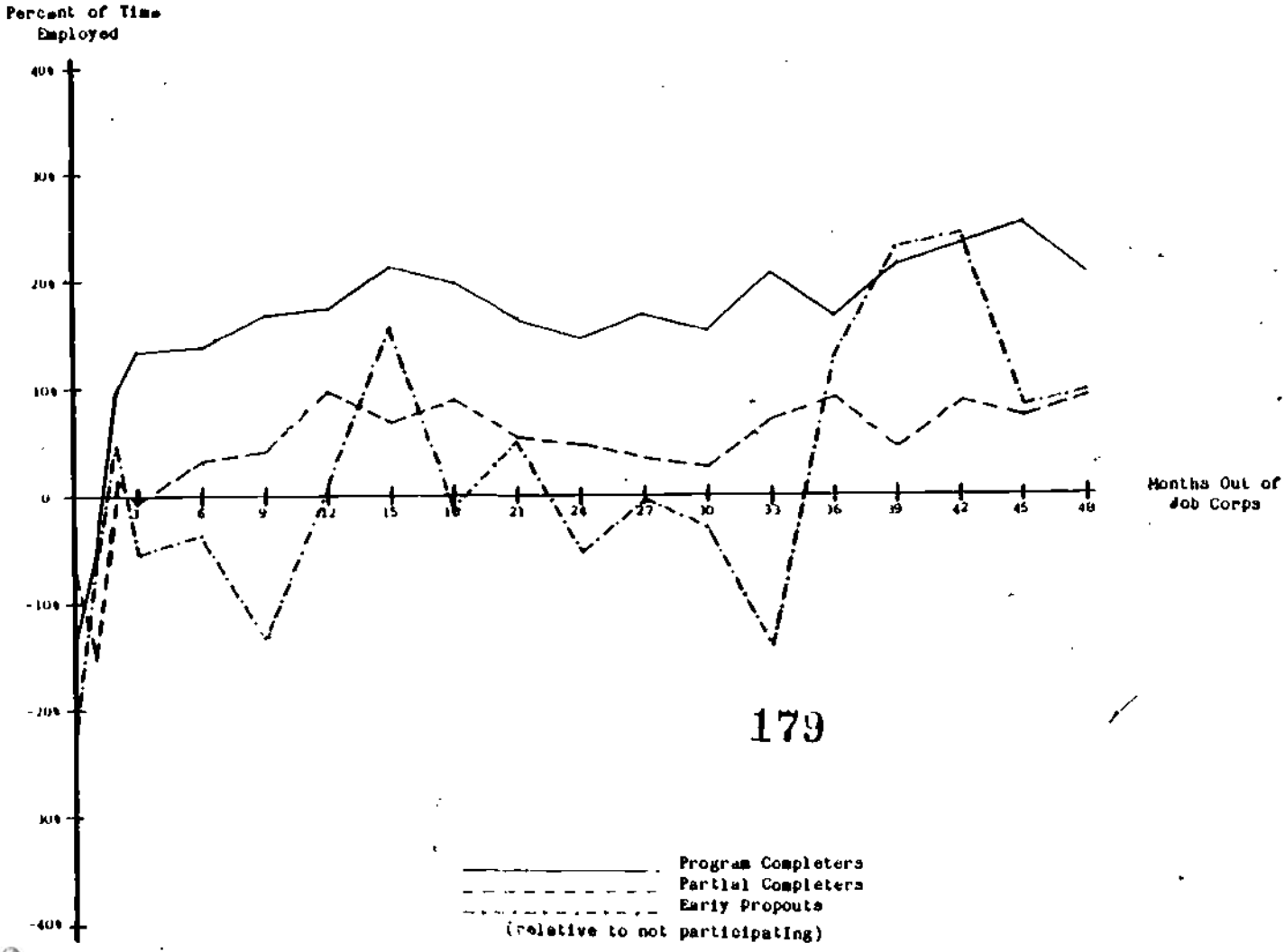


FIGURE IV.3

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED:
FEMALES WITH CHILDREN

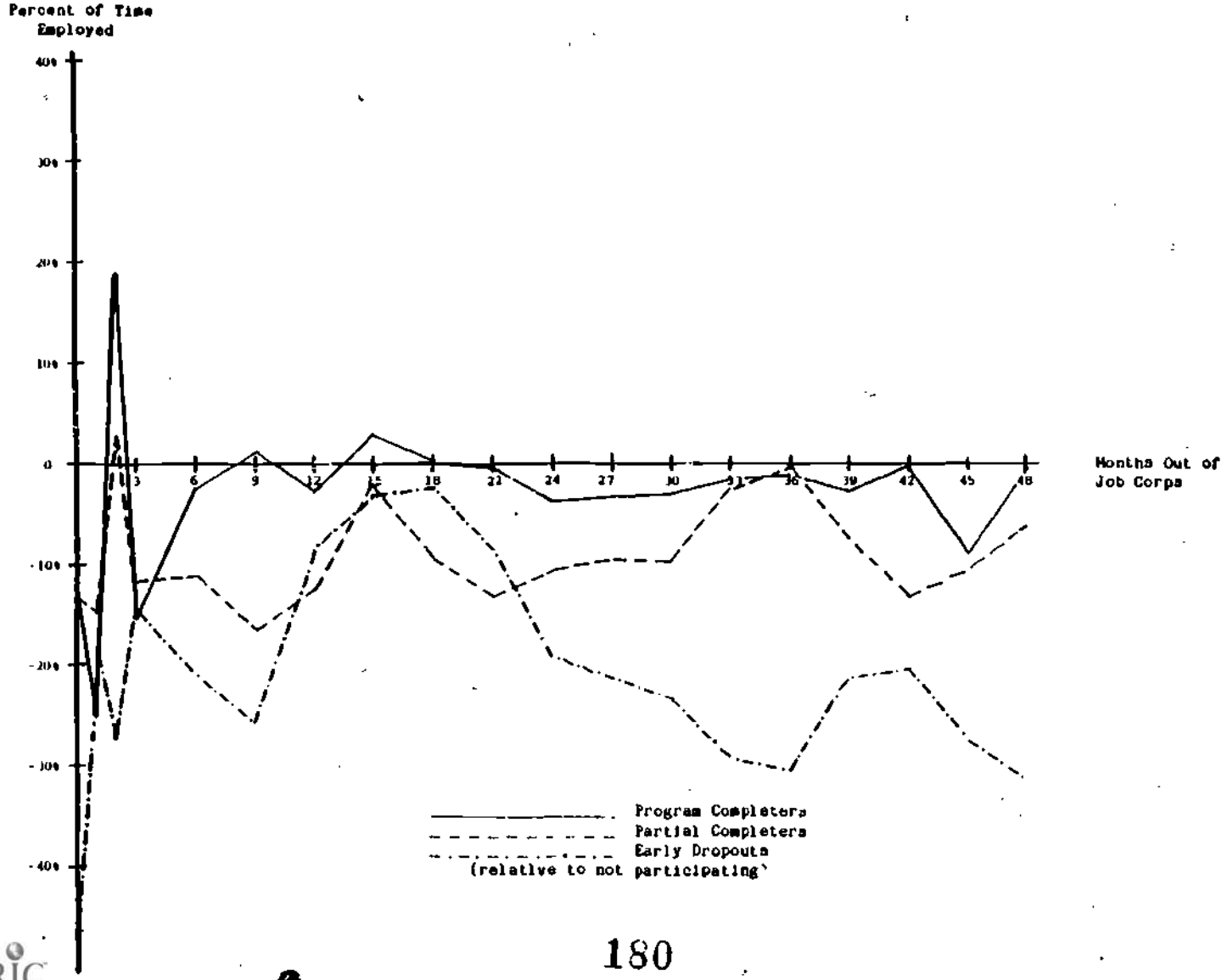


FIGURE IV.4

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED, INCLUDING MILITARY SECTOR: MALES

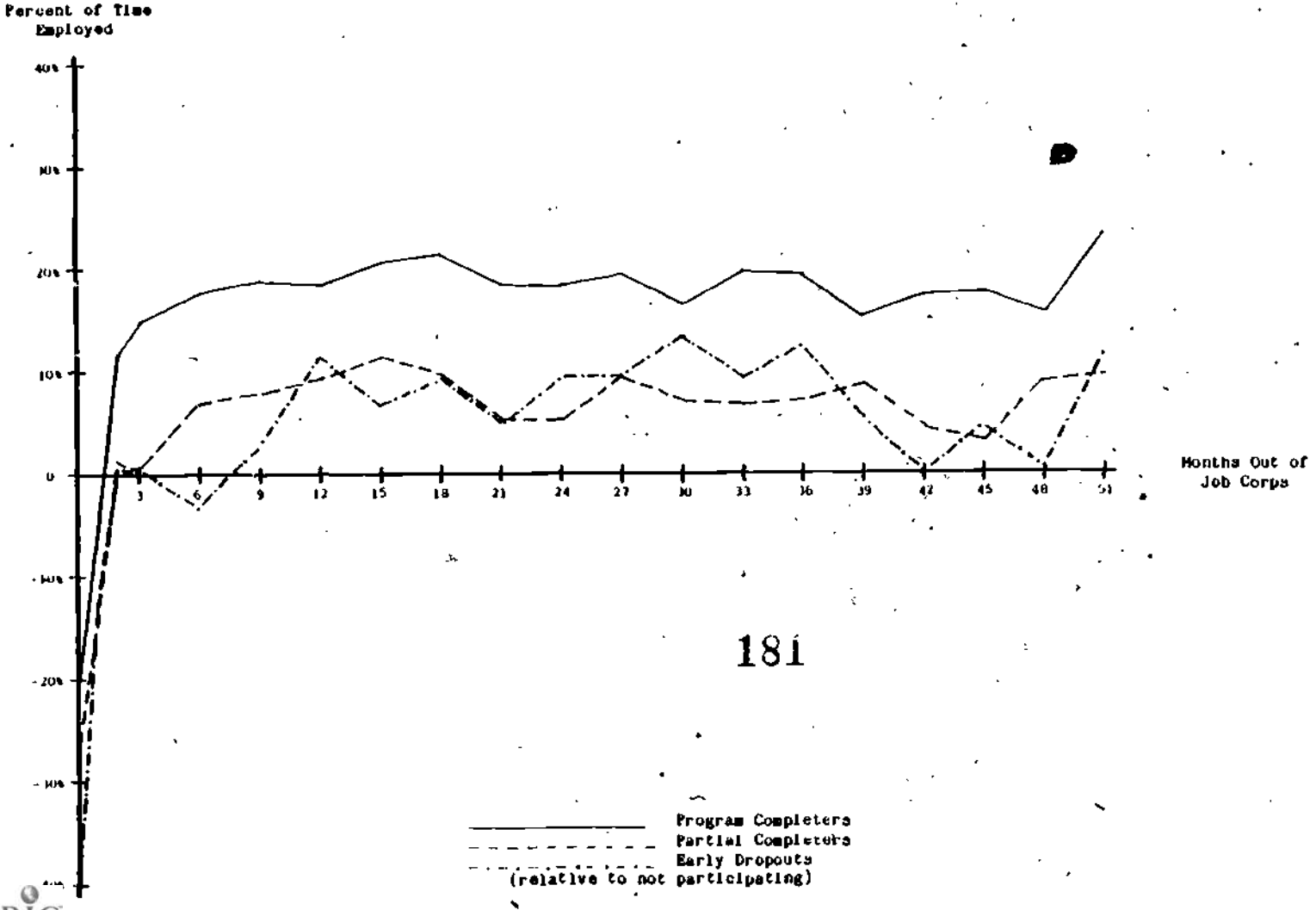


FIGURE IV.5

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED, INCLUDING MILITARY SECTOR: FEMALES WITHOUT CHILDREN

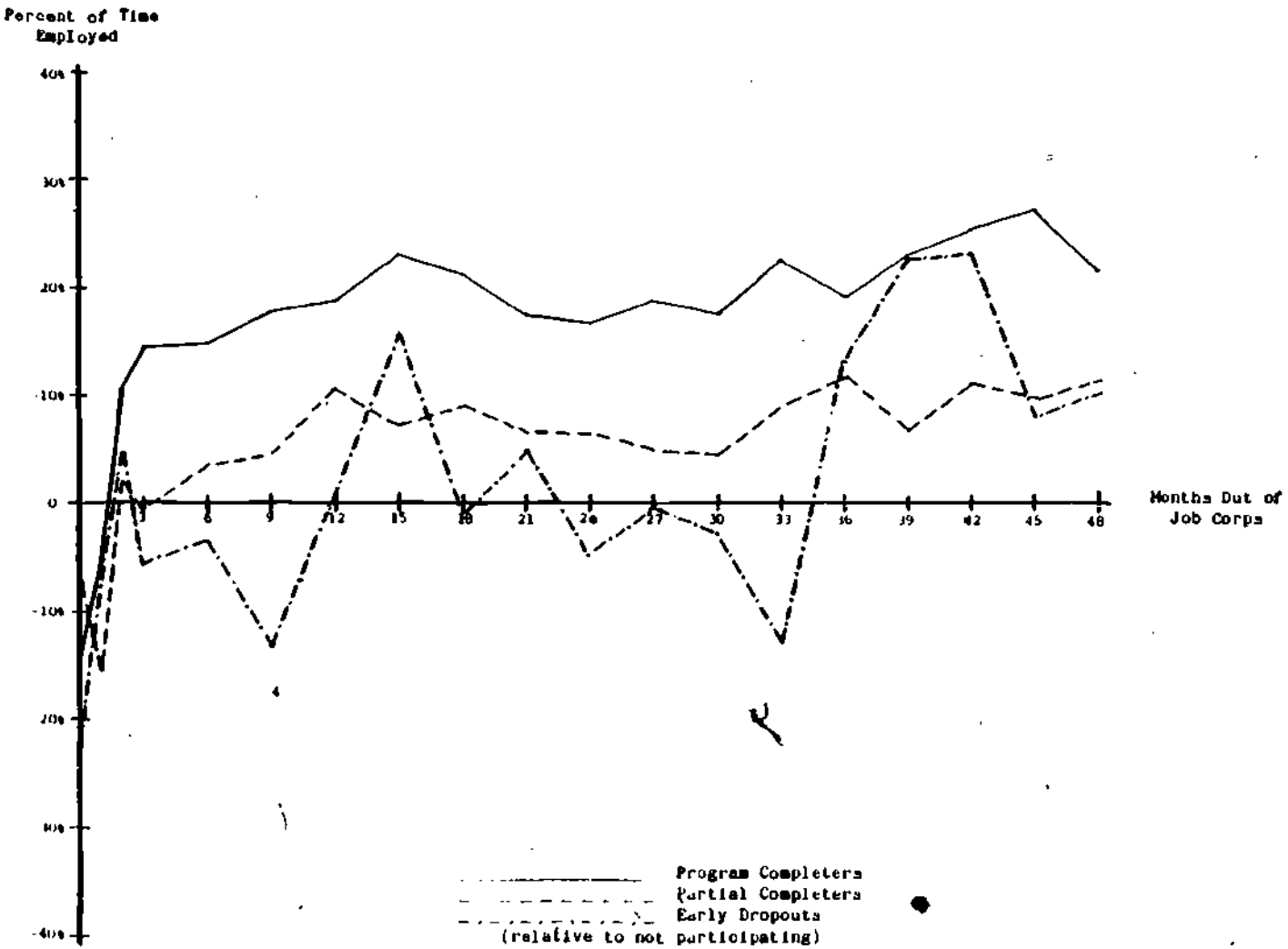
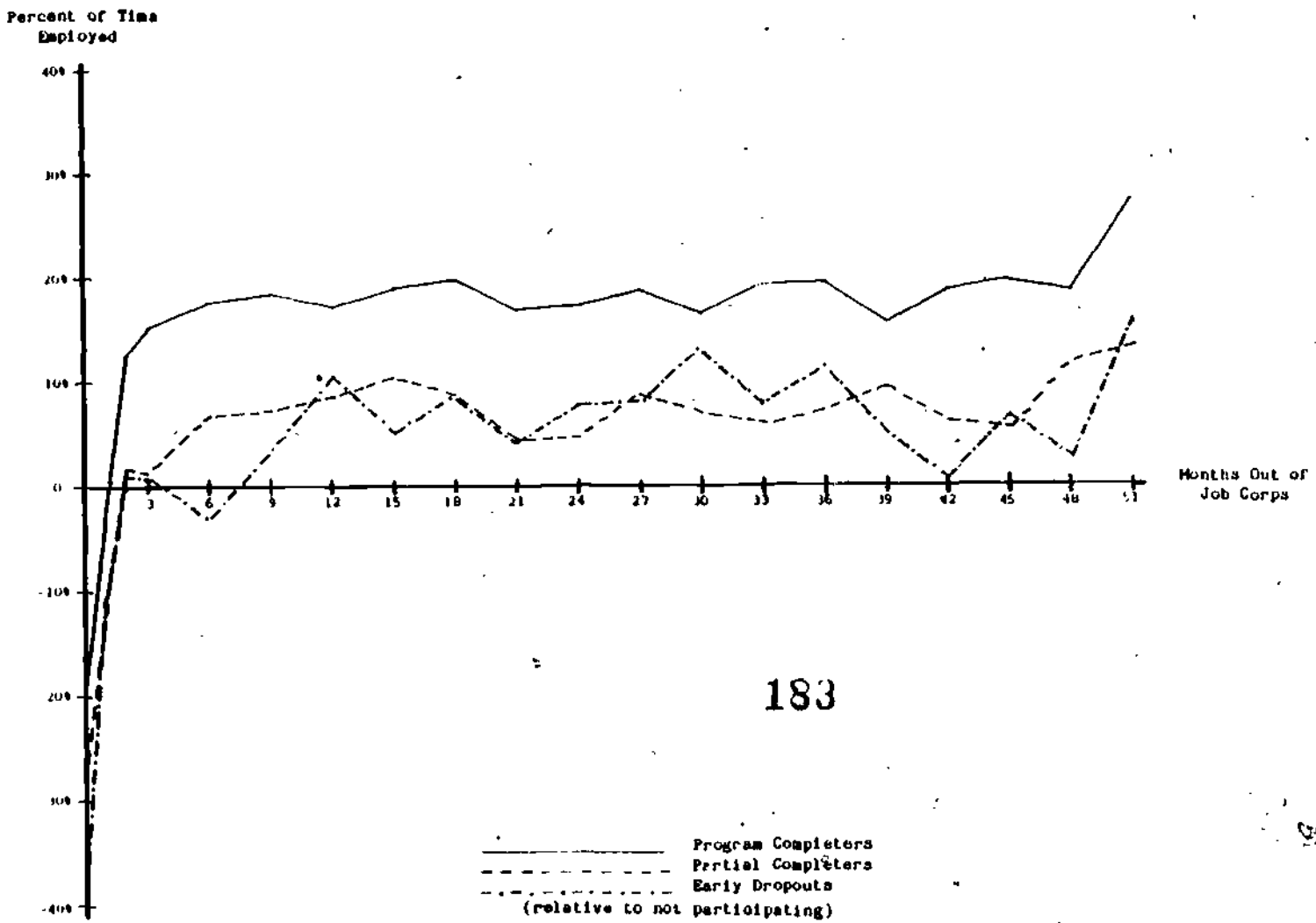


FIGURE IV.6

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED, INCLUDING MILITARY SECTOR AND CONTROLLING FOR NONLINEAR TIME TRENDS: MALES



V. IMPACT OF JCB CORPS ON INVESTMENTS IN HUMAN CAPITAL

The objective of Job Corps is to increase both the future and short-term employability of participants--in essence, to increase future employment and earnings by increasing productivity. Current activities that lead to future increases in productivity, employment, and earnings are defined in the economics literature as "investments in human capital." Work experience is one type of activity that normally leads to increased productivity and, hence, to increased employment and earnings in the future. Therefore, the short-term increases in employment and earnings for former Corpsmembers discussed in the previous chapter also indicate positive effects on future employment and earnings--and, thus, positive effects on investments in human capital.[1/] In this section we examine the impacts of Job Corps on the postprogram education, training, and other activities of former Corpsmembers--activities that could also potentially lead to future gains in employment and earnings among Corpsmembers.

A. EDUCATION AND TRAINING

As discussed in Chapter II, the a priori basis for expecting increased postprogram investments in human capital for Corpsmembers in terms of education and training is more ambiguous than for employment and for other forms of investments in human capital, for at least two reasons. First, Job Corps provides education and training, which reduces both the need for and the returns to some forms of postprogram education and

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1/ As discussed further below, military service (which we found to be increased substantially by Job Corps) is also a traditional way for low-income youths to obtain work experience and skills training that will lead to future gains in employment and earnings.

training (i.e., the Job Corps program is a substitute for some forms of education and training). Second, increased employability in the short-term provides incentives to engage in work activities that are alternatives to further education and training. Even for education or training that supplements or complements the Job Corps program, the increased postprogram employment and earnings of Corpsmembers raise the opportunity costs to attending school or training.

The postprogram findings for the effects of Job Corps on education and training are summarized in Table V.1 and are detailed with more refined estimates in Tables V.2 through V.7 and Tables V.10 through V.15.[1/] Overall, they show (1) a very large and statistically significant increase in the probability of having a high school diploma or equivalent degree, (2) a moderate-size and marginally significant increase in college attendance, (3) a moderate-size and statistically significant decrease in high school attendance, and (4) a very small and marginally significant increase in enrollments in vocational and technical schools. As shown in the detailed tables (Tables V.6, V.7, V.14, and V.15), the postprogram training effects are almost nil--generally small, erratic, and statistically insignificant. The only consistently significant estimated Job Corps effect on training is a very small reduction in WIN training (probably due to reductions in welfare dependence).

[1/] The detailed tables include (1) separate breakdowns for civilians (Tables V.2 through V.9), with military observations included (Tables V.10 through V.15); (2) background information on what Corpsmembers' postprogram behavior would have been in the absence of their participation in Job Corps (Tables V.2, V.3, V.10, and V.11); (3) estimated effects of Job Corp on education (Tables V.4, V.5, V.12, and V.13); (4) estimated effects of Job Corps on training (Tables V.6, V.7, V.14, and V.15); and (5) other estimated effects of Job Corps on investments in human capital (Tables V.8 and V.9).

TABLE V.1

SUMMARY OF MAIN FINDINGS FOR OVERALL IMPACTS ON HUMAN CAPITAL, PER CORPSMEMBER ON AN ANNUALIZED BASIS

Variable	Estimated Job Corps Effects For First Year After Termination	Estimated Job Corps Effects For Second Year After Termination	Estimated Job Corps Effects For Third Year After Termination	Estimated Job Corps Effects For Fourth Year After Termination
1. Probability of high school diploma or GED	0.239	0.265	N.A.	0.275
2. Weeks in college, civilians	0.39	0.55	0.65	1.04
3. Weeks in college, civilians and military	0.16	0.29	0.38	0.65
4. Weeks in high school, civilians	-1.51	-1.20	-0.68	-0.60
5. Weeks in high school, civilians and military	-1.22	-0.99	-0.55	-0.26
6. Weeks in vocational or technical school, civilians	0.34	0.57	0.23	0.21
7. Weeks in vocational or technical school, civilians and military	0.31	0.52	0.13	0.16
8. Weeks in any school, civilians	-1.14	-0.34	-0.08	0.55
9. Weeks in any school, civilians and military	-1.09	-0.42	-0.38	0.13
10. Weeks of serious health problems, civilians	-1.01	-1.07	-1.22	-1.09
11. Weeks of serious health problems, civilians and military	-0.96	-1.07	-1.22	-1.12

The estimates of what Corpsmembers' activities would have been during the postprogram period in the absence of Job Corps (Tables V.2, V.3, V.10, and V.11) show that very little time would have been spent in education and training activities. We estimate that Corpsmembers would have had (1) only approximately a 5 percent probability of having a high school diploma or equivalent degree, which was relatively constant over time, (2) a slight increase in college attendance over time, but only from a little less than one-half week per year to nearly three-quarters of a week per year, (3) a decrease in high school attendance over time, from nearly three weeks per year to under two-thirds of a week per year, and (4) a negligible amount of total training time for all programs (approximately one week per year), which was relatively constant but declining slightly over time.

Overall, in the postprogram period the estimated effects in Table V.1 show approximately a 25 percentage-point increase in the probability that Corpsmembers have a high school diploma, a General Educational Development (GED) degree, or an equivalent degree. As can be seen by comparing this estimate to the base in Tables V.2 and V.3, the percentage increase is extremely large--a fivefold increase in the probability of having a high school or equivalent degree (from approximately 5 percent to 25 percent).

The large increase in high school, GED, and equivalent degrees among Corpsmembers more than explains the approximately one-week-per-year average reduction in high school attendance (a small but still nearly two-thirds decrease). College attendance shows a marginally significant and growing increase of up to one week per year among Corpsmembers by the end

of our postprogram observation period. However, even this modest increase represents nearly a doubling of college attendance. Therefore, the education effects appear to indicate some clear increases in higher educational attainment (more high school degrees and greater college attendance) and, hence, human-capital investments among Corpsmembers. Furthermore, this provides additional evidence that the short-term gains in employment and earnings are not likely to fade out rapidly.

As shown in the detailed tables (Tables V.4, V.5, V.12, and V.13), the estimates of increased investments in human capital among Corpsmembers in the form of higher levels of education are largest for females without children (larger than for either males or females with children). In addition, these estimated Job Corps effects on higher levels of education are larger for males than for females with children (in terms of college attendance) but smaller for males than for females with children (in terms of receiving a high school or equivalent degree). Furthermore, the detailed tables also show that the training effects are mostly nil (small, changeable, and statistically insignificant) across all three groups, except that females without children account for almost all of the reduction in WIN training--a moderate-size and highly significant effect for them.

In summary, Job Corps-induced increases in human-capital investments are evidenced by estimates of large increases in high school or equivalent degrees and moderate-size increases in college attendance. Job Corps leads to decreases in enrollments in lower levels of education and in WIN training programs among former participants during the postprogram period.

B. OTHER INVESTMENTS IN HUMAN CAPITAL

Tables V.8 and V.9 present the findings for the Job Corps effects on other types of investments in human capital. Corpsmembers had better health, showed greater geographical mobility in general and for employment in particular, and were much more likely to have enlisted in the military.

The estimated overall reductions in serious health problems for Corpsmembers are, on average, just over one week per year and are marginally significant. However, the health gains are much larger (over three weeks per year) and highly significant for females without children--compared to one-half to one week of reductions that are statistically insignificant for males. The females-with-children group shows a large and statistically significant increase in serious health problems initially, which, however, declines rapidly over time. (Furthermore, it appears to be attributable, at least in part, to some early birth-related illnesses when, after some Job Corps-induced delays, childbirth began to become more prevalent among former Corpswomen).

The increases in geographical mobility for Corpsmembers are evidenced by [illegible]s for job opportunities, education or training, and otherwise. The overall im[illegible]t estimates are substantial--altogether, by the end of our observation period, nearly 70 additional moves for job opportunities for each 100 Corpsmembers. This added job mobility for Corpsmembers appears to occur throughout the postprogram period, which accounts for its very substantial amount by the end of the period. Furthermore, the mobility effects are probably somewhat understated because of the higher nonresponse rates for Corpsmembers relative to the comparison group, most of which is attributable to moves.

Finally, Tables V.8 and V.9 repeat the estimates of large and

statistically significant Job Corps impacts on military service (nearly a 150 percent increase by the end of the four-year observation period--approximately from 2.8 percent to 6.8 percent). These impacts can be viewed as investments in human capital, since increasing the ability of disadvantaged youths to pass military entrance exams, and the concomitant increases in military service, can be expected to lead to future increases in earnings for these youths. While the gain in military service was also reported above as an employment effect, it does have human-capital implications, since (1) entering the military (i.e., passing the Armed Forces Qualifying Examination) indicates the attainment of a certain level of human-capital development, and (2) participation in the military offers additional human capital development through vocational skills training and job experience. As already noted above, the increases in civilian employment should also produce some long-run human-capital benefits through job experience.

TABLE V.2

ESTIMATES OF CORPSMEMBERS' EDUCATION AND TRAINING HAD THEY NOT PARTICIPATED IN JOB CORPS, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Means 0 to 6 Months After Termination	Job Corps Means 6 to 12 Months After Termination	Job Corps Means 12 to 18 Months After Termination	Job Corps Means 18 to 24 Months After Termination
A. MALES				
1. Probability of having high school diploma or GED by time of interview	0.051		0.050	
2. In college (fraction of time)	0.002	0.003	0.002	0.004
3. In high school (fraction of time)	0.056	0.046	0.035	0.032
4. In any training program (fraction of time)	0.028	0.027	0.024	0.024
B. FEMALES WITHOUT CHILDREN				
1. Probability of having high school diploma or GED by time of interview	0.032		0.043	
2. In college (fraction of time)	0.020	0.020	0.018	0.023
3. In high school (fraction of time)	0.062	0.045	0.030	0.025
4. In any training program (fraction of time)	0.009	0.007	0.012	0.013
C. FEMALES WITH CHILDREN				
1. Probability of having high school diploma or GED by time of interview	0.102		0.053	
2. In college (fraction of time)	0.034	0.025	0.030	0.029
3. In high school (fraction of time)	0.032	0.029	0.020	0.013
4. In any training program (fraction of time)	0.019	0.013	0.014	0.012
D. OVERALL				
1. Probability of having high school diploma or GED by time of interview	0.047		0.050	
2. In college (fraction of time)	0.008	0.008	0.008	0.010
3. In high school (fraction of time)	0.057	0.045	0.033	0.027
4. In any training program (fraction of time)	0.022	0.021	0.021	0.021

TABLE V.3

ESTIMATES OF CORPSMEMBERS' EDUCATION AND TRAINING HAD THEY NOT PARTICIPATED IN JOB CORPS, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Means 24 to 30 Months After Termination	Job Corps Means 30 to 36 Months After Termination	Job Corps Means 36 to 42 Months After Termination	Job Corps Means 42 to 48 Months After Termination
A. MALES				
1. Probability of having high school diploma or GED by time of interview				0.041
2. In college (fraction of time)	0.004	0.006	0.004	0.004
3. In high school (fraction of time)	0.025	0.022	0.019	0.016
4. In any training program (fraction of time)	0.022	0.022	0.020	0.020
B. FEMALES WITHOUT CHILDREN				
1. Probability of having high school diploma or GED by time of interview				0.037
2. In college (fraction of time)	0.020	0.026	0.040	0.043
3. In high school (fraction of time)	0.015	0.012	0.005	0.000
4. In any training program (fraction of time)	0.020	0.022	0.011	0.009
C. FEMALES WITH CHILDREN				
1. Probability of having high school diploma or GED by time of interview				0.063
2. In college (fraction of time)	0.029	0.031	0.032	0.032
3. In high school (fraction of time)	0.006	0.008	0.006	0.003
4. In any training program (fraction of time)	0.010	0.009	0.004	0.001
D. OVERALL				
1. Probability of having high school diploma or GED by time of interview				0.043
2. In college (fraction of time)	0.010	0.012	0.014	0.015
3. In high school (fraction of time)	0.021	0.019	0.015	0.012
4. In any training program (fraction of time)	0.020	0.021	0.016	0.015

TABLE V.4

ESTIMATES OF JOB CORPS IMPACTS ON EDUCATION FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Probability of having high school diploma or GED by time of interview	0.129****		0.169****	
2. In any school (fraction of time)	-0.042***	-0.014	-0.019	-0.027*
o In college (fraction of time)	0.005	0.011	0.011	0.006
o In high school (fraction of time)	-0.033****	-0.021***	-0.023****	-0.025****
o In vocational or technical school (fraction of time)	0.001	0.002	0.006	0.003
o In other school (fraction of time)	-0.013**	-0.009	-0.010*	-0.006
B. FEMALES WITHOUT CHILDREN				
1. Probability of having high school diploma or GED by time of interview	0.518****		0.514****	
2. In any school (fraction of time)	-0.036*	0.034*	0.036*	0.066***
o In college (fraction of time)	-0.0003	0.018	0.018	0.028*
o In high school (fraction of time)	-0.047****	-0.020**	-0.024***	-0.021**
o In vocational or technical school (fraction of time)	0.009	0.020**	0.029***	0.048
o In other school (fraction of time)	-0.001	0.013*	0.012	0.028****
C. FEMALES WITH CHILDREN				
1. Probability of having high school diploma or GED by time of interview	0.303****		0.420****	
2. In any school (fraction of time)	-0.040*	-0.066***	-0.019	-0.007
o In college (fraction of time)	-0.021	-0.017	-0.009	-0.001
o In high school (fraction of time)	-0.032****	-0.019**	-0.015**	-0.009
o In vocational or technical school (fraction of time)	-0.029***	-0.025**	0.006	-0.013
o In other school (fraction of time)	0.044****	0.003	0.005	0.023***
D. OVERALL				
1. Probability of having high school diploma or GED by time of interview	0.239****		0.265****	
2. In any school (fraction of time)	-0.040****	-0.004	-0.007	-0.006
o In college (fraction of time)	0.003	0.012*	0.011*	0.010
o In high school (fraction of time)	-0.037****	-0.021****	-0.023****	-0.023****
o In vocational or technical school (fraction of time)	0.003	0.010*	0.011**	0.011**
o In other school (fraction of time)	-0.008	-0.003	-0.004	0.004

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE 7.5

ESTIMATES OF JOB CORPS IMPACTS ON EDUCATION, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Probability of having high school diploma or GED by time of interview				0.191****
2. In any school (fraction of time)	-0.031**	-0.020	-0.002	-0.001
o In college (fraction of time)	0.006	0.009	0.019**	0.012
o In high school (fraction of time)	-0.022***	-0.019***	-0.016**	-0.016**
o In vocational or technical school (fraction of time)	-0.002	-0.002	0.003	0.009
o In other school (fraction of time)	-0.009	-0.004	-0.003	-0.001
B. FEMALES WITHOUT CHILDREN				
1. Probability of having high school diploma or GED by time of interview				0.506****
2. In any school (fraction of time)	0.060***	0.099****	0.094****	0.103****
o In college (fraction of time)	0.033**	0.054****	0.072****	0.079****
o In high school (fraction of time)	-0.007	-0.009	0.001	0.005
o In vocational or technical school (fraction of time)	0.032***	0.034****	0.010	0.008
o In other school (fraction of time)	0.009	0.021***	0.008	0.009
C. FEMALES WITH CHILDREN				
1. Probability of having high school diploma or GED by time of interview				0.420****
2. In any school (fraction of time)	-0.004	0.025	-0.030*	-0.014
o In college (fraction of time)	-0.014	-0.011	-0.014	-0.011
o In high school (fraction of time)	0.021****	0.034****	-0.003	-0.003
o In vocational or technical school (fraction of time)	-0.014*	-0.016*	-0.013*	-0.009
o In other school (fraction of time)	0.024***	0.042****	0.006	0.013
D. OVERALL				
1. Probability of having high school diploma or GED by time of interview				0.275****
2. In any school (fraction of time)	-0.010	0.007	0.008	0.013
o In college (fraction of time)	0.009	0.016*	0.021****	0.019
o In high school (fraction of time)	-0.015***	-0.011*	-0.012**	-0.011*
o In vocational or technical school (fraction of time)	0.016	0.003	0.002	0.006
o In other school (fraction of time)	-0.002	0.006	0.00009	0.003

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE V.6

ESTIMATES OF JOB CORPS IMPACTS IN TRAINING, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. In any training program (fraction of time)	-0.009	-0.006	-0.008	-0.003
o In CETA training (fraction of time)	-0.011**	-0.007	-0.016*	-0.004
o In youth training (fraction of time)	0.002	-0.001	0.001	0.001
o In WIN training (fraction of time)	-0.00**	-0.00*	-0.001**	-0.001*
o In union training (fraction of time)	0.002	0.002	0.002	0.001
o In other training (fraction of time)	-0.0004	0.001	0.00002	0.001
2. Training allowances per six months in current dollars	-0.27	-12.22	-18.62	-13.24
B. FEMALES WITHOUT CHILDREN				
1. In any training program (fraction of time)	0.006	0.028***	0.016*	0.00006
o In CETA training (fraction of time)	0.003	0.015*	0.006	0.001
o In youth training (fraction of time)	0.002	0.004	-0.001	-0.001
o In WIN training (fraction of time)	-0.008***	-0.008***	-0.008***	-0.009***
o In union training (fraction of time)	0.003	0.003	0.003	0.003
o In other training (fraction of time)	0.005*	0.013****	0.014****	0.004
2. Training allowances per six months in current dollars	16.52	20.83*	8.69	8.32
C. FEMALES WITH CHILDREN				
1. In any training program (fraction of time)	0.014	0.029**	0.004	0.004
o In CETA training (fraction of time)	-0.011	0.010	0.005	-0.001
o In youth training (fraction of time)	0.005*	0.001	-0.001	-0.001
o In WIN training (fraction of time)	-0.001	0.002	-0.001	0.003
o In union training (fraction of time)	0.0	0.0	0.0	0.0
o In other training (fraction of time)	0.022****	0.017****	0.001	0.003
2. Training allowances per six months in current dollars	6.92	9.57	-1.18	10.93
D. OVERALL				
1. In any training program (fraction of time)	-0.004	0.004	-0.002	-0.002
o In CETA training (fraction of time)	-0.007	-0.001	-0.005	-0.003
o In youth training (fraction of time)	0.002	0.001	0.0004	0.00003
o In WIN training (fraction of time)	-0.003****	-0.002***	-0.003****	-0.002***
o In union training (fraction of time)	0.002****	0.002****	0.002***	0.001*
o In other training (fraction of time)	0.002	0.004	0.003	0.002
2. Training allowances per six months in current dollars	4.58	-2.76	-12.10	-6.54

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE V.7

ESTIMATES OF JOB CORPS IMPACTS ON TRAINING, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. In any training program (fraction of time)	-0.006	-0.010	-0.007	-0.009
o In CETA training (fraction of time)	-0.008	-0.010*	-0.008	-0.010*
o In youth training (fraction of time)	0.0000	-0.0001	-0.0002	-0.0001
o In WIN training (fraction of time)	-0.001**	-0.001**	-0.001**	-0.001***
o In union training (fraction of time)	-0.006	-0.001	-0.001	-0.0003
o In other training (fraction of time)	0.004	0.002	0.003	0.003
2. Training allowances per six months in current dollars	-9.55	-12.89	-12.54	-29.26**
B. FEMALES WITHOUT CHILDREN				
1. In any training program (fraction of time)	0.010	0.021**	0.030***	0.023**
o In CETA training (fraction of time)	0.014*	0.027****	0.032****	0.022**
o In youth training (fraction of time)	-0.002	-0.003	-0.001	-0.002
o In WIN training (fraction of time)	-0.009***	-0.009****	-0.010****	-0.010****
o In union training (fraction of time)	0.003	0.002	0.001	0.001
o In other training (fraction of time)	0.002	-0.001	0.004	0.006**
2. Training allowances per six months in current dollars	42.68***	83.50****	108.00****	74.60****
C. FEMALES WITH CHILDREN				
1. In any training program (fraction of time)	0.008	0.021*	0.013	0.016
o In CETA training (fraction of time)	0.001	0.019*	0.013	0.010
o In youth training (fraction of time)	-0.001	-0.001	-0.001	-0.001
o In WIN training (fraction of time)	0.003	-0.002	-0.003	-0.001
o In union training (fraction of time)	0.0	0.0	0.0	0.0
o In other training (fraction of time)	0.005*	0.003	0.003	0.005**
2. Training allowances per six months in current dollars	12.69	20.80	4.18	6.36
D. OVERALL				
1. In any training program (fraction of time)	-0.001	-0.001	0.002	-0.0004
o In CETA training (fraction of time)	-0.003	0.001	0.001	-0.002
o In youth training (fraction of time)	-0.0004	-0.001	-0.0004	-0.0004
o In WIN training (fraction of time)	-0.002***	-0.003****	-0.003****	-0.002****
o In union training (fraction of time)	-0.0001	-0.0002	-0.001	-0.0001
o In other training (fraction of time)	0.004	0.002	0.003	0.004
2. Training allowances per six months in current dollars	3.12	8.50	8.05	-8.34

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE V.8

ESTIMATES OF JOB CORPS IMPACTS IN HEALTH, MOBILITY, AND MILITARY SERVICE: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Serious health problems for civilians (fraction of time)	-0.011	-0.009	-0.010	-0.014
2. Cumulative moves to interview date for civilians				
o All moves across cities	0.264****		0.310***	
o For job opportunities	0.191****		0.229**	
o For education or training	0.063****		0.011	
3. Probability in military during interview week	0.024**		0.056***	
B. FEMALES WITHOUT CHILDREN				
1. Serious health problems for civilians (fraction of time)	-0.056****	-0.050****	-0.068****	-0.063****
2. Cumulative moves to interview date for civilians				
o All moves across cities	0.325***		0.233	
o For job opportunities	0.195***		-0.047	
o For education or training	0.050**		0.098	
3. Probability in military during interview week	0.018		0.0003	
C. FEMALES WITH CHILDREN				
1. Serious health problems for civilians (fraction of time)	0.062****	0.065****	0.038***	0.012
2. Cumulative moves to interview date for civilians				
o All moves across cities				
o For job opportunities				
o For education or training				
3. Probability in military during interview week	0.0		0.0	
D. OVERALL				
1. Serious health problems for civilians (fraction of time)	-0.022**	-0.017*	-0.019*	-0.022**
2. Cumulative moves to interview date for civilians				
o All moves across cities	0.273****		0.270***	
o For job opportunities	0.188****		0.149*	
o For education or training	0.059****		0.030	
3. Probability in military during interview week	0.019***		0.039***	

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE V.9

ESTIMATES OF JOB CORPS IMPACTS ON HEALTH, MOBILITY, AND MILITARY SERVICE: THIRD AND FOURTH POST-PROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Serious health problems for civilians (fraction of time)	-0.022*	-0.019	-0.019	-0.015
2. Cumulative moves to interview date for civilians				
o All moves across cities				0.531*
o For job opportunities				0.350***
o For education or training				-0.019
3. Probability in military during interview week				0.055****
B. FEMALES WITHOUT CHILDREN				
1. Serious health problems for civilians (fraction of time)	-0.043***	-0.073****	-0.073****	-0.075****
2. Cumulative moves to interview date for civilians				
o All moves across cities				1.021**
o For job opportunities				0.248
o For education or training				0.495***
3. Probability in military during interview week				0.006
C. FEMALES WITH CHILDREN				
1. Serious health problems for civilians (fraction of time)	0.012	0.017	0.016	0.010
2. Cumulative moves to interview date for civilians				
o All moves across cities				0.201
o For job opportunities				0.111
o For education or training				0.092*
3. Probability in military during interview week				0.0
D. OVERALL				
1. Serious health problems for civilians (fraction of time)	-0.023**	-0.024***	-0.022**	-0.020**
2. Cumulative moves to interview date for civilians				
o All moves across cities				0.580**
o For job opportunities				0.681****
o For education or training				0.087
3. Probability in military during interview week				0.040****

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE V.10

ESTIMATES OF CORPSMEMBERS' EDUCATION AND TRAINING HAD THEY NOT PARTICIPATED IN JOB CORPS, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Means 0 to 6 Months After Termination	Job Corps Means 6 to 12 Months After Termination	Job Corps Means 12 to 18 Months After Termination	Job Corps Means 18 to 24 Months After Termination
A. MALES				
1. In college (fraction of time)	0.007	0.008	0.007	0.010
2. In high school (fraction of time)	0.040	0.040	0.030	0.025
3. In any training program (fraction of time)	0.026	0.025	0.024	0.024
B. FEMALES WITHOUT CHILDREN				
1. In college (fraction of time)	0.021	0.021	0.019	0.028
2. In high school (fraction of time)	0.061	0.042	0.027	0.021
3. In any training program (fraction of time)	0.010	0.009	0.013	0.015
C. FEMALES WITH CHILDREN				
1. In college (fraction of time)	0.034	0.025	0.030	0.027
2. In high school (fraction of time)	0.032	0.029	0.020	0.012
3. In any training program (fraction of time)	0.019	0.013	0.014	0.012
D. OVERALL				
1. In college (fraction of time)	0.011	0.012	0.011	0.014
2. In high school (fraction of time)	0.045	0.040	0.028	0.024
3. In any training program (fraction of time)	0.022	0.020	0.021	0.021

TABLE 7.11

ESTIMATES OF CORPSMEMBERS' EDUCATION AND TRAINING HAD THEY NOT PARTICIPATED IN JOB CORPS, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Means 24 to 30 Months After Termination	Job Corps Means 30 to 36 Months After Termination	Job Corps Means 36 to 42 Months After Termination	Job Corps Means 42 to 48 Months After Termination
A. MALES				
1. In college (fraction of time)	0.011	0.012	0.011	0.013
2. In high school (fraction of time)	0.022	0.020	0.017	0.015
3. In any training program (fraction of time)	0.022	0.023	0.021	0.021
B. FEMALES WITHOUT CHILDREN				
1. In college (fraction of time)	0.022	0.027	0.039	0.043
2. In high school (fraction of time)	0.012	0.008	0.001	0.000
3. In any training program (fraction of time)	0.021	0.023	0.012	0.010
C. FEMALES WITH CHILDREN				
1. In college (fraction of time)	0.029	0.031	0.032	0.032
2. In high school (fraction of time)	0.007	0.008	0.006	0.004
3. In any training program (fraction of time)	0.010	0.009	0.004	0.002
D. OVERALL				
1. In college (fraction of time)	0.015	0.017	0.019	0.020
2. In high school (fraction of time)	0.018	0.016	0.013	0.010
3. In any training program (fraction of time)	0.020	0.021	0.017	0.016

TABLE V.12

ESTIMATES OF JOB CORPS IMPACTS IN EDUCATION, EXCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. In any school (fraction of time)	-0.039****	-0.014	-0.021*	-0.030***
2. In college (fraction of time)	0.00006	0.005	0.005	-0.001
3. In high school (fraction of time)	-0.025****	-0.016***	-0.018***	-0.021****
4. In vocational or technical school (fraction of time)	0.001	0.007	0.005	0.002
5. In other school (fraction of time)	-0.013**	-0.010*	-0.011*	-0.007
B. FEMALES WITHOUT CHILDREN				
1. In any school (fraction of time)	-0.036*	0.038*	0.035*	0.064***
2. In college (fraction of time)	-0.001	0.016	0.016	0.025*
3. In high school (fraction of time)	-0.046****	-0.017*	-0.022**	-0.018*
4. In vocational or technical school (fraction of time)	0.009	0.019**	0.028***	0.047****
5. In other school (fraction of time)	0.008	0.020***	0.007	0.009
C. FEMALES WITH CHILDREN				
1. In any school (fraction of time)	-0.040*	-0.066***	-0.019	-0.007
2. In college (fraction of time)	-0.021	-0.017	-0.009	-0.001
3. In high school (fraction of time)	-0.032****	-0.019**	-0.015**	-0.009
4. In vocational or technical school (fraction of time)	-0.029***	-0.025**	0.006	-0.013
5. In other school (fraction of time)	0.044****	0.003	-0.005	0.023***
D. OVERALL				
1. In any school (fraction of time)	-0.038****	-0.008	-0.008	-0.008
2. In college (fraction of time)	-0.001	0.007	0.007	0.004
3. In high school (fraction of time)	-0.031****	-0.016***	-0.019****	-0.019****
4. In vocational or technical school (fraction of time)	0.003	0.009*	0.010**	0.010**
5. In other school (fraction of time)	-0.008*	-0.004	-0.005	0.003

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE V.13

ESTIMATES OF JOB CORPS IMPACTS ON EDUCATION, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. In any school (fraction of time)	-0.035***	-0.027**	-0.011	-0.011
2. In college (fraction of time)	-0.001	0.001	0.009	0.003
3. In high school (fraction of time)	-0.019***	-0.017***	-0.014**	-0.014**
4. In vocational or technical school (fraction of time)	-0.003	-0.002	0.002	0.007
5. In other school (fraction of time)	-0.010*	-0.006	-0.005	-0.003
B. FEMALES WITHOUT CHILDREN				
1. In any school (fraction of time)	0.058***	0.095****	0.089****	0.097****
2. In college (fraction of time)	0.030*	0.050****	0.066****	0.072****
3. In high school (fraction of time)	-0.004	-0.006	0.003	0.008
4. In vocational or technical school (fraction of time)	0.030***	0.033***	0.010	0.008
5. In other school (fraction of time)	-0.0002	0.012*	0.011	0.028****
C. FEMALES WITH CHILDREN				
1. In any school (fraction of time)	-0.004	0.025	-0.030*	-0.014
2. In college (fraction of time)	-0.014	-0.011	-0.014	-0.011
3. In high school (fraction of time)	0.021****	0.034****	-0.003	-0.003
4. In vocational or technical school (fraction of time)	-0.014*	-0.016*	-0.013*	-0.009
5. In other school (fraction of time)	0.028***	0.042****	0.006	0.013
D. OVERALL				
1. In any school (fraction of time)	-0.014	0.001	0.001	0.004
2. In college (fraction of time)	0.004	0.009	0.014**	0.011*
3. In high school (fraction of time)	-0.012***	-0.009*	-0.001*	-0.009*
4. In vocational or technical school (fraction of time)	0.003	0.002	0.001	0.005
5. In other school (fraction of time)	-0.003	0.004	-0.001	0.001

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE V.14

ESTIMATES OF JOB CORPS IMPACTS ON TRAINING AND HEALTH, EXCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. In any training program (fraction of time)	-0.008	-0.005	-0.009	-0.005
o In CETA training (fraction of time)	-0.010*	-0.006	-0.009*	-0.005
o In youth training (fraction of time)	0.001	0.0004	0.001	0.0003
o In WIN training (fraction of time)	-0.001*	-0.001	-0.001**	-0.001*
o In union training (fraction of time)	0.001	0.0003	0.001	-0.0004
o In other training (fraction of time)	0.0004	0.001	-0.001	0.001
2. Training allowances per six months in current dollars	2.36	-10.26	-17.23	-12.87
3. Serious health problems (fraction of time)	-0.009	-0.008	-0.011	-0.013*
B. FEMALES WITHOUT CHILDREN				
1. In any training program (fraction of time)	0.005	0.026***	0.015	-0.002
o In CETA training (fraction of time)	0.001	0.013*	0.004	-0.001
o In youth training (fraction of time)	0.002	0.003	-0.001	-0.001
o In WIN training (fraction of time)	-0.008***	-0.008***	-0.008***	-0.009***
o In union training (fraction of time)	0.003	0.003	0.003	0.003
o In other training (fraction of time)	0.005*	0.013****	0.014****	0.004
2. Training allowances per six months in current dollars	15.18	19.77*	4.01	7.71
3. Serious health problems (fraction of time)	-0.056****	-0.050****	-0.067****	-0.063****
C. FEMALES WITH CHILDREN				
1. In any training program (fraction of time)	0.014	0.029**	0.004	0.004
o In CETA training (fraction of time)	-0.011	0.010	0.005	-0.001
o In youth training (fraction of time)	0.005*	0.001	-0.001	-0.001
o In WIN training (fraction of time)	-0.001	0.002	-0.001	0.003
o In union training (fraction of time)	0.0	0.0	0.0	0.0
o In other training (fraction of time)	0.022****	0.017****	0.001	0.003
2. Training allowances per six months in current dollars	6.92	9.57	-1.18	10.93
3. Serious health problems (fraction of time)	0.062****	0.065****	0.038***	0.012
D. OVERALL				
1. In any training program (fraction of time)	-0.004	0.004	-0.003	-0.003
o In CETA training (fraction of time)	-0.001	-0.0002	-0.005	-0.004
o In youth training (fraction of time)	0.002	0.001	0.0003	-0.0001
o In WIN training (fraction of time)	-0.003****	-0.002***	-0.003****	-0.002***
o In union training (fraction of time)	0.001*	0.001	0.001	0.0004
o In other training (fraction of time)	0.002	0.005*	0.003	0.002
2. Training allowances per six months in current dollars	6.04	-1.66	-11.28	-6.41
3. Serious health problems (fraction of time)	-0.021**	-0.016*	-0.019**	-0.022**

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE V.1

ESTIMATES OF JOB CORPS IMPACTS ON TRAINING AND HEALTH, EXCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. In any training program (fraction of time)	-0.007	-0.011	-0.009	-0.011
o In CETA training (fraction of time)	-0.008	-0.010*	-0.009	-0.010*
o In youth training (fraction of time)	-0.00003	-0.0002	-0.0002	-0.0001
o In WIN training (fraction of time)	-0.001**	-0.001**	-0.001**	-0.001***
o In union training (fraction of time)	-0.002	-0.002	-0.002	-0.002
o In other training (fraction of time)	0.004	0.002	0.004	0.002
2. Training allowances per six months in current dollars	-9.61	-13.59	-13.85	-30.00**
3. Serious health problems (fraction of time)	-0.022*	-0.019	-0.019*	-0.015
B. FEMALES WITHOUT CHILDREN				
1. In any training program (fraction of time)	0.008	0.019*	0.026**	0.020*
o In CETA training (fraction of time)	0.012	0.026***	0.028***	0.019*
o In youth training (fraction of time)	-0.002	-0.003	-0.001	-0.002
o In WIN training (fraction of time)	-0.009***	-0.009****	-0.009****	-0.010****
o In union training (fraction of time)	0.003	0.002	0.001	0.001
o In other training (fraction of time)	0.002	-0.001	-0.004	0.007*
2. Training allowances per six months in current dollars	41.07***	79.23****	100.94****	68.69****
3. Serious health problems (fraction of time)	-0.044***	-0.074****	-0.074****	-0.077****
C. FEMALES WITH CHILDREN				
1. In any training program (fraction of time)	0.008	0.021*	0.013	0.016
o In CETA training (fraction of time)	0.001	0.019*	0.013	0.010
o In youth training (fraction of time)	-0.001	-0.001	-0.001	-0.001
o In WIN training (fraction of time)	0.003	-0.002	-0.003	-0.001
o In union training (fraction of time)	0.0	0.0	0.0	0.0
o In other training (fraction of time)	0.005*	0.003	0.003	0.006**
2. Training allowances per six months in current dollars	12.69	20.80	4.18	6.36
3. Serious health problems (fraction of time)	0.012	0.017	0.016	0.010
D. OVERALL				
1. In any training program (fraction of time)	-0.003	-0.002	-0.0004	-0.002
o In CETA training (fraction of time)	-0.003	-0.0003	0.0002	-0.003
o In youth training (fraction of time)	-0.001	-0.001	-0.001	-0.001
o In WIN training (fraction of time)	-0.002***	-0.003****	-0.003****	-0.003****
o In union training (fraction of time)	-0.001	-0.001	-0.001*	-0.001
o In other training (fraction of time)	0.003	0.001	0.003	0.003
2. Training allowances per six months in current dollars	2.76	7.85	6.07	9.74
3. Serious health problems (fraction of time)	-0.023***	-0.024****	-0.022***	-0.021**

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

VI. IMPACT OF JOB CORPS ON PUBLIC-TRANSFER DEPENDENCE

The increased employment and earnings of former Corpsmembers is expected to reduce their postprogram dependence on public transfers. Because of increased earnings after leaving Job Corps, former Corpsmembers are expected to receive fewer public transfers--including AFDC, General Assistance, Food Stamps, public housing, Unemployment Insurance, and Workers' Compensation--than they would have received otherwise during the postprogram period. In addition, Corpsmembers receive much less of such transfers while they are in the Job Corps program (see Mallar et al., 1978), which is also expected to reduce their future use of such programs by breaking the link with any preprogram dependencies that had or would have developed.

The postprogram findings for the effects of Job Corps on public transfers are summarized in Table VI.1 and are detailed with more refined estimates in Tables VI.2 through VI.13.[1] Overall, they show (1) a very large and statistically significant reduction in the receipt of cash welfare (both AFDC and General Assistance); (2) a very large and statistically significant reduction in the receipt of Food Stamps; (3) a moderate-size but insignificant effect on public housing (an increase in the first two postprogram years and then a decrease in the third and fourth postprogram years); (4) a very large and statistically significant

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[1] The detailed tables include (1) separate breakdowns for civilians (Tables VI.2 through VI.7), and with military observations included (Tables VI.8 through VI.13); (2) background information on what Corpsmembers' postprogram behavior would have been in the absence of their participation in Job Corps (Tables VI.2, VI.3, VI.8, and VI.9); (3) estimated effects of Job Corps on welfare assistance (Tables VI.4, VI.5, VI.10, and VI.11); and (4) estimated effects of Job Corps on public transfers other than welfare assistance (Tables VI.6, VI.7, VI.12, and VI.13).

TABLE VI.1

SUMMARY OF MAIN FINDINGS FOR OVERALL IMPACTS ON PUBLIC TRANSFERS, PER CORPSMEMBER ON AN ANNUALIZED BASIS

Variable	Estimated Job Corps Effects For First Year After Termination	Estimated Job Corps Effects For Second Year After Termination	Estimated Job Corps Effects For Third Year After Termination	Estimated Job Corps Effects For Fourth Year After Termination
1. Weeks received any cash welfare, civilians	-2.99	-1.87	-2.31	-1.46
2. Weeks received any cash welfare, civilians and military	-2.96	-1.87	-2.37	-1.51
3. Weeks received AFDC, civilians	-1.85	-0.99	-1.38	-0.68
4. Weeks received AFDC, civilians and military	-1.90	-1.04	-1.40	-0.75
5. Weeks received GA or other, civilians	-1.25	-0.98	-1.01	-0.78
6. Weeks received GA or other, civilians and military	-1.20	-0.91	-0.99	-0.78
7. Weeks received Food Stamps, civilians	-1.48	-0.24	-1.74	-0.52
8. Weeks received Food Stamps, civilians and military	-1.51	-0.55	-2.21	-1.25
9. Weeks lived in public housing, civilians	0.86	1.14	-0.47	-0.70
10. Weeks lived in public housing, civilians and military	0.78	0.96	-0.73	-0.96
11. Weeks received Unemployment Insurance, civilians	-1.07	-0.65	-0.75	-0.70
12. Weeks received Unemployment Insurance, civilians and military	-1.04	-0.65	-0.75	-0.75
13. Weeks received Workers' Compensation, civilians	0.07	0.23	0.34	0.36
14. Weeks received Workers' Compensation, civilians and military	0.05	0.21	0.29	0.34

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reduction in the receipt of Unemployment Insurance (UI); (5) a very small but marginally significant increase in the receipt of Workers' Compensation (which is probably attributable to the increased employment of Corpsmembers); and (6) mixed and insignificant effects on the receipt of training allowances (which are consistent with the estimated null effects on training reported in Chapter V). Altogether, these findings provide strong support for the hypothesized reductions in public-transfer dependence.

The estimates of what Corpsmembers would have done during the postprogram period had they not participated in Job Corps show a large degree of dependence on public transfers. As shown in Tables VI.2, VI.3, VI.8, and VI.9, we estimate that on average (approximately): (1) 8 to 9 percent of the time, Corpsmembers would have been receiving cash welfare (four weeks per year), (2) 20 percent of the time, Corpsmembers would have been receiving Food Stamps (ten weeks per year), (3) 7 to 8 percent of the time, Corpsmembers would have been living in public housing (four weeks per year), and (4) 3 percent of the time, Corpsmembers would have been receiving Unemployment Insurance (one to two weeks per year).

Overall, in the postprogram period the estimated effects shown in Table VI.1 amount to (approximately): (1) a 50 percent reduction in the receipt of cash welfare (both AFDC and General Assistance), (2) a 10 percent reduction in the receipt of Food Stamps, and (3) a 50 percent reduction in the receipt of Unemployment Insurance. The only increase in public transfers occurs for Workers' Compensation; it is very small, only marginally significant, and probably attributable to the increased employment of Corpsmembers (so that they have a higher probability of job-

related injury, even if they hold better jobs).

These estimated effects of Job Corps on former participants' receipt of public transfers are relatively constant over time, and differ very little according to whether or not military observations are included. The estimated reductions in cash welfare assistance (see Tables VI.4, VI.5, VI.10, and VI.11) are strongest for females without children, weakest for females with children, and intermediate for males. The estimated reductions in Food Stamps (see Tables VI.4, VI.5, VI.10, and VI.11) are strongest for females without children, weakest for males, and intermediate for females with children. Finally, the estimated reductions in Unemployment Insurance (see Tables VI.6, VI.7, VI.12, and VI.13) are strongest for males, weakest for females with children, and intermediate for females without children.

TABLE VI.2

ESTIMATES OF CORPSMEMBERS' RECEIPT OF PUBLIC TRANSFERS HAD THEY NOT PARTICIPATED IN JOB CORPS, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Means 0 to 6 Months After Termination	Job Corps Means 6 to 12 Months After Termination	Job Corps Means 12 to 18 Months After Termination	Job Corps Means 18 to 24 Months After Termination
A. MALES				
1. Received any cash welfare (fraction of time)	0.043	0.041	0.037	0.036
2. Received Food Stamps (fraction of time)	0.144	0.133	0.131	0.135
3. Lived in public housing (fraction of time)	0.052	0.050	0.048	0.052
4. Received Unemployment Insurance (fraction of time)	0.030	0.034	0.033	0.037
B. FEMALES WITHOUT CHILDREN				
1. Received any cash welfare (fraction of time)	0.179	0.187	0.180	0.176
2. Received Food Stamps (fraction of time)	0.264	0.270	0.254	0.240
3. Lived in public housing (fraction of time)	0.109	0.110	0.109	0.104
4. Received Unemployment Insurance (fraction of time)	0.011	0.014	0.015	0.017
C. FEMALES WITH CHILDREN				
1. Received any cash welfare (fraction of time)	0.433	0.495	0.382	0.316
2. Received Food Stamps (fraction of time)	0.425	0.527	0.480	0.492
3. Lived in public housing (fraction of time)	0.108	0.107	0.122	0.144
4. Received Unemployment Insurance (fraction of time)	0.000	0.000	0.000	0.000
D. OVERALL				
1. Received any cash welfare (fraction of time)	0.089	0.095	0.096	0.090
2. Received Food Stamps (fraction of time)	0.189	0.184	0.186	0.189
3. Lived in public housing (fraction of time)	0.069	0.068	0.067	0.071
4. Received Unemployment Insurance (fraction of time)	0.024	0.027	0.026	0.030

TABLE VI.3

ESTIMATES OF CORPSMEMBERS' RECEIPT OF PUBLIC TRANSFERS HAD THEY NOT PARTICIPATED IN JOB CORPS, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Means 24 to 30 Months After Termination	Job Corps Means 30 to 36 Months After Termination	Job Corps Means 36 to 42 Months After Termination	Job Corps Means 42 to 48 Months After Termination
A. MALES				
1. Received any cash welfare (fraction of time)	0.034	0.033	0.031	0.028
2. Received Food Stamps (fraction of time)	0.137	0.137	0.135	0.142
3. Lived in public housing (fraction of time)	0.056	0.064	0.064	0.071
4. Received Unemployment Insurance (fraction of time)	0.036	0.039	0.037	0.041
B. FEMALES WITHOUT CHILDREN				
1. Received any cash welfare (fraction of time)	0.177	0.180	0.168	0.177
2. Received Food Stamps (fraction of time)	0.248	0.256	0.238	0.242
3. Lived in public housing (fraction of time)	0.100	0.101	0.097	0.103
4. Received Unemployment Insurance (fraction of time)	0.020	0.021	0.021	0.022
C. FEMALES WITH CHILDREN				
1. Received any cash welfare (fraction of time)	0.281	0.288	0.230	0.210
2. Received Food Stamps (fraction of time)	0.528	0.542	0.511	0.537
3. Lived in public housing (fraction of time)	0.155	0.160	0.161	0.175
4. Received Unemployment Insurance (fraction of time)	0.002	0.003	0.006	0.008
D. OVERALL				
1. Received any cash welfare (fraction of time)	0.088	0.090	0.081	0.078
2. Received Food Stamps (fraction of time)	0.198	0.207	0.207	0.216
3. Lived in public housing (fraction of time)	0.074	0.083	0.083	0.091
4. Received Unemployment Insurance (fraction of time)	0.029	0.032	0.030	0.033

TABLE VI.4

ESTIMATES OF JOB CORPS IMPACTS ON RECEIPT OF PUBLIC ASSISTANCE, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Received any cash welfare (fraction of time)	-0.030***	-0.030***	-0.021**	-0.020**
o Received AFDC (fraction of time)	-0.003	-0.003	0.002	0.002
o Received GA or other (fraction of time)	-0.026****	-0.026****	-0.023****	-0.023****
2. Received Food Stamps (fraction of time)	0.005	0.007	0.029	0.021
3. Lived in public housing (fraction of time)	0.030	0.040*	0.044*	0.038*
B. FEMALES WITHOUT CHILDREN				
1. Received any cash welfare (fraction of time)	-0.138****	-0.125****	-0.130****	-0.121****
o Received AFDC (fraction of time)	-0.128****	-0.112****	-0.117****	-0.109****
o Received GA or other (fraction of time)	-0.016*	-0.020*	-0.019*	-0.017*
2. Received Food Stamps (fraction of time)	-0.119****	-0.109****	-0.085***	-0.095***
3. Lived in public housing (fraction of time)	-0.029	-0.030	-0.036*	-0.030
C. FEMALES WITH CHILDREN				
1. Received any cash welfare (fraction of time)	-0.126***	0.019	0.095***	0.032
o Received AFDC (fraction of time)	-0.149****	0.009	0.078**	0.018
o Received GA or other (fraction of time)	0.015	0.007	0.014	0.028***
2. Received Food Stamps (fraction of time)	-0.088*	-0.027	-0.024	-0.035
3. Lived in public housing (fraction of time)	-0.035	0.017	0.019	-0.003
D. OVERALL				
1. Received any cash welfare (fraction of time)	-0.062****	-0.053****	-0.036****	-0.036****
o Received AFDC (fraction of time)	-0.041****	-0.030****	-0.018****	-0.020****
o Received GA or other (fraction of time)	-0.024****	-0.024****	-0.019****	-0.017****
2. Received Food Stamps (fraction of time)	-0.032***	-0.025**	-0.0003	-0.009
3. Lived in public housing (fraction of time)	0.012	0.021	0.024	0.020

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VI.5

ESTIMATES OF JOB CORPS IMPACTS ON RECEIPT OF PUBLIC ASSISTANCE, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Received any cash welfare (fraction of time)	-0.024***	-0.028***	-0.013	-0.013
o Received AFDC (fraction of time)	-0.001	-0.001	0.004	0.004
o Received GA or other (fraction of time)	-0.024****	-0.023****	-0.017***	-0.017***
2. Received Food Stamps (fraction of time)	-0.003	-0.026	0.008	0.010
3. Lived in public housing (fraction of time)	0.010	0.004	0.010	0.005
B. FEMALES WITHOUT CHILDREN				
1. Received any cash welfare (fraction of time)	-0.107****	-0.122****	-0.143****	-0.154****
o Received AFDC (fraction of time)	-0.099****	-0.109****	-0.131****	-0.138****
o Received GA or other (fraction of time)	-0.012	-0.016	-0.015	-0.017
2. Received Food Stamps (fraction of time)	-0.108****	-0.078**	-0.110****	-0.094***
3. Lived in public housing (fraction of time)	-0.048*	-0.065***	-0.066***	-0.048*
C. FEMALES WITH CHILDREN				
1. Received any cash welfare (fraction of time)	-0.071*	-0.045	0.015	0.031
o Received AFDC (fraction of time)	-0.071*	-0.042	0.021	0.037
o Received GA or other (fraction of time)	0.001	-0.005	-0.004	-0.004
2. Received Food Stamps (fraction of time)	-0.039	-0.049	-0.001	-0.015
3. Lived in public housing (fraction of time)	-0.023	-0.042	-0.067***	-0.071***
D. OVERALL				
1. Received any cash welfare (fraction of time)	-0.045****	-0.044****	-0.028***	-0.028***
o Received AFDC (fraction of time)	-0.028****	-0.025****	-0.014*	-0.012
o Received GA or other (fraction of time)	-0.019****	-0.020****	-0.015***	-0.015***
2. Received Food Stamps (fraction of time)	-0.028****	-0.039*	-0.011	-0.009
3. Lived in public housing (fraction of time)	-0.004	-0.014*	-0.013	-0.014*

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VI.5

ESTIMATES OF JOB CORPS IMPACTS ON RECEIPT OF OTHER PUBLIC TRANSFERS, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Received Unemployment Insurance (fraction of time)	-0.026****	-0.026****	-0.020***	-0.018***
2. Received Workers' Compensation (fraction of time)	0.0003	0.005	0.006	0.007*
3. Training allowances per six months in current dollars	-0.27	-12.22	-18.62	-13.24
B. FEMALES WITHOUT CHILDREN				
1. Received Unemployment Insurance (fraction of time)	-0.009*	-0.010*	-0.006	0.002
2. Received Workers' Compensation (fraction of time)	-0.002	-0.002	-0.001	0.0003
3. Training allowances per six months in current dollars	16.52	20.83*	4.69	8.32
C. FEMALES WITH CHILDREN				
1. Received Unemployment Insurance (fraction of time)	0.012	0.007	0.017*	0.013*
2. Received Workers' Compensation (fraction of time)	-0.0002	-0.0002	0.0004	-0.0003
3. Training allowances per six months in current dollars	6.92	9.57	-1.18	10.93
D. OVERALL				
1. Received Unemployment Insurance (fraction of time)	-0.020****	-0.021****	-0.014***	-0.011**
2. Received Workers' Compensation (fraction of time)	-0.0004	0.003	0.004	0.005*
3. Training allowances per six months in current dollars	4.57	-2.76	-12.10	-6.54

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VI.7

ESTIMATED JOB CORPS IMPACTS ON RECEIPT OF OTHER PUBLIC TRANSFERS, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Received Unemployment Insurance (fraction of time)	-0.018***	-0.020***	-0.011*	-0.024****
2. Received Workers' Compensation (fraction of time)	0.008*	0.012***	0.009**	0.011***
3. Training allowances per six months in current dollars	-9.55	-12.89	-12.54	-29.26**
B. FEMALES WITHOUT CHILDREN				
1. Received Unemployment Insurance (fraction of time)	-0.001	-0.017***	-0.011	-0.011
2. Received Workers' Compensation (fraction of time)	-0.002	-0.002	-0.002	0.0002
3. Training allowances per six months in current dollars	42.68***	83.50****	108.01****	74.59****
C. FEMALES WITH CHILDREN				
1. Received Unemployment Insurance (fraction of time)	0.008	0.004	0.003	0.005
2. Received Workers' Compensation (fraction of time)	-0.001	-0.001	-0.0001	-0.001
3. Training allowances per six months in current dollars	12.69	20.80	4.18	6.36
D. OVERALL				
1. Received Unemployment Insurance (fraction of time)	-0.012***	-0.017****	-0.009*	-0.018****
2. Received Workers' Compensation (fraction of time)	0.005*	0.008***	0.006**	0.008***
3. Training allowances per six months in current dollars	3.12	8.50	8.05	-8.34

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VI.8

ESTIMATES OF PARTICIPANTS' RECEIPT OF PUBLIC TRANSFERS HAD THEY NOT PARTICIPATED IN JOB CORPS, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Means 0 to 6 Months After Termination	Job Corps Means 6 to 12 Months After Termination	Job Corps Means 12 to 18 Months After Termination	Job Corps Means 18 to 24 Months After Termination
A. MALES				
1. Received any cash welfare (fraction of time)	0.041	0.039	0.036	0.035
2. Received Food Stamps (fraction of time)	0.138	0.127	0.128	0.133
3. Lived in public housing (fraction of time)	0.052	0.047	0.047	0.052
4. Received Unemployment Insurance (fraction of time)	0.029	0.033	0.032	0.036
B. FEMALES WITHOUT CHILDREN				
1. Received any cash welfare (fraction of time)	0.180	0.188	0.181	0.176
2. Received Food Stamps (fraction of time)	0.264	0.272	0.258	0.248
3. Lived in public housing (fraction of time)	0.109	0.111	0.110	0.103
4. Received Unemployment Insurance (fraction of time)	0.011	0.014	0.015	0.017
C. FEMALES WITH CHILDREN				
1. Received any cash welfare (fraction of time)	0.432	0.445	0.381	0.316
2. Received Food Stamps (fraction of time)	0.425	0.527	0.480	0.492
3. Lived in public housing (fraction of time)	0.107	0.107	0.123	0.144
4. Received Unemployment Insurance (fraction of time)	-0.012	-0.007	-0.005	-0.002
D. OVERALL				
1. Received any cash welfare (fraction of time)	0.088	0.098	0.095	0.090
2. Received Food Stamps (fraction of time)	0.179	0.181	0.185	0.189
3. Lived in public housing (fraction of time)	0.069	0.066	0.067	0.071
4. Received Unemployment Insurance (fraction of time)	0.023	0.026	0.025	0.029

TABLE VI.9

ESTIMATES OF EXPERIMENTERS' RECEIPT OF PUBLIC TRANSFERS HAD THEY NOT PARTICIPATED IN JOB CORPS, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Means 24 to 30 Months After Termination	Job Corps Means 30 to 36 Months After Termination	Job Corps Means 36 to 42 Months After Termination	Job Corps Means 42 to 48 Months After Termination
A. MALES				
1. Received any cash welfare (fraction of time)	0.033	0.032	0.030	0.028
2. Received Food Stamps (fraction of time)	0.139	0.140	0.140	0.147
3. Lived in public housing (fraction of time)	0.058	0.065	0.065	0.072
4. Received Unemployment Insurance (fraction of time)	0.035	0.038	0.036	0.041
B. FEMALES WITHOUT CHILDREN				
1. Received any cash welfare (fraction of time)	0.177	0.180	0.168	0.178
2. Received Food Stamps (fraction of time)	0.249	0.257	0.237	0.240
3. Lived in public housing (fraction of time)	0.099	0.099	0.095	0.010
4. Received Unemployment Insurance (fraction of time)	0.019	0.021	0.021	0.022
C. FEMALES WITH CHILDREN				
1. Received any cash welfare (fraction of time)	0.281	0.287	0.230	0.210
2. Received Food Stamps (fraction of time)	0.528	0.542	0.510	0.537
3. Lived in public housing (fraction of time)	0.154	0.160	0.161	0.175
4. Received Unemployment Insurance (fraction of time)	0.001	0.003	0.005	0.008
D. OVERALL				
1. Received any cash welfare (fraction of time)	0.087	0.089	0.081	0.078
2. Received Food Stamps (fraction of time)	0.200	0.209	0.210	0.220
3. Lived in public housing (fraction of time)	0.076	0.083	0.084	0.091
4. Received Unemployment Insurance (fraction of time)	0.029	0.031	0.029	0.033

TABLE VI.10

ESTIMATES OF JOB CORPS IMPACTS ON RECEIPT OF PUBLIC ASSISTANCE, EXCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Received any cash welfare (fraction of time)	-0.029***	-0.029****	-0.020**	-0.020***
o Received AFDC (fraction of time)	-0.003	-0.003	0.001	0.002
o Received GA or other (fraction of time)	-0.026****	-0.026****	-0.022****	-0.022****
2. Received Food Stamps (fraction of time)	0.008	0.004	0.023	0.015
3. Lived in public housing (fraction of time)	0.028	0.037*	0.040*	0.032
B. FEMALES WITHOUT CHILDREN				
1. Received any cash welfare (fraction of time)	-0.139****	-0.127****	-0.131****	-0.123****
o Received AFDC (fraction of time)	-0.130****	-0.113****	-0.118****	-0.111****
o Received GA or other (fraction of time)	-0.016*	-0.020**	-0.019*	-0.017*
2. Received Food Stamps (fraction of time)	-0.121****	-0.114****	-0.093***	-0.103****
3. Lived in public housing (fraction of time)	-0.029	-0.031	-0.038*	-0.031**
C. FEMALES WITH CHILDREN				
1. Received any cash welfare (fraction of time)	-0.126***	0.019	0.095***	0.032
o Received AFDC (fraction of time)	-0.149****	0.009	0.078***	0.018
o Received GA or other (fraction of time)	0.015	0.007	0.014	0.028***
2. Received Food Stamps (fraction of time)	-0.088*	-0.027	-0.024	-0.035
3. Lived in public housing (fraction of time)	-0.035	0.017	0.019	-0.003
D. OVERALL				
1. Received any cash welfare (fraction of time)	-0.061****	-0.053****	-0.035****	-0.037****
o Received AFDC (fraction of time)	-0.042****	-0.031****	-0.019***	-0.021***
o Received GA or other (fraction of time)	-0.023****	-0.023****	-0.018****	-0.017****
2. Received Food Stamps (fraction of time)	-0.030	-0.028	-0.007	-0.014
3. Lived in public housing (fraction of time)	0.011	0.019	0.021	0.016

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VI.11

ESTIMATES OF JOB CORPS IMPACTS ON RECEIPT OF PUBLIC ASSISTANCE, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Received any cash welfare (fraction of time)	-0.024***	-0.024***	-0.014	-0.014
o Received AFDC (fraction of time)	-0.001	-0.002	0.003	0.003
o Received GA or other (fraction of time)	-0.023****	-0.022****	-0.017***	-0.017***
2. Received Food Stamps (fraction of time)	-0.013	-0.038	-0.009	-0.009
3. Lived in public housing (fraction of time)	0.004	-0.003	0.002	-0.003
B. FEMALES WITHOUT CHILDREN				
1. Received any cash welfare (fraction of time)	-0.110****	-0.125****	-0.145****	-0.156****
o Received AFDC (fraction of time)	-0.101****	-0.111****	-0.132****	-0.140****
o Received GA or other (fraction of time)	-0.012	-0.017*	-0.015	-0.017
2. Received Food Stamps (fraction of time)	-0.116****	-0.087***	-0.119****	-0.103***
3. Lived in public housing (fraction of time)	-0.045**	-0.065***	-0.067***	-0.045*
C. FEMALES WITH CHILDREN				
1. Received any cash welfare (fraction of time)	-0.071*	-0.045	0.015	0.031
o Received AFDC (fraction of time)	-0.071*	-0.042	0.021	0.037
o Received GA or other (fraction of time)	0.001	-0.005	-0.004	-0.004
2. Received Food Stamps (fraction of time)	-0.039	-0.058	-0.001	-0.015
3. Lived in public housing (fraction of time)	-0.023	-0.042	-0.067***	-0.071***
D. OVERALL				
1. Received any cash welfare (fraction of time)	-0.046****	-0.045****	-0.029****	-0.029****
o Received AFDC (fraction of time)	-0.028****	-0.026****	-0.015*	-0.014*
o Received GA or other (fraction of time)	-0.019****	-0.019****	-0.015***	-0.015***
2. Received Food Stamps (fraction of time)	-0.036*	-0.049***	-0.024	-0.024
3. Lived in public housing (fraction of time)	-0.009	-0.019	-0.018	-0.019

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VI.12

ESTIMATES OF JOB CORPS IMPACTS ON RECEIPT OF OTHER PUBLIC TRANSFERS, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Received Unemployment Insurance (fraction of time)	-0.025****	-0.025****	-0.019****	-0.018***
2. Received Workers' Compensation (fraction of time)	0.00009	0.005	0.006	0.006*
3. Training allowances per six months in current dollars	2.36	-10.26	-17.23	-12.87
B. FEMALES WITHOUT CHILDREN				
1. Received Unemployment Insurance (fraction of time)	-0.009*	-0.010*	-0.006	0.002
2. Received Workers' Compensation (fraction of time)	-0.002	-0.002	-0.001	0.0002
3. Training allowances per six months in current dollars	15.18	19.77*	4.01	7.71
C. FEMALES WITH CHILDREN				
1. Received Unemployment Insurance (fraction of time)	0.012	0.007	0.017*	0.013*
2. Received Workers' Compensation (fraction of time)	-0.0002	-0.7002	0.0004	-0.0003
3. Training allowances per six months in current dollars	6.92	9.57	-1.18	10.93
D. OVERALL				
1. Received Unemployment Insurance (fraction of time)	-0.020****	-0.020****	-0.014***	-0.011***
2. Received Workers' Compensation (fraction of time)	-0.001	0.003	0.004	0.004
3. Training allowances per six months in current dollars	6.04	-1.66	-11.28	-6.41

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VI.13

ESTIMATES OF JOB CORPS IMPACTS ON RECEIPT OF OTHER PUBLIC TRANSFERS, EXCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Received Unemployment Insurance (fraction of time)	-0.018***	-0.020***	-0.013**	-0.026****
2. Received Workers' Compensation (fraction of time)	0.007*	0.011***	0.008**	0.010***
3. Training allowances per six months in current dollars	-9.61	-13.59	-13.85	-30.00**
B. FEMALES WITHOUT CHILDREN				
1. Received Unemployment Insurance (fraction of time)	-0.001	-0.017***	-0.012	-0.012
2. Received Workers' Compensation (fraction of time)	-0.002	-0.002	-0.002	0.0001
3. Training allowances per six months in current dollars	41.10***	79.23****	100.94****	68.69****
C. FEMALES WITH CHILDREN				
1. Received Unemployment Insurance (fraction of time)	0.008	0.004	0.003	0.005
2. Received Workers' Compensation (fraction of time)	-0.001	-0.001	-0.0001	-0.001
3. Training allowances per six months in current dollars	12.69*	20.80	4.18	6.36
D. OVERALL				
1. Received Unemployment Insurance (fraction of time)	-0.012***	-0.017****	-0.010**	-0.019****
2. Received Workers' Compensation (fraction of time)	0.004*	0.007***	0.006*	0.007**
3. Training allowances per six months in current dollars	2.76	7.25	6.07	-9.74

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

VII. IMPACT OF JOB CORPS ON CRIMINALITY

The increased legitimate opportunities for former Corpsmembers to be employed and to obtain higher earnings is expected to increase their incentives not to engage in illegal activities. As the employability of Corpsmembers becomes greater, legitimate activities should become increasingly more attractive relative to illegal activities. Thus, we expected to find reduced criminality--fewer crimes, fewer arrests, fewer convictions, and less time in jail--among Corpsmembers during the postprogram time period. In addition, a very substantial drop in criminal behavior was observed among Corpsmembers while they were in Job Corps (see Mallar et al., 1978), and this in-program effect was expected to carry over into the postprogram time period (in addition to the employment-related effects from vocational training and education services, the general counseling and center living in the Job Corps program are expected to promote more regular life-styles and, hence, reduce the criminality of Corpsmembers).

The postprogram findings for the effects of Job Corps on criminality are erratic and difficult to summarize; therefore, we present only the detailed tables in this chapter and briefly discuss their implications.[1/] An aggregation over the entire postprogram observation

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[1/] The detailed tables include (1) separate breakdowns for civilians (Tables VII.1 through VII.6), and with military observations included (Tables VII.7 through VII.12); (2) background information on what Corpsmembers' criminal behavior would have been during the postprogram period in the absence of their participation in Job Corps (Tables VII.1, VII.2, VII.7, and VII.8); (3) estimated effects of Job Corps on aggregate measures of criminality (Tables VII.3, VII.4, VII.9, and VII.10); and (4) estimated effects of Job Corps on the number of arrests by arrest charge for civilians (Tables VII.5 and VII.6), and with military observations included (Tables VII.11 and VII.12).

period shows that the total estimated effect of Job Corps on arrests or being in jail is approximately zero--with estimated reductions in arrests in the first and third postprogram years being offset almost exactly by estimated increases in the second and fourth postprogram years. However, we find a substantial estimated shift from more to less serious categories of arrest charges for Corpsmembers--fewer arrests for murder, robbery, and larceny, which are offset by more arrests for burglary, other personal crimes (minor personal crimes, such as threats), and other miscellaneous crimes (primarily for traffic offenses, drunkenness, and disturbing the peace, in that order). For the most part, the crime estimates show a reduction in thefts, as expected, but offsetting increases elsewhere that relate primarily to traffic offenses.

The estimates of what Corpsmembers would have done during the postprogram period in the absence of Job Corps (see Tables VII.1, VII.2, VII.7, and VII.8) show the familiar pattern for severely disadvantaged youths--very high crime rates that decline as the youths become older. We obtained estimates of sample means beginning at 6 reported arrests per 100 youths in the first six-month period and declining to 4 reported arrests per 100 youths in the fourth six-month period.

The estimated effects of Job Corps on aggregate measures of crime are presented in Tables VII.3 and VII.4 for civilians and in Tables VII.9 and VII.10 when military observations are included. These aggregate measures show (1) no overall reductions in arrests, (2) a reduction in theft arrests, and (3) no effects for being in jail. The pattern of Job Corps effects on arrests over time shows a decrease in the first postprogram year (inexplicably smaller than we have found before), an increase in the second postprogram year, a decrease in the third

postprogram year, and an increase in the fourth postprogram--which, altogether, add up to a zero effect on total postprogram arrests.

Estimates based on disaggregations of reported arrests by major categories of arrest charges (with the most serious charge being used for cases of multiple arrest charges) show a substantial shift from more to less serious crimes for Corpsmembers. There are estimated effects of fewer arrests for murder, robbery, and larceny, and more arrests for burglary, other personal crimes, and other miscellaneous crimes. In fact, the estimated increases in crimes appear to relate primarily to traffic offenses in the other-miscellaneous category. Overall, there is a significant reduction in thefts, which was expected. However, we did not anticipate the offsetting increase in arrests for traffic offenses, which, speculatively, may be due to the fact that Corpsmembers are driving more because of their increased earnings and because they received drivers training at Job Corps centers (intended to increase their job mobility).

Thus, these disaggregated effects show substantial benefits for society (see further in Chapter VIII), because crimes of murder, robbery, and larceny are much more costly to society than are traffic offenses. Together with the extremely large crime reductions during the in-program period, this shift from more to less serious crime leads to a substantial benefit to society in the benefit-cost analysis.

Finally, in previous reports we found small but statistically significant reductions in the use of drug/alcohol treatment programs among Corpsmembers during the early postprogram period. In the subsequent postprogram period for the third follow-up survey (our last two-and-one-half years of observation) the incidence of drug/alcohol treatment was so low among our observed youths that the effects could not reasonably be estimated.

TABLE VII.1

ESTIMATES OF CORPSMEMBERS' CRIMINALITY HAD THEY NOT PARTICIPATED IN JOB CORPS, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Means 0 to 6 Months After Termination	Job Corps Means 6 to 12 Months After Termination	Job Corps Means 12 to 18 Months After Termination	Job Corps Means 18 to 24 Months After Termination
A. MALES				
1. Total number of arrests per six months	0.083	0.077	0.071	0.063
2. Number of theft arrests per six months	0.039	0.039	0.034	0.033
3. Probability in jail during survey week	0.031		0.016	
4. Fraction of time in jail if out of jail during survey week	0.028	0.028	0.025	0.027
B. FEMALES WITHOUT CHILDREN				
1. Total number of arrests per six months	0.020	0.023	0.021	0.022
2. Number of theft arrests per six months	0.000	0.000	0.000	0.000
3. Probability in jail during survey week	0.0		0.0	
4. Fraction of time in jail if out of jail during survey week	0.003	0.001	0.000	0.000
C. FEMALES WITH CHILDREN				
1. Total number of arrests per six months	0.021	0.021	0.018	0.021
2. Number of theft arrests per six months	0.022	0.020	0.018	0.019
3. Probability in jail during survey week	0.0		0.0	
4. Fraction of time in jail if out of jail during survey week	0.014	0.007	0.002	0.002
D. OVERALL				
1. Total number of arrests per six months	0.064	0.060	0.056	0.051
2. Number of theft arrests per six months	0.027	0.028	0.025	0.025
3. Probability in jail during survey week	0.022		0.011	
4. Fraction of time in jail if out of jail during survey week	0.021	0.020	0.017	0.019

TABLE VII.2

ESTIMATES OF CORPSMEMBERS' CRIMINALITY HAD THEY NOT PARTICIPATED IN JOB CORPS, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Means 24 to 30 Months After Termination	Job Corps Means 30 to 36 Months After Termination	Job Corps Means 36 to 42 Months After Termination	Job Corps Means 42 to 48 Months After Termination
A. MALES				
1. Total number of arrests per six months	0.057	0.053	0.052	0.046
2. Number of theft arrests per six months	0.030	0.029	0.028	0.026
3. Probability in jail during survey week				0.048
4. Fraction of time in jail if out of jail during survey week	0.028	0.021	0.019	0.020
B. FEMALES WITHOUT CHILDREN				
1. Total number of arrests per six months	0.022	0.022	0.021	0.022
2. Number of theft arrests per six months	0.000	0.000	0.001	0.0002
3. Probability in jail during survey week				0.0
4. Fraction of time in jail if out of jail during survey week	0.000	0.000	0.000	0.000
C. FEMALES WITH CHILDREN				
1. Total number of arrests per six months	0.019	0.021	0.018	0.022
2. Number of theft arrests per six months	0.018	0.019	0.017	0.019
3. Probability in jail during survey week				0.0
4. Fraction of time in jail if out of jail during survey week	0.003	0.002	-0.001	-0.001
D. OVERALL				
1. Total number of arrests per six months	0.046	0.044	0.042	0.039
2. Number of theft arrests per six months	0.023	0.023	0.022	0.021
3. Probability in jail during survey week				0.033
4. Fraction of time in jail if out of jail during survey week	0.019	0.014	0.012	0.013

TABLE VII.3

ESTIMATES OF JOB CORPS IMPACTS ON OVERALL CRIMINALITY, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Total number of arrests per six months	-0.009	-0.006	0.008	0.015
2. Number of theft arrests per six months	-0.003	-0.005	-0.004	-0.012
3. Probability in jail during survey week	-0.002		0.027	
4. Fraction of time in jail if out of jail during survey week	-0.006	0.006	-0.004	-0.007
B. FEMALES WITHOUT CHILDREN				
1. Total number of arrests per six months	-0.014*	-0.019***	-0.009	0.002
2. Number of theft arrests per six months	0.005	0.003	0.005	0.002
3. Probability in jail during survey week	0.005		0.0	
4. Fraction of time in jail if out of jail during survey week	0.005***	0.008****	0.005**	0.003
C. FEMALES WITH CHILDREN				
1. Total number of arrests per six months	0.041****	-0.013	-0.016*	-0.002
2. Number of theft arrests per six months	0.040****	-0.016	-0.016**	-0.0002
3. Probability in jail during survey week	0.0		0.0	
4. Fraction of time in jail if out of jail during survey week	-0.014****	-0.007****	0.012****	0.014****
D. OVERALL				
1. Total number of arrests per six months	-0.009	-0.010**	0.002	0.011
2. Number of theft arrests per six months	0.0001	-0.003	-0.003	-0.006
3. Probability in jail during survey week	-0.0003		0.019	
4. Fraction of time in jail if out of jail during survey week	-0.003	0.006	-0.0007	-0.003

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

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TABLE VII.4

ESTIMATES OF JOB CORPS IMPACTS ON OVERALL CRIMINALITY, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Total number of arrests per six months	-0.007	0.007	0.006	0.030*
2. Number of theft arrests per six months	-0.017*	-0.010	-0.010	0.002*
3. Probability in jail during survey week				0.014
4. Fraction of time in jail if out of jail during survey week	-0.016*	-0.005	0.002	-0.012
B. FEMALES WITHOUT CHILDREN				
1. Total number of arrests per six months	-0.020**	-0.019**	-0.021**	-0.010
2. Number of theft arrests per six months	0.0001	0.0004	-0.001	-0.0002
3. Probability in jail during survey week				0.0
4. Fraction of time in jail if out of jail during survey week	0.004*	0.004*	0.005*	0.005*
C. FEMALES WITH CHILDREN				
1. Total number of arrests per six months	-0.019**	-0.021**	-0.015*	-0.013
2. Number of theft arrests per six months	-0.019***	-0.019***	-0.014**	-0.014**
3. Probability in jail during survey week				0.0
4. Fraction of time in jail if out of jail during survey week	0.003**	-0.002	0.001	0.001
D. OVERALL				
1. Total number of arrests per six months	-0.011**	-0.001	-0.001	0.018
2. Number of theft arrests per six months	-0.014***	-0.009	-0.009	-0.0007
3. Probability in jail during survey week				0.010
4. Fraction of time in jail if out of jail during survey week	-0.010*	-0.003	0.002	-0.007

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VII.5

ESTIMATES OF JOB CORPS IMPACTS ON NUMBER OF ARRESTS BY ARREST CHARGE, FOR CIVILIANS: FIRST AND SECOND POSTPROGRAM YEARS

Arrest Charge for Number of Arrests per Six Months	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Murder	-0.002**	-0.002	-0.001	-0.001
2. Felonious assault	-0.001	0.004	0.001	0.011**
3. Robbery	-0.006**	-0.003	-0.005*	-0.005*
4. Burglary	0.008	0.004	0.002	0.001
5. Larceny and motor vehicle theft	-0.008	-0.011	-0.006	-0.014*
6. Drug law violation	-0.003	-0.0003	0.001	0.005
7. Other personal crimes	0.001	-0.003	0.001	0.006
8. Other miscellaneous crimes	0.007	0.001	0.014	0.009
9. Unspecified crimes	-0.004***	-0.004***	-0.004***	-0.003**
B. FEMALES WITHOUT CHILDREN				
1. Murder	0.0	0.0	0.0	0.0
2. Felonious assault	-0.003*	-0.003*	-0.002	-0.003
3. Robbery	0.0	0.0	0.0	0.0
4. Burglary	-0.001	-0.0004	-0.001	-0.001
5. Larceny and motor vehicle theft	0.006*	0.004	0.006*	0.002
6. Drug law violation	-0.0003	-0.001	-0.001	-0.001
7. Other personal crimes	-0.002*	-0.002	-0.001	0.001
8. Other miscellaneous crimes	-0.014***	-0.016***	-0.011*	0.003
9. Unspecified crimes	0.0004	0.0001	0.0003	-0.00002
C. FEMALES WITH CHILDREN				
1. Murder	0.0	0.0	0.0	0.0
2. Felonious assault	0.0	0.0	0.0	0.0
3. Robbery	0.0	0.0	0.0	0.0
4. Burglary	0.0	0.0	0.0	0.0
5. Larceny and motor vehicle theft	0.040****	-0.016	-0.016**	-0.0002
6. Drug law violation	-0.001	-0.0003	-0.0001	0.0001
7. Other personal crimes	0.0	0.0	0.0	0.0
8. Other miscellaneous crimes	0.002	0.004	0.001	-0.003
9. Unspecified crimes	-0.001	-0.001	-0.001	-0.001
D. OVERALL				
1. Murder	-0.001**	-0.001*	-0.001	-0.001
2. Felonious assault	-0.002	0.002	0.0003	0.007
3. Robbery	-0.004**	-0.002	-0.003*	-0.004*
4. Burglary	0.005*	0.003	0.001	0.001
5. Larceny and motor vehicle theft	-0.003	-0.007	-0.004	-0.009*
6. Drug law violation	-0.002	-0.0003	0.001	0.003
7. Other personal crimes	-0.0002	-0.003	0.001	0.004
8. Other miscellaneous crimes	0.001	-0.003	0.009	0.007
9. Unspecified crimes	-0.003**	-0.003***	-0.003***	-0.002***

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VII.6

ESTIMATES OF JOB CORPS IMPACTS ON NUMBER OF ARRESTS BY ARREST CHARGE, FOR CIVILIANS: THIRD AND FOURTH POSTPROGRAM YEARS

Arrest Charge for Number of Arrests per Six Months	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Murder	-0.001	-0.001	-0.001	-0.001
2. Felonious assault	0.002	-0.003	-0.004	-0.003
3. Robbery	0.001	-0.004	-0.004	0.002
4. Burglary	-0.002	0.004	0.009*	0.002
5. Larceny and motor vehicle theft	-0.021***	-0.016**	-0.021***	-0.009
6. Drug law violation	-0.001	0.011**	0.009*	0.005
7. Other personal crimes	0.001	0.003	0.002	0.001
8. Other miscellaneous crimes	0.012	0.009	0.010	0.028*
9. Unspecified crimes	-0.003**	-0.003*	-0.001	-0.002
B. FEMALES WITHOUT CHILDREN				
1. Murder	0.0	0.0	0.0	0.0
2. Felonious assault	-0.001	-0.003	-0.003	-0.002
3. Robbery	0.0	0.0	0.0	0.0
4. Burglary	-0.001	-0.001	-0.0004	-0.0004
5. Larceny and motor vehicle theft	0.001	0.001	-0.0002	0.0002
6. Drug law violation	-0.002	-0.002	-0.002	-0.002
7. Other personal crimes	-0.001	-0.001	-0.001	-0.0001
8. Other miscellaneous crimes	-0.018***	-0.015**	-0.016*	-0.006
9. Unspecified crimes	0.0001	-0.0001	0.0001	-0.0001
C. FEMALES WITH CHILDREN				
1. Murder	0.0	0.0	0.0	0.0
2. Felonious assault	0.0	0.0	0.0	0.0
3. Robbery	0.0	0.0	0.0	0.0
4. Burglary	0.0	0.0	0.0	0.0
5. Larceny and motor vehicle theft	-0.018***	-0.019***	-0.014**	-0.014**
6. Drug law violation	-0.00002	0.0003	0.0003	0.001
7. Other personal crimes	0.0	0.0	0.0	0.0
8. Other miscellaneous crimes	0.00002	-0.001	0.0001	0.002
9. Unspecified crimes	-0.002	-0.002	-0.002	-0.002**
D. OVERALL				
1. Murder	-0.001	-0.001	-0.001	-0.001
2. Felonious assault	0.001	-0.003	-0.003	-0.002
3. Robbery	0.001	-0.003	-0.003	0.001
4. Burglary	-0.002	0.003	0.006**	0.001
5. Larceny and motor vehicle theft	-0.016***	-0.013**	-0.017***	-0.008
6. Drug law violation	-0.001	0.007**	0.006*	0.003
7. Other personal crimes	0.001	0.002	0.001	0.0003
8. Other miscellaneous crimes	0.007	0.004	0.005	0.019*
9. Unspecified crimes	-0.002**	-0.003**	-0.001	-0.002*

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VII.7

ESTIMATES OF PARTICIPANTS' CRIMINALITY HAD THEY NOT PARTICIPATED IN JOB CORPS, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Variable	Job Corps Means 0 to 6 Months After Termination	Job Corps Means 6 to 12 Months After Termination	Job Corps Means 12 to 18 Months After Termination	Job Corps Means 18 to 24 Months After Termination
A. MALES				
1. Total number of arrests per six months	0.076	0.070	0.065	0.058
2. Number of theft arrests per six months	0.036	0.036	0.032	0.031
3. Probability in jail during survey week	0.031		0.016	
4. Fraction of time in jail if out of jail during survey week	0.026	0.026	0.023	0.026
B. FEMALES WITHOUT CHILDREN				
1. Total number of arrests per six months	0.020	0.022	0.021	0.021
2. Number of theft arrests per six months	0.000	0.000	0.000	0.000
3. Probability in jail during survey week	0.0		0.0	
4. Fraction of time in jail if out of jail during survey week	0.003	0.001	0.000	0.000
C. FEMALES WITH CHILDREN				
1. Total number of arrests per six months	0.021	0.021	0.018	0.020
2. Number of theft arrests per six months	0.022	0.019	0.018	0.019
3. Probability in jail during survey week	0.0		0.0	
4. Fraction of time in jail if out of jail during survey week	0.014	0.007	0.002	0.002
D. OVERALL				
1. Total number of arrests per six months	0.059	0.056	0.051	0.047
2. Number of theft arrests per six months	0.025	0.026	0.024	0.023
3. Probability in jail during survey week	0.022		0.011	
4. Fraction of time in jail if out of jail during survey week	0.019	0.019	0.016	0.017

TABLE VII.8

ESTIMATES OF CORPSMEMBERS' CRIMINALITY HAD THEY NOT PARTICIPATED IN JOB CORPS, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Means 24 to 30 Months After Termination	Job Corps Means 30 to 36 Months After Termination	Job Corps Means 36 to 42 Months After Termination	Job Corps Means 42 to 48 Months After Termination
A. MALES				
1. Total number of arrests per six months	0.053	0.049	0.049	0.044
2. Number of theft arrests per six months	0.028	0.027	0.017	0.025
3. Probability in jail during survey week				0.048
4. Fraction of time in jail if out of jail during survey week	0.026	0.019	0.018	0.019
B. FEMALES WITHOUT CHILDREN				
1. Total number of arrests per six months	0.021	0.021	0.021	0.021
2. Number of theft arrests per six months	0.000	0.000	0.0004	0.0001
3. Probability in jail during survey week				0.0
4. Fraction of time in jail if out of jail during survey week	-0.003	-0.004	-0.004	-0.005
C. FEMALES WITH CHILDREN				
1. Total number of arrests per six months	0.019	0.021	0.019	0.022
2. Number of theft arrests per six months	0.018	0.019	0.018	0.019
3. Probability in jail during survey week				0.0
4. Fraction of time in jail if out of jail during survey week	0.003	0.002	0.000	0.000
D. OVERALL				
1. Total number of arrests per six months	0.043	0.041	0.040	0.037
2. Number of theft arrests per six months	0.021	0.021	0.015	0.021
3. Probability in jail during survey week				0.033
4. Fraction of time in jail if out of jail during survey week	0.018	0.013	0.012	0.012

TABLE VII.9

ESTIMATES OF JOB CORPS IMPACTS ON OVERALL CRIMINALITY, INCLUDING MILITARY SECTOR: FIRST AND SECOND POST-PROGRAM YEARS

Variable	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Total number of arrests per six months	-0.003	-0.004	0.009	0.016
2. Number of theft arrests per six months	-0.0002	-0.004	-0.004	-0.011
3. Probability in jail during survey week	-0.002		0.027	
4. Fraction of time in jail if out of jail during survey week	-0.004	0.006	-0.004	-0.006
B. FEMALES WITHOUT CHILDREN				
1. Total number of arrests per six months	-0.018**	-0.018***	-0.009	0.002
2. Number of theft arrests per six months	0.005	0.003	0.005	0.002
3. Probability in jail during survey week	0.005		0.0	
4. Fraction of time in jail if out of jail during survey week	0.005***	0.008****	0.005**	0.003
C. FEMALES WITH CHILDREN				
1. Total number of arrests per six months	0.041****	-0.013	-0.016*	-0.002
2. Number of theft arrests per six months	0.040****	-0.016	-0.016**	-0.0002
3. Probability in jail during survey week	0.0		0.0	
4. Fraction of time in jail if out of jail during survey week	-0.014****	-0.007****	0.012****	0.014****
D. OVERALL				
1. Total number of arrests per six months	-0.005	-0.008	0.003	0.012
2. Number of theft arrests per six months	0.002	-0.002	-0.003	-0.007
3. Probability in jail during survey week	-0.0003		0.019	
4. Fraction of time in jail if out of jail during survey week	-0.002	0.006	-0.001	-0.002

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VII.10

ESTIMATES OF JOB CORPS IMPACTS ON OVERALL CRIMINALITY, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Variable	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Total number of arrests per six months	-0.006	0.006	0.004	0.026
2. Number of theft arrests per six months	-0.016*	-0.010	-0.010	0.0003
3. Probability in jail during survey week				0.014
4. Fraction of time in jail if out of jail during survey week	-0.015*	-0.004	0.001	-0.011
B. FEMALES WITHOUT CHILDREN				
1. Total number of arrests per six months	-0.019**	-0.019**	-0.021**	-0.011
2. Number of theft arrests per six months	0.0003	0.001	-0.0004	-0.00007
3. Probability in jail during survey week				0.0
4. Fraction of time in jail if out of jail during survey week	0.004*	0.004*	0.004*	0.005*
C. FEMALES WITH CHILDREN				
1. Total number of arrests per six months	-0.0[illegible]**	-0.021**	-0.015*	-0.013
2. Number of theft arrests per six months	-0.019***	-0.019***	-0.014**	-0.014**
3. Probability in jail during survey week				0.0
4. Fraction of time in jail if out of jail during survey week	0.003**	-0.002	0.001	0.001
D. OVERALL				
1. Total number of arrests per six months	-0.010	-0.002	-0.003	0.014
2. Number of theft arrests per six months	-0.013**	-0.009	-0.009*	-0.002
3. Probability in jail during survey week				0.010
4. Fraction of time in jail if out of jail during survey week	-0.009*	-0.003	0.002	-0.007

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VII.11

ESTIMATES OF JOB CORPS IMPACTS ON NUMBER OF ARRESTS BY ARREST CHARGE, INCLUDING MILITARY SECTOR: FIRST AND SECOND POSTPROGRAM YEARS

Arrest Charge for Number of Arrests per Six Months	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES				
1. Murder	-0.002**	-0.002*	-0.001	-0.001
2. Felonious Assault	-0.001	0.004	0.001	0.010**
3. Robbery	-0.006***	-0.003	-0.004*	-0.005*
4. Burglary	0.008*	0.004	0.002	0.001
5. Larceny and motor vehicle theft	-0.006	-0.010	-0.006	-0.012*
6. Drug law violation	-0.003	-0.0004	0.001	0.005
7. Other personal crimes	0.001	-0.003	0.001	0.006
8. Other miscellaneous crimes	0.009	0.002	0.015	0.010
9. Unspecified crimes	-0.004****	-0.004****	-0.003***	-0.003**
B. FEMALES WITHOUT CHILDREN				
1. Murder	0.0	0.0	0.0	0.0
2. Felonious Assault	-0.003*	-0.003*	-0.002	-0.003
3. Robbery	0.0	0.0	0.0	0.0
4. Burglary	-0.001	-0.0004	-0.001	-0.001
5. Larceny and motor vehicle theft	0.006*	0.004	0.006*	0.002
6. Drug law violation	-0.0003	-0.0009	-0.001	-0.001
7. Other personal crimes	-0.002*	-0.002	-0.001	0.001
8. Other miscellaneous crimes	-0.014****	-0.016****	-0.011*	0.003
9. Unspecified crimes	0.0003	0.00004	0.0002	-0.00002
C. FEMALES WITH CHILDREN				
1. Murder	0.0	0.0	0.0	0.0
2. Felonious Assault	0.0	0.0	0.0	0.0
3. Robbery	0.0	0.0	0.0	0.0
4. Burglary	0.0	0.0	0.0	0.0
5. Larceny and motor vehicle theft	0.040****	-0.016	-0.016**	-0.0002
6. Drug law violation	-0.001	-0.0003	-0.0001	0.0001
7. Other personal crimes	0.0	0.0	0.0	0.0
8. Other miscellaneous crimes	0.002	0.004	0.001	-0.0003
9. Unspecified crimes	-0.001	-0.001	-0.001	-0.001
D. OVERALL				
1. Murder	-0.001**	-0.001*	-0.001	-0.001
2. Felonious Assault	-0.002	0.002	0.0002	0.007
3. Robbery	-0.004**	-0.002	-0.003*	-0.003*
4. Burglary	0.006*	0.002	0.001	0.001
5. Larceny and motor vehicle theft	-0.002	-0.007	-0.004	-0.008*
6. Drug law violation	-0.002	-0.001	0.0003	0.003
7. Other personal crimes	0.00003	-0.002	0.00004	0.004
8. Other miscellaneous crimes	0.002	-0.003	0.008	0.008
9. Unspecified crimes	-0.002****	-0.003**	-0.002***	-0.002

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

TABLE VII.12

ESTIMATES OF JOB CORPS IMPACTS ON NUMBER OF ARRESTS BY ARREST CHARGE, INCLUDING MILITARY SECTOR: THIRD AND FOURTH POSTPROGRAM YEARS

Arrest Charge for Number of Arrests per Six Months	Job Corps Effects 24 to 30 Months After Termination	Job Corps Effects 30 to 36 Months After Termination	Job Corps Effects 36 to 42 Months After Termination	Job Corps Effects 42 to 48 Months After Termination
A. MALES				
1. Murder	-0.001	-0.001	-0.001	-0.001
2. Felonious Assault	0.002	-0.002	-0.004	-0.003
3. Robbery	0.001	-0.004	-0.004	0.001
4. Burglary	-0.002	0.003	0.008*	0.002
5. Larceny and motor vehicle theft	-0.020***	-0.015**	-0.020***	-0.009
6. Drug law violation	-0.001	0.010**	0.006*	0.005
7. Other personal crimes	0.001	0.003	0.001	0.0003
8. Other miscellaneous crimes	0.012	0.008	0.009	0.026*
9. Unspecified crimes	-0.003**	-0.002*	-0.001	-0.002
B. FEMALES WITHOUT CHILDREN				
1. Murder	0.0	0.0	0.0	0.0
2. Felonious Assault	-0.001	-0.003	-0.002	-0.002
3. Robbery	0.0	0.0	0.0	0.0
4. Burglary	-0.001	-0.001	-0.0004	-0.0003
5. Larceny and motor vehicle theft	0.001	0.001	-0.00008	0.0003
6. Drug law violation	-0.002	-0.002	-0.002	-0.002
7. Other personal crimes	-0.001	-0.001	-0.001	-0.0001
8. Other miscellaneous crimes	-0.017***	-0.014**	-0.016*	-0.007
9. Unspecified crimes	0.0001	-0.0001	0.00008	-0.00007
C. FEMALES WITH CHILDREN				
1. Murder	0.0	0.0	0.0	0.0
2. Felonious Assault	0.0	0.0	0.0	0.0
3. Robbery	0.0	0.0	0.0	0.0
4. Burglary	0.0	0.0	0.0	0.0
5. Larceny and motor vehicle theft	-0.018***	-0.019***	-0.014**	-0.018**
6. Drug law violation	0.00002	0.0003	0.0003	0.001
7. Other personal crimes	0.0	0.0	0.0	0.0
8. Other miscellaneous crimes	0.00002	-0.001	0.0001	0.002
9. Unspecified crimes	-0.002	-0.002	-0.002	-0.002*
D. OVERALL				
1. Murder	-0.001	-0.001	-0.001	-0.001
2. Felonious Assault	0.001	-0.002	-0.003	-0.002
3. Robbery	0.001	-0.003	-0.003	0.001
4. Burglary	-0.001	0.002	0.006*	0.001
5. Larceny and motor vehicle theft	-0.015***	-0.013***	-0.016***	-0.008
6. Drug law violation	-0.001	0.007**	0.006*	0.003
7. Other personal crimes	0.0003	0.002	0.001	0.0002
8. Other miscellaneous crimes	0.005	0.003	0.004	0.018*
9. Unspecified crimes	-0.002**	-0.002*	-0.001	-0.002*

* Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
** Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
*** Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
**** Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

VIII. COMPARATIVE EVALUATION OF BENEFITS AND COSTS

Benefit-cost analysis attempts to resolve a problem that faces policy analysts in evaluating public programs--that is, how to systematically compare the wide range of program effects with the costs of the various resources necessary to operate a program. With respect to Job Corps, the difficulty of this problem is illustrated by the diverse set of effects measured in the evaluation: employment and earnings, the use of alternative and supplemental training and education programs, dependence on welfare and other transfer programs, and anti-social behavior (criminal activity and drug/alcohol abuse). In addition to these program effects, Job Corp uses a wide array of resources, including many different types of personnel, materials, land, buildings, vehicles, and supplies.

In this chapter we use a benefit-cost analysis to organize our findings on the various effects and costs of the Job Corps program in order to facilitate using these findings to assess and formulate public policy.[1/] Our discussion begins with an overview of the benefit-cost methodology used and then examines each of the benefit and cost components separately. These components are then aggregated and the overall results are examined, particularly with respect to the sensitivity of the overall findings to changes in the most speculative of the valuation assumptions and

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[1/] This chapter summarizes the benefit-cost techniques that were used and their results. The basic methodology used is the same as in our two earlier benefit-cost evaluations of Job Corps and is presented in more detail in Technical Report Q, "A Comparative Evaluation of the Benefits and Costs of Job Corps after Forty-Eight Months of Postprogram Observation."

estimates. The chapter concludes with a summary of the findings from our benefit-cost analysis.

The overall conclusion is that Job Corps is a worthwhile public investment. Our benchmark estimate (based on the assumptions and estimates with which we are most comfortable) is that measured benefits to society exceed costs by over $2,300 per Corpsmember in 1977 dollars, or, equivalently, by approximately 45 percent.[1/] Furthermore, the overall finding of positive net social benefits is obtained under a wide range of alternative assumptions and estimates, and the additional follow-up data allow more confidence to be placed in this overall finding than previously. With the longer postprogram observation period, we rely less on relatively imprecise extrapolation procedures, and, in fact, we estimate that social benefits exceed costs during the time frame now covered by the interviews.

The information obtained from organizing all of the estimated program effects into a systematic comparison of the benefits and costs is much more powerful than can be summarized by a few aggregate numbers on the estimated net economic benefits to society. We have been careful in this chapter to provide adequate detail to ensure that informed policymakers can form their own judgments and value the estimated effects in alternative ways.

A. BENEFIT-COST METHODOLOGY

Benefit-cost analysis attempts to provide an appropriate framework within which program effects and their costs can be compared. The usual

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[1/] To obtain rough estimates of the 1982 dollar values, the numbers in this chapter can be multiplied by 1.457, reflecting the estimated increase in the GNP price deflator between 1977 and 1982. This implies that the difference in value between social benefits and costs is approximately $3,350 per Corpsmember in 1982 dollars.

approach entails estimating constant dollar values (dollar values denominated at a particular point in time, so as not to be biased by inflation) for each benefit and cost and then aggregating those values through standard accounting procedures. By measuring the benefits and costs of a program in common units such as constant dollars, the worth of a program can readily be assessed for measured effects.

An appropriate procedure for comparing benefits and costs is to calculate the program's "net present value"--a term that refers to the difference between total benefits and total costs when dollar values accruing in different time periods have been adjusted to "present-value" units for a base time period.[1/] To control partially for program size, we divide all dollar values by the number of Corpsmembers, so that all figures reflect benefits or costs per Corpsmember. Thus, the resulting criterion used to judge the program is whether the program's net present value per Corpsmember is greater than zero. If it is, the program is judged to be worthwhile from the perspective of measured economic efficiency; otherwise, the program is judged to be undesirable unless the value of unmeasured benefits exceeds the value of unmeasured costs by at least the measured shortfall.

While the net present value criterion is easy to state, a high degree of uncertainty often surrounds its estimation, making it difficult to apply. Sometimes, in fact, equally plausible estimates of a program's net present value can lie on opposite sides of zero, making it

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[1/] In discounting to present-value units, we adjust the value of estimated benefits or costs that accrue in different time periods to reflect their worth in the base time period (see Gramlich, 1981, for a detailed discussion of discounting and other benefit-cost principles). The specific discounting assumptions that we use are outlined below in Section B.7 of this chapter.

impossible to apply the net present value rule directly and, thus, impossible to use this rule in a simple way to judge the worth of a program.

This uncertainty stems from four sources: the estimates of program effects, the estimates of the "shadow prices" used to value these effects,[1/] unmeasured effects, and the accounting assumptions. Of these four areas, the uncertainty surrounding the estimated effects is best understood, because the estimated effects are obtained from statistical computations that also provide estimates of their error variances. Less is known about the accuracy of the shadow prices. These prices are generally estimated on the basis of published data for which measures of error or uncertainty are often unavailable. With unmeasured effects we can only document the existence and expected direction of bias from omitting known effects (see further below). Finally, many of the accounting assumptions used in the analysis approximate reality, with the magnitude of the approximation error often being impossible to determine.

Because of the error associated with any single estimate of net present value, much of the usefulness of benefit-cost analysis is due to its comprehensiveness in drawing together measures of the various program effects and costs. The general patterns that emerge from the attempts to assign relative values are often more useful than any specific estimate of net present value. For that reason, the analysis in this chapter does not focus on a single net present value estimate but, rather, on a set of

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[1/] The term shadow price is used to refer to the estimated value per unit of effects.

estimates. This set includes (1) a benchmark estimate, incorporating the assumptions and estimates with which we feel most comfortable, and (2) several estimates based on sensitivity tests, each illustrating the effect of changing one or more of the assumptions used in the benchmark calculations while holding all others constant. When equally plausible estimates are available, we adopt a conservative convention for our benchmark estimate--that is, we use the value that yields lower estimated benefits.

The conclusions of our benefit-cost analysis are based on all these estimates. Thus, they do not rely on a single set of uncertain assumptions and estimates but, rather, on a range of possible assumptions and estimates. By examining the different assumptions, the underlying outcome estimates, and the techniques used to value outcomes, we believe that reasonable judgments can be made about the relative value of a program's benefits and costs.

Most benefit-cost evaluations adopt the perspective of society as a whole. They focus on "economic efficiency"--that is, on a program's effect on the total value of the goods and services available to society. Is the value of those goods and services greater as a result of the program under study, or would the value have been greater had the resources used for the program been devoted to alternative use ? In essence, this analytical focus assumes that a dollar of benefit or cost to one person is equal to a dollar of benefit or cost to another person.

While an evaluation from the perspective of society as a whole is most useful, it is clear that all groups in society do not share equally in the benefits and costs of a program, and that distributional effects can be important and, indeed, are often part of a program's objectives. Thus, it

is useful to consider the effect of a program on the distribution of resources, as well as its effect on the total amount of resources. For example, one objective of Job Corps is to transfer resources to Corpsmembers.

In order to address both the economic efficiency of the Job Corps program and its important distributional consequences, we will compute estimates for three key perspectives: society as a whole, Corpsmembers, and non-Corpsmembers (general taxpayers).[1/] One analytically useful feature of using these three perspectives is that the sum of the net present values calculated from the Corpsmember and non-Corpsmember perspectives are computed so as to equal the social net present value. This adding-up property pertains because Corpsmembers and non-Corpsmembers constitute mutually exclusive groups that, when combined, include all members of society (i.e., mutually exclusive and exhaustive). Therefore, transfers of income between these two groups (e.g., reduced welfare transfers or Job Corps stipends) cancel out from the social perspective, because the benefit to one group is equal to the cost to the other.[2/] Benefits or costs that accrue to one group and that are not offset by corresponding costs or benefits to the other group (e.g., increased work output) do not cancel out from the social perspective and cause a change in the amount of resources available to society.

[1/] The term non-Corpsmember is used consistently throughout this benefit-cost discussion to refer to all members of society other than those who enroll in Job Corps. It should be pointed out that this term does not refer specifically to that group of non-Corpsmembers interviewed in our study as a comparison group, although these individuals of course constitute a very small fraction of the total non-Corpsmember group.

[2/] This assumes that a dollar of benefit or cost to one person is equal to a dollar of benefit or cost to anyone else.

The examination of net present value from the three perspectives enables the analysis to estimate the redistribution of resources between non-Corpsmembers and Corpsmembers. However, the value of such a redistribution is not quantified, since we do not have a practical technique for doing so. Furthermore, the analysis masks any redistributions within these two broad groups. For example, a person using public facilities renovated or built by Corpsmembers, or someone who is not a crime victim as a result of Job Corps, receives a disproportionate share of the benefits that accrue to non-Corpsmembers. These aspects of the effects of Job Corps on the distribution of resources should be kept in mind when interpreting the benefit-cost findings.

Table VIII.1 presents the components of our benefit-cost analysis of Job Corps and the relationships among the Corpsmember, non-Corpsmember, and social perspectives. The table lists the principal components of the benefit-cost analysis, suggests whether a component is, on average, a benefit, a cost, or neither from each of the three perspectives, and indicates the data sources used to measure and value each component.[1/] The separate benefit-cost components listed in Table VIII.1 are the subject of the next two sections.

Finally, before proceeding, it is important to comment upon the comprehensiveness of the approach adopted in our evaluation. In general, we attempt to value <u>all</u> of the major resources saved or used as a result of

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[1/] Whether the effect on a component is a net benefit or cost is sometimes problematic. Table VIII.1 reflects prior judgments about the social value of components from the perspective of society as a whole. The treatment of all components in the final net present value calculations is, of course, determined by the estimates of the actual Job Corps effects.

TABLE VIII.1

COMPONENTS OF THE JOB CORPS
BENEFIT-COST ANALYSIS

	Perspective[a]			Data
	Social	Non-Corpsmember	Corpsmember	Source(s)[b]
BENEFITS				
1. Output Produced by Corpsmembers				
o In-program output	+	+	+	S,P
o Increased postprogram output	+	0	+	I,P
o Increased postprogram tax payments	0	+	-	I,P
2. Reduced Dependence on Transfer Programs				
o Reduced public transfers	0	+	-	I,P
o Reduced administrative costs	+	+	0	I,P
o Increased utility from reduced welfare dependence	+	+	+	N
3. Reduced Criminal Activity				
o Reduced criminal justice system costs	+	+	0	I,P
o Reduced personal injury and property damage	+	+	0	I,P
o Reduced stolen property	+	+	-	I,P
o Reduced psychological costs	+	+	+	N
4. Reduced Drug/Alcohol Abuse				
o Reduced drug/alcohol treatment costs	+	+	0	I,P
o Increased utility from reduced drug/alcohol dependence	+	+	+	N
5. Reduced Utilization of Alternative Services				
o Reduced costs of training and education programs other than Job Corps	+	+	0	I,P
o Reduced training allowances	0	+	-	I,P
6. Other Benefits				
o Increased utility from redistribution	+	+	+	N
o Increased utility from improved well-being of of Corpsmembers	+	+	+	N
COSTS				
1. Program Operating Expenditures				
o Center operating expenditures, excluding transfers to Corpsmembers	-	-	0	A
o Transfers to Corpsmembers	0	-	+	A
o Central administrative costs	-	-	0	A,S
2. Opportunity Cost of Corpsmember Labor During the Program				
o Foregone output	-	0	-	I,P
o Forgone tax payments	0	-	+	I,P
3. Onbudgeted Expenditures Other than Corpsmember Labor				
o Resource costs	-	-	0	S,P
o Transfers to Corpsmembers	0	+	+	S,P

[a] The columns indicate whether the net impact of a particular item is a net benefit (+), a net cost (-), or neither (0). In addition to the value to society as a whole, the estimates are calculated from the non-Corpsmember and Corpsmember perspectives in order to indicate redistributional effects. In doing so, Corpsmembers are treated as nontaxpayers (except for their own taxes) to simplify the exposition, and non-Corpsmembers encompass everyone in society other than Corpsmembers.

[b] The codes of data sources are: S = special study; I = interview; P = published data source; A = Job Corps financial accounting system; N = not measured.

the program. When market values are not observed directly, we estimate implicit shadow prices whenever possible. However, in some cases, we will be unable to estimate reliable shadow prices. In addition, we will sometimes be unable to value the magnitude of Job Corps effects, such as the utility to society of reduced welfare transfers due to individuals' preferences for reduced welfare dependence (increased self-esteem of Corpsmembers and reduced social fears among non-Corpsmembers).[1/] In cases like these, we will provide a qualitative discussion of the unmeasured benefit or value, so that readers can form their own judgments about its worth.

B. BENEFIT COMPONENTS

Five major benefit components are measured and valued. All of them are expected to derive, at least in part, from the increased employability of Corpsmembers. Improved job opportunities should lead to benefits from increases in the production of goods and services and from reductions in (1) welfare dependence, (2) criminal activities, (3) drug and alcohol abuse, and (4) the use of alternative training and education services. Our estimates for each of these five benefit components are briefly discussed below.

1. Output Produced by Corpsmembers at Centers and in Postprogram Employment

The increase in goods and services produced by Corpsmembers constitutes a major benefit of the program. For analytical purposes, it is

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[1/] One way to interpret quantitative benefit-cost findings when some benefits and costs are not measured is that if measured costs exceed measured benefits, society must value the difference between unmeasured benefits and costs by at least the amount of the measured shortfall in net present value in order for the program to be considered a worthwhile economic investment.

useful to distinguish between goods and services that Corpsmembers produce while they are enrolled in Job Corps and those that they produce after they leave the program. This distinction is convenient because different techniques are necessary to value changes in postprogram versus in-program output, and the different outputs have different distributional consequences. Output produced by Corpsmembers after they leave the program is produced in the regular labor market, and we can estimate its value on the basis of their earnings. Indeed, employer payments represent the values that we attach to Job Corps effects on postprogram employment and earnings.

In contrast, the output produced by Corpsmembers while they are enrolled in Job Corps is produced under nonmarket circumstances, and Corpsmembers are not paid for that output (their Job Corps stipends, or "pay allowances," are determined on a different basis and bear no relationship to in-program output). Furthermore, non-Corpsmembers benefit from postprogram employment effects through taxes on earnings, but they benefit from in-program output differently from one work activity to another. To value in-program output, we had to undertake a separate sampling and study of Job Corps work activities.[1]

In-Program Output. The in-program output produced by Corpsmembers in connection with their vocational training provides benefits to Corpsmembers, to non-Corpsmembers, and to society as a whole. These outputs include goods produced in work projects (for instance, the addition built for a hospital in rural Colorado by Corpsmembers who were receiving

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[1] For more details on the estimated values of in-program output and the techniques used to obtain these estimates, see Technical Reports E and Q.

on-the-job training in various construction trades) and services provided in work-experience programs (for instance, the nursing assistance provided by Corpsmembers at a county hospital in Guthrie, Oklahoma, as they were obtaining work experience in nursing). The value of these goods and services was estimated on the basis of twenty-two special studies of randomly chosen work projects and work-experience programs at eleven Job Corps centers.

The recipients of this Corpsmember-produced output might be either the non-Corpsmember community or the Job Corps centers themselves. In the first case (community-serving output), the entire value of the output produced is considered to be a benefit to non-Corpsmembers.[1/] In the second case (center-serving output), the output benefits both Corpsmembers and non-Corpsmembers. Corpsmembers benefit from center-serving output because they consume some of the output they produce (e.g., housing services provided in dormitories built or rehabilitated with Corpsmember labor, part of which is included as a capitalized cost in the Job Corps financial data); non-Corpsmembers benefit when the capital stock available to society increases as a result of Corpsmember labor in these activities.

The value of the goods and services produced by Corpsmembers in community- and center-serving projects is estimated by the price that alternative suppliers would have charged to provide those same goods and services.[2/] After estimates are made for the value of center-serving output

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[1/] As general members of society, Corpsmembers also benefit from community-serving output. For the most part, however, we will use the approximate (and computationally convenient) assumption that only non-Corpsmembers benefit from such output.

[2/] The value of all materials and labor inputs provided by Job Corps are subtracted from the alternative supplier's price. In many cases, this net value of Corpsmembers' output was quite close to the alternative supplier's labor cost.

used by Job Corps and the amount transferred to Corpsmembers, the net benefits per Corpsmember year of service for in-program output are estimated to be $1,364 for non-Corpsmembers, $175 for Corpsmembers, and, thus, $1,539 for society as a whole. Because the average Corpsmember remains in the program approximately half a year (5.9 months in fiscal 1977), the average social benefit per Corpsmember is $757.[1] On average, Corpsmembers receive $86 of this benefit, while the remaining $671 accrues to non-Corpsmembers.

These values are based on the prices charged by alternative suppliers and indicate the value of the resources that would be required to produce the in-program output of Corpsmembers. However, these prices do not directly measure the value that society places on the output. While only imprecise estimates of this demand value can be made, tests presented in Technical Report E suggest that, under reasonable assumptions, the demand value will be between 103 and 62 percent of the supply-price estimate.[2] Thus, using supply price as a measure of the value of in-program output appears to provide a reasonably accurate estimate of the demand value (although probably high by a small amount).

Increased Postprogram Employment Output. The increase in the value of output produced by Corpsmembers after they leave the program is estimated by the increase in their gross compensation (i.e., earnings plus

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[1] That is, (5.9/12) x $1,539 ≐ $757.

[2] Essentially, the value can be greater than 100 percent of the supply price when people other than the recipient obtain benefits (externalities to other people or external benefits), and the value can be less than 100 percent of the supply price when recipients value it less than it costs an alternative supplier to produce it (i.e., demand price is less than supply price).

fringe benefits).[1] The use of gross compensation as a measure of output produced is based on the assumption that labor markets function in a competitive manner (although, in any case, gross compensation is the correct measure from the Corpsmember perspective, since that is what they receive). This assumes that employers pay total compensation to their workers that reflects the value of the output produced by the workers, which should generally be correct for competitive markets. The increase in output produced by Corpsmembers is then estimated by the difference between their gross compensation and the amount they would have received had they not entered the program.[2]

The earnings component of gross compensation was estimated from the interview data and includes both civilian and estimated military earnings (see Chapter IV). The nonwage components--retirement, health, and insurance benefits, the employer's share of FICA payroll taxes, and payments into Unemployment Insurance and Workers' Compensation funds--can be estimated on the basis of secondary data (e.g., Social Security statutes and U.S. Department of Labor estimates of fringe-benefit rates). For workers like the Corpsmembers, the value of nonwage items is estimated to equal, approximately, 15 percent of wages. Thus, increased total compensation (the social benefit derived from the increase in output) is

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[1] This assumes that Corpsmembers do not displace other workers who subsequently become unemployed (see Technical Report Q for more discussion on this topic).

[2] See the discussion in Chapter III for details on the econometric procedures used to obtain the estimates of Job Corps effects. Chapters IV through VII present the resulting impact estimates that are valued in this chapter. The estimates used in the benefit-cost analysis include youths in military jobs.

estimated to be 1.15 times the estimated increase in Corpsmembers' earnings.

Table VIII.2 provides estimates of the value of increases in postprogram output (earnings and fringe benefits). The estimated present value of increased postprogram output (gross compensation) is approximately $1,930 per Corpsmember during our postprogram observation period. As discussed in Chapter IV, these estimates might be on the low side (approximately $100 per year too low for earnings), because they do not account for nonlinearities in the time trends for the economy. Here, however, in line with our conservative approach, and in order to use a consistent basis for all estimates, we use the lower estimate, which has the same linear time-trend assumption as all of our other estimates (some of which could not be computed, except at great expense, with nonlinear time trends for the underlying economy).

Increased Tax Payments on Postprogram Income. As the incomes of Corpsmembers rise, they pay more taxes. Such an increase in tax payments is a cost to Corpsmembers, but is an offsetting benefit to non-Corpsmembers (i.e., all other taxpayers); hence, it does not enter the social perspective.[1/] However, it does represent a transfer of resources from Corpsmembers to non-Corpsmembers.

To estimate taxes paid, we used an estimate of the change in Corpsmembers' taxable income and an estimate of the overall tax rate applicable to that income for low-income households. This tax rate was

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1/ As is the case with all transfers, changes in the resource costs of making the transfer should be included in the social perspective. With respect to tax payments, however, the change in administrative costs is probably very small and is treated as zero.

TABLE VIII.2

ESTIMATED VALUE OF INCREASED POSTPROGRAM OUTPUT AND TAXES PER CORPSMEMBER
(1977 DOLLARS)

Source of Estimated Increase	Year After Leaving Job Corps 1	2	3	4	Estimated Total Discounted Value[a]
1. Earnings[b]	$262	$652	$567	$405	$1,681
2. Fringe benefits (0.15 x earnings)	39	98	85	61	252
3. Postprogram output (earnings + fringe benefits)	301	750	652	466	1,933
4. Taxable income[c]	161	683	537	405	1,587
5. Tax payments (0.23 x taxable income)	37	157	124	93	365

NOTE: For an explanation and justification of these estimation procedures, see the text (for more details, see Technical Report Q).

[a] The postprogram amounts are discounted to the in-program period (fiscal 1977) at a 5 percent real annual rate. The numbers in this column are more accurate than can be obtained from the preceding four columns, because they are based on more refined, six-month breakdowns from Tables IV.10 and IV.11 in Chapter IV and because they have less rounding error.

[b] These are the summary estimates for civilian and military jobs in 1977 dollars from Table IV.1 in Chapter IV. As noted in the discussion in Chapter IV, these estimates are probably somewhat low (on the order of $100 per year) because we do not take account of nonlinearities in the time trends for earnings aside from Job Corps, which make the first and fourth postprogram years especially low. However, we use these estimates to keep them on the same basis as other estimated effects, some of which would have been very difficult to compute had controls for nonlinear time trends in the economy been included.

[c] Taxable income is defined as in the source for the tax rate (Pechman and Okner, 1974) and includes earnings plus fringe benefits plus public transfers plus training allowances.

estimated by Pechman and Okner (1974) to be approximately 23 percent of total taxable income, defined to include earnings, fringe benefits, public transfers, training allowances, etc.[1/] The major components of this tax rate are payroll, sales, and excise taxes. These taxes are difficult to avoid, especially those levied on consumption. Thus, even though Corpsmembers might face low tax rates on earnings and might in fact avoid paying some payroll and income taxes, their total tax burden (as a percentage of income) is not significantly different from the tax burden of most taxpayers (although the composition of taxes does vary considerably by income level).

The change in taxable income (as defined by Pechman and Okner, 1974) was calculated from estimates of the changes in gross compensation, transfer payments, and training allowances. Table VIII.2 provides the resulting estimates, along with the estimates of the changes in tax payments. Because there were estimated declines in the receipt of public transfers and training allowances, the changes in "taxable" income are smaller than the changes in gross compensation. Altogether, Corpsmembers received an average of approximately $1,600 more in income during the first four postprogram years than they would have had they not entered Job Corps; they also paid approximately $365 more in taxes per Corpsmember during that period.

[1/] This discussion draws on the results of Pechman and Okner (1974). Their data show that the combined effect of all taxes is equivalent to a proportional tax of approximately 23 percent of income. These data are rather old (1966) and contain some inaccuracies when applied to populations of low-income youths. However, they are the best estimates currently available--particularly because they are comprehensive in covering all major taxes and are able to incorporate the effects of tax avoidance.

2. Reduced Dependence on Public Transfers

As reported earlier (see Mallar et al., 1978), Corpsmembers reduce their use of public transfers while they are in the Job Corps program. In addition, as discussed in Chapter VI, associated with the postprogram increases in employment and earnings, we find that Job Corps substantially reduced Corpsmembers' postprogram dependence on public assistance and Unemployment Insurance. This will cause a decline both in transfers to Corpsmembers and in the level of resources necessary to administer those programs. Aside from the unmeasured utility from reduced welfare dependence, the reductions in public transfers represent a cost to Corpsmembers and a corresponding benefit to non-Corpsmembers, who otherwise would have had to pay for them. Therefore, these transfers net out from the social perspective. In contrast, the associated administrative cost savings are a benefit to non-Corpsmembers and do not represent a cost to Corpsmembers; hence, the administrative savings are a benefit to society.

Reduced Transfers. Seven transfer programs were examined as part of the analysis: Aid to Families with Dependent Children (AFDC), Medicaid, General Assistance, Food Stamps, public housing, Unemployment Insurance, and Workers' Compensation. Transfers were estimated on the basis of the estimated differences in the number of months during which the transfers were received (see Chapter VI) and the average benefit levels of the programs. Average benefit levels were estimated on the basis of published data, except in the cases of Unemployment Insurance and Workers' Compensation, for which we used the average benefit reported by the youths in our sample (a smaller amount than the average benefit levels of the program).

For AFDC, Medicaid, General Assistance, Food Stamps, and public

housing, separate estimates were made for Corpsmembers with children living with them and for those without children.[1/] Cases in which children were present were valued using the average benefit amount per case. For Corpsmembers without children, the average benefit per recipient was used. Table VIII.3 provides the resulting estimates and the value of these estimated effects of Job Corps. The largest values of reductions were for the cash welfare programs (AFDC and General Assistance) and for the receipt of Unemployment Insurance. The discounted value of the net reduction in all seven types of transfers is just over $500 per Corpsmember.

Reduced Administrative Costs. With the decline in transfers, the amount of resources necessary to administer the programs also declines. As mentioned, this resource saving is a benefit to non-Corpsmembers and to society as a whole. The savings are estimated on the basis of the estimated changes in months of program use and the average costs per month of processing a case in each program. Table VIII.4 presents the estimated benefits, which total approximately $100 per Corpsmember in present-value terms during the observation period.

Unmeasured Benefits. In addition to the reduced transfer payments and reduced administrative costs of welfare, benefits also accrue directly to both Corpsmembers and non-Corpsmembers to the extent that individuals prefer not to depend on welfare. Corpsmembers are expected to benefit

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[1/] For females, separate impact estimates were computed for women with children and for those without children. However, this distinction was not necessary for males. For benefit-cost purposes, we have assumed that the fraction of males with children equals the corresponding fraction for females; we have apportioned the Job Corps effects for males accordingly. This procedure differs slightly from that used in our previous benefit-cost analyses of Job Corps and is described more fully in Technical Report 4.

TABLE VIII.3

ESTIMATED VALUE OF BENEFITS PER CORPSMEMBER FROM REDUCED PUBLIC TRANSFERS
(1977 DOLLARS)

Transfer Program[a]	Estimated Months of Reduction: In-Program Period	Postprogram Period: Year 1	Year 2	Year 3	Year 4	Estimated Value Per Month	Estimated Total Discounted Value[b]
1. AFDC[c]							
o With children	0.111	0.019	-0.051	0.078	-0.065	$239	$ 23
o Without children	0.222	0.417	0.287	0.249	0.233	76	99
2. Medicaid[d]	0.569	0.476	0.127	0.493	0.030	27	43
3. General Assistance[c]							
o With children	0.018	0.018	0.031	0.073	0.078	152	29
o Without children	0.036	0.255	0.179	0.153	0.100	116	77
4. Food Stamps[e]	1.965	0.371	0.096	0.810	0.394	25	85
5. Public Housing[c]							
o With children	0.180	-0.030	-0.090	0.042	0.126	61	10
o Without children	0.683	-0.184	-0.126	0.120	0.102	32	19
6. Unemployment Insurance (UI)	—	0.240	0.150	0.174	0.174	275	183
7. Workers' Compensation (WC)	—	-0.012	-0.048	-0.066	-0.078	$74	-84
8. UI and WC[f]	$29.30[b]	—	—	—	—	—	29
Total Benefits							$514

[a] The estimated reductions for the in-program period were computed for earlier reports (for more details, see Technical Report Q). For the postprogram period, the estimated reductions for public assistance programs (rows 1 through 5) are based on the six-month breakdowns from Tables VI.10 and VI.11 in Chapter VI, and the estimated reductions for Unemployment Insurance and Workers' Compensation are based on the six-month breakdowns from Tables VI.12 and VI.13 in Chapter VI.

[b] The postprogram amounts are discounted to the in-program period (fiscal 1977) at a 5 percent real annual rate. The numbers in this column are more accurate than can be obtained from the preceding six columns, because they are based on more refined, six-month breakdowns of estimated reductions and because they have less rounding error.

[c] The estimated values of these transfers differ substantially according to whether or not the recipients have children living with them. Therefore, the estimated months of reduction are calculated separately by the presence or absence of children--the "with children" estimates include females with children plus the relevant portions of the estimated male effects, and the "without children" estimates include females without children plus the relevant portions of the estimated male effects.

[d] The estimated value of Medicaid transfers is based on recipients of AFDC (for more details, see Technical Report Q). We assume one recipient for the AFDC effects on Corpsmembers without children and the AFDC program average per case (family) of 3.13 recipients for the AFDC effects on Corpsmembers with children (females with children plus the relevant portions of the estimated male effects).

[e] The estimated value of Food Stamps transfers is based on recipients. We assume one recipient for the effects on Corpsmembers without children and the program average per case (household) of 3.00 recipients for the effects on Corpsmembers with children (females with children plus the relevant portions of the estimated male effects).

[f] For the in-program period we have data only on the aggregate of UI and WC. The estimate is based on the sampled youths' reports of the total amount of UI and WC received (for more details, see Technical Report Q).

TABLE VIII.4

ESTIMATED VALUE OF BENEFITS PER CORPSMEMBER FROM REDUCED ADMINISTRATIVE COSTS OF TRANSFER PROGRAMS
(1977 DOLLARS)

Transfer Program[a]	Estimated Months of Reduction: In-Program Period	Postprogram Period: Year 1	Year 2	Year 3	Year 4	Estimated Value Per Month	Estimated Total Discounted Value[b]
1. AFDC	0.333	0.436	0.236	0.327	0.168	$28	$ 39
2. Medicaid[c]	0.569	0.476	0.127	0.493	0.030	3	5
3. General Assistance	0.053	0.273	0.210	0.226	0.178	17	15
4. Food Stamps[d]	1.447	0.347	0.128	0.514	0.290	9	28
5. Public Housing	0.822	-0.174	-0.216	0.162	0.228	13	10
6. Unemployment Insurance[e]	0.144	0.240	0.150	0.174	0.174	30	28
7. Workers' Compensation[e]	—	-0.012	-0.048	-0.066	-0.078	$4	-10
Total Benefits							$108

[a] The estimated reductions for the in-program period were computed for earlier reports (for more details, see Technical Report Q). For the postprogram period, the estimated reductions for public assistance programs (rows 1 through 5) are based on the six-month breakdowns from Tables VI.10 and VI.11 in Chapter VI, and the estimated reductions for Unemployment Insurance and Workers' Compensation are based on the six-month breakdowns from Tables VI.12 and VI.13 in Chapter VI.

[b] The postprogram amounts are discounted to the in-program period (fiscal 1977) at a 5 percent real annual rate. The numbers in this column are more accurate than can be obtained from the preceding six columns, because they are based on more refined, six-month breakdowns of estimated reductions and because they have less rounding error.

[c] The estimated value of the administrative cost of Medicaid is based on recipients of AFDC (for more details, see Technical Report Q). We assume one recipient for the AFDC effects on Corpsmembers without children and the AFDC Program average per case (family) of 3.13 recipients for the AFDC effects on Corpsmembers with children (females with children plus the relevant portions of the estimated male effects).

[d] The estimated value of the administrative cost of Food Stamps is based on cases of receipt. Therefore, the estimated months of reduction can be calculated directly from the overall Job Corps effects from Tables VI.10 and VI.11 in Chapter VI similar to the other entries in this table, except for Medicaid (see above). The differences between cases (households) and individual recipients explain the differences for estimated months of reduction in Food Stamps for this table compared to the entries in Table VIII.3.

[e] For the in-Program period we have data only on the aggregate of UI and WC. We use the estimated administrative cost value for UI because, by far, the greatest portion of these youths receive UI compared to WC. (This is slightly conservative in the sense of undervaluing benefits, since the value for UI is less than that for WC.)

from increased independence and self-esteem and reduced stigma. Non-Corpsmembers are expected to benefit to the extent that they would prefer that the resources be used to give Corpsmembers an opportunity to increase their human capital and earnings from Job Corps participation, rather than used to provide public transfers to Corpsmembers. These changes in well-being are intangible and could not be estimated accurately for this analysis.

3. Reduced Criminal Activities[1/]

Four benefits from the decline in criminal activities among Corpsmembers relate to reductions in (1) the resources used in the criminal justice system, (2) the personal injury and property damage that accompany victimizations, (3) the value of stolen property, and (4) the fear and anxiety associated with crime. The resource savings associated with the first two items are benefits to society and to non-Corpsmembers. The reduced value of stolen property will be a benefit to non-Corpsmembers, but part of its value should be viewed as a cost to Corpsmembers, who no longer receive that theft income. The social benefit of a reduction in stolen property (the difference between the non-Corpsmember benefit and the Corpsmember cost) relates to the decreased social resources attributable to fencing (which uses resources), damage to stolen property, and the loss of legal titles. Reductions in the fear and anxiety associated with crime are not valued directly, but will be noted.

[1/] By necessity, this section is a brief summary of the crime-evaluation procedures used. Technical Report Q contains a more complete discussion of the procedures and their justifications.

The method used to value the crime-reduction benefits focuses on the effect of changes in arrests among nine crime categories (see Table VIII.5). The estimates from Chapter VII of Job Corps-induced reductions in arrests are adjusted upward by 70 percent to correct for underreporting in the interview self-reports.[1/] These estimated reductions in arrests were then valued by multiplying them by shadow prices equal to the cost savings per arrest. This disaggregation of arrests was made according to most serious charge and enables us to value changes in both the mix of arrest charges and the overall level of arrests. Table VIII.5 presents the shadow prices applicable to the social perspective.

Reduced Criminal Justice System Costs. Processing persons through the criminal justice system (police, prosecution, courts, and corrections) is expensive. Court time alone is estimated to cost $15 per minute; thus, even the simple process of entering a plea cost close to $450 in 1977.[2/] The shadow prices in the table reflect the probability and cost of an arrested person passing through each stage of the system--police custody, arraignment, detention, trial, and incarceration.

[1/] Schore, Maynard, and Piliavin (1979) found that arrests were underreported by between 41 and 48 percent when interview questions (essentially the same as those used in this analysis) were compared to official court records. Most of this underreporting stemmed from the underreporting of multiple arrests and not from the failure to report any arrest. Thus, the self-reports must be multiplied by 1.7 to obtain an estimate of the actual number of arrests. For further details, see Technical Report Q.

[2/] These estimates are based on computations made by Greenwood et al. (1973); their estimates have been adjusted to 1977 dollars.

TABLE VIII.5

ESTIMATES OF AVERAGE SOCIAL COST OF CRIME PER ARREST, BY ARREST CHARGE
(1977 DOLLARS)

Arrest Charge	Criminal Justice System Costs	Personal Injury and Property Damage Costs[a]	Stolen Property Resource Costs[b]	Total Estimated Cost Per Arrest
1. Murder	$24,767	$100,538	$0	$125,305
2. Felonious assault	2,732	489	0	3,221
3. Robbery	12,087	569	497	13,135
4. Burglary	5,895	537	2,317	8,749
5. Larceny and motor vehicle theft	2,618	408	1,268	4,294
6. Drug law violation	2,590	0	0	2,590
7. Other personal crimes	756	94	0	850
8. Other miscellaneous crimes	919	0	0	919
9. Unspecified crimes	2,048	171	348	2,567

[a] The "drug law violation" and "other miscellaneous crimes" categories contain primarily "victimless" crimes, and a value of zero is assumed in these categories for personal injury and property damage costs.

[b] Stolen-property resource costs (associated with fencing, loss of legal title, and property damage), estimated only for theft crimes, are estimated to be 65 percent of the average property stolen per arrest (see the text and Technical Report Q for more details).

[c] The "unspecified-crimes" category contains arrests for which the arrest charge was either not recorded or undecipherable. Costs for this crime category are estimated to be the weighted average of the costs of the other categories.

Reduced Personal Injury and Property Damage. Another major benefit associated with reduced criminal activity is the decrease in victimizations. The victimization benefits included in this analysis are the resource savings from reductions in the amount of personal injury and property damage. Using data collected as part of the National Crime Panel Survey program, we estimated (1) the average value of property damage from criminal acts, (2) the average value of the medical care needed by victims of personal crimes, (3) the average output lost when victims lose time from work while they are recovering from personal crimes, and (4) the average costs of administering the insurance used to compensate victims.[1/] The average cost-per-victimization figures were multiplied by the ratio of victimizations to arrests in order to estimate the average cost per arrest.

Reduced Value of Stolen Property. Estimates for the value associated with the reduction in stolen property were obtained from victimization data in a manner similar to that used to estimate the cost of reductions in property damage and personal injury, with one major difference: part of the value of stolen property represents a transfer from victim to thief. The remaining part of the value of stolen property is the social cost associated with fencing the goods, with a decline in the value of the goods because they cannot be sold with a legal title, or with a decline in value because of damage. The relative magnitudes of these

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1/We would like to thank Wesley G. Skogan for his help in obtaining the necessary estimates from the victimization-incident data that were gathered as part of the National Crime Panel program. Technical Report Q presents a detailed breakdown and analysis of the average costs of victimizations by crime category.

components were estimated on the basis of a study which found that thieves were able to realize only 35 percent of the value to victims when they converted stolen goods into cash.[1/]

Non-Corpsmembers are assumed to benefit from the estimated Corpsmember reductions in stolen property in an amount equal to the full dollar value of the reductions. However, 35 percent of the dollar value of these reductions in stolen property is estimated (from above) to represent a loss of income to Corpsmembers, and thus, 65 percent of the dollar value of these reductions is assumed to represent a benefit to society.

Reduced Psychological Costs. The values presented above capture only part of the costs of criminal victimizations. In particular, they fail to capture completely the emotional benefits that individuals derive from reductions in crime. These benefits are undoubtedly important. However, because there is no accurate way to directly estimate their magnitude, they have not been counted in the numerical estimates. This exclusion will bias our benefit-cost estimates downward, and that must be kept in mind when interpreting the results.

The Value of Social Benefits from Job Corps Reductions in Crime. Table VIII.6 presents the estimates of the Job Corps-induced changes in arrests for the nine arrest types adjusted for underreporting. The measured shadow prices to society (i.e., the average social costs from Table VIII.5) of the different arrest types have been entered in the next-to-last column. The final column provides the total discounted value of the crime benefits generated during the in-program period and the first four postprogram years.

1/ U.S. Drug Enforcement Administration, Heroin-Related Crimes (1977). The 35 percent estimate takes into account that stolen cash and other liquid assets need not be fenced.

TABLE VIII.6

ESTIMATED VALUE OF SOCIAL BENEFITS PER CORPSMEMBER FROM REDUCED CRIMINAL ACTIVITIES
(1977 DOLLARS)

Arrest Charge[b]	Estimated Reduction in Number of Arrests[a]					Estimated Social Value Per Arrest	Estimated Total Discounted Value[c]
	In-Program Period	Postprogram Period					
		Year 1	Year 2	Year 3	Year 4		
1. Murder	0.002	0.004	0.002	0.003	0.002	$125,305	$1,468
2. Felonious assault	0.005	0.000	-0.012	0.002	0.008	3,221	8
3. Robbery	0.002	0.010	0.010	0.003	0.003	13,135	349
4. Burglary	0.052	-0.014	-0.003	-0.002	-0.012	8,749	214
5. Larceny and motor vehicle theft	0.059	0.015	0.020	0.048	0.041	4,294	720
6. Drug law violation	0.026	.005	-0.006	-0.010	-0.015	2,590	10
7. Other personal crimes	0.019	.003	-0.007	-0.004	-0.002	850	9
8. Other miscellaneous crimes	0.050	0.002	-0.027	-0.014	-0.037	919	-15
9. Unspecified crimes[d]	-0.003	0.008	0.007	0.007	0.005	2,567	55
Total Benefits							$2,819

[a] Estimates of the Job Corps-induced reductions in arrests per Corpsmember from Tables VII.11 and VII.12 in Chapter VII have been multiplied by 1.7 to correct for underreporting of arrests in the interviews. This procedure and its justification are described in Technical Report Q. For an analysis of underreporting of arrests in survey data such as ours, also see Schore, Maynard, and Piliavin (1979).

[b] In those cases where there was more than one arrest charge, only the most serious charge was used.

[c] The postprogram amounts are discounted to the in-program period (fiscal 1977) at a 5 percent real annual rate. The numbers in this column are more accurate than can be obtained from the preceding six columns, because they are based on more refined, six-month breakdowns of estimated reductions and because they have less rounding error.

[d] The "unspecified crimes" category contains arrests for which the arrest charge was either not recorded or undecipherable. Costs for this crime category are estimated to be the weighted average of the costs for the other categories.

The estimates indicate that a large reduction in arrests occurs while Corpsmembers are in Job Corps, particularly in burglary and larceny arrests. As discussed in Chapter VII, after Corpsmembers leave the program, the pattern of changes in arrests becomes more erratic. During the first four postprogram years, substantial reductions in larceny and consistent reductions in murder and robbery arrests occur. At the same time, increases in arrests for burglary and traffic offenses are observed. The net results appear to be a large value of reduced crime during the program, and an erratic but beneficial pattern during the postprogram period, with some shift to less serious crimes. The total discounted value of the social benefits from reductions in arrests during the observation period (the in-program period plus the first four postprogram years) is just over $2,800 per Corpsmember.[1/] When the redistributional aspects of stolen property are taken into account, we estimate a $2,975 per-Corpsmember benefit to non-Corpsmembers and an average net cost of approximately $160 to Corpsmembers.

Approximately one-half of these savings are derived from reductions in murder arrests. While none of the estimates of annual reductions in murder arrests is large (they range from reductions of 2 to 4 arrests per 1,000 Corpsmembers) the high average cost of both this crime ano adjudicating these arrests implies substantial cost savings. Most of the remaining benefits from reduced crimes relate to theft crimes--that is, reductions in robbery and larceny both during and after Job Corps participation and reductions in burglary during the program.

[1/] Detailed estimates of the components of the social benefit and tne non-Corpsmember and Corpsmember benefits are presented in Technical Report Q.

4. Reduced Drug and Alcohol Abuse

The principal estimated benefit of the reduction in drug and alcohol abuse is from the in-program and early postprogram decrease in treatment (see Mallar et al., 1980). The types of drug-alcohol treatments included in the estimates are residential and nonresidential drug detoxification (principally heroin), residential and nonresidential "drug-free" treatment, alcohol detoxification, and education and counseling services. The resource savings associated with the reduction in drug-alcohol treatments will benefit both non-Corpsmembers and society as a whole. The emotional benefits from reduced drug and alcohol abuse, while unmeasured in this report, will also accrue to both Corpsmembers and non-Corpsmembers.

In general, Corpsmembers participate very little in drug- or alcohol-treatment programs, especially as they become older. The largest effect was observed while they were enrolled in Job Corps. During the postprogram period, the decreases in treatment use were all very small (a difference of less than one-half day in treatment per year), since, for the most part, they would not have been involved in drug treatment in any case. The present value of the resources saved because of these reductions is estimated to be approximately $31 per Corpsmember for both the in-program period and the first four years of the postprogram period (approximately $22 of this benefit accrues during the program).[1/]

[1/] As noted in Chapter VII, because the use of drug treatment was almost nonexistent for either Corpsmembers or comparison youths, it was not estimated for the new data, which cover the third and fourth postprogram years. In the benefit-cost analysis, we have used a value of zero for drug- and alcohol-abuse benefits during those years.

5. Use of Alternative Training and Education Programs

Decisions by Corpsmembers to enter into training and schooling outside Job Corps will generate benefits and costs to both Corpsmembers and non-Corpsmembers. To the extent that Job Corps influences these decisions, the resulting changes in benefits and costs must be included in the benefit-cost analysis. While the in-program effects are clearly in the direction of reduced participation in alternative education and training, as reported in Chapter V the postprogram effects are somewhat offsetting--reductions in high school (an alternative program) but increases in college (a supplemental program). The effect of Job Corps on resource use in education and training can be captured by using estimates of the effect of Job Corps on Corpsmembers' use of these programs and the average-cost data for the affected programs.

Fully capturing changes in the earnings and other benefits generated by these alternative programs is more problematic. Ideally, the impact estimates for earnings and other effects would include these changes directly, since they are measured in relation to what Corpsmembers would have done in the absence of Job Corps. This is a reasonable assumption for some types of education and training that were alternatives to Job Corps (i.e., high school education and CETA training) and that were obtained primarily during or shortly after the time Corpsmembers were in the program. However, for supplemental higher education, such as college, which is increased later, the benefits will occur primarily after our observation period. In reality, not all of the effects from alternative education and training will have been observed during the period covered by our interview data, especially for the additional higher education. Thus,

by including all of these changes as resource costs, we probably bias our net present value estimates downward.[1]

Table VIII.7 presents the estimated value of changes in Corpsmembers' use of education and training programs; these include the traditional school programs--college, high school, vocational and technical school, and other schooling (mostly adult education programs)--and three general categories of employment and training programs--the Work Incentive Program (WIN), CETA and other non-WIN programs, and Public Service Employment (primarily CETA). The largest reduction in costs occurs in the use of high school, which is due, in part, to the fact that many Corpsmembers earn a GED degree while in Job Corps. Also, small reductions occur in the use of employment and training programs, especially during the in-program and early postprogram periods. The increase in the use of college and vocational education programs increases costs, which, as noted above, might lead to future benefits. The net result is a resource savings of approximately $250 per Corpsmember during the observation period.

In addition to the changes in resource use for the various education and training programs, there will also be a transfer due to changes in training allowances paid to Corpsmembers. Deflating the estimates from Chapter V to 1977 dollars, we estimate that Corpsmembers received an average of $27 less in training allowances from alternative programs because of their participation in Job Corps. Over 80 percent of

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[1] Technical Report Q discusses this situation in more detail.

TABLE VIII.7

ESTIMATED VALUE OF SOCIAL BENEFITS PER CORPSMEMBER FROM REDUCED UTILIZATION OF ALTERNATIVE TRAINING AND EDUCATION PROGRAMS
(1977 DOLLARS)

Alternative Program[a]	Estimated Months of Reduction					Estimated Value Per Month	Estimated Total Discounted Value[b]
	In-Program Period	Postprogram Period					
		Year 1	Year 2	Year 3	Year 4		
1. College	0.065	-0.036	-0.066	-0.078	-0.150	$368	-$82
2. High school	0.548	0.282	0.228	0.126	0.060	199	237
3. Vocational or technical school	0.092	-0.072	-0.120	-0.030	-0.036	228	-33
4. Other school	0.163	0.072	0.012	-0.006	0.000	265	64
5. CETA and related training[c]	0.123	-0.068	0.014	0.017	0.005	164	15
6. WIN training	0.000	0.030	0.030	0.030	0.036	320	36
7. Public service employment (PSE)	0.208	0.090	-0.114	-0.078	0.138	61	14
Total Benefits							$251

[a] The estimated reductions for the in-program period were computed for an earlier report (for more details, see Technical Report Q). For the postprogram period, the estimated reductions for education programs (Rows 1 through 4) are based on the six-month breakdowns from Tables V.12 and V.13 in Chapter V, the estimated reductions for training programs (Rows 5 and 6) are based on the six-month breakdowns from Tables V.14 and V.15 in Chapter V, and the estimated reductions for PSE (Row 7) are based on the six-month breakdowns from Tables IV.12 and IV.13 in Chapter IV.

[b] The postprogram amounts are discounted to the in-program period (fiscal 1977) at a 5 percent real annual rate. The numbers in this column are more accurate than can be obtained from the preceding six columns, because they are based on more refined, six-month breakdowns of estimated reductions and because they have less rounding error.

[c] This entry includes the joint impact of all the training effects listed under CETA training, youth training, union training, and other training in Tables V.14 and V.15 of Chapter V.

this reduction is estimated to have occurred while they were enrolled in Job Corps.

6. Other Benefits

In addition to the benefit components discussed above, there are two benefits that cannot be directly measured and valued; however, some indirect evidence on them does exist. One important benefit that cannot be measured directly is the utility that Corpsmembers and non-Corpsmembers derive from the income redistribution per se that is implicit in the Job Corps program. The other benefit is the improved personal well-being of participants beyond what they derive from both increased earnings and the value of Job Corps expenditures for room, board, medical and dental services, etc. In particular, it is likely that the full value of observed improvements in the health status of Corpsmembers and their basic education are not fully captured in this analysis.

7. Benefits After the Observation Period

To this point, the discussion of benefits has focused on the time period over which the Corpsmembers were observed--the in-program period plus the four years after they left Job Corps. However, there is strong reason to believe that at least some of the benefits discussed will continue after this period. This is particularly true of the earnings gains and of those effects closely related to increased earnings--increased taxes and reduced public transfers.

The existence of these future benefits creates a difficult problem for the benefit-cost analysis: in order to provide an accurate assessment, it is necessary to estimate the value of all benefits and costs, not just those that accrue during the period covered by the interviews.

While the extrapolations required for a full assessment are important, they are much more inaccurate than the estimates of benefits and costs for the observation period. Estimates of future benefits are imprecise not only because of questions about the appropriateness of the valuation methods, but also because of the lack of direct observations upon which to estimate the effects. Future benefits and costs must be estimated on the basis of trends observed to occur during the observation period, or on the basis of long-run studies of other groups--both of which are subject to substantial error and serious controversy when applied to Job Corps. The importance of such extrapolation to the overall benefit-cost estimates will be assessed in the sensitivity tests.

The central hypothesis of the extrapolation procedure adopted is that all benefits observed during a base time period will persist into the future, and that their magnitude will decline continuously over time.[1/] Thus, for example, an observed increase in earnings per year during the base period would be assumed to continue for future periods, with the size of the benefit becoming progressively smaller in each succeeding year.

The base period we have adopted is the last 6 months of the observation period (which covers postprogram months 43 to 48). The time horizon over which benefits are assumed to persist is assumed to be the expected worklife of the average Corpsmember (43 years after enrolling in Job Corps--up to approximately age 62). The rate at which earnings, taxes, and transfer-program benefits are assumed to decay is the same as was

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1/ Technical Report Q presents a more detailed explanation and justification of the extrapolation procedures and issues covered in this section.

used throughout the evaluation (approximately 14 percent per year), based on a study which found that earnings differentials for adult training had declined by one-half after five years.[1/] The evidence from our study is that this decay rate is very conservative, and the alternative of no decay might be more plausible (see the sensitivity tests below). However, because our observation period has increased, the particular decay rate matters less, since future years are subject to heavier discounting when calculating present values. For crime and alternative-program use (drug/alconol abuse had already declined to zero), a much higher decay rate was used, both because participation in these activities generally declines rapidly with age and because we observed some substantial declines over our observation period.[2/]

[1/] A study of Manpower Development and Training Act (MDTA) programs by Ashenfelter (1978) provides this evidence on the future magnitude of the earnings effect. He found that the earnings gains for adult men who had participated in MDTA employment and training programs had declined by approximately 50 percent after five years, while the gains for adult women did not fade out. If we assume a decline for Job Corps similar to the larger magnitude that Ashenfelter found for males, on a continuous basis it would imply a rate of decline of just under 14 percent per year. In the absence of better information, Ashenfelter's decay rate for adult males has been adopted for both males and females. This probably overstates the decay rates for Corpsmembers for three reasons: (1) Corpsmembers are young and are still benefitting from increased investments in human capital, (2) Ashenfelter's estimated decay rate for males is much larger than he found for females, and (3) we find little evidence of such a decay rate over the first four postprogram years.

[2/] Specifically, it was assumed that these effects decay at a continuous rate that reduces them to 25 percent after one year and to 1 percent after three years. This assumption (a decay rate of 140 percent per year) is very conservative and probably understates the crime benefits (see the sensitivity tests below).

In addition to the extrapolation of effects into the future, two other issues regarding benefits and costs over time had to be addressed. The first was to correct for the effect of inflation on dollar-denominated benefits and costs. This was done by measuring all items in terms of fiscal 1977 dollars. This time period corresponds approximately to the period when our sample of Corpsmembers were in Job Corps. As discussed in the next section, cost data were derived from fiscal 1977 records and require no special adjustment. For benefits that were not denominated in dollars, shadow prices measured in fiscal 1977 dollars were used. For postprogram earnings and training allowances (the only benefits estimated using dollar-denominated data from the interviews), the estimates were deflated to 1977 dollars on the basis of the changes in the implicit price deflators for the gross national product.[1/]

The second adjustment was to account for the fact that benefits or costs generated in the future will not be worth as much as the same benefits or costs generated at present. This adjustment process of discounting to present values converts the observed extrapolated streams of benefits and costs into equivalent present-value dollar amounts. We use a real (i.e., net of inflation) discount rate of 5 percent per year. (With an inflation rate of 10 percent per year, this would correspond to an annual nominal interest rate of 15 percent.)[2/]

Table VIII.8 presents the extrapolated values for the various benefits and costs from the social perspective. The largest effect of

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[1/] This procedure is explained more fully and justified in Technical Report Q.

[2/] For more discussion, see Technical Report Q.

TABLE VIII.8

ESTIMATED TOTAL VALUE OF SOCIAL BENEFITS PER CORPSMEMBER
(1977 DOLLARS)

Benefit Component	Estimated Discounted Present Value[a]		
	Observation Period	Extrapolation Period	Total Benefits
1. Output Produced by Corpsmembers			
o In-program output	$ 757	$ 0	$ 757
o Increased postprogram output	1,933	1,343	3,276
o Increased postprogram tax payments[b]	0	0	0
2. Reduced Dependence on Transfer Programs			
o Reduced public transfers[b]	0	0	0
o Reduced administrative costs	108	64	172
o Increased utility from reduced welfare dependence	+	+	+
3. Reduced Criminal Activities			
o Total observed reductions in resource use	2,819	100	2,919
o Reduced psychological costs[c]	+	+	+
4. Reduced Drug/Alcohol Abuse			
o Reduced drug/alcohol treatment costs	31	0	31
o Increased utility from reduced drug/alcohol dependence[c]	+	+	+
5. Reduced Utilization of Alternative Services			
o Reduced costs of training and education programs other than Job Corps	251	-7	244
o Reduced training allowances[b]	0	0	0
6. Other Benefits			
o Increased utility from redistribution[c]	+	+	+
o Increased utility from improved well-being of Corpsmembers	+	+	+
Total Estimated Benefits	$5,899	$1,500	$7,399

NOTE: Details may not sum exactly to totals because of rounding.

[a] The postprogram amounts are discounted to the in-program period (fiscal 1977) at a 5 percent real annual rate.

[b] Item does not enter the social perspective (i.e., exclusively transfers between Corpsmembers and non-Corpsmembers).

[c] Item is not observed and cannot be estimated for the analysis.

extrapolation relates to earnings, for which the extrapolation increases the value of the benefit by approximately 70 percent. The smaller numbers for crime-reduction benefits and the use of alternative education and training programs reflect (1) the smaller Job Corps effects observed during postprogram months 43 to 48 for these benefits and (2) the higher fade-out rate assumed for them. The estimated total value of all the benefits is almost $7,400 per Corpsmember (in 1977 dollars).

C. COSTS

Table VIII.9 provides the breakdown of program costs by category and analytical perspective. There are three basic cost categories: program operating expenditures, the opportunity cost of Corpsmember labor, and the nonbudgeted costs other than for Corpsmember labor. The total social cost of Job Corps (i.e., excluding all transfers) is estimated to be $5,070 per Corpsmember, while the cost to non-Corpsmembers is $5,736 per Corpsmember enrolled. The difference is the net value of the transfers provided to Corpsmembers ($665 per Corpsmember).

1. Program Operating Expenditures

The breakdown of program operating expenditures into three components--center operating expenditures (excluding Corpsmember transfers), Corpsmember transfers, and the central administrative costs--reflects the different nature and sources of Job Corps expenditures. Center operating expenditures are costs to non-Corpsmembers and to society. These expenditures and figures were obtained from the Job Corps Financial Reporting System. The Corpsmember transfers were also obtained from the Job Corps Financial Reporting System, but they are not social costs;

TABLE VIII.9

SUMMARY OF ESTIMATED VALUE OF COSTS PER CORPSMEMBER,
BY ANALYTICAL PERSPECTIVE
(FISCAL 1977)

Cost Component	Estimated Value of Costs by Perspective		
	Social	Non-Corpsmember	Corpsmember[a]
1. Program Operating Expenditures			
o Center operating expenditures, excluding transfers to Corpsmembers	$2,796	$2,796	$ 0
o Transfers to Corpsmembers	0	1,208	-1,208
o Central administrative costs	1,347	1,347	0
2. Opportunity Cost of Corpsmember Labor During the Program			
o Foregone output	881	0	881
o Foregone tax payments	0	153	-153
3. Unbudgeted Expenditures Other than Corpsmember Labor			
o Resource costs	46	46	0
o Transfers to Corpsmembers	0	185	-185
Total Costs	$5,070	$5,736	$ -665

[a] Corpsmember benefits from transfers are shown here as negative costs.

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instead, they represent a transfer of resources from non-Corpsmembers to Corpsmembers.[1] Finally, data on the central administrative expenditures were provided by the Office of Management and Budget. These expenditures represent costs to both non-Corpsmembers and society as a whole.

2. Opportunity Cost of Corpsmember Labor

Youths who participate in Job Corps forego employment opportunities that otherwise they would have taken. The wages they would have earned are a cost to them of participating in Job Corps. This "opportunity cost" of Corpsmember labor is not balanced by corresponding benefits to non-Corpsmembers and thus enters the social benefit-cost calculation as a cost.[2]

Another way to view this cost is that, from society's point of view, the decision to enroll a person in Job Corps implies that the output that person would have produced in the absence of the program must now be foregone. The loss of this output is a net cost to society; the value of this foregone output is measured by the foregone earnings. As was the case in estimating the increase in output produced, the estimate of foregone

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[1] These transfers are expenditures for items that many Corpsmembers would have consumed in the absence of Job Corps (e.g., food, clothing, and housing) and, hence, can be assumed to be valued near the supply price.

[2] However, if the labor markets are in disequilibrium (i.e., if disadvantaged youths are unemployed in the labor market), non-Corpsmembers receive benefits by replacing Corpsmembers on jobs; thus, social costs are reduced. While replacement is obviously an important factor given the labor markets that Corpsmembers leave when they enter the program, we have no basis currently to estimate either the magnitude or value of replacement activities. Furthermore, we have no way to measure displacement during the postprogram period or to compare its value to that for replacement.

earnings includes the amount of fringe benefits in order to measure the total value of the lost output. We also estimate the fraction of this foregone output borne by non-Corpsmembers in the form of reduced tax payments (reduced taxes offset part of the lost earnings for Corpsmembers).

3. Nonbudgeted Costs Other than for Corpsmember Labor

The opportunity cost of Corpsmember labor described above is an unbudgeted item. In addition, there are other types of expenditures whose costs do not appear in the Job Corps financial accounts: government-surplus goods, for which the centers pay only transportation charges; meal costs reimbursed by the National School Lunch program; medical supplies and services provided by state and local agencies; and other resources acquired at below-market prices. The use of these resources is a cost to non-Corpsmembers and to society. However, the use of many of these items represents a transfer to Corpsmembers and, hence, does not enter the social perspective. The opportunity cost of these resources was estimated on the basis of special studies conducted at thirteen Job Corps centers (see Technical Report F for more details).

D. OVERALL FINDINGS FOR NET PRESENT VALUE

Once the various effects of Job Corps have been valued, calculating the net present value is straightforward. Table VIII.10 presents the values of the various benefit and cost components with their associated net present values from the three perspectives. As can be seen, the program yields net benefits to society and to Corpsmembers under our benchmark assumptions. From the social perspective, the increases in output and the

TABLE VIII.10

ESTIMATED NET PRESENT VALUES PER CORPSMEMBER UNDER THE BENCHMARK ASSUMPTIONS[a]
(1977 DOLLARS)

	Perspective		
	Social	Non-Corpsmember	Corpsmember
BENEFITS			
1. Output Produced by Corpsmembers			
o In-program output	$ 757	$ 673	$ 83
o Increased postprogram output	3,276	0	3,276
o Increased postprogram tax payments	0	596	-596
2. Reduced Dependence on Transfer Programs			
o Reduced transfer payments	0	791	-791
o Reduced administrative costs	172	172	0
o Increased utility from reduced welfare dependence	+	+	+
3. Reduced Criminal Activity			
o Reduced criminal justice system costs	1,253	1,253	0
o Reduced personal injury and property damage	1,366	1,366	0
o Reduced stolen property	300	462	-162
o Reduced psychological costs	+	+	+
4. Reduced Drug/Alcohol Abuse			
o Reduced drug/alcohol treatment costs	31	31	0
o Increased utility from reduced drug/alcohol dependence	+	+	+
5. Reduced Utilization of Alternative Services			
o Reduced costs of training and education programs other than Job Corps	244	244	0
o Reduced training allowances	0	33	-33
6. Other Benefits			
o Increased utility from redistribution	+	+	+
o Increased utility from improved well-being of of Corpsmembers	+	+	+
Total Benefits	$7,399	$5,621	$1,777
COSTS			
1. Program Operating Expenditures			
o Center operating expenditures, excluding transfers to Corpsmembers	$2,796	$2,796	$ 0
o Transfers to Corpsmembers	0	1,208	-1,208
o Central administrative costs	1,347	1,347	0
2. Opportunity Cost of Corpsmember Labor During the Program			
o Foregone output	881	0	881
o Foregone tax payments	0	153	-153
3. Unbudgeted Expenditures Other than Corpsmember Labor			
o Resource costs	46	46	0
o Transfers to Corpsmembers	0	185	-185
Total Costs	$5,070	$5,735	-$665
Net Present Value (Benefits minus Costs)	$2,327	-$115	$2,442
Benefit-Cost Ratio[b]	1.46	0.98	1.99

[a] See the text for a review of the assumptions, estimation procedures, and their implications for the values presented in this table.

[b] The numerators for the benefit-cost ratios include all of the benefits listed in this table as either positive benefits or negative costs, and the denominator includes all of the costs listed in this table as either positive costs or negative benefits.

crime-related benefits constitute the bulk of the benefits. The largest social costs are for the resources necessary to operate the program. The net present value for society is estimated to be $2,327 per Corpsmember in 1977.

Corpsmembers benefit primarily from the increase in their post-program earnings and from the program's transfer (shown as negative costs) in the form of room and board. Their major costs are the foregone earnings while they are in the program, as well as the reductions in the transfer payments that accompany their increases in earnings. The net present value from the Corpsmember perspective is estimated to be $2,442, on average. Non-Corpsmembers, who bear both the costs of program operation and the costs of the transfers to Corpsmembers, incur an estimated net cost of $115.[1/] They do receive substantial benefits from reductions in Corpsmembers' criminal activities, but these are not sufficient to outweigh their share of program cost. Thus, Job Corps is estimated to be a socially efficient use of resources and to lead to a net redistribution of resources from non-Corpsmembers to Corpsmembers.

The overall benchmark numbers are essentially unchanged from the benchmark estimates reported in the Second Follow-Up Report.[2/] The current

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[1/] Of course, because there are many more non-Corpsmembers than Corpsmembers, the net cost to non-Corpsmembers will be quite small, on average (much smaller than the $115 per Corpsmember). However, some non-Corpsmembers (e.g., recipients of the value of output and additional victims of crimes in the absence of Job Corps) might benefit substantially.

[2/] Mallar et al. (1978), Mallar et al. (1980), Technical Report D, and Technical Report K present the earlier results. Technical Report Q provides a more detailed comparison of the results in the three studies.

benchmark estimate of social net present value is only 2.5 percent greater than the earlier estimate of $2,271 per Corpsmember. However, both estimates are substantially greater than our initial net present value estimate of $251, which was calculated after only seven months of post-program observation. The changes reflect the reduced reliance on conservative extrapolation procedures and the substantial earnings gains that persist over time. In addition, two aspects of the current benchmark estimate reflect important changes in the estimates of the separate benefits and costs compared to the second follow-up: (1) the current estimates indicate that benefits exceed costs without extrapolating past the period of direct observation, and (2) the estimated increase in value of output produced (both in Job Corps and in postprogram employment) is slightly lower and the estimated value of crime reductions is substantially higher, which leads to the slightly higher overall estimate.

The data used in this report provide two years of observation beyond that available in the Second Follow-Up Report. The new data indicate that the substantial postprogram earnings gains estimated earlier persist to a large degree for at least two more years. Thus, we now observe earnings gains (including fringe benefits) of over $1,900 without any extrapolation. In addition, the large benefits from reductions in arrests total approximately $2,800 from the social perspective before extrapolation. When these two benefits are combined with the other observed benefits (including the value of output produced by Corpsmembers while they are in Job Corps), they are worth almost $5,900 per Corpsmember. The average social cost of Job Corps was approximately $5,100, so the net present value without any extrapolation is approximately $800.

Because this positive net present value estimate is based on direct observations rather than on extrapolation, we feel that more confidence can be placed in the positive overall finding than in the previous studies (i.e., the statement that benefits exceed costs does not rely on relatively imprecise extrapolation beyond the period of direct observation). In addition, an analysis of the time pattern of observed effects suggests that the average social investment per Corpsmember enrolled in Job Corps is paid back in approximately three years.

The new estimates of Job Corps' effects made for this report show a shift in the relative magnitudes of the earnings and crime-related benefits. Increased output produced by Corpsmembers accounts for 55 percent of the total benefits in the current benchmark estimate. This can be compared to a 63 percent share in the Second Follow-Up Report benchmark. An opposite shift in the estimate of crime-related benefits (from 29 percent to 40 percent) has left the overall social net present value estimate essentially unchanged, although the crime benefits are now relatively more important than in the previous estimate.

Despite the differences in the three reports, the major policy conclusion from all of them is the same--Job Corps is a socially desirable investment. The additional follow-up data have allowed more precise estimates of net present value to be made but have left the basic finding unchanged. The current analysis confirms the overall finding of the Second Follow-Up Report and provides a far greater level of confidence in the results.

As mentioned earlier, substantial uncertainty surrounds any single estimate of net present value. To better understand the extent of this

uncertainty, net present value estimates are made under a variety of alternative assumptions. Each alternative illustrates the effect of changing one assumption or estimate while maintaining the other benchmark assumptions and estimates. Table VIII.11 presents the benchmark net present value estimates and seven alternative estimates.[1/]

An examination of these alternative estimates (and some others reported in Technical Report Q) indicates that, while the point estimates of net present value are sensitive to changes in assumptions, the overall finding of economic efficiency remains under widely different sets of assumptions. Assumptions would have to be changed so as to eliminate virtually all the earnings-related benefits or the reduced crime benefits before the social conclusion would be changed.[2/] On average, Corpsmembers will receive a net benefit from Job Corps under almost any set of assumptions. However, the tests indicate that the negative net present value estimated for non-Corpsmembers could become positive if earnings gains faded out more slowly than assumed, or if more persistent long-run benefits occurred for crime.

The first three sets of alternative assumptions in Table VIII.11 concern the rate at which the Job Corps effects fade out over time. The first alternative has already been discussed and assumes that no effects

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[1/] Only brief summaries of the sensitivity tests are given here. More details and discussions of tests not presented here are provided in Technical Report Q.

[2/] If all of the effects were changed in the same direction, the overall estimates would obviously change considerably (see Technical Report Q).

TABLE VIII.11

ALTERNATIVE ESTIMATES OF NET PRESENT VALUE PER CORPSMEMBER
(1977 DOLLARS)

Alternative Assumptions	Perspective		
	Social	Non-Corpsmember	Corpsmember
1. Benchmark assumptions[a]	$2,327	-$115	$2,442
2. Effects are zero after the first 48 postprogram months (our observation period)	827	-792	1,618
3. Earnings and transfer effects do not fade out but other effects do fade out at the benchmark rate	5,653	1,238	4,415
4. Crime, drug, and alternative program effects fade out at the same rate as earnings effects (14 percent per year)	3,203	869	2,334
5. Three percent annual real discount rate	2,801	125	2,676
6. Ten percent annual real discount rate	1,438	-581	2 019
7. No underreporting of arrests	1,125	-1,384	2,509
8. Reduced crime benefits are zero after the in-program period	464	-2,035	2,498

[a] The benchmark assumptions are as follows: earnings, tax, and transfer effects fade out at a real rate of 14 percent per year; all other effects fade out at a real rate of 140 percent per year (which assumes that these other effects fade out to 25 percent of their base amount after one year and 1 percent after three years); the real discount rate is 5 percent per annum; the expected worklife of a Corpsmember is forty-three years after leaving Job Corps (i.e., until age 62). (For a more complete discussion of these and other assumptions, see Chapter V of Technical Report Q.)

exist other than those already observed by the end of the observation period (four years after leaving Job Corps). These estimates indicate that benefits exceed costs without extrapolation. Furthermore, these benefits indicate a real (i.e., net of inflation) internal rate of return for Job Corps of almost 18 percent per year. The second alternative assumption is that the earnings and corresponding tax and transfer effects do not fade out, while the other effects (reduced criminal activity, drug- and alcohol-treatment use, and education- and training-program use) fade out at the rapid benchmark rate. In this instance, the social, Corpsmember, and non-Corpsmember net present values are all positive, with the total social benefits outweighing social costs in 1977 dollars by over $5,600 per Corpsmember.

The third alternative illustrates the effect of lowering the rate at which effects on crime, drug treatment, and alternative education- and training-program use fade out. If these effects fade out at the lower rate assumed for earnings and transfers (14 percent per year), the estimated net present value would be greater than with the benchmark assumptions, and would be positive from all three perspectives. The social net present value would increase by 38 percent, to $3,203 per Corpsmember. Increasing the rate at which these effects are assumed to fade out has little effect on net present value because the future value of the estimated effects is small under the benchmark assumptions of an extremely rapid fade-out rate (approximately 140 percent per year).[1/]

The next two alternative estimates pertain to the discount rate.

[1/] For example, one year after the end of the observation period the value of these effects would be reduced by 75 percent under the benchmark fade-out assumptions. After two years, the value would be reduced by 95 percent; after three years, the value would be reduced by 99 percent.

The appropriate discount rate to use when evaluating government training and education programs is always a controversial issue because, while its choice is very important for the evaluation and is well established theoretically, there has never been a completely satisfactory way to estimate social discount rates. Imperfections in the markets for capital, the existence of risk and uncertainty and inflation, and the fact that many tax-incidence questions are still unresolved have made it impossible to determine an appropriate single discount rate to evaluate government investments. Consequently, we have adopted a 5 percent per annum real rate (i.e., net of inflation) as our benchmark.[1/] This number is in the range of discount rates used by other studies. Because of the somewhat arbitrary nature of this assumption, we test the sensitivity of the findings to variations in this assumption.

To undertake this test, we calculated net present value estimates using real discount rates of 3 and 10 percent per annum. As can be seen, the social net present value changes in the opposite direction from the discount rate. Lower discount rates increase the present value of social benefits, but leave social costs (all of which are incurred during the in-program time period) unchanged. Thus, using a 3 percent discount rate increases the social net present value by approximately $475 per Corps-member as compared to the benchmark estimate. In a similar manner, increasing the discount rate from the 5 percent benchmark rate to 10 percent decreases the social net present value by almost $900 per Corpsmember, although social net present value remains positive, $1,438.

[1/] For further documentation, see Technical Report Q.

The last two sensitivity tests presented in Table VIII.11 concern the estimation of the crime-reduction benefits. In particular, they examine the estimation of the reduction in arrests and the relative importance of the in-program and postprogram crime-reduction benefits. The benchmark procedure adjusts the interview arrest data to account for underreporting. However, the estimate of underreporting is subject to some uncertainty and might not be entirely appropriate for the sample of youths enrolled in Job Corps (although larger underreporting is also plausible).[1/] The effect of this adjustment is illustrated by estimating net present value using the unadjusted self-reported data on arrests. The resulting net present value estimates exhibit the same pattern as the benchmark values, but the social net present value estimate is 50 percent smaller (net present value from the Corpsmember perspective rises because of a reduction in the estimate of foregone theft income).

The large in-program reduction in all types of arrests create social benefits that exceed $1,000 per Corpsmember. These effects appear to be due to the intensive supervision provided by Job Corps, the legitimate alternatives provided to Corpsmembers while enrolled, and the lack of criminal opportunities at most Job Corps centers.[2/] These benefits are substantial, and we have strong confidence in our estimates of their value.

[1/] The underreporting estimate was made by examining official records and interview data that used questions and survey techniques similar to those used in the Job Corps interviews. However, the interview data used in the validation study were for a sample of slightly older ex-offenders and ex-addicts. Thus, while the results are the best available for our purposes, they have a large potential for error.

[2/] In many cases where Corpsmembers do commit crimes while enrolled in Job Corps, the costs of those crimes are included in center operating costs. To avoid double counting, the corresponding savings to the regular criminal justice system must be included as a benefit.

During the postprogram period, the crime-reduction effects of Job Corps are more erratic, with the benefits being generated as much by a shift in types of crime as by reductions in the overall level of crime. With less confidence in the postprogram estimates of crime effects, it is useful to assess their importance in the benefit-cost findings. The last row of Table VIII.11 presents estimates of the net present value under the assumption that postprogram crime benefits are zero. For the social and non-Corpsmember perspectives, this alternative assumption leads to sizable reductions in net present value. However, the resulting social estimate, $464 per Corpsmember, is still positive, which supports the finding of economic efficiency even under this extreme assumption.

Finally, the assumption that the estimated increase in a Corpsmember's total earned compensation (earnings plus fringe benefits) represents a gain in social output should be examined. This assumption is customary in benefit-cost analysis, but, under alternative assumptions, earnings increases might not represent net gains to society. The key determinants of whether the earnings increase are social benefits are the labor-market context in which Job Corps operates and the indirect program effects that can arise in those markets.[1/]

For Corpsmembers, any increase in their earnings is clearly a benefit. However, if their earnings and employment gains affect other workers, an increase in the total output produced might not occur at all

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[1/] Indirect labor-market effects, which are potentially complicated and far-reaching, are discussed in more detail in Technical Report Q, Kemper (1980), Johnson (1979), and Hall (1979).

or might be magnified. One possible indirect labor-market effect is that Corpsmembers displace other workers. In the extreme, Job Corps training might simply shuffle workers among a fixed number of jobs, with no net increase in output; in essence, the Job Corps experience would enable Corpsmembers to move to the front of the job queue. In this case, Corpsmembers would have higher earnings, but this gain would be at the expense of non-Corpsmembers who are displaced.

The other extreme is that Job Corps training might allow Corpsmembers to move from markets with an excess supply of labor to markets with an excess demand for labor. Withdrawing from an excess-supply market to participate in a program will not affect total output, since any labor withdrawn would be replaced immediately by a previously unemployed worker. From the perspective of society as a whole, the opportunity cost of Corpsmember labor during the program is zero in this case. When the Corpsmember becomes employed in the excess-demand market, the social value would now equal the full amount of the Corpsmember's earnings, rather than just the gain in earnings. Thus, the costs are reduced and the benefits increase, such that the net social present value increases substantially.

Measuring the extent and direction of possible indirect labor-market effects is beyond the scope of this research effort. All employment program evaluations face decisions that pertain to these labor-market effects; these decisions are all the more difficult because of the lack of evidence on the magnitude of these types of effects. We believe that the benchmark assumption--that Job Corps increases the productivity of Corpsmembers, and, thus, that the increase in their gross compensation

represents a social gain--is the most reasonable that can be made in the absence of specific information about indirect labor-market effects. However, the benchmark net present value estimates must be interpreted in relation to the possibility of indirect labor-market effects.

E. SUMMARY AND CONCLUSIONS

The principal issue analyzed in this chapter is whether the investment in the Job Corps program is economically efficient in the sense that it provides greater value to society than the value of resources used by Job Corps. Specifically, does society have more goods and services at its disposal because of the investment in Job Corps? The findings of this analysis suggest that public investment in Job Corps is economically efficient. Our benchmark estimate indicates that the social value of benefits in fiscal 1977 exceeds costs by over $2,300 per Corpsmember, or by approximately 45 percent of costs. In addition, the program is found to be efficient under a wide range of alternative assumptions and estimates. Because over 40,000 youths enrolled in Job Corps during fiscal 1977, our benchmark estimate indicates that the net social benefit exceeds $90 million for that year.

We estimate that over 50 percent of the social benefits are generated by increases in the value of output produced by Corpsmembers while they are in Job Corps and after they leave. Another 40 percent of the social benefits are attributable to reductions in criminal activities among Corpsmembers--particularly larceny and robbery. These benefits from reductions in crime include reductions in personal injury, property damage, stolen property, and criminal justice system costs and include substantial reductions in crime while the Corpsmembers are in Job Corps. The bulk of the social costs are incurred from operating the program.

While the analysis of social benefits and costs abstracts from the fact that members of society share disproportionately in the benefits and costs, the equity effects of the program are important. As a result, we also analyzed the benefits and costs of investments in Job Corps from the perspectives of both Corpsmembers and all other members of society (i.e., non-Corpsmembers). Our benchmark distributional estimates indicate that, on average, Corpsmembers receive a net benefit of $2,442 from participating in Job Corps. We estimate that non-Corpsmembers as a group incur a net cost of $115 per Corpsmember (or a few cents per non-Corpsmember).

Approximately 70 percent of the benefits to Corpsmembers are accounted for by their increased earnings. The other major benefits are the transfers they receive while they are in Job Corps. The major costs borne by Corpsmembers are the reduction in their transfer income and the earnings they forego while enrolled in Job Corps.

Non-Corpsmembers receive over $5,600 per Corpsmember in benefits, primarily from reductions in Corpsmembers' criminal activities the use of transfer programs. However, non-Corpsmembers incur almost all of the costs for operating and administering the program, including a large transfer to Corpsmembers while they are enrolled in Job Corps. Consequently, measured costs slightly exceed measured benefits from the non-Corpsmember perspective.

Estimating the present value of benefits and costs required numerous assumptions and approximations. Alternative benefit-cost estimates calculated for a wide range of assumptions confirm that Job Corps is an economically efficient program. They suggest that social benefits will exceed costs for Job Corps, as long as displacement in the labor markets

that Corpsmembers enter is not severe and as long as the observed crime reductions are at least minimally valued.

The benefit-cost results presented in this report are based on the additional data from the third follow-up survey. These updated results are very similar to those estimated in the Second Follow-Up Report (the social net present value estimates are within 3 percent of each other). However, because the more recent data indicate that social benefits exceed social costs without extrapolating benefits beyond the period covered by interview data, we feel that more confidence can be placed in the overall finding that Job Corps is economically efficient. In fact, the new data suggest that the social investment in Job Corps is paid back in approximately three years. They also indicate that the internal rate of return for social investments in Job Corps is almost 18 percent per year (net of inflation), even if no further benefits occur after the four-year period covered by our data.

IX. GENERALIZABILITY OF THE FINDINGS

One goal of our evaluation was to provide a general statement about the overall effectiveness of Job Corps as an employment and training program for disadvantaged youths. The statement was to be made on the basis of information obtained on the differences between a random sample of Corpsmembers and a carefully selected comparison group. However, several issues that pertain to this strategy could possibly affect our ability to generalize the findings from the evaluation sample to the Job Corps population as it was in 1977 when the sample was drawn, as well as to potential Job Corps populations of the future:

> Is our sample of Corpsmembers similar to the Job Corps population in 1977, to later Job Corps populations, and to more general populations of disadvantaged youths that might be of interest (at least similar enough to facilitate correct statistical inferences)?
>
> Has the Job Corps program changed in any major substantive ways since 1977?
>
> Has the social and economic background against which the Job Corps evaluation sample was observed changed in any ways that are likely to influence our estimates of the effectiveness of Job Corps?
>
> To what degree are our estimates biased by our having used a comparison sample rather than a "true" control group, or by other peculiarities of the analytical approach?
>
> And, finally, what is the overall quality of the data used in the evaluation?

Each of these important questions will be discussed in turn in the remainder of this chapter.

A. REPRESENTATIVENESS OF THE SAMPLE

In previous reports we compared the percentage distributions for several demographic characteristics of our Job Corps evaluation sample to those for the Job Corps populations in 1977 and in prior and subsequent years.[1] We found that the evaluation sample is, on average, similar to the 1977, prior, and subsequent populations of Corpsmembers in terms of sex, age, race/ethnicity, and educational background. Our sample should be very similar to the Job Corps population in 1977, since ours is a random sample of nearly one-third of this population. Compared to prior and subsequent years, as the 1980 Employment and Training Report of the President concluded (pp. 37-38):

> Characteristics of Job Corps enrollees have remained relatively constant over the years. In any 1 year, about 70 percent of the enrollees are male, and an equal proportion represent racial minorities. In addition, between 85 and 90 percent have not completed high school and about 50 percent read at a 6th-grade level or below (including 20 percent who are functionally illiterate).

It is reasonable to question further whether future generations of Corpsmembers will be affected by the Job Corps program in ways similar to our evaluation sample--that is, can we expect future enrollees to exhibit increased employability and earnings, and to derive the other benefits that were found for our evaluation sample? Because our observations show no evidence that contradicts the underlying theory of the traditional economic models of behavior tested in our analysis, we have

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[1] See Kerachsky and Mallar (1978) and Mallar et al. (1980).

no reason to believe that the behavior of future Job Corps participants will contradict this theory and thus, as compared with our evaluation sample, be affected in any qualitatively different way by the program. Of course, it is unlikely that the exact or nearly identical numerical estimates of the effectiveness Job Corps would be replicated.

B. PROGRAMMATIC CHANGES

In 1977, the Job Corps program was mandated to double in size--from 22,000 program slots to 44,000. However, after achieving over 86 percent of this expansion (up to 41,000 slots in fiscal 1981), plans are now being considered to scale back the program and reduce the amount of program slots to the earlier number of 22,000. The expansion entailed adding centers in high-poverty, high-unemployment areas, and encouraging the enrollment of women (especially those with children), Hispanics and American Indians, handicapped individuals, and other target groups. However, as noted above, the overall demographic composition of Job Corps changed very little between fiscal 1977 and the present.

The education and training components of the program have been enhanced somewhat by an expansion of Job Corps opportunities--more positions in junior colleges and, for individuals who had previously failed to meet military admissions requirements, specialized preparation for military service. In addition, efforts have been made to strengthen the ties between Job Corps and other employment programs. It would be difficult to imagine that any of these changes would have a negative effect on the ability of Job Corps to improve employment opportunities for disadvantaged youths. In sum, the essence of the Job Corps program has not changed since 1977, although some improvements have been made that would

probably strengthen some of our overall findings on the beneficial impacts of Job Corps.

C. SOCIAL AND ECONOMIC CHANGES

In terms of the social and economic context against which the program was set, the employment rate for youths in the regular labor market (i.e., other than special government programs for unemployed youths) has not improved since the sample period, might even have become worse, and does not look promising for the immediate future. Economic fluctuations no doubt affect Job Corps impacts, and this could be studied further using the differences in economic conditions over time and across sites in our samples. However, to some extent, this is a separate issue and one over which Job Corps exercises no control. Generally, fiscal 1977 was relatively typical of the recent labor experiences of disadvantaged youths. In addition, relatively constant Job Corps effects occur across the four years of our postprogram observation period, despite large cyclical fluctuations in the underlying economy and labor markets.

Furthermore, many more alternative employment and training programs for disadvantaged youths existed between 1978 and 1980 than in 1977. However, our analysis has shown that Job Corps had a larger impact on earnings than did other training programs available to our sample, and that it was an efficient social investment. (See Technical Report M for comparisons of Job Corps with other programs, and Chapter VIII for the most up-to-date and comprehensive results for the benefit-cost analysis.) We conclude that, although the Job Corps program has continued to grow and change and the economy and the general plight of disadvantaged youths might have become worse, our estimates of the benefits of Job Corps participation

can be generalized, at least in broad implications, to potential future generations of Corpsmembers, and might in fact be somewhat understated for them due to program improvements.

D. THE VALIDITY OF THE COMPARISON SAMPLE STRATEGY

In a perfectly controlled experiment, individuals are randomly assigned either to a group that receives a treatment or to a control group that receives no treatment. This randomization ensures that the between-group differences measured after treatment can be attributed to the treatment and not to unobserved differences between the groups. However, randomization was not possible for the evaluation of an ongoing program in the context of Job Corps. Therefore, instead of using a true control group, we selected a comparison sample that matched our participant sample as closely as possible.

A two-stage process was used to select comparison-group members. In the first stage, fifteen areas of the country were chosen on the basis of their similarity to the areas in which Corpsmembers lived, but in which Job Corps did not recruit extensively. In the second stage, 100 youths at each site were then selected from school drop-out and employment-service lists, to yield a sample of 70 percent young, recent dropouts, and 30 percent older dropouts who had been out of school longer (the assigned selection probabilities ensured comparability in terms of educational levels, age, race/ethnicity, etc.).

Regression analysis was used to control for differences between the participant and comparison samples with respect to various demographic and socioeconomic characteristics, such as age, sex, race/ethnicity, education, prior health, prior drug use, criminal history, and prior employment.

Other econometric techniques were used to control for unmeasured preprogram traits, such as motivation and innate employability. (See Chapter III for a more detailed discussion of the econometric methodology used in the evaluation.)

Even though we were able to control for inherent differences between participants and comparison-group members with respect to both measured and unmeasured preprogram characteristics, we were forced to make a second departure from the controlled experimental design. While participants were receiving the Job Corps "treatment," comparison-group members were not treatment-free (many other employment and training programs were potentially available to them). However, as mentioned earlier, we found that participation in other education and training programs was not substantial, and that Job Corps had a significantly larger impact on earnings than other training programs available to our sample. Therefore, we conclude that the comparison sample provided an adequate, although somewhat nebulous, standard against which the effectiveness of Job Corps could be measured.

E. DATA QUALITY

When analysis data are culled from survey interviews, data quality always boils down to two questions. First, are the results biased by our inability to interview certain individuals, either because they could not be located or refused to be interviewed? Second, given that we are able to interview an individual, how accurate are the responses recorded for him or her?

An investigation into the problems associated with nonresponse to Job Corps evaluation interviews showed that overall response rates were

relatively high. Altogether, nearly 90 percent of the sample responded to at least one follow-up interview and were thus available for the analysis of postprogram behavior. Even though the response rate for Corpsmembers was as much as 15 percentage points lower than it was for comparison-group members (e.g., at second follow-up, when we did not attempt in-person interviews for half of the Corpsmembers who could not be interviewed by telephone), we did not find that nonresponse led to biased estimates of the impact of Job Corps on employment, earnings, or frequency of arrests. (For details on this investigation, see Technical Report L, "An Analysis of Nonresponse to Job Corps Evaluation Interviews.") If anything, nonresponse adjustments tend to show slightly larger impacts than we have presented.

It should also be noted that the second and third follow-up interviews were administered to some individuals in person and to some by telephone, whereas the baseline and first follow-up interviews were all administered in person. Due to budget restrictions, the sample eligible for second and third follow-up interviews were first attempted by telephone. Then, for most of the sample,[1/] if a telephone interview could not be completed, additional attempts were made to interview them in person. Previous studies that compare the quality of data from telephone interviews with those from in-person interviews have found minimal differences for the type of data that we collected. No substantive differences have been found in individuals' cooperation for these two modes

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[1/] In the second follow-up, in-person attempts were scheduled for only one-half of the Job Corps group; however, in the third follow-up, in-person attempts were scheduled for everyone except those (15 percent) who lived in remote locations (see Technical Report P).

of interview, and the two different modes do not appear to affect the accuracy or reliability of reporting to any great extent for the type of data we collected (see Groves, 1977; Rogers, 1976; and Colombotos, 1969).[1]

Not surprisingly, we found that a combined telephone and in-person approach resulted in substantially higher response rates (an average of approximately 25 percentage points more) than for a telephone contact only (see Technical Reports L and P). Furthermore, as mentioned above, nonresponse did not seem to lead to biased estimates of program impacts. Finally, item nonresponse was very low (almost nonexistent, except for some recall problems at baseline). With the follow-up interviews, for example, almost every data field was fully complete, despite both the large number of questions and the existence of several data items that served only as interviewer checks.

F. OVERALL

While the exact estimates and single numbers do not generalize very well, we are relatively confident about the broad implications of our findings for disadvantaged youths in general and for Job Corps in particular. The evaluation has largely been successful within its narrow range of objectives (including the development of innovative procedures for comparison-group methodologies). Furthermore, useful data have been provided for additional research on the difficult employment problems faced by disadvantaged youths.

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[1] The data quality appears similar for our surveys, but this direct evidence is somewhat doubtful, since respondents were not assigned randomly to telephone and in-person interviews.

X. SUMMARY AND CONCLUSIONS

Information obtained in the baseline survey through the second follow-up interviews indicated that the Job Corps program was successful in the short run at achieving its primary objective--to improve the economic prospects of Corpsmembers--and that, on average, it appeared to be a worthwhile investment of public funds. Of course, these two primary findings (especially the latter) were severely constrained by the short length of that previous postprogram observation period (between 12 and 24 months, with an average per Corpsmember of approximately 18 months) and, hence, by the necessity of relying on imprecise extrapolation procedures to judge the economic success of the program.

Thus, prior to the third follow-up, relatively little empirical evidence existed on the longevity of the economic impact of the Job Corps program on participants and its overall economic effect upon society. For example, it was thought that the effects might decline over time (as they had for some previous adult employment and education programs) or, in contrast, grow further over time--especially for a program such as Job Corps, which, by causing its participants to increase postprogram investments in human capital, could lead to future increases in earnings. Furthermore, the weak empirical evidence precluded a reliable, precise projection of its benefits into the future, and we could not place much confidence in the results of the benefit-cost findings.

Thus, the value of the third follow-up is obvious: occurring nearly two and a half years ago after the second follow-up survey, it significantly extends the postprogram observation period, thereby yielding

a sufficient sample, a sufficient time period of observation, and, hence, sufficient information/data to obtain more policy-relevant and reliable empirical estimates of the effect of Job Corps on participants.

With this wealth of information, numerous findings have been discussed in this report. These findings were based on estimates made of the difference between the postprogram behavior of Corpsmembers and what their behavior would have been had they not participated in Job Corps (which includes alternative education, training, and work experience that they forego in favor of participating in the program), and included a variety of impacts--some significant, some insignificant. However, throughout, they have been reported in detail to ensure that informed policymakers can form their own judgments and interpret the information in alternative ways. The purpose of this chapter is to summarize our discussion and to break down our evaluation into several areas that can be thought of as highlighting the contents of the report: the longevity or persistence of effects; the consistency of findings with the hypothesized effects of the program; the most noteworthy effects; the differential impacts of the program; the sensitivity of impact estimates to alternative econometric specifications; and the timing, distribution, and findings of the benefit-cost analysis.

A. LONGEVITY OF EFFECTS

The positive overall impacts generally persist throughout the four years of the postprogram observation period. The trend over the four-year period (as shown in Figures 1 and 2, and Table 1) appears to indicate an increase in program benefits during the first few months (especially for employment and earnings during the period of transition from center life to

re-entering the regular labor market), and then relatively stable effects throughout the rest of the four-year period.

However, the evidence is mixed as to whether the program effects were growing or shrinking toward the end of the observation period. When only civilian jobs are considered and a simple linear time trend is assumed for the economy in general, the empirical estimates show a substantial shrinkage of the employment and earnings effects for the fourth postprogram year. However, when military jobs are included (as we believe they should be) and better account is taken of the time trends in the economy, the employment effects are relatively constant for the fourth postprogram year, and the earnings effects show a sizable (21 percent) growth. Furthermore, for males--the largest group of Corpsmembers (70 percent of all Corpsmembers)--we have adequate observations to extend our analysis further, from 48 to 51 postprogram months. We find a significant upturn in the estimated effects for males with respect to both employment and earnings during that time period. (Through month 48, the estimated effects for females without children are similar on average to males, but are more erratic; the estimated effects for females with children, the smallest group, are much lower and more erratic than males.) In light of all this, we believe that the most prudent conclusion about the longevity of Job Corps effects is that they persist at a relatively stable rate from approximately three months after termination until the end of the four-year observation period; beyond that point, our ability to extrapolate is very limited.

B. CONSISTENCY OF FINDINGS WITH HYPOTHESIZED EFFECTS

The estimated effects of Job Corps on former participants' postprogram behavior are generally consistent with hypothesized economic impacts and the primary goal of the program--to improve the economic prospects of Corpsmembers. During the four postprogram years, we find that Job Corps is at least moderately successful in achieving its desired effects of (1) increasing employment and earnings, (2) improving future labor-market opportunities through work experience, military service, higher education and training, better health, and geographic mobility, (3) reducing dependence on welfare assistance and other public transfers, and (4) reducing criminality.

C. NOTEWORTHY EFFECTS

In terms of statistical significance and size, some of the most important impacts of Job Corps on the behavior of former participants (on a per-Corpsmember basis and including military jobs) are (1) an increase in employment of nearly four weeks per year, (2) an increase in earnings of over $600 per year, (3) a very substantial increase in the probability of obtaining a high school diploma or its equivalent, (4) higher college attendance, (5) a decrease in high school attendance associated with more high school degrees, (6) better health, with a reduction in serious health problems of over one week per year on average, (7) a reduction in the receipt of financial welfare assistance amounting to over two weeks per year on average, and (8) a reduction in the receipt of Unemployment Insurance of nearly one week per year. The crime effects are erratic over the postprogram period, yet, on average, exhibit little overall decline (after a large decline during the program) but a shift from more to less serious crimes (fewer thefts and more traffic offenses).

D. DIFFERENTIAL IMPACTS

Differential impacts among Corpsmembers are found to be associated with sex and child responsibility. Relatively larger impacts for males are found for the probability of being in the military service (more than doubled by the end of our observation period) and the receipt of Unemployment Insurance, while relatively larger impacts for females who have no children present are found for education, health, and the receipt of welfare. The estimated Job Corps impacts for females who have children living with them are generally much less positive than for either males or females without children. This can possibly be attributed to delays in childbirth among Corpswomen, so that they are more likely to be faced temporarily with labor-market constraints from pregnancy (which also increases their reporting of health problems) and from having very young children during the period of postprogram observation.

Differential impacts are also found among categories of program completion. A substantial, positive correlation is found between the estimated Job Corps impacts and the proportion of the Job Corps program completed. Program completers consistently benefit the most, particularly in terms of employment, earnings, and welfare dependence (more than double the overall impacts). Early program dropouts are found to benefit little or not at all. Furthermore, these differential impacts by completion category seem to be attributable in part to the effect of remaining in the program longer and completing the program, which indicates the potential for additional benefits to the program from increasing participants' lengths of stay and obtaining more completions.

E. ROBUSTNESS OF IMPACT ESTIMATES

In checking the sensitivity of our findings to alternative econometric specifications, we find that (1) adding controls for differences in marital status (even pre-enrollment values) makes the estimates consistently more favorable for Job Corps among all three major sex and child-responsibility groups (males, females with children, and females without children present), (2) adding controls for differences in the age composition of children makes the findings much more positive for females with children, and (3) not controlling for differences between the Job Corps and comparison groups makes the findings much less favorable for males, much more favorable for females with children, and barely changes the findings at all for females without children.

F. BENEFITS AND COSTS OF THE PROGRAM

Alternative benefit-cost estimates have been made for a wide range of assumptions. A sensitivity analysis based on this range of alternatives generally confirms that Job Corps is an economically efficient program. As long as displacement in the labor markets that Corpsmembers enter is not severe and the observed crime reductions are minimally valued, Job Corps is estimated to provide net economic benefits to society.

1. Timing of Benefits

The estimated timing of benefits suggests that the average social investment per Corpsmember is paid back in approximately three years. With the estimated benefits for the first four postprogram years, Job Corps has an internal rate of return of approximately 18 percent under the assumption that no further benefits occur after that point.

2. The Distribution of Benefits

In assessing the distribution of benefits and costs, we find a net transfer from non-Corpsmembers as a group (everyone in society other than Corpsmembers) to Corpsmembers. The primary economic benefits to Corpsmembers are derived from increased earnings (approximately 70 percent of the benefits) and transfers received while they are enrolled in Job Corps. The primary economic benefits to non-Corpsmembers are derived from reductions in Corpsmembers' criminal activities, Corpsmembers' reduced use of transfer programs, and increased tax payments.

3. Numerical Findings

The findings gleaned from a comprehensive evaluation of the Job Corps program suggest that the program is a worthwhile public investment. Our benchmark estimate is that benefits to society exceed costs by over $2,300 per Corpsmember (in fiscal 1977 dollars), or, equivalently, by approximately 45 percent of costs. Thus, Job Corps is an economically efficient use of public resources in the sense that the program provides a greater value to society than the value of the resources it consumes. Because over 40,000 youths enrolled in Job Corps during fiscal 1977, our benchmark estimate of the net social benefit for the entire program exceeds $90-million for that year.

We estimate that over 50 percent of the social benefits are derived from the increased value of output produced by Corpsmembers. Another 40 percent of the social benefits are attributable to reductions in criminal activities among Corpsmembers, particularly murder, larceny, and robbery (including substantial reductions of these and burglary crimes during the program).

The benefit-cost findings based on the additional data from the third follow-up interview are very close to those estimated in the Second Follow-Up Report (the social net present value estimates are now higher, but by less than 3 percent). However, because benefits are now estimated to exceed costs without extrapolating benefits into the future, we feel that more confidence can be placed in the overall finding that Job Corps is an economically efficient investment.

G. SUMMATION

While the estimates presented above are not exact, and while any single number will not generalize very well, after a careful analysis we are relatively confident about the broad implications of our findings for disadvantaged youths in general and for the Job Corps program in particular.

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