Contract No.: 600-83-0227
MPR Reference No.: 7573

THE TRANSITIONAL EMPLOYMENT TRAINING DEMONSTRATION: ANALYSIS OF PROGRAM IMPACTS

July 1989

Authors:

Craig Thornton
Paul Decker

Editor:

Thomas Good

Prepared for:

Social Security Administration
Baltimore, MD 21235

Project Officer:
Aaron J. Prero

Prepared by:

Mathematica Policy Research, Inc.
P.O. Box 2393
Princeton, NJ 08543-2393

Principal Investigator:
Craig Thornton

ACKNOWLEDGMENTS

Large demonstrations and evaluations happen only through the joint efforts of many individuals, and in the seven years since its conception the Transitional Employment Training Demonstration has not been an exception. It had its origins in the urgings of the Association for Retarded Citizens and Transitional Employment Enterprises, which encouraged the Social Security Administration to use the demonstration authority granted in the Social Security Disability Amendments of 1980 to provide transitional-employment services to increase the self-sufficiency of Supplemental Security Income (SSI) recipients with mental retardation. In response, a preliminary meeting was held in Baltimore on September 22, 1982, to discuss the initial design of the demonstration and its evaluation, which were developed by Aaron Prero, with the assistance of Vincent Geraci.

Mathematica Policy Research (MPR) was awarded the design and evaluation contract in September 1983. Our subsequent efforts were assisted by an extremely helpful advisory panel, consisting of Robert Bruininks, Gary Clark, Frank Rusch, and Paul Wehman. We have also benefited from the ongoing assistance of the government project officer, Aaron Prero.

We also owe much to our colleagues at MPR who assisted in the design and conduct of the evaluation. In particular, we are indebted to Shari Miller Dunstan and Jennifer Schore, whose analysis of demonstration operations shaped our thinking about the delivery of transitional-employment services. In addition, David Long, Debi Slatkin, and Paul Rynders helped collect the implementation information and guided the demonstration through its initial stages. George Carcagno, Anne Ciemnecki, Alan Hershey, Stuart Kerachsky, and Susan Stephens helped develop the demonstration design and the data collection procedures. They also helped read and assess the eighty proposals that were submitted by organizations that wanted to operate demonstration projects.

The data collection work for the evaluation was directed by Anne Ciemnecki and Roderick Pannell. Joan Mattei constructed the data bases and programmed the statistical work. Thomas Good edited the report, and the production of the report (as well as the production of the hundreds of preceding memos and drafts) was managed by Marjorie Mitchell, with assistance

from Donna Adubato, Monica Capizzi, Stephanie Collins, Debbie Jones, and Yvette McKnight.

We have also benefited more than we can say from our work with the staffs of the eight training organizations that operated the transitional-employment programs: AHEDD, Inc.; the Association for Retarded Citizens, Monmouth Unit; The CENTER for the Rehabilitation and Training of the Disabled; The Children's Hospital of Boston, Massachusetts; Exceptional Children's Foundation; Goodwill Industries, Milwaukee Area; the University of Washington and the Portland Employment Program at Portland Community College; and the Vocational Development Center at the University of Wisconsin, Stout.

Finally, we want to acknowledge the contributions made by the men and women who volunteered to be in the demonstration research sample. This report is dedicated to them and to the hope that it will help improve the delivery of vocational services and thus improve the quality of life for them and other persons who face similar challenges.

Craig Thornton
Paul Decker

Princeton, New Jersey
July 1989

CONTENTS

Chapter		Page
	EXECUTIVE SUMMARY	ix
I.	THE TRANSITIONAL-EMPLOYMENT TRAINING DEMONSTRATION	1
	A. TRANSITIONAL EMPLOYMENT SERVICES IN THE DEMONSTRATION	5
	1. The Transitional-Employment Model	8
	2. SSI Rules in the Demonstration	12
	3. The Implementation of the Demonstration Model	13
	B. THE EVALUATION DESIGN AND THE ANTICIPATED IMPACTS OF THE DEMONSTRATION	19
	1. Making the Policy Comparison: The Experimental Design	20
	2. The Sample Size for the Evaluation	21
	3. Data Sources	24
	4. Expectations about the Effects of Transitional-Employment Service	28
II.	THE EXPERIENCE OF SAMPLE MEMBERS IN THE ABSENCE OF THE DEMONSTRATION	33
	A. THE DEMONSTRATION SAMPLE RECRUITMENT PROCESS	34
	B. THE PRE-ENROLLMENT CHARACTERISTICS OF THE SAMPLE MEMBERS	38
	C. A COMPARISON OF THE RESEARCH SAMPLE WITH THE ELIGIBLE POPULATION	47
	D. THE ACTIVITIES OF SAMPLE MEMBERS IN THE ABSENCE OF THE DEMONSTRATION	49
	1. Service Use	50
	2. Income and Earnings	52
III.	IMPACTS OF THE DEMONSTRATION	57
	A. STATISTICAL METHODOLOGY	59
	B. IMPACTS DURING THE FIRST 24 MONTHS AFTER ENROLLMENT	61
	1. Net Impacts on the Use of Employment Services	61
	2. Impacts on Earnings and SSI Receipt	64

CONTENTS (continued)

Chapter		Page
Chapter III (continued)		
	C. ESTIMATED IMPACTS AFTER THE FIRST 24 MONTHS FOLLOWING ENROLLMENT	78
	1. Estimated Impacts Derived from Social Security Records	79
	2. Estimated Impacts at the Time of the Interview	82
	D. ESTIMATED IMPACTS FOR SUBGROUPS OF THE SAMPLE	88
	1. Differences in Earnings Impacts across Projects	92
	2. Differences in Earnings Impacts across Other Subgroups	98
	E. THE DECAY OF IMPACTS OVER TIME	101
IV.	CONCLUSIONS AND IMPLICATIONS	107
	A. DEMONSTRATION OUTCOMES FROM THE PERSPECTIVE OF THE TREATMENT-GROUP MEMBERS	108
	B. DEMONSTRATION OUTCOMES FROM THE PERSPECTIVE OF THE SSI BUDGET	109
	C. DEMONSTRATION OUTCOMES FROM THE PERSPECTIVE OF ALL GOVERNMENT BUDGETS	112
	D. DEMONSTRATION OUTCOMES FROM THE PERSPECTIVE OF SOCIETY AS A WHOLE	116
	E. SUGGESTIONS FOR FUTURE RESEARCH	117
REFERENCES		123
APPENDIX A:	SURVEY PROCEDURES AND RESULTS	A.1
APPENDIX B:	STATISTICAL METHODOLOGY	B.1
APPENDIX C:	SUPPLEMENTAL TABLES	C.1

TABLES

Table		Page
I.1	MILESTONES IN THE PLANNING AND IMPLEMENTATION OF THE TRANSITIONAL EMPLOYMENT TRAINING DEMONSTRATION	6
I.2	DEMONSTRATION PROJECTS AND SITES	15
I.3	DATA SOURCES USED IN THE IMPACT ANALYSIS	25
II.1	CHARACTERISTICS OF THE RESEARCH SAMPLE AT ENROLLMENT, BY RANDOMIZATION STATUS	40
II.2	A COMPARISON OF THE CHARACTERISTICS OF SAMPLE MEMBERS WITH THE CHARACTERISTICS OF THOSE INDIVIDUALS WHO WERE SENT INVITATION LETTERS	48
III.1	ESTIMATED IMPACTS ON MONTHS OF SERVICE RECEIPT DURING THE FIRST 24 MONTHS AFTER ENROLLMENT	63
III.2	ESTIMATED IMPACTS ON THE PERCENTAGE OF SAMPLE MEMBERS WHO RECEIVED SERVICES DURING THE FIRST 24 MONTHS AFTER ENROLLMENT	65
III.3	ESTIMATED IMPACTS ON EARNINGS AND INCOME BY SOURCE DURING THE FIRST 24 MONTHS AFTER ENROLLMENT	66
III.4	ESTIMATED IMPACTS ON EARNINGS AND INCOME BY SOURCE DURING THE FIRST 12 MONTHS AFTER ENROLLMENT	76
III.5	ESTIMATED IMPACTS ON EARNINGS AND INCOME BY SOURCE DURING MONTHS 13 THROUGH 24 AFTER ENROLLMENT	77
III.6	ESTIMATED IMPACTS ON EARNINGS AND INCOME BY SOURCE DURING MONTHS 25 THROUGH 36 AFTER ENROLLMENT	80
III.7	ESTIMATED IMPACTS ON EMPLOYMENT AND EARNINGS FOR PRINCIPAL JOB AT THE TIME OF THE INTERVIEW	84
III.8	ESTIMATED IMPACTS ON LIVING ARRANGEMENT AT THE TIME OF THE INTERVIEW	89

TABLES (continued)

III.9	ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS BY PROJECT DURING MONTHS 1 THROUGH 12 AFTER ENROLLMENT	93
III.10	ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS BY PROJECT DURING MONTHS 13 THROUGH 24 AFTER ENROLLMENT	94
III.11	ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS BY PROJECT DURING MONTHS 25 THROUGH 36 AFTER ENROLLMENT	95
III.12	ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS FOR KEY SUBGROUPS OF THE SAMPLE DURING MONTHS 1 THROUGH 24 AFTER ENROLLMENT	99

FIGURES

Figure		Page
I.1	THE GENERAL FLOW OF DEMONSTRATION SERVICES	9
II.1	PERCENT OF CONTROL GROUP RECEIVING EACH OF THREE SERVICES MONTH 1 TO 36 AFTER ENROLLMENT	51
II.2	AVERAGE MONTHLY INCOME BY SOURCE, MONTH 1 TO 36 AFTER ENROLLMENT, CONTROL GROUP	53
III.1	AVERAGE MONTHLY EARNINGS FOR TREATMENT AND CONTROL GROUPS MONTH 1 TO 24 AFTER ENROLLMENT	68
III.2	AVERAGE MONTHLY SSI RECEIPT FOR TREATMENT AND CONTROL GROUPS MONTH 1 TO 24 AFTER ENROLLMENT	70
III.3	AVERAGE COMPUTED MONTHLY SSI PAYMENT FOR TREATMENT AND CONTROL GROUPS MONTH 1 TO 24 AFTER ENROLLMENT	72
III.4	AVERAGE TOTAL MONTHLY INCOME FOR TREATMENT AND CONTROL GROUPS MONTH 1 TO 24 AFTER ENROLLMENT	74
III.5	AVERAGE MONTHLY EARNINGS FOR TREATMENT AND CONTROL GROUPS MONTH 1 TO 40 AFTER ENROLLMENT	104

THE TRANSITIONAL EMPLOYMENT TRAINING DEMONSTRATION: ANALYSIS OF PROGRAM IMPACTS

EXECUTIVE SUMMARY

Background

The Transitional Employment Training demonstration was conducted to determine the effectiveness of transitional employment as a vehicle for enhancing the economic self-sufficiency of Supplemental Security Income (SSI) recipients with mental retardation.

Specifically, the demonstration sought to assess the extent to which the provision of time-limited placement, on-the-job training, and support services could increase the employment and earnings and reduce the SSI payments of SSI recipients with mental retardation.

Definition of Transitional Employment

As implemented in the demonstration, transitional employment consisted of three core services intended to help SSI recipients with mental retardation obtain and hold "competitive" jobs--that is, economically productive jobs that are essentially undifferentiated from other jobs that exist in the economy. The three core services were:

- Placement on potentially permanent competitive jobs
- Specialized on-the-job training that was gradually phased out over time to enable the SSI recipient to gain independence on the job
- Postplacement support and follow-up as necessary for job retention

The other distinguishing feature of transitional employment is that services were time-limited. In the demonstration, the core services were to be provided within one year after an SSI recipient enrolled in the demonstration. Long-term job-retention services were to be arranged as necessary, but services that were provided beyond the one-year period were to be funded by a source other than the demonstration.

Previous Evidence about Transitional Employment

In the last fifteen years, transitional-employment and similar programs have become an established part of the vocational service system for persons with mental retardation. The growing interest in this service model reflects the efforts of a number of program operators, advocates, and researchers to demonstrate the feasibility and potential of transitional employment.

The demonstration built on this earlier work by testing transitional employment on a large scale for a wide range of persons whose level of impairment was sufficiently severe to meet the SSI eligibility requirements. In addition, an evaluation was conducted as an integral part of the demonstration in order to provide a rigorous basis for assessing the effectiveness of transitional employment.

The Evaluation Design

The evaluation was based on an experimental design to measure the net effect of introducing transitional employment into the existing service system. Program applicants were assigned randomly to either a treatment group (that was offered the demonstration services) or a control group (that was precluded from receiving demonstration services, but was free to seek any other services available in the community). The activities of the treatment-group members indicate what happens to persons who are offered transitional employment. The experience of the control-group members indicates what would have happened to the treatment-group members in the absence of the demonstration. Because the random assignment process ensures that the pre-enrollment characteristics of the two groups are identical, any postenrollment differences between the groups can be attributed to the demonstration services.

Persons Served in the Demonstration

The demonstration targeted SSI recipients with mental retardation for several reasons. First, previous research indicated that transitional employment could help such persons obtain and hold jobs. Second, persons with mental retardation constitute one of the largest recipient groups, accounting for approximately 25 percent of all adult disabled SSI recipients and for approximately $2.4 billion per year in SSI payments. Third, transitional employment was seen as a vehicle for helping SSI recipients with mental retardation reach their goal of achieving greater integration into economic and community life.

Eligibility Criteria

Applicants to the demonstration were to be SSI recipients who were between 18 and 40 years old with a diagnosis of mental retardation. In addition, they were to be living in one of the communities served by the demonstration.

Recruitment Process

The case folders of approximately 25,000 SSI recipients were screened to identify recipients with mental retardation who lived in one of the thirteen demonstration communities. Invitation letters that described the demonstration were mailed to virtually all of the 13,800 determined to be eligible. In addition, follow-up letters, telephone calls, and outreach to service providers in the communities were also used to recruit persons for the demonstration.

Intake workers described the available demonstration services to all applicants and explained that participation in the demonstration was strictly voluntary. The intake workers also collected basic information about the applicant. If the applicant consented to participate and the intake worker decided that the applicant could be served, the applicant was formally enrolled in the demonstration.

A total of 745 SSI recipients with mental retardation were enrolled in the demonstration; 375 of these persons were assigned randomly to the treatment group, and 370 to the control group.

The SSI recipients who were enrolled in the demonstration represent approximately 5 percent of the persons who were sent initial invitation letters. While the recipients who were enrolled in the demonstration are not a representative sample of all SSI recipients with mental retardation, they are a group who are indicative of the recipients who would volunteer for transitional-employment services.

Characteristics of the SSI Recipients Enrolled

The average age of the persons enrolled in the demonstration was 27 years; 22 percent were younger than age 22, and 10 percent older than age 35. Forty-one percent of the persons who were enrolled were female, and approximately 30 percent were black.

The average IQ score of the persons who enrolled was 57. The general distribution of IQ scores for the persons who enrolled was:

10 percent greater than 70
49 percent between 55 and 70
35 percent between 40 and 55
6 percent below 40

Many also exhibited physical, social, or emotional problems that could be expected to impair their ability to function in the labor market.

Approximately a third of the persons who were enrolled had no vocational activity during the year prior to their application, and another third had been in sheltered workshops. Only 10 percent of the persons had held a competitive job in the previous year. Overall, the average earnings of these persons in the previous year was only $450.

SSI benefits accounted for almost 75 percent of the total annual income of the persons who enrolled. On average, these persons had received over $3,638 in SSI benefits during the year prior to enrollment.

Summary of Demonstration Operations

Demonstration operations began in June 1985. Persons were enrolled throughout the following year, and transitional-employment services were provided through June 1987.

The Projects

Transitional-employment services in the demonstration were provided by eight organizations that were awarded grants by the Social Security Administration. The eight were selected from the 80 training providers that submitted proposals in a competitive process. The eight projects (three were universities or university affiliates, three were local units of the Association for Retarded Citizens or Goodwill, and two were independent rehabilitation organizations) provided services in thirteen communities across the country.

Delivery of Transitional-Employment Services

The basic elements of transitional employment--job development and placement, on-the-job training, and short-term support and follow-up--were implemented in the demonstration.

The demonstration projects were able to place two-thirds of the treatment-group members on jobs.

Half of these persons (or one-third of all treatment-group members) were successfully stabilized on a potentially permanent job; this success rate is consistent with the rate observed for other large transitional-employment programs.

An analysis of program operations found that the demonstration was equally successful at serving treatment-group members with a wide range of characteristics and placing them on jobs. The existence of physical, emotional, social, or speech problems or low IQ scores did not appear to effect the length of participation in the demonstration, the likelihood of being placed in a job, or the expected weekly earnings from the job held at the time that a person exited from the demonstration. However, the extent to which the demonstration could have served the persons who were screened out during the intake process is unclear. For example, because intake workers felt that the time-limited demonstration services would have been inadequate for persons with severe emotional problems, they did not enroll such persons.

The delivery of services and outcomes differed across the eight projects, even after differences in the characteristics of the SSI recipients who were enrolled were controlled for. In particular, the proportion of treatment-group members who were successfully placed and stabilized on jobs differed across projects.

Estimated Costs of Services in the Demonstration

Average expenditures per person enrolled in the demonstration varied across the eight projects, from $3,800 to almost $14,000. The variation across individual treatment-group members was even more substantial, with costs for some persons as high as $25,000.

Costs would likely be lower in an ongoing nondemonstration program. An analysis of program operations indicated that a replication of the service model that was implemented in the demonstration would cost $5,600 per person enrolled.

Experience of the Target Population in the Absence of the Demonstration

The experience of the control group shows that the employment experience of persons who enrolled in the transitional-employment program in the absence of the demonstration. Their

experience forms the basis for determining the net impacts of the demonstration services. Findings for the control group highlight the fact that even in the absence of the demonstration the target population would have made limited progress.

Changes in Service Use in the Absence of the Demonstration

Without demonstration services, sheltered workshop employment would have remained the dominant vocational activity for the persons enrolled in the demonstration. During the three years following the enrollment of the treatment group in the demonstration, approximately 25 percent of the control-group members reported being enrolled in a sheltered workshop.

The use of transitional employment rose over time, although the rate of use remained relatively low: in the absence of the demonstration, we estimate that fewer than 6 percent of the persons who were enrolled in the demonstration would have enrolled in a transitional-employment program.

Changes in Earnings in the Absence of the Demonstration

Average earnings would have increased somewhat even in the absence of the demonstration services. We estimate that, during the three years following enrollment, the average earnings of the persons who enrolled in the demonstration would have risen by 21 percent relative to earnings in the month of enrollment. In addition, we found that by the third year following enrollment 30 percent of the control-group members held jobs outside of sheltered workshops, and approximately 13 percent held community jobs that paid at least the minimum wage. This earnings and employment growth highlights the need for a control group in the evaluation in order to estimate the net effect of transitional employment on employment and earnings.

Impacts of the Demonstration Services

The impacts of the demonstration indicate the net change expected from adding transitional-employment services similar to those fielded in the demonstration to the existing mix of services. In addition, the impacts indicate the effects on SSI recipients with mental retardation who are similar to those who were enrolled in the demonstration.

Service Use

The demonstration dramatically increased the rate of employment-service receipt among members of the treatment group. Not surprisingly, members of the treatment group received substantially more

transitional-employment services. In addition, the use of nondemonstration services (such as supported-employment and follow-up services) was estimated to have increased: during the 24 months following enrollment, treatment-group members spent an average of almost six weeks more in these employment-support programs than did control-group members. Accompanying this greater use of transitional- and supported-employment services was a 32 percent reduction in the average amount of time that treatment-group members spent in sheltered workshops.

Community Employment

Reflecting the success of the transitional-employment services and the shift away from the use of sheltered workshops, the demonstration increased the extent of employment in integrated, or "community," jobs. Interview data collected in the demonstration indicate that by the third year after enrollment 45 percent of the treatment-group members were in community jobs, a 50 percent increase over the experience of the control group.

A majority of the persons in community employment had supports available from a job coach or training program. Most of the increase in community employment appears to have occurred for persons in this type of support-oriented job. The proportion of persons in such jobs was estimated to have increased from 17 to 30 percent due to the demonstration. This result suggests that many of the persons who obtained community jobs through their activities in the demonstration had access to job-retention services, although the exact level of support that was actually provided was not measured.

Earnings

The transitional-employment services clearly increased earnings relative to what they would have been in the absence of the demonstration. Average earnings for the three years following enrollment were estimated to be 85 percent greater than in the absence of the demonstration. The estimated impacts for the three years are as follows:

Year	Impact	Percentage Change
1	$665	108%
2	$909	96%
3	$742	63%

The decline in the percentage change over time reflects the overall increase in average earnings that we estimated would have occurred even in the absence of the demonstration, as well as some decline in the employment and earnings of persons in the treatment group.

SSI Receipt

The transitional-employment services provided in the demonstration had little effect on the receipt of SSI payments. Over the three years following enrollment, average SSI payments were reduced only by 2 percent (approximately $240 per treatment-group member). The continued receipt of SSI reflects the fact that, while the average earnings gains of treatment-group members were proportionately large, total earnings remained low relative to the levels that would imply economic self-sufficiency. SSI regulations disregard a large proportion of any small increase in earnings; consequently, treatment-group members were able to maintain their eligibility and much of their benefits even though their average earnings increased.

Cross-Project Variation in Impacts

The impacts of the demonstration services appear to differ across the projects, although it is difficult to determine analytically whether such differences are due to differential effectiveness or differences in the local labor-market and service environments. Nevertheless, when the project-specific impact estimates are considered in light of the analysis of project operations (Thornton, Dunstan, and Schore, 1988), we conclude that impacts were greater in those projects that emphasized (1) placing persons in potentially permanent jobs as soon as possible, (2) matching jobs and participants carefully, and (3) being flexible in response to individual client needs.

A closer examination of the training practices of the demonstration projects clearly seems warranted. In particular, it is important to assess whether the experience of the most successful project could be replicated. That project essentially raised average earnings by $2,000 per year over the three years following enrollment, an increase of 134 percent.

Conclusions about the Demonstration

As fielded in the demonstration, transitional-employment increased the employment of SSI recipients with mental retardation. The

demonstration services also led to small reductions in SSI receipt and changed the mix of services used by the treatment-group members. The assessment of these impacts and the costs of providing the services depend on the analytical perspective adopted.

Perspective of SSI Recipients Who Enrolled

We conclude that treatment-group members received pecuniary and nonpecuniary net benefits from their enrollment in the demonstration. Since the estimated 85 percent increase in earnings far outweighed the reduction in SSI benefits, treatment-group members benefited financially from their participation; we estimate that average income for the treatment-group members was more than 10 percent higher than it would have been in the absence of the demonstration.

Given the importance of work in our society, this combination of impacts suggests that treatment-group members will view the offer of transitional-employment services favorably. The demonstration-induced increase in job-holding not only increases the income of treatment-group members, but also provides nonpecuniary benefits as treatment-group members increase their interaction with other members of society and are able to adopt roles that are more in line with those held by their nondisabled peers. At the same time, the continuation of SSI benefits provides basic income support and security that are likely to be important to such persons as the demonstration participants, who were seeking to enter an often unsure and volatile labor market.

Perspective of the SSI Budget

For the SSI budget, the small reductions in SSI payments represent the only financial benefit from the investment in transitional-employment services (essentially no effect in the receipt of Social Security Disability Insurance benefits occurred among treatment-group members). The estimated $240 reduction in average SSI payments would offset only 4 percent of the $5,600 that we estimate it would cost to provide services. This finding indicates that the SSI program could not justify bearing the full costs for transitional employment on financial grounds alone.

Consequently, it seems that the SSI program should investigate the possibility of providing partial program funding in line with the

anticipated SSI reductions. Such partial funding would provide an incentive for other agencies responsible for providing assistance to persons with mental retardation to expand the availability of transitional-employment services, and, if properly designed, could ensure that the SSI budget would not incur net costs.

At least two possible funding plans deserve consideration: to provide vocational rehabilitation agencies with grants based on the number of SSI recipients served in state-operated transitional employment, or to provide funding for ongoing job-retention services to agencies that work with SSI recipients who are placed and trained on jobs by transitional-employment programs. In either case, the funding could be based on the estimated SSI savings attributable to the transitional-employment services, so that funding could be kept in line with the expected reduction in SSI payments.

Perspective of the Aggregate Government Budget

Transitional employment affects the use of alternative vocational services paid for by the government, and thus will affect aggregate government expenditures for SSI recipients with mental retardation. Our analysis indicates that the demonstration-induced shifts in service use were cost-neutral from the aggregate government perspective: the costs of the increased job-retention services approximately equal the savings from a reduction in the use of sheltered workshops.

The demonstration experience holds important implications for the relationship between program targeting and program funding if transitional employment is to be undertaken on a broader scale. If transitional employment were provided to persons who would have used relatively few services otherwise, the transitional-employment services represent an overall expansion of assistance to persons with mental retardation. Our findings indicate that such an expansion would require an increase in expenditures.

The situation might be different if transitional-employment services were targeted toward persons currently in sheltered workshops. Such a case would represent a shift in government funding rather than an expansion of services to previously unserved persons. While the

demonstration did not directly test this scenario, the impact analysis suggests that by recruiting persons from existing programs a transitional-employment program would be more likely to create a situation in which savings could be generated. In the first year, a switch of persons from workshops to transitional employment might increase total costs to the extent that the costs of transitional employment exceeded the costs of workshops. In subsequent years, it is likely that savings would accrue, since the costs of long-term job-retention services appear to be less than the costs of workshops, and it would be expected that some persons would succeed in the labor market to the extent that they no longer needed any special government-funded services.

Perspective of Society as a Whole

When all groups in society are considered together, it appears that transitional employment has the potential of creating impacts that are sufficiently large to justify the costs of this type of service. This conclusion depends on the relative consideration given to the perspectives of the SSI recipients who enroll and of the various government budgets. It also depends on the value placed on increasing the community-based employment of SSI recipients with mental retardation.

As long as transitional-employment services are targeted toward a mix of SSI recipients with mental retardation, many of whom apparently would not receive vocational services otherwise, then it seems likely that the government would incur a net cost for operating the program (although some savings would offset the gross operating costs of transitional-employment programs).

Balancing this net expenditure are the earnings gains of the SSI recipients, as well as the nonpecuniary benefits of their increased integration into community life. Indeed, it is the increased self-esteem of persons with mental retardation who are able to enter and participate in the labor market, as well as the satisfaction that we as a society derive from assisting these individuals in their efforts, that represents the major justification of transitional-employment services.

Additional Information about the Demonstration

The operational experience of the demonstration, including descriptions of the eight projects, the recruitment procedures, the specific transitional-employment services provided, placement rates, and program costs, is assessed in:

> "The Transitional Employment Training Demonstration: Analysis of Program Operations," by Craig Thornton, Shari Miller Dunstan, and Jennifer Schore. Princeton, NJ: Mathematica Policy Research, Inc., 1988.

A more individual-oriented view of the demonstration based on case studies of several clients is presented in:

> "Making the Move: Case Histories of Persons in the Transitional Employment Training Demonstration." Princeton, NJ: Mathematica Policy Research, Inc., 1987.

I. THE TRANSITIONAL-EMPLOYMENT TRAINING DEMONSTRATION

Employment support programs for persons with mental retardation have become an established part of the vocational service system in this country. Under a variety of names--transitional employment, supported employment, and supported competitive employment--these support programs have demonstrated the feasibility and potential for placing, training, and maintaining persons with mental retardation on jobs similar to those held by persons without cognitive or physical limitations. Through the efforts of program operators, researchers, and administrators across the country, this program model has been transformed from an experimental venture about which considerable skepticism was initially expressed into an integral part of the vocational rehabilitation system and a mandated service to be provided by state vocational rehabilitation agencies funded by the reauthorized Rehabilitation Act of 1973 (P.L. 99-506).

Along with the expansion of employment support programs for persons with mental retardation, the focus of associated research and evaluation efforts has changed. The initial research efforts demonstrated that this approach to vocational habilitation was feasible, and that the primary barrier to employment for many persons with mental retardation was the lack of training and support rather than the lack of ability or motivation. The documented success of the early employment support programs (Moss, 1980; Rusch and Mithaug, 1980; and Wehman, 1981) led researchers to move beyond efforts to demonstrate feasibility and to focus instead on how employment support programs fit into the overall social service system, how training techniques could be improved, how services could be targeted effectively, whether these programs could be operated on a

large scale and as part of the general vocational service system, and whether this program model was cost-effective.

The Transitional Employment Training Demonstration was initiated in 1982 by the Social Security Administration as part of this second stage of research. In particular, the demonstration was fielded to determine the effectiveness of transitional employment as a vehicle for enhancing the economic self-sufficiency of Supplemental Security Income (SSI) recipients with mental retardation. That is, is it possible to increase the employment and earnings of this group by providing them with time-limited placement, training, and support services? Previous research had suggested that transitional employment could be effective, but its effectiveness for persons with mental retardation whose disability met the standard of severity required for SSI eligibility had not been subjected to a rigorous evaluation.

While groups who exhibit similar levels of retardation had previously been served, mentally retarded SSI recipients represented a special challenge, since, beyond their functional limitations, they tend to be poor and to have extremely limited, or nonexistent, work experience; moreover, the income support provided to SSI recipients gives them different work incentives than those which face the nonrecipient population. These distinctive characteristics of the SSI recipients with mental retardation, along with the fact that they constitute approximately 20 percent of all adult SSI recipients (see McCoy and Weems, 1989), generated considerable policy interest in the effects of transitional employment on this particular group of individuals.

The demonstration sought to address five basic issues as they pertained to SSI recipients with mental retardation:

1. Do transitional-employment services improve their labor-market performance?

2. Does the provision of transitional-employment services reduce their SSI payments?

3. To what extent do any such SSI savings offset the costs of providing the services?

4. How do the characteristics and experience of participants influence the effectiveness of the service?

5. Can transitional-employment programs be operated at policy-relevant scales, how do the approaches that are used to deliver transitional-employment services differ, and do those differences influence the effectiveness of the program?

Findings pertaining to the first four of these points are presented in this report. Specifically, this report describes the impacts of the demonstration services on the employment, earnings, SSI receipt, and service use of the sample of SSI recipients with mental retardation who were enrolled in the demonstration. We begin our presentation in Chapter II by considering what the activities of these SSI recipients would have been in the absence of the demonstration. Then, in Chapter III, we examine the extent to which the actual activities of these SSI recipients differed from what they would have been--that is, the net impacts of the demonstration services. The report concludes in Chapter IV with a brief assessment of the estimated impacts from the perspectives of the SSI recipients, the SSI budget, the overall government budget, and society as a whole.

The fifth issue--the operational feasibility and characteristics of transitional employment for SSI recipients with mental retardation--was addressed in an earlier report (Thornton, Dunstan, and Schore, 1988). That report presented information on the training organizations selected to provide transitional-employment services in the demonstration, the recruitment and

enrollment of SSI recipients with mental retardation into the demonstration, and the nature and extent of the services actually provided.

Together, these two reports provide a complete picture of the demonstration and its effects. The impact estimates presented herein indicate whether the demonstration services were successful--that is, the extent to which the transitional-employment services increased the economic self-sufficiency of a sample of SSI recipients with mental retardation. The analysis of program operations, presented in the other report, provides a basis for understanding the specific services that produced the impacts and for developing future programs that could be expected to generate even greater impacts.

The two reports also highlight the use of quantitative and qualitative information in the evaluation. The rigorously derived estimates of the overall impacts of the demonstration and the statistics on program operations provide the hard numbers about the success of the demonstration services. At the same time, qualitative information derived from observations of transitional-employment projects, along with the suggestive, but imprecise, estimates of the impacts by subgroup, provides a sense of the potential of transitional-employment services and how the delivery of these services could be improved. Policymakers require both types of information in order to make decisions about the future of transitional employment for SSI recipients with mental retardation.

The remainder of this chapter provides some of the program-operations information developed by Thornton, Dunstan, and Schore (1988). Section A describes the transitional-employment services that were provided in the demonstration and that were expected to generate the desired increase in self-sufficiency among SSI recipients with mental retardation. The process by which

those SSI recipients were enrolled in the demonstration is summarized in Section B. Section C then outlines the design of the demonstration, highlighting the use of randomly assigned treatment and control groups to provide a rigorous foundation for estimating the impacts of the demonstration.

A. TRANSITIONAL EMPLOYMENT SERVICES IN THE DEMONSTRATION

Operations in the demonstration essentially began when the Social Security Administration (SSA) funded eight training organizations to provide transitional-employment services to a sample of SSI recipients with mental retardation who lived in thirteen communities across the country. As indicated in Table I.1, these projects were selected in November 1984. The first persons were enrolled in the demonstration in June 1985, and services were provided until June 1987.

Overall, the demonstration projects appear to have operated successfully and in accordance with the experience of similar large-scale demonstration programs. As discussed in the remainder of this chapter (and in greater detail in Thornton, Dunstan, and Schore, 1988), the key features of the demonstration and its implementation are as follows:

- During the thirteen months of enrollment, the eight demonstration projects enrolled 745 SSI recipients--almost 5 percent of all SSI recipients who were between 18 and 40 years of age, who had a diagnosis of mental retardation, and who lived in the demonstration catchment areas.

- The SSI recipients who enrolled in the demonstration comprise an unrepresentative sample of all SSI recipients with mental retardation; in particular, enrollees volunteered to participate in the program, having expressed an interest in obtaining a job and having been judged by program intake workers to be appropriate for transitional employment.

- To support a rigorous evaluation, the 745 recipients were assigned randomly to a treatment group which was offered the demonstration services and to a control group which was precluded from

TABLE I.1

MILESTONES IN THE PLANNING AND IMPLEMENTATION OF THE TRANSITIONAL EMPLOYMENT TRAINING DEMONSTRATION

Date	Event
June 1980	The President signs the Social Security Disability Amendments of 1980 (P.L. 96-265) that authorize the demonstration.
September 1982	The Social Security Administration (SSA) holds a public conference to solicit comments on the initial demonstration design.
September 1983	SSA hires an evaluation contractor (Mathematica Policy Research)
March 1984	SSA publishes a Federal Register notice to solicit bids from training organizations to operate demonstration projects.
November 1984	After reviewing 80 proposals, SSA awards grants to 8 projects.
May 1985	SSA starts to mail invitation letters to mentally retarded SSI recipients who are between the ages of 18 and 40 and live in the areas served by the 8 projects.
June 1985	Projects begin to enroll SSI recipients into the demonstration.
July 1986	Demonstration enrollment ends.
June 1987	Projects complete demonstration training activities.
December 1988	The data collection for the report is completed.

receiving demonstration services but was permitted to seek any other available services; due to the analytical strengths and nature of the random assignment process, post-enrollment differences between these groups will indicate the impact of adding transitional-employment services to the current service system.

- The basic elements of the demonstration--the enrollment of SSI recipients with mental retardation, job development and placement, on-the-job training, and short-term support and followup--were implemented successfully.

- Waivers to the SSI regulations were implemented to enable recipients to participate in the demonstration without risking their eligibility for benefits; these waivers essentially provided many of the work incentives that were enacted as part of the Employment Opportunities for Disabled Americans Act in effect as of July 1, 1987.[1]

- The demonstration projects were able to place, train, and maintain approximately one-third of the treatment-group members on potentially permanent jobs; this success rate is consistent with the rate observed for other large transitional-employment programs.[2]

The demonstration design specified the nature of the transitional-employment services that were to be provided by the projects and encompassed several modifications to the SSI regulations. The following two sections delineate the service model and regulatory modifications; the third section assesses the general implementation of the service model at the overall demonstration level.

---

[1]In particular, the demonstration waivers provided essentially the same SSI work incentives as those provided under the recently enacted Section 1619 provisions of the Social Security Act as long as persons were enrolled in the demonstration. The legislation applied the work incentives to all SSI recipients (see Thornton, Dunstan, and Schore, 1988, pp. 20-23).

[2]As discussed later in Section A, the placement rate for the demonstration is lower than that reported for a number of other programs (see, for example, the results reported for the employment-support programs described in Rusch, 1986). Because placement rates are affected by enrollment and screening practices, comparing these rates across programs is extremely problematic. After reviewing the evidence from the demonstration, it is our assessment that the placement rates are generally consistent with current practice.

1. The Transitional-Employment Model

The basic transitional-employment model specified in the demonstration design required that all projects provide a specified set of transitional-employment services, but allowed the individual projects considerable flexibility in how they delivered those specified services. The general approach was adapted from the findings of previous research into transitional employment, particularly the studies of Wehman (1981), Rusch and Mithaug (1980), and Kerachsky et al. (1985). Specifically, all program operators were to work toward the goal of ensuring permanent competitive employment for all participants. To achieve this goal, they were required to:

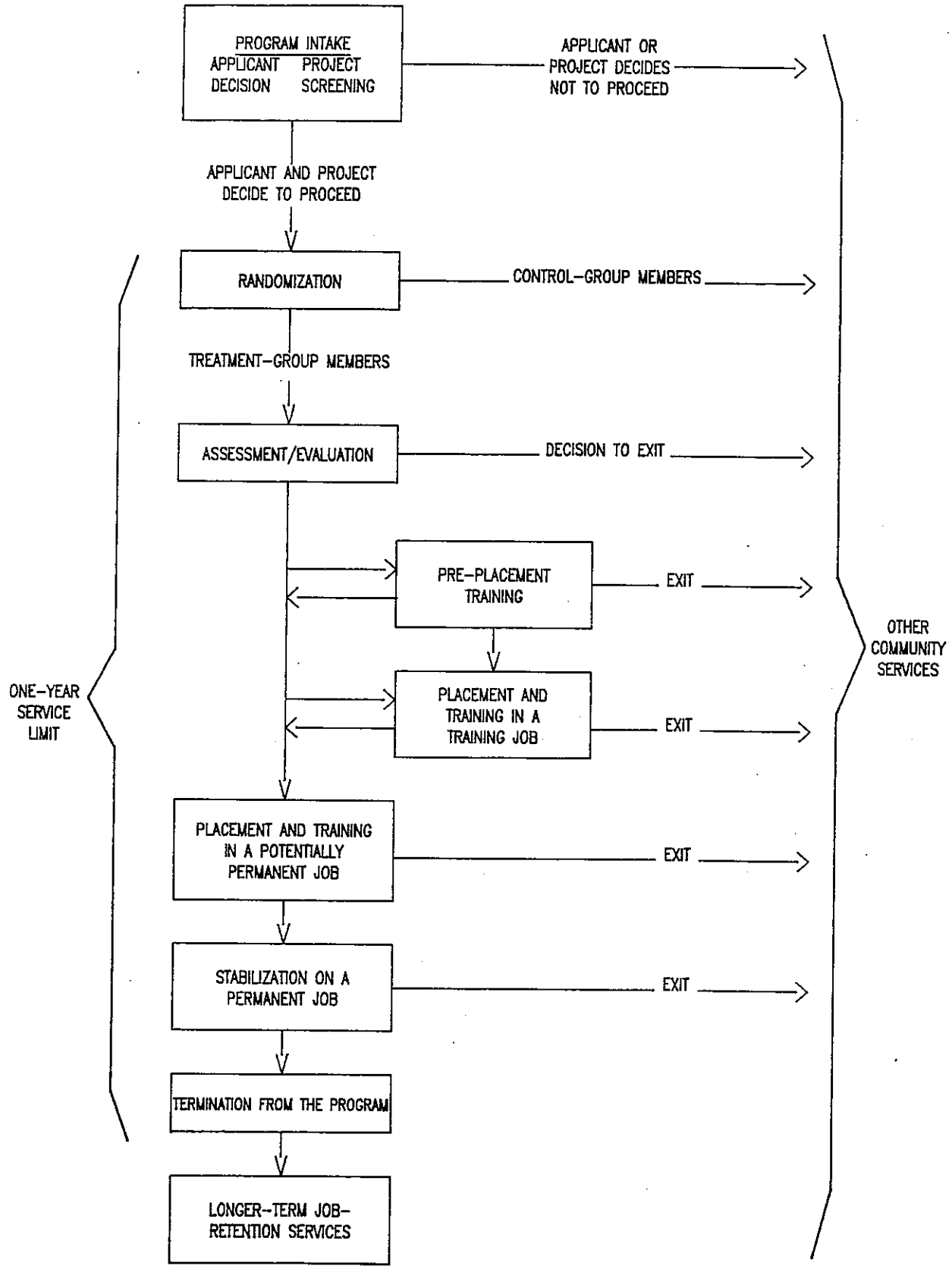
- Place participants in competitive jobs with the potential for permanent employment
- Provide participants with training in specific vocational skills, where such training was provided primarily, if not entirely, in conventional work environments alongside nonhandicapped co-workers
- Provide postplacement support and follow-up as necessary to promote the retention of jobs

The projects were to provide all demonstration-funded services for a client within one year after the date on which the client enrolled in the demonstration. Any additional services required after the year (in particular, job-retention services) were to be funded through other than demonstration resources.

Figure I.1 indicates that the general demonstration service model encompassed five stages for persons assigned to the treatment group: assessment/evaluation, pre-placement training, training-job placement, permanent-job placement, and stabilization. All projects were expected to provide assessment/evaluation, as well as placement and stabilization on

FIGURE I.1

THE GENERAL FLOW OF DEMONSTRATION SERVICES



a permanent job. Projects had the option of including pre-placement training and training-job placements in their model.

Following enrollment--that is, at the point of random assignment--all treatment-group members were assessed to determine their skills, interests, and limitations. This assessment provided the information necessary for developing an effective training plan and identifying appropriate job placements. Following assessment, some projects provided treatment-group members with pre-placement training designed to prepare them for jobs. This training typically stressed job-search skills and appropriate work-place behavior.

Treatment-group members were then placed on jobs. While the goal for all projects was to place treatment-group members on potentially permanent jobs, some projects opted to place treatment-group members on temporary training jobs as a preliminary step. The training jobs provided a forum for more detailed assessment of skills and for providing vocational and behavioral training.

The core service of the demonstration was the placement and training of treatment-group members on potentially permanent jobs. The job-placement services sought to match each treatment-group member with a job that reflected his or her skills and interests. Once on the job, a project staff member, generally termed a "job-coach," provided on-the-job training to enhance the work performance of the treatment-group member. This training covered the specific job tasks that a treatment-group member would be expected to perform on his or her job, as well as the interactions between co-worker and supervisor that the treatment-group member would have to master to be successful on the job. In addition, the job coaches helped treatment-group members develop the skills and arrangements necessary to travel to and from the job. Such transportation assistance proved to be one of the most important demonstration services. The

projects also provided a range of support services to help treatment-group members adjust to their new role in the labor force. These services included counseling and assistance in their interactions with SSI and other programs.

The training and support services were gradually phased down as the treatment-group member learned the job. The purpose of this process was to promote the independence of and to stabilize the treatment-group member on his or her job. Stabilization was to be achieved within one year from the point at which a person was assigned to the treatment group--that is, from the time of his or her enrollment in the demonstration. In order to promote job retention, part of the stabilization process was to entail arranging for the long-term support services necessary to ensure that treatment-group members could hold their jobs. These long-term services were to be funded from sources other than the demonstration, and could include additional on-the-job training in response to changes in job tasks or structure, case management to arrange for support or other services, and even placement and training on new jobs for those persons who lost their jobs.

As indicated in Figure I.1, treatment-group members could exit from the demonstration during any of the five stages. The reasons for program exits were both client- and program-related. On the one hand, some treatment-group members exited the program prior their full period of participation because they wished to pursue other activities (such as sheltered workshops), or because they lost interest in the demonstration or found jobs on their own.[3] On the other, program operators decided that they could not successfully serve some treatment-group

[3]Typically, if a demonstration project was aware of such jobs it would attempt to provide the training and support services that the treatment-group member thought necessary and desirable for taking those jobs.

and thus either initiated the exit themselves or referred clients to alternative programs. Consequently, while all treatment-group members were initially offered the full range of demonstration services, not all of these persons actually participated in all stages of the services model.

2. SSI Rules in the Demonstration

The SSI program provides income to persons who are poor and who are disabled, blind, or aged (see Social Security Administration, 1988). As such, it institutes a two-part eligibility standard. To be eligible for SSI benefits, a person with mental retardation (or other condition) must be poor and be sufficiently disabled that he or she cannot engage in substantial gainful activity. Poverty is defined in terms of a person's income and assets, and essentially requires that a person's income be less than $368 a month (as of January 1, 1989). To be considered disabled, a person must have a medically determinable reason that he or she cannot hold gainful employment where he or she earns more than $300 per month (excluding subsidies and certain impairment-related work expenses).

Persons who meet the income, assets, and disability criteria are entitled to a monthly federal benefit of up to $368. In addition, depending on the policies adopted by their state, eligible recipients may also receive a monthly state supplemental benefit and medical benefits under the Medicaid program. Recipients continue to receive these benefits (which are adjusted annually for inflation) as long as they remain eligible.

The incentives and disincentives created by the SSI eligibility and benefit-determination rules have been the subject of considerable academic and policy debate. The demonstration addressed the work-disincentive issue by

implementing four waivers to SSI regulations (see Thornton, Dunstan, and Schore, 1988, pp. 20-23). In effect, these waivers allowed SSI recipients who enrolled in the demonstration to maintain their eligibility for SSI benefits while they received training. These same types of provisions were ultimately made permanent and applicable to all jobs rather than just demonstration jobs by the Employment Opportunities for Disabled Americans Act (P.L. 99-643), which took effect on July 1, 1987.[4] Essentially, the waivers and subsequent permanent provisions modified the criteria pertaining to the definition of disability while leaving the income and asset criteria essentially unchanged. Under the new rules, SSI recipients who sought and held jobs would still be considered to meet the disability criteria for SSI eligibility, as long as their earnings did not exceed the level at which they would no longer be receiving SSI benefits.[5]

3. The Implementation of the Demonstration Model

The demonstration model was implemented by eight training organizations that were selected competitively on the basis of their responses to the announcement of the availability of funding for operating a transitional-employment intervention. More than 700 organizations requested application materials in response to the announcement, and 80 of them submitted proposals.

---

[4]Rocklin and Mattson (1987) review the policy debate and the provisions of the Act. The Social Security Administration (1986) reviews the experience with the provisional work-incentive regulations.

[5]The SSI regulations reduce the size of a recipient's monthly benefit to reflect any income received by the recipient. The first $20 of income from any source is ignored in these calculations. After that, the monthly benefit is reduced by the amount of any unearned income. For recipients with earnings, the first $65 of earnings is disregarded, and benefits are reduced by half of any earnings that exceed $65. Thus, a recipient with no unearned income could earn up to $821 a month before his or her benefit would be reduced to zero (this figure reflects the $20 general disregard and the $65 earnings disregard, as well as the current monthly federal benefit of $368).

Proposals were screened by a technical review panel, Social Security Administration staff, and staff at Mathematica Policy Research, and eight organizations were selected to operate the demonstration projects.

These eight organizations and the areas they served are listed in Table I.2.[6] Because the AHEDD project served six different areas, the eight projects served thirteen different sites. The eight organizations varied along many dimensions. Some operated ongoing transitional-employment programs, while others developed new programs for the demonstration. The organizational affiliation of the projects also varied (some were university-based, while others were nonprofit organizations), as did their staffing patterns, their approach to providing nonvocational support services, and of course the environments in which they operated.

The analysis of program operations at the eight projects (Thornton, Dunstan, and Schore, 1988, Chapters V and VI) drew several conclusions about the implementation of demonstration services. The major conclusions are as follows.

The Basic Programmatic Elements of the Demonstration Were Implemented Accordingly by the Projects. The demonstration projects enrolled a sample of mentally retarded persons who largely satisfied the basic eligibility criteria. Indeed, each project enrolled individuals who exhibited diverse characteristics, but who were deemed likely to benefit from demonstration services.

In addition, despite adopting different service approaches, all projects provided the essential service elements of the demonstration--job development and placement, on-the-job training, and short-term support and follow-up

[6]Thornton, Dunstan, and Schore (1988, Chapter III) describe the eight organizations that operated demonstration programs and the communities in which they operated.

TABLE I.2

DEMONSTRATION PROJECTS AND SITES

Grantee Name	Abbreviated Name	Site
AHEDD, Incorporated	AHEDD	Dover, Delaware Harrisburg, Pennsylvania Lancaster, Pennsylvania Philadelphia, Pennsylvania York, Pennsylvania
Association for Retarded Citizens, Monmouth Unit	ARC/MU	Monmouth County, New Jersey
The CENTER for the Rehabilitation and Training of the Disabled	The Center	Chicago, Illinois
The Children's Hospital	Children's Hospital	Boston, Massachusetts
Exceptional Children's Foundation	ECF	Los Angeles, California
Goodwill Industries, Milwaukee Area	Goodwill	Milwaukee, Wisconsin
University of Washington and Portland Community College	UWash/PCC	Portland, Oregon
University of Wisconsin, Stout, Vocational Development Center	UWis/Stout	Chippewa, Dunn, Eau Claire and Pepin counties, Wisconsin

services. In addition, projects provided these services to most clients within a one-year time frame.

Over two-thirds of the treatment-group members were placed on potentially permanent or training jobs in the demonstration, and half of the persons who were placed on jobs were successfully stabilized on potentially permanent jobs. In addition, the demonstration appears to have been successful at providing a similar and equivalent set of services to clients who exhibited a wide range of disabilities and personal characteristics.

While the placement rates of the demonstration cannot be compared directly with the placement rates reported for other transitional-employment programs, we feel that the overall placement performance of the demonstration is consistent with the performance that would be observed for similar transitional-employment programs. In particular, the success of the demonstration projects at placing and stabilizing one-third of the treatment-group members on potentially permanent jobs is similar to the results reported for the STETS program, the only other large-scale transitional-employment demonstration whose evaluation was based on an experimental design (Kerachsky et al., 1985; and Kerachsky and Thornton, 1987).

Because placement rates are determined by the recruitment, screening, and enrollment practices of programs, as well as by the actual delivery of services, comparisons of programs that differ along these dimensions require considerable care. In addition, the manner in which placement rates are defined varies across programs. The demonstration projects were precluded from extensively evaluating or screening applicants, which meant that the demonstration projects enrolled persons before conducting the assessment and job-matching activities that constitute the first step of transitional employment. In addition, the

demonstration projects were encouraged to enroll a broad cross-section of SSI recipients with mental retardation, many of whom may have had only a rough idea of what it meant to hold a job. Finally, placement rates in the demonstration are calculated by dividing the number of persons who were placed and stabilized successfully on a job by the number of treatment-group members.[7] These aspects of the demonstration imply that placement rates will be lower for the demonstration than for transitional-employment projects that are able to conduct more extensive pre-enrollment screening. Of course, such differences in placement rates imply little about any cross-program differences in net impacts, the criteria by which programs are ultimately judged.

A 12-Month Service Period Is Adequate for Placing Persons on Potentially Permanent Jobs. Although some clients appeared to need demonstration services beyond the 12-month service period, it is clear that the one-year time frame was adequate to meet the short-run demonstration goals for at least two-thirds of the clients who were terminated with potentially permanent jobs. Overall, the average number of enrollment months for all clients terminated with jobs was only 12.5.[8]

A Variety of Support Services Are Necessary To Respond to the Diverse Needs of Various Clients. The demonstration projects enrolled SSI recipients who exhibited diverse backgrounds and personal characteristics. Such diversity

---

[7]An alternative method would be to divide the number of persons successfully stabilized by the number of treatment-group members placed on jobs. This calculation would imply a placement rate of 50 percent for the demonstration.

[8]It is important to note that, even after exiting the program, some of these persons did receive additional services (under other funding) from the agencies that operated the demonstration projects. These services ranged from continued active on-the-job monitoring to problem resolution on an as-needed basis.

meant that projects had to be able to provide more than merely vocational-training services. For instance, in preparation for employment, project staff helped clients acquire job-search skills and develop good hygiene and grooming for job interviews; staff also worked with clients on the job to deal with problems not associated with skills training (such as interacting with co-workers), and provided support to clients off-the-job in other areas of their lives that affected or were affected by their employment (for example, in budgetary matters and residential moves).

Arranging Transportation Is a Critical Element in Providing Transitional-Employment Services. While many of the aforementioned support services were client-specific and were arranged on an as-needed basis, a critical element of the transitional-employment services for all clients was transportation arrangements. Because transportation was crucial to successful job placements and maintenance, some projects decided not to enroll clients who could not travel independently or who were unwilling to be travel-trained. For clients who were enrolled, resolving their travel barriers was often more difficult than was teaching them how to perform their job.

The Demonstration Was Feasible at a Variety of Scales. This demonstration experience was unique in that the eight transitional-employment projects operated at a variety of scales. The measures of service delivery and costs presented in Thornton, Dunstan, and Schore (1988) indicate that demonstration services were feasible at a variety of scales, ranging from 20 to 50 clients per year.

In addition, it is also clear that programs which might serve a larger number of clients than were served by the projects in the demonstration are feasible. While no single demonstration project served more than 50 persons a

year, the demonstration as a whole could be considered a multi-site program that served 250 persons a year. Thus, transitional employment is a feasible policy option for serving large groups of SSI recipients with mental retardation, particularly if replicated with a multiple-vendor model.

Transitional Employment Represents a Substantial Investment. As was known from the start, transitional employment is an intensive intervention. Its costs confirm this fact. In the eight demonstration projects, average expenditures per treatment-group member ranged from $3,800 to almost $14,000. Furthermore, expenditures for some specific individuals were as high as $25,000.

A substantial part of these costs was due to the special nature of the demonstration. Thornton, Dunstan, and Schore (1988, pp. 119-130) estimated that it would cost $5,600 per client enrolled to operate an ongoing transitional-employment program similar to the one fielded in the demonstration. The extent to which the net impacts of the demonstration are sufficiently large to justify these substantial costs is addressed in the conclusions to this report (Chapter IV).

B. THE EVALUATION DESIGN AND THE ANTICIPATED IMPACTS OF THE DEMONSTRATION

The evaluation of the Transitional-Employment Training Demonstration was designed to provide accurate and unbiased estimates of the effects of transitional employment on the employment, earnings, SSI benefits, and service use of the mentally retarded SSI recipients who were enrolled relative to what would have happened to them in the absence of the demonstration. The key features of the evaluation are the experimental design on which the estimated impacts are based, the relatively large number of persons followed as part of the evaluation, and the mix of data sources used in the evaluation.

1. Making the Policy Comparison: The Experimental Design

One of the most straightforward and accurate methods for estimating the impact of adding transitional employment to the service system is to use an experimental design. Under this design, eligible program applicants are assigned randomly to either a treatment group (which is eligible for demonstration services) or a control group (which is precluded from receiving demonstration services, but is allowed to use all other services available in the community). The goal of this procedure is to produce two groups that are virtually identical in terms of both observable characteristics (e.g., age, IQ score, gender, and pre-enrollment activities) and unobservable characteristics (e.g., motivation and ability). Some differences might still arise by chance, but they should be small, and those that are measured can be controlled for statistically in the course of the research.

The experience of the treatment group can be used to determine what happens when the transitional-employment services are available. The experience of the control group indicates what would have happened in the absence of the demonstration. Because the two groups differ only with respect to the opportunity to receive demonstration services, any postrandomization differences that emerge between the groups can reasonably be attributed to the effect of the demonstration services. Experimental designs have been used widely to study social programs (Greenberg and Robins, 1986, review many of such studies), and they have proved to be very powerful, generally leading to more definitive conclusions than other available evaluation designs (see Betsey, Hollister, and Papageorgiou, 1985).

Because the demonstration was introduced in the thirteen sites as a new or expanded service, its introduction was compatible with an experimental design.

That is, randomization allocated new positions that would have been unavailable in the absence of the demonstration. In general, persons who were assigned to the control group were not denied services that they could have obtained in the absence of the demonstration.[9]

Random assignment was conducted as the final stage of the intake process. When an applicant indicated his or her willingness to cooperate with the requirements of the program and the research, and when the project determined its willingness to accept the applicant, the project's intake worker called a staff member at Mathematica Policy Research to verify that the individual was a first-time applicant (i.e., had not previously been assigned a research status) and to receive the applicant's randomly determined assignment to treatment- or control-group status. Each applicant had a 50 percent chance of being assigned to the treatment group.

2. The Sample Size for the Evaluation

Many of the previous efforts to evaluate transitional-employment programs have relied on samples of fewer than 100 persons (see, for example, Hill and Wehman, 1983; Vogelsberg, 1986; and Moss, 1980). Such samples are adequate for assessing the feasibility of transitional employment and for providing information about potentially effective program designs. Nevertheless, impact

[9]However, access to services was restricted in those projects that operated a transitional-employment program in addition to the demonstration program. To keep the distinction between the treatment and control groups clear, the demonstration design precluded control-group members from enrolling in any alternative transitional-employment program operated by the demonstration training organization. Control-group members could obtain transitional-employment services from other program operators, although such services were not widely available. Three of the eight projects operated a second transitional-employment program and were subject to this restriction. Under the demonstration guidelines, the eight grantee organizations could begin serving control-group members (but could not actively recruit them) after April 1988.

estimates based on such samples are likely to lack the precision needed for formal tests of hypotheses about the impacts of programs.

The demonstration evaluation was designed to yield relatively precise estimates of program impacts and to provide a basis for testing whether the transitional-employment services fielded in the demonstration increased earnings and reduced SSI payments. Specifically, sample sizes were selected so as to make the probability of two undesirable events small: (1) drawing the conclusion that transitional employment had an impact when in fact it did not, and (2) drawing the conclusion that transitional employment had no impact when in fact it did. The probability that a statistical test will not lead to the first of these errors is called the "confidence level" of the test, and the probability that a statistical test will not lead to the second of these errors is referred to as the "power" of the test.

The specific tests of interest in the demonstration are whether the transitional-employment services as fielded in the demonstration increased earnings and reduced SSI payments relative to what would have been the case in the absence of the demonstration. This test requires that we compare the experience of the treatment and control groups and determine whether any observed difference between the two groups is sufficiently large that we can confidently reject the idea that the difference arose by chance, and can attribute the difference instead to the effect of the demonstration services. As we noted earlier, our ability to conduct this test depends on the confidence and power standards that are adopted. In addition, the precision of the test will depend on the variation in earnings and SSI payments among the demonstration sample members and on the number of persons included in the sample.

While any confidence and power standards are essentially arbitrary, a requirement that tests have 90 percent power and 95 percent confidence seems consistent with general practice. Under these standards, the minimum treatment-control difference in earnings that we can detect with the sample of 375 treatment-group members and 370 control-group members would be about $20 per month. Stated differently, if the true difference in the average earnings between the treatment and control groups were $20 per week, we would have a 90 percent chance of concluding that the difference was not due to chance if we used statistical tests based on a 95 percent level of confidence. If the difference were smaller than $20, the probability that we would attribute the difference to chance would be greater. In addition, if we examined subgroups of the sample, the minimum effect that we would have a 90 percent chance of detecting (with a statistical test based on 95 percent confidence) would be larger than $20.[10]

The implication of these figures is that we should be able to detect policy-relevant impacts--that is, increases in earnings of $20 per month or more and reductions in SSI payments of $35 or more. We expect that impacts of at least this size would be necessary to justify the substantial costs of operating the transitional-employment programs. The statistical tests used in the evaluation may be able to detect smaller treatment-control differences or similar differences for subsamples; however, we would be much more likely to fail to attribute such differences to the workings of the demonstration.

[10]The corresponding minimum detectable impact on SSI payments would be approximately $35 per month.

3. Data Sources

The evaluation drew on data from several sources in order to estimate the impacts of the demonstration services. The primary goal of the data collection process was to obtain equivalent data for the treatment and control groups--specifically, to ensure that inconsistencies in the data for both groups did not compromise the experimental design by introducing systematic differences between the two experimental groups.

Table I.3 summarizes the data sources used in the impact analysis:

- Baseline information collected by the demonstration projects as part of the intake process
- SSI program records on SSI payments and recipient earnings
- Information from interviews administered to sample members at nine of the demonstration sites
- Information extracted from the records of vocational service agencies in nine of the demonstration sites.[11]

Together, these four data sources provide a comprehensive perspective on the employment, earnings, SSI receipt, and vocational-service use of the treatment- and control-group members enrolled in the demonstration. However, because these data sets vary in terms of their time period and accuracy, it is thus useful to review the characteristics of each data set.

Program-Intake Data. A major source of information on the personal characteristics of sample members prior to their enrollment in the demonstration

[11]As discussed in Appendix A, the follow-up interview and extraction of data from agency records were undertaken at nine of the thirteen demonstration sites and pertain to seven of the eight projects. Specifically, no interviews were conducted in Chicago or in three of the six sites served by AHEDD: Dover, Philadelphia, and Pittsburgh.

TABLE I.3

DATA SOURCES USED IN THE IMPACT ANALYSIS

Source	Sample Coverage	Time Period Coverage	Available Data
Baseline information from intake process	All enrollees (745 individuals)	Point of enrollment and prior year	Data on demographic characteristics, living arrangements, previous work history, types of disabling conditions
SSI records	All enrollees	Monthly data covering the year prior to enrollment through November 1988 (between 29 and 42 months after enrollment, depending on the date of enrollment)	Monthly totals on earnings, income, and SSI receipt
Follow-up interview	92% of the enrollees in 9 sites (480 individuals)	Point-in-time data from fall 1988--between 26 and 44 months after enrollment, depending on the date of enrollment	Point-in-time data on wages, hours, types of jobs, occupations, service use, and hiring arrangement
Records extracted from vocational service agencies	Same as the interview sample	Covers the period between enrollment and the follow-up interview	Dates of entry into and exit from specified nondemonstration vocational service programs

was the Intake Data Collection Form.[12] This instrument, administered by intake workers at the projects prior to random assignment, collected information on the characteristics of sample members, including their basic demographic profile, living arrangements, previous work history, and types of disabling conditions.

SSI Records Data. Information on SSI payments, earnings, and other income sources was collected from computerized SSI records maintained by the Social Security Administration. These records provided data on the actual SSI payments made to recipients and, in most cases, verified reports of the earnings and income of recipients.[13]

These records were obtained for each sample member for the period beginning with their initial receipt of SSI payments and concluding with November 1988. Data for the year prior to enrollment in the demonstration and from 29 to 42 months after enrollment were extracted from these records for each sample member. The length of available postenrollment data varied because sample members were enrolled in the demonstration in different months. Due to the November 1988 cut-off date for extracting records from the Social Security Administration, data for those sample members who were enrolled in June 1985 (the first month of enrollment) cover a longer postenrollment period than do the data for those who were enrolled in July 1986 (the last month of enrollment).

---

[12]Copies of this form and other data-collection instruments can be obtained from Mathematica Policy Research, Inc.

[13]SSI recipients are required to report all earnings and other income to the Social Security Administration. SSA staff verify these reports by checking the pay-stubs provided by recipients and requesting verification from employers. Most instances in which the SSI records indicated that the reported data were unverified occurred in the three months prior to our November 1988 cut-off date.

Follow-Up Interviews. From September to December 1988, interviews were scheduled with all sample members who lived in nine of the thirteen demonstration sites. As described in Appendix A, interviews were successfully completed with 92 percent (524) of the sample members in these sites. Due to budget limitations, no interviews were fielded in the Chicago, Dover, Philadelphia, and Pittsburgh sites. The follow-up interview collected information on employment activities (including wages, hours, types of jobs, and occupations), and on the use of vocational services, particularly the use of transitional employment, supported employment, job-retention services, and sheltered workshops. Due to the difficulty of collecting retrospective data, these interviews focused on the activities of sample members at the time of the interview, a point between 26 and 44 months after enrollment. Appendix A describes the follow-up interview effort in more detail.

Vocational Service Agency Records. In order to obtain information on the use of vocational services other than those offered in the demonstration program, the follow-up interview asked sample members whether they had received any vocational services since enrolling in the demonstration, including transitional employment, supported employment, job-retention services, and sheltered workshops. Attempts were made to contact all of the vocational service agencies reported in the follow-up interviews in order to obtain information on the service use of sample members. A total of 113 vocational agencies were contacted, and records were obtained on 420 separate cases in which a sample member was enrolled in one of these programs. While these data appear to provide an accurate measure of enrollment in the various vocational services, the data do not fully capture the actual level and quality of the services received by sample members while enrolled in these programs.

4. Expectations about the Effects of Transitional-Employment Service

The design of the demonstration and its evaluation reflects the expected impacts of transitional employment. The primary objective of transitional employment is to increase the extent to which clients hold jobs in the competitive labor market. We would thus expect to find higher employment rates among members of the treatment group. Correspondingly, we should also find higher earnings for the treatment group as a whole. Given the SSI regulations pertaining to earned income, the increased earnings should be accompanied by a reduction in SSI payments to treatment-group members. A key issue for the analysis is the extent to which the reductions in SSI payments will offset the costs of providing the transitional-employment services.

The magnitude of any reductions in SSI payments will be affected by the special provisions for treating recipient earnings. In general, a recipient can receive $20 in income from any source without a reduction in his or her benefit. After that $20, a recipient's SSI benefit is reduced dollar for dollar by the amount of his or her income. If, however, the income is from earnings, some of the income is ignored in this calculation. In particular, an additional $65 in monthly earnings is disregarded, as are half of any earnings above that amount. Thus, SSI benefits fall in response to earnings increases, but, at most, the decline in benefits is half the size of the increase in earnings. For example, a recipient with no other income who began earning $100 per month would have his or her SSI check reduced by $7.50.[14]

[14]This reduction is calculated in three steps. First, $20 of the earnings are disregarded under the standard procedure. Second, the next $65 of earnings are disregarded under the special provisions for earned income. Finally, half of the remaining $15 is disregarded. Thus, only $7.50 of the $100 in earnings is treated as "countable" income and is taken out of the person's SSI benefit. As earnings rise, the effect of the two fixed disregards becomes relatively less important, and the main incentive is the one that excludes half of the earnings from consideration.

One of the issues addressed by the demonstration is whether the characteristics and experience of the participants influence the effectiveness of transitional employment. Such information can be helpful in planning and targeting future programs. For the most part, expectations about the differences among subgroups are ambiguous. We have thus specified the groups for which we might expect to find different impacts, but have not specified the direction of those different impacts.

The subgroups to be considered are categorized by the following:

- Demographic characteristics
- Personal characteristics
- Activities and experience prior to the demonstration
- The features and characteristics of program services received

The first category, demographic characteristics, includes such factors as age, race or ethnicity, and gender. Previous studies of disadvantaged groups (although typically not persons with mentally retardation) often show that the effectiveness of programs can vary along such dimensions. Such variation can be caused by many factors, including actual social or cultural differences among individuals or labor-market discrimination.

The second category, personal characteristics, pertains to individual-specific traits, such as intellectual ability, personal motivation, and physical ability. These types of traits are difficult to measure, and we have used only two to define subgroups in the study. The first is intellectual ability, as defined by IQ score. This partial measure of mental retardation was used because no data on adaptive behavior were available. A weakness of this measure is that, in general, the IQ data were drawn from Social Security records and

have a number of limitations when used for research or treatment purposes. The other subgroup is based on the intake workers' assessment of the probability of the person's success in transitional employment. This variable incorporates many personal characteristics into a single measure.

The third category encompasses the activities and experience of sample members in the period before they enrolled in the demonstration. Prior experience is expected to influence the effectiveness of the demonstration services and could provide a basis for determining the appropriateness of transitional-employment services. Variables in this category include living arrangements, work experience, and the receipt of Social Security benefits in addition to SSI.

While the fourth category--the specific demonstration services received by the treatment-group members--would appear to include obvious candidates that would condition the effectiveness of the demonstration services, it presents two serious problems. In terms of the variables that describe the demonstration projects themselves, the distinctions among them cannot easily be quantified, nor do the eight projects provide enough variation to distinguish among all their different features. In addition, it is extremely difficult to identify the differential effects of specific services provided to individual clients on the basis of need, rather than on the basis on random selection. Therefore, site is the only variable of this type that can be incorporated into the analysis. Of course, any impacts that are associated with site might also reflect differences among the local areas (i.e., in terms of job opportunities, alternative services, etc.).

In addition to the impacts on employment, earnings, and SSI payments, transitional-employment services will affect many other aspects of clients'

lives. Some of these other effects are closely associated with basic employment outcomes. For example, we would expect to find an increase in hours of work for the treatment group and possibly improvements in wage rates and the characteristics of the jobs held by treatment-group members who are working.[15] We would also hope to find improvements in job retention.

The demonstration may also have effects on the use of other programs by treatment-group members. An evaluation of a previous transitional-employment demonstration (Kerachsky et al., 1985) found that persons who received transitional-employment services were much less likely to be in sheltered workshops or work-activity centers. To the extent that transitional employment substitutes for these and other vocational services, important savings may accrue to the government. However, costs may appear in the form of increases in the use of services that complement transitional employment--for example, such services as transportation assistance, counseling, and long-term job-retention services.

Finally, a number of effects pertain to the well-being of the clients. The demonstration should improve the economic status of the clients, with their earning increases outweighing the expected reductions in SSI payments. The services are also expected to have an effect on clients whereby their living arrangements, social behavior, service use, and levels of self-sufficiency

[15]The effects on wages for persons who are employed may be positive or negative, because the program will affect the mix of persons who work, as well as the wage rates of those persons. The program may make it more likely for marginal workers to be employed at low wages rather than to be unemployed. Thus, the number of low-wage workers may be higher among the treatment group than among the control group, which could lead to lower average wages for working treatment-group members relative to working control-group members. Of course, the average earnings of all treatment-group members are expected to rise, since the proportion of such persons who work will be greater than among controls.

become more similar to those of the general population. The demonstration may also affect the confidence of clients in their income stream. While the recent changes in the SSI laws provide some income protection for recipients who lose their jobs, some recipients may still be anxious about exchanging the secure stream of SSI payments for a higher, but often more variable, stream of earnings.

II. THE EXPERIENCE OF SAMPLE MEMBERS IN THE ABSENCE OF THE DEMONSTRATION

Before we assess the effects of the demonstration, it is useful to examine the characteristics of the sample of SSI recipients who were enrolled in the demonstration and to establish what would have happened to them in its absence. This information establishes a basis for interpreting the effects presented in Chapter III.

We begin our presentation by examining the SSI recipients who were enrolled in the demonstration. Clearly, any attempts to replicate or build upon the demonstration requires information on the characteristics of the persons whose experiences are analyzed in the demonstration. Our description of the pre-enrollment characteristics of these SSI recipients encompasses both treatment- and control-group members, since the random assignment process ensured that individuals in both groups exhibited the same characteristics on average. We begin our description in Section A by summarizing the recruitment procedures by which SSI recipients were identified for and enrolled in the demonstration. An understanding of this process in essential for interpreting how the SSI recipients who enrolled in the demonstration might differ from SSI recipients in general and from persons served in other transitional-employment programs. We continue our description in Section B by presenting descriptive statistics on the SSI recipients who were enrolled.

Section C presents the available evidence on the differences between the characteristics of the SSI recipients with mental retardation who were enrolled in the demonstration and those of the overall population of eligible SSI

recipients. Again, this information is useful when considering the generalizability of the evaluation findings.

We conclude Chapter II by examining what would have happened in the absence of the demonstration to the SSI recipients who were enrolled. Our examination is based only on the experience of the control-group members and represents the situation with which the demonstration is being compared. Specifically, the demonstration was introduced into communities in which SSI recipients could have used some services, and some changes in their earnings and SSI receipt would probably have occurred over time even in the absence of the demonstration. It is relative to this "status quo" that the demonstration is evaluated. Thus, it is essential that we understand the extent to which the SSI recipients who enrolled in the demonstration would have enrolled in other programs, and the extent to which their employment, earnings, and SSI receipt would have changed over time even if the demonstration services had not been available.

A. THE DEMONSTRATION SAMPLE RECRUITMENT PROCESS

Any effort to interpret and build on the results of the demonstration must consider the characteristics of the persons who were enrolled. In particular, the impacts of the demonstration are likely to be influenced by the fact that all demonstration sample members were SSI recipients who responded to the demonstration outreach activities and who volunteered to participate. Furthermore, it should be noted that the SSI recipients who were enrolled in the demonstration do not constitute a representative sample of all SSI recipients, and thus efforts to expand transitional employment to other recipient groups must account for any differences in the experience or abilities of the demonstration sample members and those of the other recipient groups.

Applicants to the demonstration were recruited from within a specified target population, consisting of persons who were:

- o SSI recipients
- o Between the ages of 18 and 40
- o Diagnosed as having mental retardation
- o Residing within the area served by one of the eight demonstration projects

Social Security records indicate that about 13,800 persons met these basic eligibility criteria across the demonstration projects. Projects used various forms of outreach to attract a broad sample of SSI recipients to the demonstration--mailings to prospective enrollees, follow-up telephone calls, and other forms of outreach.

The outreach effort was initiated when personalized letters were sent to eligible SSI recipients (or, when appropriate, their representative payees) to invite them to apply to their local demonstration project.[1] Eligible persons were identified by the Social Security Administration, which screened the case folders of over 25,000 SSI recipients who lived in the demonstration catchment areas and were between 18 and 40 years of age to identify those persons with mental retardation. A total of 12,174 letters were sent to eligible persons (or

[1]A representative payee was appointed to receive and manage the use of a recipient's SSI payments in the event that project staff judged that the recipient was incapable of handling his or her own funds. Over 80 percent of those SSI recipients who were eligible to receive a letter had a representative payee.

their representative payees) across the demonstration sites, representing almost 90 percent of the eligible population.[2]

In addition, follow-up letters were sent and telephone calls were made to persons who did not respond to the initial mailing. Each project also conducted a variety of other forms of outreach, which included making presentations about the project, mailing material to other service providers and community groups, and encouraging service providers with whom they had contact on a regular basis to refer clients. Projects often worked with the school system to target students who were close to graduation. The media also helped disseminate information about the projects to a wider audience. At least one project also worked with the SSA district offices to obtain referrals from that source.

Approximately 2,400 persons, or 17 percent of the 13,800 eligible recipients, responded to the outreach efforts with interest. Each project established an intake system through which interested persons were assessed on an individual basis in order to determine their suitability for the project. Projects were encouraged to devise an intake system that met both their own needs and those of the overall demonstration. However, demonstration guidelines in this area were minimal. Beyond the basic criteria identified earlier (SSI recipients between the ages of 18 and 40 who had a diagnosis of mental retardation and resided in the catchment area), projects were encouraged to enroll individuals who had a reasonable chance of success but who also represented a wide range of abilities and characteristics, including individuals who showed severely limited abilities. Moreover, once persons had applied to

[2]Letters were sent to all eligible persons identified by the Social Security Administration, with the exception of persons who lived in state institutions to whom the project which served that catchment area requested that letters not be sent.

demonstration projects their contact with project staff was to be relatively brief, so as to avoid raising unduly high expectations for services among those individuals who would eventually be assigned to the control group. Thus, direct contact with applicants prior to random assignment was to be limited to a total of two hours, and the process was to be completed within two weeks' elapsed time, starting with the point of application.

In the intake sessions, applicants and their families (or guardians or other service providers who accompanied the client to the session) were typically provided with additional information about the program and in turn provided information about the applicant to intake staff, to enable the staff to assess his/her appropriateness for the program. Both the criteria for defining and the methods of determining "appropriateness" varied among the projects. Factors that led to immediate rejection at some projects included the following: mental illness, uncontrollable seizures, an inability to self-administer medication, and an inability to travel alone and an unwillingness to accept travel training, as well as certain physical limitations, such as the inability to work in a standing position. Beyond these criteria, project staff considered many other dimensions which helped predict vocational success: behavioral and emotional conditions, attitude and enthusiasm towards working, the nature of the applicant's support system, the availability of transportation, grooming practices, communication skills, vocational history, attendance and punctuality for the intake sessions, IQ score, behavior toward others, and medical problems. Essentially, each applicant was assessed as having certain positive attributes and certain negative attributes along these dimensions. The staff member or group of staff members who made the enrollment decision assessed these attributes and weighed the attributes in a fairly

subjective manner when determining an applicant's suitability for the demonstration.

In addition to meeting these program screening criteria, each eligible SSI recipient who came into contact with the program had to make an explicit or implicit decision to be enrolled. This "self-screening" took several forms. For example, some individuals chose not to respond to outreach efforts, while others contacted the projects but subsequently chose to decline the offer of demonstration services after finding out more about the nature of the program.

A total of 745 persons completed all aspects of this recruitment and enrollment process. Clearly, these persons constitute a select sample of all SSI recipients with mental retardation in the sense that they (1) expressed interest in transitional-employment services, (2) volunteered to participate in the demonstration program, (3) were judged by project intake workers to be appropriate for the demonstration, and (4) had sufficient desire and interest in employment that they completed all stages of the intake process. The characteristics of these 745 SSI recipients are examined in the next section.

B. THE PRE-ENROLLMENT CHARACTERISTICS OF THE SAMPLE MEMBERS

The members of the control group in the demonstration exhibited the characteristics of a segment of the population that is difficult to employ, regardless of their mental capabilities: they were young, were receiving income-maintenance payments, and had relatively little education or work experience. A substantial body of literature indicates that such persons would have difficulty in the labor market even if they were not also constrained by

cognitive limitations.[3] Thus, the projects faced a substantial challenge in placing and maintaining the demonstration sample members in jobs.

Table II.1 presents the characteristics of the full sample of 745 persons as measured at intake, disaggregated by treatment- and control-group status. Due to the random-assignment procedures used in the demonstration, these two groups are essentially identical, and the subsequent discussion will focus on the overall sample of SSI recipients enrolled in the demonstration. (Appendix B discusses the statistical tests of the equivalence of the treatment and control groups.)

As shown in Table II.1, the demonstration projects successfully enrolled persons who met the basic demonstration eligibility criteria. Specifically, sample members:

- Were SSI recipients
- Had a diagnosis of mental retardation
- Were between the ages of 18 and 40
- Resided in the catchment areas of the sites

In addition, the sample included individuals of diverse demographic characteristics, impairments, and employment backgrounds. However, despite having serious impairments, the persons enrolled in the demonstration had generally engaged in some type of vocational activity in the year prior to enrollment, although they were not very productive economically, as

[3]The labor-market implications of education and work experience have been widely studied; Ehrenberg and Smith (1985) provide an overview of this literature, as well as a number of references. The labor-market problems of young workers have also received considerable attention and are summarized by Betsy, Hollister, and Papageorgiou (1985) and by Rees (1986).

TABLE II.1

CHARACTERISTICS OF THE RESEARCH SAMPLE
AT ENROLLMENT, BY RANDOMIZATION STATUS
(Entries in the Table Are the Percent of the Sample with the Characterisitic unless Otherwise Specified)

	Treatment-Group Members	Control-Group Members	Total Sample
Project (percent)			
AHEDD	21.6	20.8	21.2
ARC/MU	10.4	11.1	10.7
The CENTER	7.2	7.0	7.1
Children's Hospital	7.7	7.6	7.7
ECF	20.5	20.8	20.7
Goodwill	9.6	9.5	9.5
UWash/PCC	12.0	12.7	12.3
UWis/Stout	10.9	10.5	10.7
Age (percent)			
Younger than 22	22.4	22.2	22.3
22-25	30.4	28.9	29.7
26-30	25.1	26.8	25.9
31-35	11.5	12.2	11.8
36-40	8.8	6.8	7.8
Older than 40	1.9	3.2	2.6
Average age (years)	26.4	26.6	6.5
Gender (percent)			
Female	41.1	40.5	40.8
Male	58.9	59.5	59.2
Race (percent)			
Black	32.5	28.1	30.3
White and other	67.5	71.9	69.7
Measured IQ Score (percent)			
Greater than 70	10.9	8.9	9.9
55 to 70	50.4	46.8	48.6
40 to 54	32.5	37.6	35.0
Below 40	6.1	6.8	6.4
Average IQ score	57.1	56.0	56.6
Total Income per Person during Year Prior to Enrollment (Dollars)	$5,113	$5,004	$5,058
Average Time on SSI (years)	6.4	6.6	6.5
Total SSI Received per Person during Year Prior to Enrollment (Dollars)[a]	$3,691	$3,584	$3,638

TABLE II.1 (continued)

	Treatment-Group Members	Control-Group Members	Total Sample
Receipt of Transfers (percent)[b]			
Food stamps	19.0	17.8	18.4
Welfare[c]	15.6	12.4	14.0
Medicaid	93.0	93.0	93.0
Concurrently Receiving Social Security Benefits (percent)	32.3	29.7	31.0
Total Other Unearned Income per Person during Year Prior to Enrollment (Dollars)	$970	$971	$970
Vocational Activity during the Year Prior to Enrollment			
Percent of sample with some vocational activity:[d]	66.7	70.5	68.6
Regular job	9.9	11.1	10.5
Mainstream job training or volunteer job	8.3	8.4	8.3
Work in sheltered workshop or enclave	32.8	34.1	33.4
Other type of job	15.7	17.0	16.4
Percent of sample with no vocational activity	33.3	29.5	31.4
School (percent)			
In school at enrollment	15.7	13.6	14.7
Not in school	84.3	86.4	85.3
Total Earned Income per Person during Year Prior to Enrollment (Dollars)	$452	$449	$450
Living Arrangement (percent)			
In a supervised or semi-supervised setting	15.6	21.2	18.4
With parents	64.5	60.6	62.6
Independent	19.9	18.2	19.1
Unassisted Use of Public Transportation (percent)			
Has used regularly	76.7	77.6	77.2
Has not used regularly	23.3	22.4	22.8
Physical, Social, and Emotional Characteristics[b] (percent)			
Has been institutionalized	17.9	18.1	18.0
Is receiving psychiatric treatment	12.5	16.1	14.3
Has one or more physical disabilities that limit employment[e]	38.9	40.8	39.9

TABLE II.1 (continued)

	Treatment-Group Members	Control-Group Members	Total Sample
Has one or more emotional problems that limit employment[f]	22.3	26.3	24.3
Exhibits social behavior inadequate for job interview situation[g]	46.9	45.3	46.1
Cannot speak clearly in sentences	27.0	25.7	26.4
Exhibits at least one of these characteristics	82.7	82.9	82.8
Intake Worker's Opinion of Probability of Success in Competitive Job (percent)			
High	36.0	34.1	35.0
Medium	52.3	53.5	52.9
Low	11.7	12.4	12.1
Sample Size	375	370	745

SOURCE: Intake Data Collection Form and SSA records data.

[a] SSI payment includes an imputation of the state supplemental payment for sample members at the CENTER and UWash/PCC because individual-level data on the state supplementation were not available for Illinois and Oregon, which have state-administered SSI supplements.

[b] Categories are not mutually exclusive.

[c] Welfare includes Aid to Families with Dependent Children (AFDC) and General Assistance.

[d] For persons with a job, the classifications are hierarchical and mutually exclusive. Thus, some persons who held regular jobs may also have been in a workshop for part of the year.

[e] Physical disabilities include severe visual or hearing impairment, seizure disorders, cerebral palsy, general health problems, arm/head mobility problems, whole body range-of-motion limitations, and ambulatory limitations.

[f] Emotional problems include emotional impairment, mental illness, chemical or drug dependency or abuse, and maladaptive, anti-social, or disruptive behavior.

[g] Inadequate social behavior includes inattention to interview, inability to respond appropriately to questions and conversation, inability to make eye contact with interviewer, inability to display appropriate greetings, postures, or gestures, inadequate grooming or attire, physical appearance and characteristic not "normal," and exhibition of unusual behavior or gestures.

evidenced by their low earnings and the small proportion of the sample who had worked in a regular job during the previous year.

The basic demographic characteristics of the sample indicate that the average age at enrollment was 27 years; 22 percent of the sample members were younger than age 22 (and thus potentially eligible for special education services), and 10 percent were older than age 35. Forty-one percent of the sample members were female. Approximately 30 percent of the sample members were black; the others were predominantly white.

Over 40 percent of the sample members were enrolled in two projects, AHEDD and ECF. The remainder of the sample was distributed fairly evenly across the other projects.

The sample members exhibited a wide range of measured intellectual capabilities. While the average IQ score for the sample was 57 (which is considered to be in the mild range, close to moderate), 6 percent of the sample members would be considered severely or profoundly retarded, with an IQ score of below 40. Thirty-five percent of the sample members would be considered moderately retarded (with an IQ score of between 40 and 54), while 49 percent of the sample had IQ scores in the mild range (an IQ score of between 55 and 70). Ten percent of the sample members had an IQ score of higher than 70 (indicating no mental retardation). All of the persons whose IQ scores were higher than 70 contacted the program after receiving an invitation letter from

SSA. Given SSI disability criteria, these person had other handicapping conditions that made employment difficult.[4]

The projects enrolled a group of persons who were relatively economically disadvantaged. The average total income received by sample members from all sources during the year prior to enrollment was just over $5,000; SSI payments accounted for roughly 72 percent of this total. On average, sample members had been receiving SSI payments for over 6 years, and about 20 percent also lived in households that received food stamps or welfare (including Aid to Families with Dependent Children and General Assistance). Almost all (93 percent) of the sample members reported receiving Medicaid benefits, and 31 percent received SSI and Social Security benefits concurrently.

Not surprisingly, very few of the recipients who enrolled in the demonstration had recent work experience in the regular unsubsidized labor market. While almost 70 percent of the sample members had engaged in some type of vocational activity during the year prior to enrollment, only 11 percent had held a regular job--that is, paid employment that entailed working without special supervision with nonhandicapped co-workers, and performing work that was typically undertaken by nonhandicapped workers. The predominant vocational activity was participation in sheltered workshops; one-third of the sample members had engaged in such activity in the prior year. In addition, at the

[4]It is possible that individuals whose measured IQ scores were higher than 70 had adaptive-behavior problems that would lead to their classification as having mental retardation (Grossman, 1983, discusses this aspect of the definition of mental retardation). Alternatively, when an applicant for SSI alleges more than one impairment and provides evidence to support the existence of one of them, the case records may cite both impairments even if the evidence to support the second diagnosis is incomplete. Hence, it is possible that some individuals could have been identified as mentally retarded and been enrolled in the demonstration even though their measured intellectual limitations were not sufficiently severe to contribute to a finding of disability.

time of enrollment, 15 percent of the sample members were attending school or participating in an educational program.

Given the small percentage of the sample who had worked in regular jobs during the year prior to enrollment, it is not surprising that the average earned income during that year was only $449. This earnings figure constitutes only 9 percent of the total income for this sample.

In addition to the various types of financial support received by sample members, the majority appeared to need other types of support in their daily living. Almost two-thirds of the sample members lived with their parents, and another 21 percent lived in a supervised or semi-supervised setting (including a supervised apartment or setting in which a counselor dropped in at least once a week, as well as group homes, care homes, and halfway houses). Only 18 percent lived independently (or with a spouse, children, or unrelated housemate). Further evidence of the level of support provided to sample members is that 23 percent had never traveled unassisted via public transportation on a regular basis.

When individuals applied to the projects, staff were asked to record any conditions or characteristics that would seriously limit the applicant's ability to obtain and retain a job. Since intake staff had only a limited opportunity to meet with the applicants prior to random assignment, they could record only those conditions that were reported by applicants or referral agencies or those that became obvious during the intake process. This information indicates that 83 percent of the sample members had at least one such limitation, and, although the types of impairments exhibited by individuals varied widely, social or physical limitations were more common than emotional problems. Almost half (46 percent) of the sample members were assessed as exhibiting a social behavior

that was inappropriate for a job interview situation (such as inattentiveness to the interview or an inability to respond appropriately to questions and conversation; an inability to make eye contact with the interviewer; an inability to display appropriate greetings, postures, or gestures; inadequate grooming or attire; physical appearance and other characteristics judged as not "normal"; and exhibition of unusual behavior or gestures). Forty percent of the sample members were assessed as having one or more physical disabilities that might limit employment success (such as severe visual or hearing impairment, seizure disorders, cerebral palsy, general health problems, arm or head mobility problems, whole body range-of-motion limitations, and ambulatory limitations). Over one-quarter of the sample members could not speak clearly in sentences. Twenty-six percent were assessed as having an emotional problem (such as an emotional impairment, mental illness, chemical or drug dependency or abuse, and maladaptive, anti-social, or disruptive behavior). In addition, 18 percent of the sample members had been institutionalized prior to enrolling in the program, and 14 percent were receiving psychiatric treatment at the time of enrollment. In sum, the sample members were a seriously impaired group who would have found it difficult to obtain or to hold a job on their own.

Finally, intake staff were asked to record in the intake interview their opinion about the probability of the applicant's completing training and being successful in a competitive job. Only 35 percent of the sample members were judged to have a high probability of success. Staff predicted a low probability of success for 12 percent of the cases, indicating that staff did appear to take some risks in order to enroll a sample that included severely impaired persons.

C. A COMPARISON OF THE RESEARCH SAMPLE WITH THE ELIGIBLE POPULATION

The process of recruiting and screening eligible persons for the demonstration did not generate a representative sample of all SSI recipients with mental retardation. Thus, it is of interest to examine how persons who were enrolled differed from those who were not. A limited comparison can be made by using Social Security records, but much of this comparison must be undertaken by considering how the recruitment and screening process would have affected different types of persons.

Table II.2 presents the available records data on sample members and on those SSI recipients who were sent invitation letters (invitees) but were not enrolled. A comparison of these data indicates that the research sample is somewhat younger and much more likely to have held a job prior to the demonstration. Sample members were about a year younger than the group of invitees and, correspondingly, had received SSI benefits for a shorter period of time. Sample members were almost twice as likely to have reported wages prior to the demonstration, although reported wages for both groups tended to be quite low.

In addition to these measured differences, the recruitment and screening process is likely to have generated a sample that differed from all SSI recipients with mental retardation. The demonstration asked SSI recipients to make a potentially large change in their lives--that is, to enter the labor market, with its attendant risks and opportunities. The SSI recipients who accepted this challenge are likely to have differed from those who chose otherwise--they may have been more motivated and less satisfied with their current service arrangements, had better supports for dealing with work and life, or possessed better adaptive skills. These differences suggest that

TABLE II.2

A COMPARISON OF THE CHARACTERISTICS OF SAMPLE MEMBERS WITH THE CHARACTERISTICS OF THOSE INDIVIDUALS WHO WERE SENT INVITATION LETTERS

Characteristics	Total	
	Sample Members	Other Invitees
Age		
Younger than 22	23.9	20.4
22-30	54.7	48.6
Older than 30	21.5	30.9
Average age	26.7	27.9
Gender[a]		
Female	41.2	45.2
Male	58.8	54.6
Race[a]		
Black	28.3	26.7
White or other	63.0	64.6
Marital Status		
Married	3.3	2.8
Not married	96.7	97.2
Mean Number of Years on SSI	5.4	6.2
Percent Receiving Title II Benefits[b]	31.1	31.8
Percent with Wages	30.2	16.2
Mean Monthly Earnings (Dollars)[b]	31.64	15.72
Mean SSI Monthly Payment[c]	301.95	296.62
Sample Size[d]	633	11,462

SOURCE: Social Security records.

[a]Percentages may not sum to 100 due to missing data.

[b] These data refer to the month during which the data were drawn from the SSA files (November 1984 and January and August 1986).

[c]Includes both federal and state payments.

[d] The sample members included herein include only the 633 SSI recipients who responded to the invitation letter. Data were unavailable for the 112 sample members who were referred to the demonstration.

any attempt to generalize the demonstration findings beyond the sample of SSI recipients who volunteered and were accepted into the demonstration must be undertaken with extreme care.

Nevertheless, the sample that was enrolled is relevant for developing future policy. In one way or another, it is likely that future policy in this area would give SSI recipients a choice about whether to enroll in transitional employment. The persons who would be served would thus be likely to exhibit many of the traits found in the demonstration sample. The major differences would pertain to the selection criteria used by future transitional-employment projects, which might use alternative screening and recruitment procedures.

D. THE ACTIVITIES OF SAMPLE MEMBERS IN THE ABSENCE OF THE DEMONSTRATION

As we noted in Chapter I, the comparison addressed in the demonstration is the net effect of adding transitional-employment services to the existing service system. We estimate this effect by calculating the difference between what treatment-group members actually do in response to their demonstration experience and what they would have done in the absence of the demonstration. Determining the actual response of treatment-group members is relatively straightforward, since their postenrollment experience (i.e., their experience after the random assignment process) can be observed. What their activities would have been--that is, the basis against which impacts will be measured--can be determined by observing the postenrollment experience of the control group.

The experience of control-group members over the three years following their enrollment in the demonstration is described in this section. When considering the data on postenrollment activities, the reader should keep in mind that data for the full sample are available only for the first 29 months

following enrollment; over the subsequent seven-month period, the control group declines in size, until at 36 months the sample consists of 193 of the 370 members who enrolled during the early months of the demonstration. Because the characteristics of the individuals from the early enrollment cohorts are nearly identical to those of the later cohorts, we expect that the experience of the reduced sample over months 29 to 36 accurately represents what was occurring with the entire sample over this time period (see Chapter III). However, the smaller sample size available for these last few months reduces the precision of the corresponding estimates (recall our discussion in Section B of Chapter I).

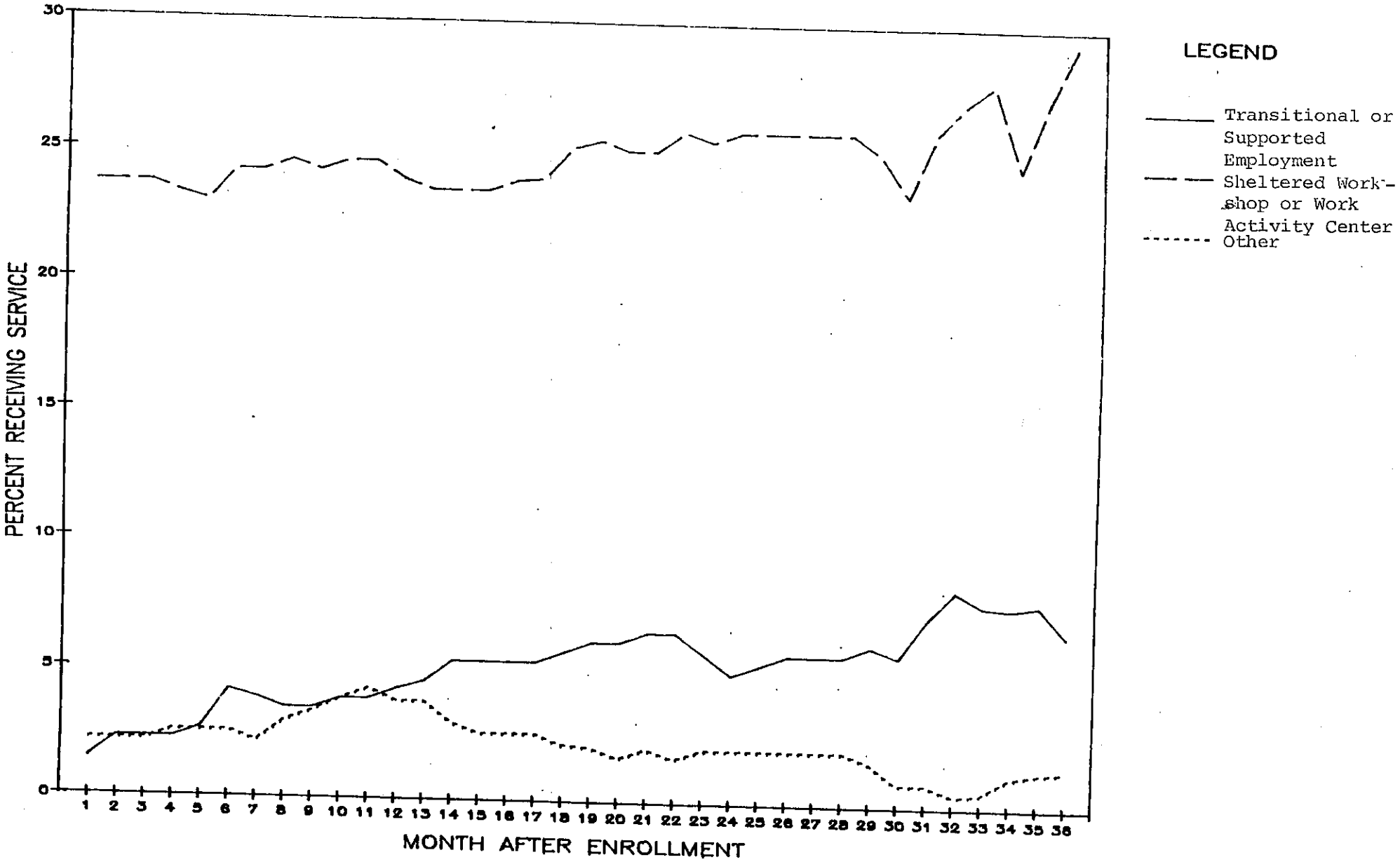
In general, the data on the postenrollment activities of the control-group members indicate that the these individuals experienced slight increases both in their receipt of nondemonstration services and in their earnings and income. However, the predominant service received by the control-group members remained sheltered workshops, and their average earnings remained low.

1. Service Use

The postenrollment pattern of service use by control-group members indicates that a few control-group members sought and found transitional- and supported-employment services outside the demonstration. Nevertheless, the predominant vocational service for the control-group members continued to be sheltered workshops; 25 percent were enrolled in such programs. The trends in service receipt over the 36-month period are shown in Figure II.1.[5] Only about 1.5 percent of the control group were enrolled in a transitional-employment or supported-employment program in the month of randomization. This number

[5]Table C.1 provides the specific estimates used to construct Figure II.1.

FIGURE II.1
PERCENT OF CONTROL GROUP RECEIVING EACH OF THREE SERVICES MONTH 1 TO 36 AFTER ENROLLMENT



increased to 5 percent of the control group by month 14, tripling the rate of transitional-employment service receipt, and it increased slightly to 6 percent by month 36. The rate of participation in sheltered workshops and work activity centers also increased slightly over the 36 months after enrollment, from 24 percent to 26 percent. Roughly 3 percent of the control group received other services (such as classroom training) over the 36-month period, and the receipt of these other services remained fairly constant across the 36-month period.

2. Income and Earnings

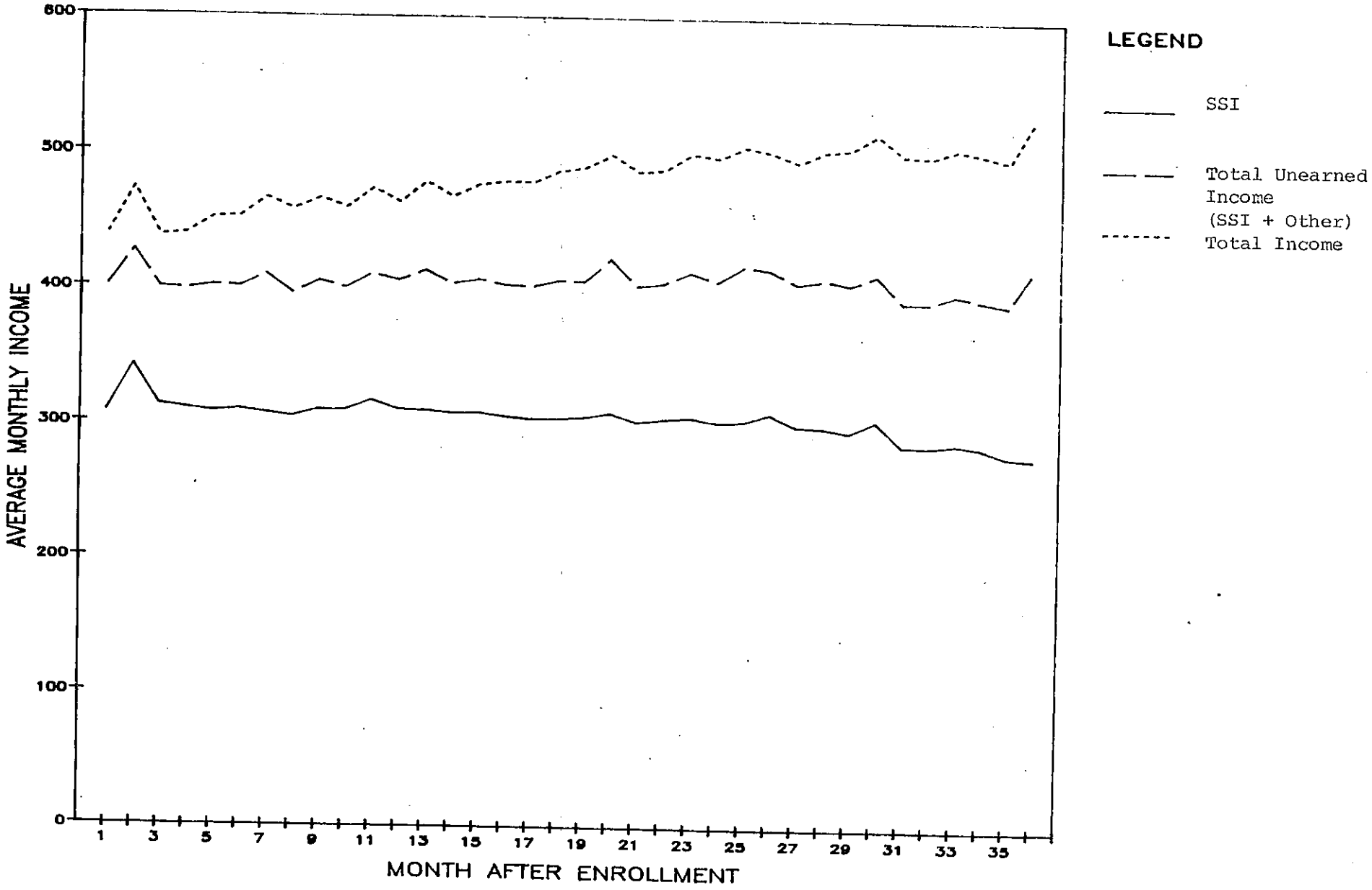
The total income of the control group increased steadily over the 36-month period following enrollment. As total income increased for persons in this group, they came to rely somewhat less on transfer income, such as SSI benefits. However, it appears that even by month 36 transfer payments still constituted the bulk of the total income of the control group.

The 36-month pattern of income receipt by the control group is shown in Figure II.2.[6] Average monthly income increased from $439 in the first month after enrollment to just over $500 in month 29 (the final month for which we have data for the entire sample), an increase of approximately 13 percent. The growth in average monthly income is estimated to have continued; in month 36, average income was about $530, a 21 percent increase from the average level of monthly earnings during the first month following enrollment.

Figure II.2 shows that the majority of the increase in total income over the 36-month period was generated by an increase in earnings. Earnings are represented in Figure II.2 as the difference between total income (shown as the dotted line at the top of the figure) and unearned income (SSI + other

[6]Table C.2 provides the numerical input for Figure II.2.

FIGURE II.2
AVERAGE MONTHLY INCOME BY SOURCE, MONTH 1 TO 36 AFTER ENROLLMENT, CONTROL GROUP



unearned income) (shown as the dashes in the middle of the figure). Although remaining relatively low, their average monthly earnings more than doubled over the first 29 months following enrollment, increasing from $38 to $100 per month. Their earnings level then continued to increase over the next nine months, reaching an average level of $112 in month 36. The primary reason for this increase in average earnings was an increase in the proportion of control-group members who reported having earnings: this proportion grew from 36 percent in the month of randomization to 46 percent by month 29.

The average SSI payments of the control group exhibited little variation over the first 29 months of the demonstration, but then declined slightly between months 29 and 36.[7] SSI payments averaged just over $300 in the first month following enrollment, a figure that represents approximately 70 percent of the average total income received by individuals in the control group. Twenty-nine months after randomization, SSI payments were still approximately $300 per month, but accounted only for 61 percent of the income of control-group members in that month. Average SSI payments fell to $277 by month 36, a 10 percent reduction from average SSI receipt in the first month following enrollment. Because total income increased over time, SSI payments in month 36 also represented a smaller portion (54 percent) of total income.

Thus, we see that the control group did not remain static while the treatment-group members were in the demonstration. While the changes were not dramatic, some control group members were able to enroll in transitional- and supported-employment programs, and, overall, the control-group members were able

[7]It should be noted that the maximum federal SSI benefit is adjusted annually to reflect inflation. From June 1985 to June 1988, this benefit level was increased by approximately 9 percent (Social Security Administration, 1988).

to increase their average earnings. With this background in mind, we now turn to the experience of the treatment-group members and the impacts generated by the demonstration services.

III. IMPACTS OF THE DEMONSTRATION

In this chapter we examine the extent to which the services provided under the demonstration increased the economic self-sufficiency of treatment-group members relative to what it would have been in the absence of the demonstration. Specifically, we estimate the effect that the transitional-employment services provided in the demonstration had on the earnings, SSI payments, total income, and vocational-service use of treatment-group members. We measure this effect by comparing the outcomes for treatment-group members with those for control-group members during the same postenrollment time period. The estimates indicate the net effect of adding the demonstration services to the services that already existed in the communities in which the demonstration was fielded.

As discussed in Chapter I, an experimental design was applied in the demonstration whereby eligible applicants were assigned randomly to the treatment or control group. This design provides a firm basis for estimating the impact of adding the demonstration services to the existing service system, because the random assignment process generated a control group that was virtually identical to the treatment group except for the offer of demonstration services. As indicated in Chapter II, the control-group members were not isolated from their normal environment. They were permitted to participate in any services available outside the demonstration, and were precluded from receiving only those transitional-employment services available from the demonstration projects. In addition, treatment-group members were not required to continue receiving services (or to receive any services) when they no longer desired them. Consequently, the treatment group contained individuals who left the program before being placed in a potentially permanent job, as well as those who were

terminated from the demonstration with jobs. The implementation of the demonstration thus implies that the estimated impacts of the program are not based on comparisons of treatment-group members with individuals who were living in a service vacuum, nor will all treatment-group members have received the same degree of program services. Rather, impacts strictly reflect the demonstration as it was implemented relative to the status quo.

Our presentation of the impact analysis begins by considering the impacts during the first two years following enrollment in the demonstration (that is, the 24 months following each sample member's date of randomization); we then examine the longer-term results by presenting estimated impacts for the third year following enrollment. We conclude our analysis in this chapter by examining the effects for specific subgroups and by assessing whether and the extent to which the impacts decayed over time.

This order of presentation reflects both the varying nature of the post-enrollment period and the differential availability of data over that period. We have data from Social Security Administration records for all 745 sample members for the first two years following enrollment. Because treatment-group members were eligible for demonstration services for one year after enrollment, impacts during the first two years will reflect their activities in the demonstration projects and in the immediately ensuing months of the postdemonstration period. The results for the third year cover a crucial period, since they are indicative of the extent to which the demonstration was able to procure job placements that extended beyond the period in which treatment-group members received demonstration supports. Of course, the success of long-term job retention will have depended on the quality of the demonstration placements

and training, as well as on the ability of the demonstration projects to arrange for any necessary long-term job-retention services.

We have two types of data that can be used to assess the third-year impacts. First, we have SSI records data on all sample members for months 25 to 29 after enrollment, and data on 386 of the 745 sample members for the entire third year after enrollment. In addition, interview data, which were collected for sample members in nine of the thirteen demonstration sites, cover activities at a point between 26 and 44 months after enrollment and thus were used to estimate the third-year impacts of the demonstration.

A. STATISTICAL METHODOLOGY

Our estimates of the impacts of transitional employment were derived with multiple regression procedures to reflect the differences between the treatment and control groups. To the extent that demonstration-induced outcomes are associated with the characteristics of sample members prior to their enrollment in the demonstration, regression analysis explains some of the systematic variation among individuals. This explanatory power enables the analysis to yield estimates of treatment-control differences that are more precise than estimates that are obtained simply by comparing the means for the two groups. In addition, multiple regression adjusts for any minor differences in the measured pre-enrollment characteristics of the treatment and control groups that may have occurred despite random assignment, and which, if not controlled for, might affect the outcomes of interest.[1] The following pre-enrollment characteristics were used in the regression models:

[1]Appendix B provides additional information on the statistical methods applied in the analysis.

- The project in which the sample member was enrolled
- Demographic characteristics
- IQ-score range
- Secondary disabling condition(s)
- Economic status
- Length of time on SSI
- Employment during the year prior to enrollment
- Living arrangement
- Ability to use public transportation
- Month of enrollment in the demonstration
- The intake worker's assessment of the likelihood of the individual's succeeding in a competitive job

Whether an estimated treatment-control difference is interpreted as a real effect of transitional employment rather than as a random occurrence is judged according to standard rules of statistical significance. We deem that an estimated treatment-control difference is evidence that transitional employment has had an effect if that difference is statistically significant at the 95 percent confidence level in a one-tailed test.[2] This criterion means that we will not attribute estimates of treatment-control differences to transitional employment unless they are large enough that only a 5 percent or less probability exists that the differences are due simply to chance.

Of course, simply because we are relatively certain that an observed treatment-control difference is unlikely to have occurred by chance--that is,

[2]We expected the demonstration to affect outcomes only in one direction--SSI payments would fall while earnings would rise. Consequently, we used one-tailed tests of statistical significance.

the difference is found to be statistically significant at conventionally accepted levels--does not mean that the observed difference is large enough to be of substantial interest to policymakers. In this Chapter, we will focus primarily on whether the demonstration services generated the anticipated effects. In the next chapter, we will consider whether these impacts are sufficiently large to justify the investment necessary to operate a transitional-employment program.

B. IMPACTS DURING THE FIRST 24 MONTHS AFTER ENROLLMENT

Our analysis of the impacts of the demonstration through the first 24 months begins by examining the net increase in employment-service use induced by the demonstration. It is the magnitude of this increase that is expected to drive all other impacts. The analysis then focuses on the impact of the demonstration on earnings and SSI benefit receipt.

1. Net Impacts on the Use of Employment Services

The first step in the evaluation is to determine the extent to which the demonstration increased the use of transitional-employment services among treatment-group members. While the demonstration clearly did increase the use of such services, it is the magnitude of this increase that will be important for interpreting the estimated impacts on employment, earnings, and SSI receipt. Because treatment members were free to leave the program at any time and because some control-group members enrolled in transitional-employment programs other than those provided as part of the demonstration, the net increase in the use of transitional employment will be less than the level indicated by the aggregate participation rate observed for the treatment group. In addition, it is important to understand the extent to which the demonstration led to a net

change in the use of other employment services--that is, the demonstration was expected to prompt treatment-group members to reduce their use of sheltered workshops and to increase their use of supported-employment and job-retention services to help them maintain their jobs in the long-term.

An examination of the estimates presented in Table III.1 shows that the demonstration dramatically increased the rate of employment-service receipt among members of the treatment group relative to the rate among control-group members. Members of the treatment group received substantially more transitional-employment services (from both demonstration and nondemonstration programs) than did control-group members. Despite the increased use of transitional employment by control-group members that was documented in Chapter II, treatment-group members were enrolled in such programs for an average of 11 months longer than were control-group members during the 24 months following enrollment in the demonstration.

The treatment-group members were also more likely to be enrolled in non-demonstration programs that provided supported-employment and follow-up services. During the 24 months following enrollment, treatment-group members spent an average of almost six weeks (1.32 months) more in these programs than did control-group members.

Table III.1 also shows that the demonstration effected a 32 percent reduction in the average amount of time spent by members of the treatment group in sheltered workshops. This negative effect on participation in sheltered workshops is consistent with the objective of the demonstration--to reduce the rate at which individuals chose employment in a facility-based setting. Finally, Table III.1 also reveals that the receipt of other vocational services remained low and was not significantly affected by the demonstration.

TABLE III.1

ESTIMATED IMPACTS ON MONTHS OF SERVICE RECEIPT
DURING THE FIRST 24 MONTHS AFTER ENROLLMENT

Service	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Demonstration Transitional-Employment Services	10.80	0	10.80**	47.41
Nondemonstration Transitional Employment	0.52	0.31	0.21	0.93
Supported Employment	1.96	0.75	1.08**	3.01
Follow-Up Services	0.26	0.02	0.24*	2.12
Activity Center	1.49	1.09	0.40	1.04
Sheltered Workshop	3.26	4.76	-1.50*	-2.26
Other	0.31	0.57	-0.26	-0.99
Sample Size	221	234	455	

SOURCE: Extracted data from agency records (see Appendix A).

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Control group means are raw means; they are not regression-adjusted.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

The impacts of the demonstration on the use of employment services are also clear when measured by the proportion of the sample who received services rather than by average months of enrollment. Table III.2 shows the same pattern of results observed in Table III.1. All treatment-group members spent at least some time in a transitional-employment program. In addition, the demonstration increased the proportion of treatment-group members who enrolled in nondemonstration programs that provided transitional-employment, supported-employment, and follow-up services outside the demonstration. The reduction in the rate of sheltered-workshop participation by treatment-group members is also shown by the reduction in the percentage of treatment-group members who attended shelter workshops (21 percent compared with 27 percent of the control-group members).

2. Impacts on Earnings and SSI Receipt

The central goal of the demonstration was to enhance the economic self-sufficiency of SSI recipients with mental retardation. Table III.3 presents the basic estimates pertinent to addressing this objective: estimated impacts on employment, earnings, SSI receipt, and total income for the 24 months following enrollment in the demonstration.

a. Impacts on Earnings and Employment

The driving force behind all these impacts is the increase in employment and earnings. The estimated impacts indicate that the demonstration nearly doubled the average earnings of the treatment-group members relative to

TABLE III.2

ESTIMATED IMPACTS ON THE PERCENTAGE OF SAMPLE MEMBERS WHO RECEIVED SERVICES DURING THE FIRST 24 MONTHS AFTER ENROLLMENT

Service	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Demonstration Transitional-Employment Services	100.0	0.0	100.0**	0
Nondemonstration Transitional Employment	33.5	3.8	2.7**	1.24
Supported Employment	20.2	6.0	14.2**	4.65
Follow-Up Services	3.1	0.8	2.2*	1.71
Activity Center	9.1	8.5	0.6	0.23
Sheltered Workshop	20.8	26.9	-6.1*	-1.72
Other	1.9	4.7	-2.8	-1.53
Sample Size	221	234	455	

SOURCE: Extracted data from agency records (see Appendix A).

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Control group means are raw means; they are not regression-adjusted.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE III.3

ESTIMATED IMPACTS ON EARNINGS AND INCOME BY SOURCE
DURING THE FIRST 24 MONTHS AFTER ENROLLMENT

Income Source	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Percent Who Received Earnings	74.7	56.7	18.0**	5.71
Number of Months Received Earnings	12.5	9.9	2.6**	4.27
Earned Income (Dollars)	$3,129	$1,556	$1,574**	7.06
Unearned Income Other Than SSI (Dollars)	$2,294	$2,320	$-26	-0.19
Federal plus State Supplemental SSI Payments[a] (Dollars)	$7,147	$7,413	$-266*	-1.91
Federal SSI payments	$4,989	$5,213	$-224*	-2.02
State supplemental SSI payments[a]	$2,157	$2,199	$-42	-0.71
Total Income (Dollars)	$12,569	$11,288	$1,281**	5.75
Sample Size	367	358	725	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Control group means are raw means; they are not regression-adjusted.

[a]SSI payments include an imputation of the state supplemental payment for sample members at The CENTER and UWash/PCC because individual-level data on the state supplementation were not available for Illinois and Oregon from the Social Security records.

* Statistically significant at the 95 percent confidence level in a one-tailed test.
** Statistically significant at the 99 percent confidence level in a one-tailed test.

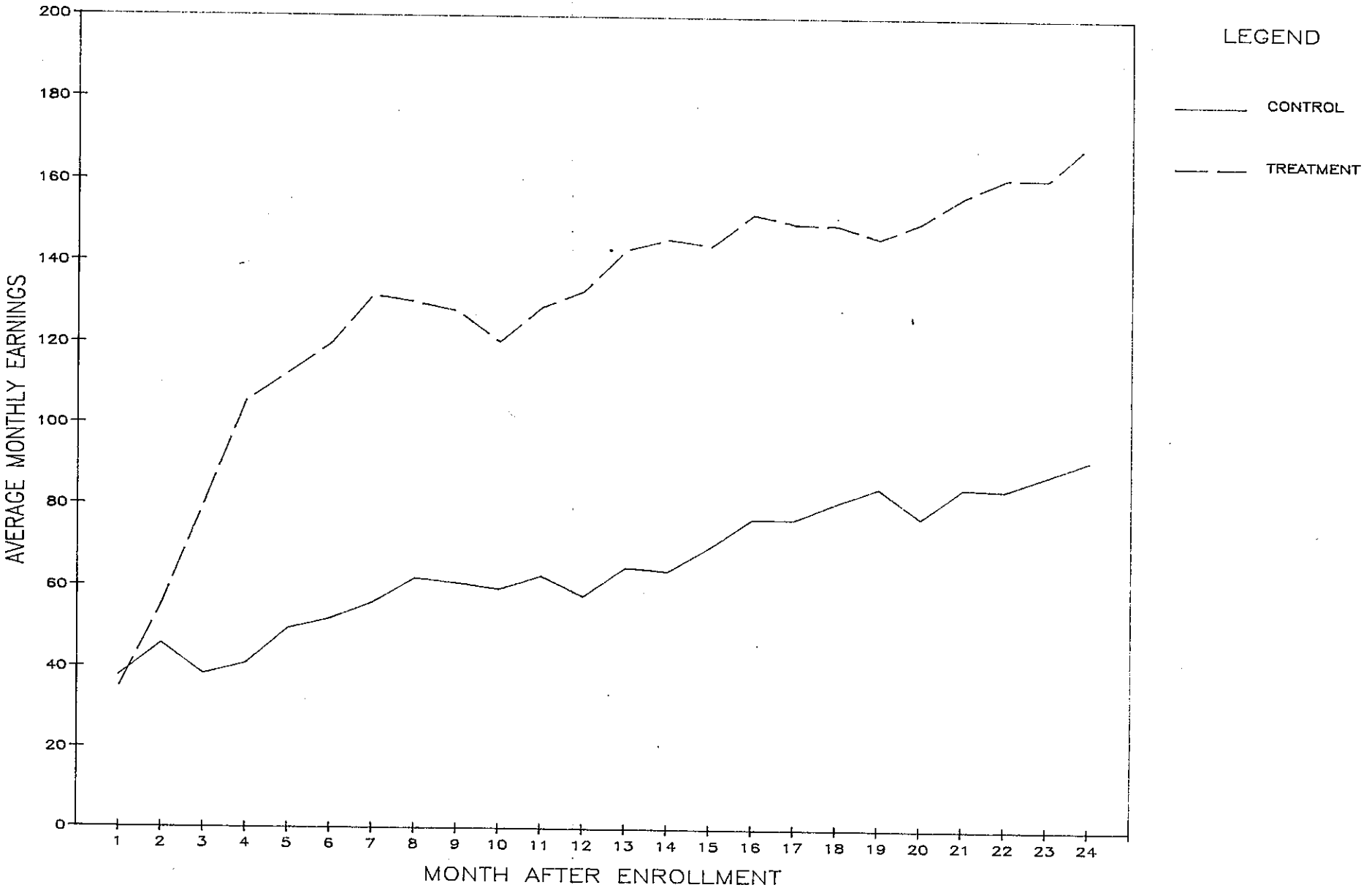
what their earnings would have been in the absence of the demonstration.[3] This estimate includes earnings from all types of employment, including regular jobs as well as those in sheltered workshops. The total earnings of the treatment group averaged just over $3,100, compared with under $1,600 for the control group. This treatment-control difference in earnings represents an increase of approximately $66 in the average monthly earnings of treatment-group members.

Figure III.1 demonstrates that the average earnings for the treatment-group also rose quickly; by the fourth month following enrollment, they reached a level more than $60 higher than the average earnings of the control group. Thereafter, average monthly earnings for the treatment group exceeded the average earnings of the control group by $60 to $85 for the remainder of the 24-month period.

The overall increase in earnings was effected by an increase both in the rate of employment and in the average earnings of those treatment-group members who were employed. The increased employment rate is evident in the proportion of persons who reported earnings and the average number of months reportedly worked, which are cited in Table III.3. We found that the proportion of the treatment group who received earnings at some point during the 24-month period increased by 18 percent relative to the control group, and that treatment-group members received earnings for an average of 12.5 months during the first 24 months, compared with 10 months of earnings for control-group members. The increased earnings of working treatment-group members relative to working control-group members is indicated by regression-adjusted average earnings of over $4,300 for working treatment-group members relative to the approximately

[3]The earnings measures from SSI records pertain to those from all employment, including training jobs and workshop jobs in addition to regular jobs. The interview data disaggregate earnings by job category.

FIGURE III.1
AVERAGE MONTHLY EARNINGS
FOR TREATMENT AND CONTROL GROUPS
MONTH 1 TO 24 AFTER ENROLLMENT



$2,700 of earnings for working control-group members (these estimates are not reported Table III.3).

b. SSI Benefits and Other Unearned Income

As discussed at the close of Chapter I, earnings increases should lead to lower average SSI benefits for treatment-group members, since SSI regulations require that a recipient's benefits be reduced to reflect any earnings and other income that exceed specified thresholds. Because the total benefit amount is reduced by less than the total increase in earnings, we would also expect an increase in total income, unless unearned income other than SSI declined along other dimensions.

The estimates in Table III.3 show that the demonstration services generated only small reductions in the average SSI benefits payments to treatment-group members relative to what they would have been in the absence of the demonstration. Total SSI payments declined by 4 percent ($266) over the 24-month period. For the two-year period, SSI benefit payments to treatment-group members averaged $7,147; for the control group, $7,413. Figure III.2 demonstrates that the impact on average monthly SSI payments fluctuated between no impact and a $25 per-month reduction.[4] It also indicates that the reduction in SSI payments began to take effect in about the sixth month after enrollment, reflecting the two-month lag specified in SSI regulations for adjusting benefits in response to recipient earnings. The disaggregation of payments into federal and state payments indicates that the impact on overall SSI payments occurred primarily through reductions in federal payments. The small difference in state supplemental payments did not differ statistically from zero.

[4]The treatment means used in Figure III.2 are not regression-adjusted.

FIGURE III.2
AVERAGE MONTHLY SSI RECEIPT
FOR TREATMENT AND CONTROL GROUPS
MONTH 1 TO 24 AFTER ENROLLMENT

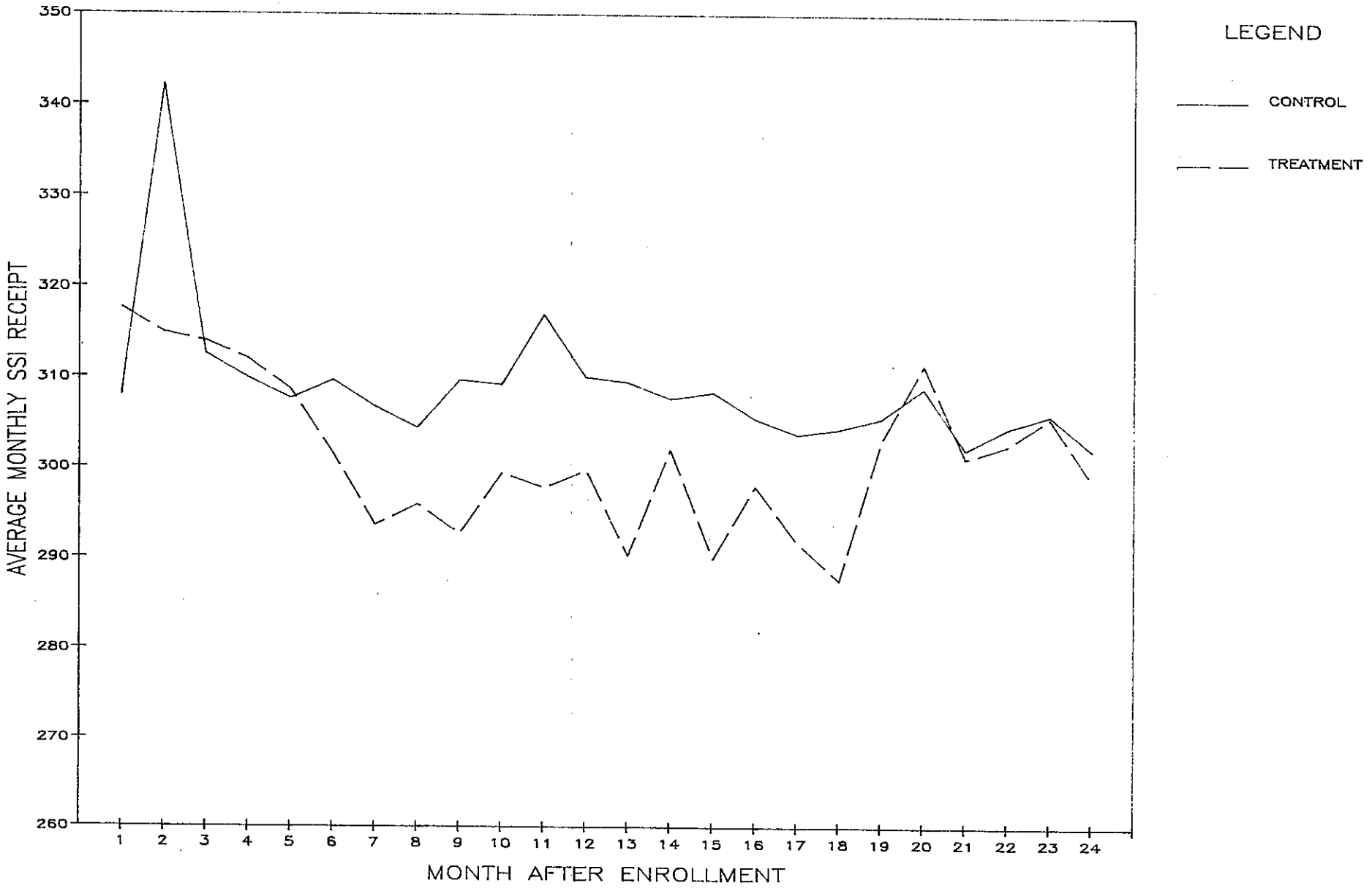


Figure III.2 presents the actual payments made to individuals. Due to various case-processing difficulties, actual payments may have occasionally included adjustments to correct for previous under- and overpayments. While infrequent, these adjustments can in fact be substantial: during the two years following enrollment, eight cases required adjustments that necessitated an excess of $4,000 in monthly SSI payments.[5] These infrequent and large payments give the curves in Figure III.1 their jagged shape.

When the Social Security Administration makes these types of adjustments, it computes what the recipient's payment history should have been. Figure III.3, which is based on this "computational history" of payments, shows much smoother patterns of average SSI payments for both the treatment and control groups. However, the general pattern of impacts does not differ from the pattern observed with the data on actual payments that were shown in Figure III.2.

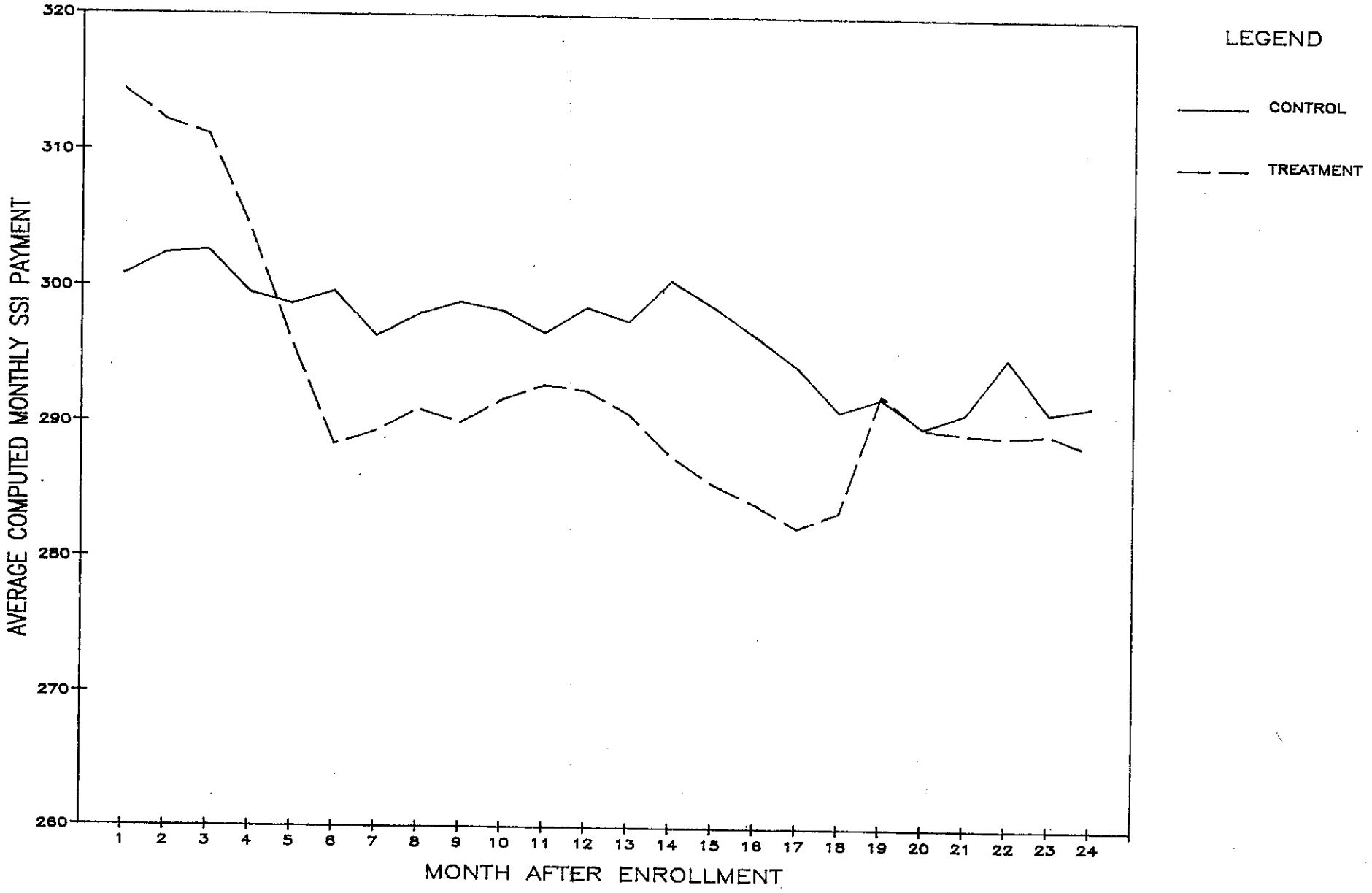
The reduction in SSI benefits over the 24-month period is consistent with what one would expect given the reductions in earnings. Because the unearned income of the treatment and control groups did not differ significantly over the 24-month period (it averaged approximately $2,300 for both groups), any reductions in SSI payments for the treatment group were due to increased earnings.[6] If the entire $66 increase in average monthly earnings were subject to the $65 monthly earnings disregard under the benefit-calculation algorithm,

---

[5]The largest adjustments appeared for some new SSI recipients who experienced a lag between their application for SSI benefits and the time of their first check. The benefits for this entire period were provided in a lump-sum payment in the first check.

[6]Among the demonstration sample members, the primary source of unearned income other than SSI payments was Social Security Disability Insurance payments.

FIGURE III.3
AVERAGE COMPUTED MONTHLY SSI PAYMENT FOR TREATMENT AND CONTROL GROUPS MONTH 1 TO 24 AFTER ENROLLMENT



the 50 percent benefit reduction rate on earnings would have led to a $0.50 per-month reduction in SSI payments, or $12 over 24 months. Conversely, if none of the increase were subject to the basic $65 disregard, then a $33 per month reduction in SSI payments would have been observed for the 24-month period (implying a $792 reduction for the 24-month period). Since it is likely that some proportion of the increased earnings would have been subject to the basic disregard, a reduction in SSI payments due to increased earnings may have been expected to fall somewhere between $12 and $792 for the 24-month period. The measured reduction in SSI payments of $266 did indeed fall within this range.

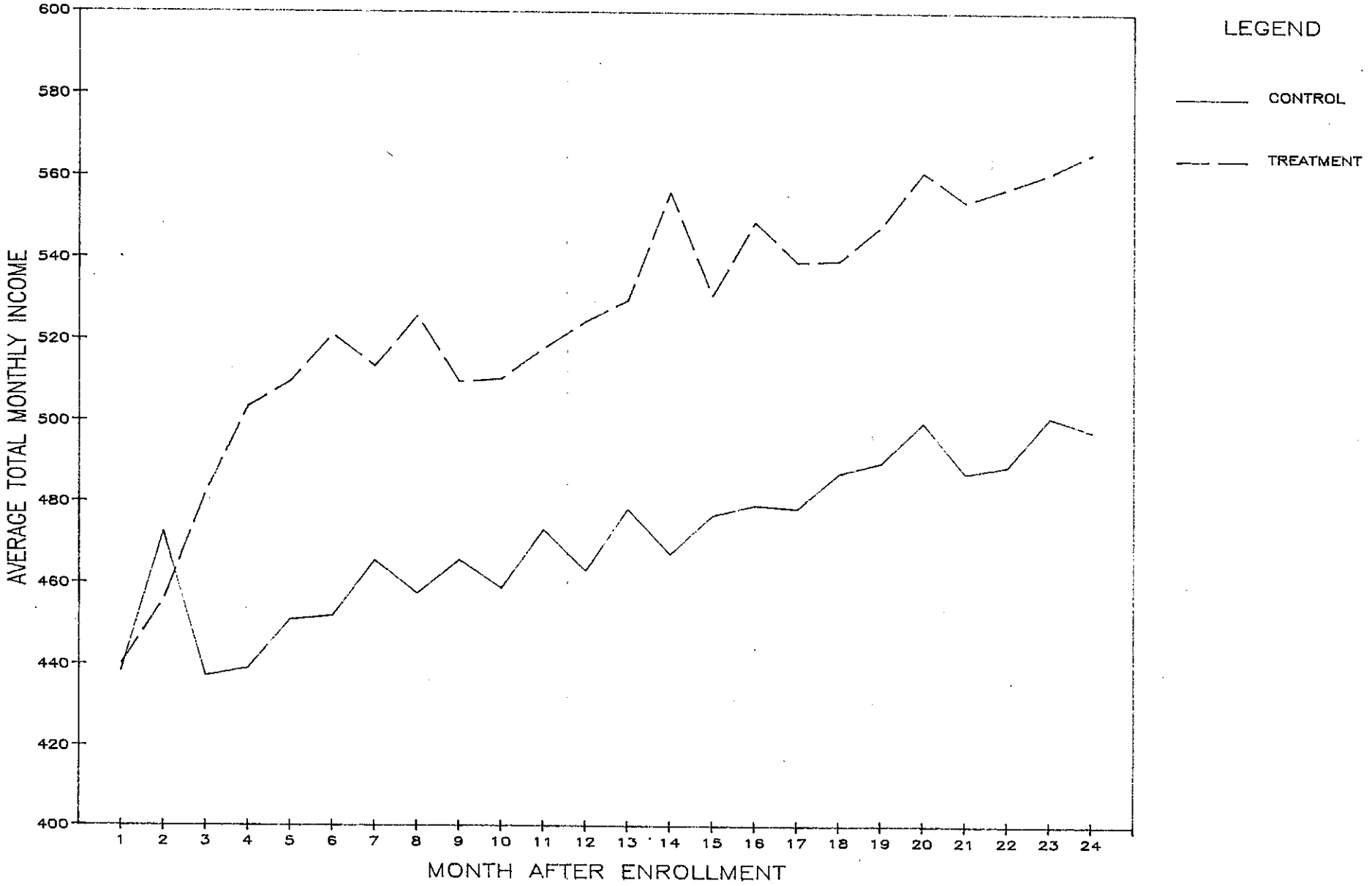
c. Total Income

The combination of the increase in earnings and the small reduction in SSI payments raised the income of the treatment group by almost $1,300 over the 24-month period. As reflected in Figure III.4, total income was higher for the treatment group than for the control group for most of the 24-month period. Monthly income for the control group rose slowly throughout the period, averaging $470. Total income for the treatment group, while identical to the total income of the control group during the first month after enrollment and slightly lower than for the control group during the second month, increased quickly to a level $60 to $70 greater than the control-group level. Thereafter, income for both groups rose steadily over time at roughly the same rate.

d. Annual Impacts during the First 24 Months

The overall impacts measured for the 24-month period can be separated into the impacts for months 1 to 12, which represent the impacts that occurred while most treatment-group members were still enrolled in the demonstration, and the impacts for months 13 to 24, the period immediately after the treatment

FIGURE III.4
AVERAGE TOTAL MONTHLY INCOME
FOR TREATMENT AND CONTROL GROUPS
MONTH 1 TO 24 AFTER ENROLLMENT



group had left the demonstration. The estimated impacts for each of the 12-month periods are presented in Table III.4 and Table III.5.

The impacts of the demonstration on the earnings of treatment-group members in both the in-program and immediate postprogram periods are statistically significant. The observed increases in employment rates and average earnings for treatment-group members during the in-program period are not surprising, given the placement and support services provided by the demonstration projects. The persistence of these impacts into the postprogram period indicates the potential permanence of the job placements.

When the estimates for the two periods are compared, it appears that the employment rate of treatment-group members fell slightly relative to their employment rate for months 1 to 12 and relative to the control group (whose employment rate remained essentially constant over the two-year period). This decline probably reflects the fact that some treatment-group members who were placed on jobs during the demonstration were unable to become stabilized on those jobs. However, despite the slight decline in the impact on employment rates between the two periods, the impact on average earnings appears to have increased over time. While a slight increase in average earnings between the two periods was observed for the control group, the increase for the treatment group was even greater, and so the estimated impact on earnings rose by over 35 percent (from $665 for the first year to just over $900 for the second year). The demonstration services thus appear to have increased both the employment and the earnings of recipients even in the year after which they were no longer receiving direct assistance from the demonstration.

TABLE III.4

ESTIMATED IMPACTS ON EARNINGS AND INCOME BY SOURCE
DURING THE FIRST 12 MONTHS AFTER ENROLLMENT

Income Source	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Percent Who Received Earnings	69.8	50.3	19.48**	6.15
Number of Months Received Earnings	5.91	4.62	1.29**	4.31
Earned Income (Dollars)	$1,283.35	$618.13	$665.22**	6.80
Unearned Income Other Than SSI (Dollars)	$1,104.40	$1,094.42	$9.98	0.17
Federal plus State Supplemental SSI Payments[a] (Dollars)	$3,593.35	$3,746.44	$-153.09*	-2.30
Federal SSI payments	$2,537.21	$2,651.07	$-113.86*	-2.20
State supplemental SSI payments[a]	$1,056.14	$1,095.37	$-39.23	-1.31
Total Income (Dollars)	$5,981.10	$5,458.99	$522.11**	4.70
Sample Size	367	358	725	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Control group means are raw means; they are not regression-adjusted.

[a]SSI payments include an imputation of the state supplemental payment for sample members at The CENTER and UWash/PCC because individual-level data on the state supplementation were not available for Illinois and Oregon from the Social Security records.

* Statistically significant at the 95 percent confidence level in a one-tailed test.
** Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE III.5

ESTIMATED IMPACTS ON EARNINGS AND INCOME BY SOURCE
DURING MONTHS 13 THROUGH 24 AFTER ENROLLMENT

Income Source	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Percent Who Received Earnings	65.02	51.11	13.91**	4.30
Number of Months Received Earnings	6.63	5.30	1.33**	3.67
Earned Income (Dollars)	$1,846.11	$937.55	$908.56**	6.22
Unearned Income Other Than SSI (Dollars)	$1,188.90	$1,225.11	$-36.21	-0.42
Federal plus State Supplemental SSI Payments[a] (Dollars)	3,553.20	3,666.29	-113.09	-1.25
Federal SSI payments	2,452.22	2,562.30	-110.08	-1.50
State supplemental SSI payments[a]	1,100.97	1,103.98	-3.01	-0.09
Total Income (Dollars)	6,588.20	5,828.94	759.26**	(5.46)
Sample Size	367	358	725	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Control group means are raw means; they are not regression-adjusted.

[a]SSI payments include an imputation of the state supplemental payment for sample members at The CENTER and UWash/PCC because individual-level data on the state supplementation were not available for Illinois and Oregon from the Social Security records.

* Statistically significant at the 95 percent confidence level in a one-tailed test.
** Statistically significant at the 99 percent confidence level in a one-tailed test.

C. ESTIMATED IMPACTS AFTER THE FIRST 24 MONTHS FOLLOWING ENROLLMENT

While the observed impacts during the first two years following enrollments are encouraging, the demonstration cannot entirely reach its goal of improving the economic self-sufficiency of SSI recipients with mental retardation without producing effects that persist beyond those two years. The ability of transitional-employment programs to increase employment in the short-term had already been strongly suggested by previous research. The greater uncertainty was whether the demonstration services would be successful at enabling treatment-group members to remain employed in the long-term.

We used both SSI program records and interview data to address this issue. As we discussed in Chapter I, each of these two data sets offers specific advantages and disadvantages in terms of the number of sample members included and the extent of the information collected. Social Security Administration records data are available for the third year after enrollment for the 386 of the 745 demonstration sample members who enrolled early in the demonstration. These records contain information on employment, earnings, SSI benefit receipt, and total income. In contrast, data from the follow-up interview, which was administered between 26 and 44 months after enrollment, provide more detailed information on employment and living arrangements than do the Social Security records, but are available only for sample members who lived in nine of the thirteen demonstration communities.

Statistical tests presented in Appendix B indicate that the cohort of sample members for whom we have third-year records data are generally representative of the entire sample; thus, estimates derived from their records are a valid basis for assessing the demonstration. The representativeness of estimates based on the interview sample is less clear. As discussed in Chapter

I, the characteristics of both sample members and the demonstration projects at the four sites excluded from the follow-up survey differed from those of the sites that were included. Nevertheless, the interview data are representative for the nine sites in which they were collected, and thus provide a reasonable basis for drawing conclusions about the effectiveness of transitional-employment services for SSI recipients with mental retardation.

Our presentation of the findings for the third year begins with the estimates based on the records data and then turns to the estimates derived from the interview data.

1. Estimated Impacts Derived from Social Security Records

The demonstration-induced increase in earnings observed during months 1 to 24 appears to have persisted into months 25 to 36, although at a somewhat reduced size. Table III.6 presents estimates of the impact of the demonstration on employment, earnings, SSI receipt, and other unearned income during months 25 to 36 after enrollment. Relative to the estimates for the two previous years (Tables III.4 and III.5), the estimates for the third year show that the decline in the impact on employment rates continued. In the third year, we estimate that approximately 11 percent more treatment-group members than control-group members were employed at some type of job (including both regular and sheltered-workshop jobs). The corresponding impacts estimated for the first and second years were 19 and 14 percent, respectively. Thus, while we estimate that the demonstration-induced increase in employment persists, it appears to be declining. This decline in the impact of employment rates appears to be responsible for the lower estimated impact on average earnings relative to the impact observed for the second year after enrollment.

TABLE III.6

ESTIMATED IMPACTS ON EARNINGS AND INCOME BY SOURCE
DURING MONTHS 25 THROUGH 36 AFTER ENROLLMENT

Income Source	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Percent Who Received Earnings	63.85	53.19	10.66**	2.34
Number of Months Received Earnings	6.89	5.52	1.37**	2.69
Earned Income (Dollars)	$1,928.80	$1,186.83	$741.97**	3.11
Unearned Income Other Than SSI (Dollars)	$1,341.78	$1,391.04	$-49.26	-0.41
Federal plus State Supplemental SSI Payments[a] (Dollars)	$3,358.21	$3,333.26	$24.95	0.18
Federal SSI payments	$2,358.18	$2,412.19	$-54.01	-0.47
State supplemental SSI payments[a]	$1,000.03	$921.07	$78.96	1.50
Total Income (Dollars)	6,628.79	5,911.13	717.66**	3.44
Sample Size	198	188	386	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Control group means are raw means; they are not regression-adjusted.

[a]SSI payments include an imputation of the state supplemental payment for sample members at The CENTER and UWash/PCC because individual-level data on the state supplementation were not available for Illinois and Oregon from the Social Security records.

* Statistically significant at the 95 percent confidence level in a one-tailed test.
** Statistically significant at the 99 percent confidence level in a one-tailed test.

The declining impact on employment rates appears to be due to a reduction in the proportion of treatment-group members who report earnings, rather than to increases in the proportion of control-group members who were working. In the third year, approximately 50 percent of the control-group members reported earnings to the Social Security Administration, a figure that is approximately the same as that observed during the first two years following enrollment. In contrast, the proportion of treatment-group members who reported earnings continues to decline relative to the levels observed during the in-program period, although this proportion remains higher than the corresponding proportion for the control. It is interesting to note that, while the estimated impact on employment rates seems to have fallen slightly over time, the estimated impact on the average number of months employed per year seems to have remained relatively constant at approximately 1.3 months. Because this estimate pertains to working and nonworking sample members, it suggests that the treatment-group members who no longer reported earnings were those who tended to work fewer months per year.

While the impacts of the demonstration on employment and earnings persist into the third year (although at a somewhat reduced rate), the impacts on SSI receipt seem to disappear. During months 25 to 36, the average amount of SSI benefit payments received by treatment-group members appears to have been virtually unchanged by the demonstration.[7] In addition, the demonstration did not affect unearned income from sources other than SSI over this period, a finding consistent with the previous 24-month period. The lack of impacts on these income sources, combined with the observed increase in earnings, generates

[7]This result is not substantially altered if the "computational payment history" is used rather than actual payments.

an increase in total income for treatment-group members of just over $700 (relative to the income of the control-group members).

The absence of a reduction in SSI payments is somewhat surprising given the estimated increase in average earnings. Depending on how much of the increased earnings were subject to the SSI earnings disregard of $65 per month, the $700 increase in earnings should have reduced SSI payments by between 0 and $350. The observed impact on federal SSI payments is generally consistent with this range (under the assumption that a large part of the earnings increases were disregarded), but the observed increase in state supplemental payments appears to be inconsistent with the other information. One possible explanation is that the demonstration had an impact on living arrangements whereby treatment-group members moved to living arrangements in which they were entitled to greater payments. We will examine this explanation when we review the interview data, which contain information on the place of residence.

2. Estimated Impacts at the Time of the Interview

The purpose of the follow-up interview was to provide information on the postprogram status of participants that was unavailable from Social Security records. The most significant feature of the follow-up interview for our analysis is that it provided information on types of employment and living arrangements. However, it provides this information only for 478 of the 745 sample members--specifically, interviews were administered only to sample members in nine of the demonstration sites: Milwaukee (Goodwill), Boston (Children's Hospital), Northern Wisconsin (UWis/Stout), Los Angeles (ECF), Portland (UWash/PCC), Monmouth County (ARC/MU), Harrisburg (AHEDD), Lancaster (AHEDD), and York (AHEDD). While the interview data are representative of the

impacts at these sites, the exclusion of three AHEDD sites and The CENTER site in Chicago means that estimates based on interview data cannot be compared directly with the estimates based on the entire sample.

a. Impacts on Employment and Earnings

Table III.7 presents the estimated impacts of the demonstration on the employment and earnings of treatment-group members as measured at the time of the follow-up interview. The interview was administered in fall 1988, between 26 and 44 months after the enrollment of participants in the program, depending on when the interview was administered and when respondents had enrolled in the demonstration. An impact estimated from the interview responses thus represents the impact which was observed at about an average of 35 months after enrollment. Given the timing of the interview, impacts that were estimated with the interview data provide a measure of the effect of the demonstration on the employment and earnings experience of respondents beyond the 24-month period for which we have SSI program records for the entire demonstration sample.

In general, the findings based on interview data, which are presented in Table III.7, are consistent with those based on the records data, which are presented in Table III.6. Both sets of estimates indicate that the demonstration increased the employment rate among the treatment-group members relative to what would have been expected in the absence of the demonstration. In addition, the demonstration-induced increase in earnings appears in both sets of estimates.

The interview data provide some additional information on the characteristics of the jobs held by sample members. In particular, it provides information on two crucial job characteristics: whether the job was in

TABLE III.7

ESTIMATED IMPACTS ON EMPLOYMENT AND EARNINGS FOR PRINCIPAL JOB AT THE TIME OF THE INTERVIEW

Employment/Earnings[a]	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Percent Employed	69.8	60.0	9.8**	2.41
Percent in community employment:	45.4	30.2	15.2**	3.51
Independent (percent)	15.0	12.8	2.2	0.69
Wages $\geq$ 3.35	8.2	7.7	0.5	0.21
Wages < 3.35	6.8	5.1	1.7	0.75
With supports (percent)	30.4	17.4	13.0**	3.49
Wages $\geq$ 3.35	11.4	5.1	6.3**	2.54
Wages < 3.35	19.0	12.3	6.7	2.09
Percent in workshop job	24.5	29.8	-5.3	-1.43
Monthly Earnings (Dollars)	$190	$141	$49**	2.42
Percent Whose Monthly Earnings Exceed $300	24.3	17.0	7.2*	2.02
Sample Size	243	235	478	

SOURCE: Follow-up interviews.

NOTE: For the 18 persons who reported more than one job, the job in which they spent the most time was designated as the principal job. Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Control group means are raw means; they are not regression-adjusted.

[a]All persons with any type of job are considered to be employed. Community jobs are those not in sheltered workshops. Independent jobs are community jobs that involve no support from a job coach, training program, or school, while jobs with support do involve assistance from one of these sources.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test

the community (that is, outside of a sheltered workshop), and whether the sample member received support services to help him or her keep the job.

These jobs, often referred to as "community jobs," involve work on an employer's job site rather than in a sheltered workshop. This type of job provides an opportunity for the person with mental retardation to interact with nondisabled persons who are not paid trainers or caregivers. In contrast, sheltered workshops provide employment for groups of persons with impairments who work in segregated facilities or who work together at an employer's site where they are typically differentiated from other workers at the site. The extent to which the demonstration enabled persons to move from workshop jobs to community jobs, thereby increasing their interaction with nondisabled co-workers, supervisors, and customers, was expected to be an important benefit beyond any increases in earnings associated with such moves.

The interview data presented in Table III.7 indicate that the demonstration did have the desired effect on community-job holding. At the time of the interview, 50 percent more treatment-group than control-group members held community jobs (45 percent of the treatment group held a community job, compared with 30 percent of the control group).

The interview data also indicate that the impact on community-job holding occurred primarily for persons who received support services. Approximately equal percentages of treatment-group and control-group members held independent jobs--that is, jobs that (1) were not in a sheltered workshop, and (2) involved no job assistance from someone other than a work supervisor or co-workers.[8]

[8]Jobs in which support was provided by a job coach or that were part of a school or training program were considered to be jobs with supports rather than independent jobs.

However, treatment-group members were almost twice as likely to hold jobs in which some type of support was available. While we do not know the magnitude of this support, this finding suggests a potential role for postdemonstration job-retention services.

One aspect of our analysis of interview data that is not readily apparent from the numbers in Table III.7 is that the estimated impact of the demonstration services on earnings and employment varied greatly according to the time between enrollment and the interview. The impacts appear to have been larger for the group, or cohort, of sample members who enrolled later in the demonstration than for the early enrollees. For example, the estimated impact on average earnings at the time of the interview for the sample members who enrolled before January 1986 was approximately a $15 increase, while the impact on sample members who enrolled in or after January 1986 was about a $108 increase. The $49 increase cited in Table III.7 for the full interview sample is an average of the impacts of the demonstration on all of the respondents.

There are two possible explanations for the variations in treatment impacts on interview outcomes across cohorts. First, because the time between enrollment and the interview differs across sample members, the observed differences in impacts between the sample members who enrolled early and those who enrolled later may be accounted for by changes in the magnitude of impacts over time. Alternatively, the observed differences in impacts may be caused by underlying differences between the two sample-member cohorts.

Both types of explanations would be consistent with the observed impacts. The lower impacts estimated for the cohort who was enrolled prior to January 1986 (the approximate mid-point of demonstration enrollment) would be consistent with a decay in the effect of the demonstration services, since the elapsed time

between enrollment and the interview is longest for this cohort. Similarly, underlying differences between cohorts would also possibly explain the variation in estimated impacts based on interview data if the characteristics of early enrollees or of the demonstration services provided to these enrollees made them less likely to maintain their employment in the long-term.

In the last section of this chapter, we present evidence which suggests that the impacts on the cohorts do not differ significantly when the outcomes are measured at the same point relative to the time of enrollment, as is possible with the Social Security records data. This finding implies that decays in the impacts are probably the more likely explanation for the variation of interview-based impact estimates across cohorts.

c. Impacts on Living Arrangement

At the time of enrollment, most sample members lived in their parents' households. Some of these persons would be expected to move to living arrangements in which they would become less dependent upon family members or guardians, and the demonstration may have helped facilitate such moves. Demonstration-induced shifts in living arrangements would have occurred if the demonstration services enabled treatment-group members to gain greater self-confidence, or if the relatively greater earnings of treatment-group members provided them with greater financial independence. Moves to more independent living arrangements would generally be considered a positive outcome for sample members, since it would mean that they would be residing in settings similar to those used by their nonhandicapped peers. Such moves could also induce higher SSI benefit payments, both because payments are lower for persons who reside in

the household of another person and because many state supplements often provide greater benefits to persons who live in group-home-type settings.

Table III.8 reveals that the demonstration-induced changes in living arrangements were not of sufficient size that we can confidently attribute them to the effect of the demonstration rather than to random chance. Nevertheless, the point estimates are consistent with a slight tendency for treatment-group members to have moved out of their parents' households. The estimates in Table III.8 suggest that a smaller proportion of the treatment-group members were living with a parent, guardian, or other adult at the time of the interview relative to the control group.[9] While the evidence of this shift in living arrangements is weak, such a move would be one explanation for the increases in state supplemental SSI payments.

D. ESTIMATED IMPACTS FOR SUBGROUPS OF THE SAMPLE

It was hypothesized that the size of the impact of transitional employment on earnings might vary for different segments of the demonstration sample. In this section, we present the estimated impacts on earnings for specific subgroups. These estimates, which are based on records data from the Social Security Administration, will be useful for interpreting the overall impacts and for assessing alternative program configurations and targeting strategies. We define the subgroups on the basis of characteristics that are widely used to describe program participants or to target program services. Knowing the

[9]Although not shown in the table, an analysis of the less detailed data on living arrangements contained in the Social Security Administration records provides a similar indication of the potential effects of living arrangements. For the sample members who were enrolled early enough that we had three years of records data, we observed a smaller shift out of the category "living in the households of another person" for treatment-group members than for controls.

TABLE III.8

ESTIMATED IMPACTS ON LIVING ARRANGEMENT
AT THE TIME OF THE INTERVIEW
(Percent)

Living Arrangement	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Living Independently	27.0	23.7	3.3	1.12
Living with Parent, Guardian, or Other Adult Relative	46.5	51.0	-4.5	-1.39
Living in a Supervised Dwelling	26.5	25.3	1.2	0.39
Sample Size	258	253	511	

SOURCE: Follow-up interviews.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Control group means are raw means; they are not regression-adjusted.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

relative effects for these various groups will facilitate comparing the estimated impacts for the demonstration with those for programs (past and future) that enroll groups whose mix of characteristics differs from those of the demonstration sample.

In estimating subgroup impacts, we have focused on the differences in impacts across specific subgroups, holding all other factors constant. This process provides estimates of the impact of the demonstration on persons in a specific subgroup under the assumption that those subgroup members are identical to the overall sample in all respects except for the characteristics used to define the subgroup.[10] For example, the estimated project-specific impacts indicate the effect that each project would have had if all projects served sample members whose pre-enrollment characteristics were identical, and thus the only cross-site differences would have pertained to the services provided and the local environment. Thus, we have used statistical procedures to control for differences among the projects in terms of the pre-enrollment

[10]Alternatively, subgroup-level impacts could have been estimated under the assumption that sample members exhibited the characteristics of the particular subgroup in the analysis sample. If a strong correlation existed among the characteristics used to define the subgroups and if one (or more) of the characteristics had a large influence on an outcome of interest, then the results of tests based on this assumption could differ markedly from those based on the assumption described above. This was, however, not the case. Subgroup-level impact estimates based on this assumption were similar to those presented in this section.

In addition, as with estimates based on any subsample of the full analysis sample of 725 individuals, estimates of treatment-control differences computed for subgroups will have less statistical power than estimates computed for the full sample. Thus, only relatively large subgroup impacts are likely to be statistically significant.

characteristics of sample members that could have influenced the impact of the program.[11]

Subgroups were chosen for this analysis based on our review of the general employment and training literature. As indicated in Chapter I, the subgroups examined were those defined by:

- Project: AHEDD, ARC/MU, The CENTER, Children's Hospital, ECF, Goodwill, UWash/PCC, and UWis/Stout
- IQ score: greater than 70, 55 to 70, 40 to 54, and less than 40
- Age: those younger than 22, and those older than 22
- Gender
- Race/ethnicity: black as compared with white and other
- Living arrangement: living with parents, living in a supervised setting, and living independently
- Work experience in the year prior to enrollment: had a regular job, had a mainstream job training or a volunteer job, worked in a sheltered workshop or enclave, had some other type of job, and had no job
- Received Social Security benefits at enrollment: the receipt of a Social Security benefit in the month prior to enrollment
- Program's opinion of the probability of program completion and success in competitive employment: high as compared with medium and low
- Cohort: enrolled in or prior to September 1985, October 1985 to December 1985, January 1986 to March 1986, or after March 1986

In analyzing the different impacts for these subgroups, we have focused on the demonstration-induced impacts on earnings during the first two years after

[11]The control-group means presented in this chapter are the actual unadjusted means for controls in the subgroup. The treatment-group means are calculated by adding the estimated impact estimates to the control-group means. (See Appendix B for details on the model and the statistical tests used in the subgroup analysis.)

enrollment. This focus reflects the goal of the demonstration to foster economic self-sufficiency, and the fact that employment and earnings gains were expected to be the primary mechanism by which the demonstration achieved its goal.

1. Differences in Earnings Impacts across Projects

In this section, we consider the different earnings impacts generated by the eight demonstration projects under the assumption that the measured characteristics of the sample members at each project were identical.[12] Identifying any differences in earnings impacts will facilitate interpreting the operational differences between demonstration projects as cited in Thornton, Dunstan, and Schore (1988), and will help program operators identify efficient program designs. Of course, since cross-project differences in earnings impacts are due to differences in both the nature of the demonstration services provided at each project and the different characteristics of the local economies and service environments, it is difficult to know the precise cause of any observed differences.

The project-specific impacts on average earnings are shown for each of the three years following enrollment in Tables III.9, III.10, and III.11. In interpreting the estimates in these tables, the reader must keep in mind that the small number of sample members at each project make the project-specific estimates relatively imprecise. Such imprecision is particularly true for the estimates drawn for the third year after enrollment (months 25 to 36).

[12]Appendix B provides a list of the characteristics that were taken into account in the statistical procedures underlying the estimates in this report.

TABLE III.9

ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS BY PROJECT
DURING MONTHS 1 THROUGH 12 AFTER ENROLLMENT
(Dollars)

Project	Sample Size	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Total Sample	725	1,283.29	618.13	665.16**	7.03
Project				#	
AHEDD	153	800.45	628.08	172.37	0.79
ARC/MU	77	3,109.17	947.91	2,161.26**	7.19
The CENTER	50	2,048.47	340.64	1,707.83**	4.50
Children's Hospital	57	1,947.62	997.35	950.27*	2.73
ECF	149	726.67	219.13	507.54*	2.25
Goodwill	70	951.49	467.11	484.38	1.56
UWash/PCC	89	990.82	580.21	410.61	1.36
UWis/Stout	80	1,137.65	1,093.01	44.64	0.14
Sample Size		367	358	725	

SOURE: Social Security Administration records.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Treatment/control differences (and therefore treatment-group members) were evaluated at the overall sample mean. Control group means are raw means; they are not regression-adjusted.

#Treatment-control differences differ statistically across subgroups defined by this variable at the 95 percent confidence level.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE III.10

ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS BY PROJECT
DURING MONTHS 13 THROUGH 24 AFTER ENROLLMENT
(Dollars)

Project	Sample Size	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Total Sample	725	1,851.77	937.55	914.22**	6.2
Project				#	
AHEDD	153	1,392.84	906.53	486.31	1.45
ARC/MU	77	3,797.52	1,666.83	2,130.69**	4.60
The CENTER	50	1,191.90	254.95	936.95	1.60
Children's Hospital	57	3,108.60	2,095.97	1,012.63*	1.89
ECF	149	1,595.79	515.02	1,080.77**	3.11
Goodwill	70	1,591.69	598.86	992.83*	2.07
UWash/PCC	89	1,870.32	760.76	1,109.56**	2.39
UWis/Stout	80	1,026.69	1,152.34	-125.65	0.25
Sample Size	367	358	725		

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Treatment-control differences (and therefore treatment-group members) were evaluated at the overall sample mean. Control group means are raw means; they are not regression-adjusted.

#Treatment-control differences do not differ statistically across subgroups defined by this variable at the 95 percent confidence level.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE III.11

ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS BY PROJECT
DURING MONTHS 25 THROUGH 36 AFTER ENROLLMENT
(Dollars)

Project	Sample Size	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Total Sample	386	1,922.51	1,186.83	735.68	2.51**
Project				#	
AHEDD	108	1,190.91	1,146.70	44.21	0.09
ARC/MU	44	3,862.80	1,985.92	1,876.88**	2.40
The CENTER	17	761.96	195.00	566.96	0.48
Children's Hospital	22	2,333.64	3,346.64	-1,013.00	0.94
ECF	58	2,057.10	725.11	1,331.99**	2.04
Goodwill	44	1,758.44	779.44	979.00	1.35
UWash/PCC	50	2,242.90	723.06	1,519.84*	2.05
UWis/Stout	43	1,502.52	1,392.91	109.61	0.12
Sample Size		198	188	386	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Treatment/control differences (and, therefore, treatment-group members) were evaluated at the overall sample mean. Control group means are raw means; they are not regression-adjusted.

#Treatment/control differences do not differ statistically across subgroups defined by this variable at the 95 percent confidence level.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

A number of interesting patterns of effects emerge in the three tables. In particular, the impacts estimated for ARC/MU, ECF, and UWash/PCC indicate that these projects were most successful at increasing the average earnings of the treatment group relative to those of the control group. The success of the ARC/MU project is particularly noteworthy, since its demonstration-induced impact on earnings appeared quickly and remained relatively constant over the three-year observation period. Over the three-year period, the project was able to more than double the average earnings of the treatment group, with annual earnings increases of approximately $2,000. This success seems to reflect efforts undertaken by ARC/MU to place clients as quickly as possible into competitive jobs whose hours were as close to full time as possible. Moreover, as noted by Thornton, Dunstan, and Schore (1988, Table VI.2), ARC/MU successfully sought to place clients in light manufacturing and assembly jobs that appeared to have the potential for long-term stability and that offered good wages. It seems likely that some of the stability in the average earnings of the treatment group observed for ARC/MU is due to the emphasis on these types of jobs rather than on jobs in food service and cleaning.

The pattern of effects at ECF and UWash/PCC also indicates a successful increase in average earnings. At these two projects, impacts were quite small during the in-program period of the first year. However, they grew substantially during the second and third years. This growth is even more impressive considering the relatively low earnings estimated for control group members at these projects, suggesting that sample members at these projects would have had a difficult time in obtaining employment in the absence of the demonstration.

In contrast, the patterns of impacts at Children's Hospital and The CENTER suggest that, while these projects were successful at increasing short-term earnings, the impacts tended to fade over time. The successful earnings increases in the first year reflect the service models at these projects, which emphasized the use of training sites. Both The CENTER and Children's Hospital placed clients quickly into temporary jobs at training sites that paid at least the minimum wage and required that sample members work a set number of hours per week. Hours of work increased to full time (or nearly full time) as sample members progressed through the training. Thus, a high proportion of sample members at these projects were able to increase their earnings during the early months following enrollment.

While both The CENTER and Children's Hospital sought to find permanent job placements for clients who successfully mastered the training job, it appears that the net impact of these efforts on average earnings dissipated over time. In the strong economy and service environment in which the Children's Hospital project operated, it appears that the average earnings of the control-group rose substantially over time, thereby eliminating the impact of the demonstration services. In contrast, control-group earnings remained low over the entire period at The CENTER, and the decline in the estimated impact appears to be due to a decay in the effect for treatment-group members.

These results highlight the fact that the performance of projects in multi-site programs will vary. While the impact results alone are relatively imprecise (given the small samples in each site), combining these results with the observations from the analysis of program operations suggests that the direct-placement models used by ARC/MU, ECF, and UWash/PCC are particularly effective. Because local service and economic conditions varied across these

projects and their sites, their success does not appear to be determined strictly by either the availability of nondemonstration services or strong local economic conditions.[13]

2. Differences in Earnings Impacts across Other Subgroups

Table III.12 contains the estimated impacts on earnings during the first two years after enrollment for various subgroups of the demonstration sample not defined by project. While the treatment impacts did vary in magnitude across subgroups, statistical tests indicate that none of the within-category variation was statistically significant. Thus, for example, while the specific estimates derived for sample members younger and older than age 22 differ, their difference is not so large (given the precision level of the estimates) that we could be reasonably certain that the differences had not occurred by chance. The demonstration thus does not provide conclusive evidence for different impacts for any of the subgroups examined when the estimates are adjusted to control for all measured characteristics other than those used to define the subgroup.[14]

---

[13]Tables C.8, C.9, and C.10 present project-specific information on the impacts on the use of nondemonstration services for those sites that were included in the follow-up survey. These results suggest that the long-term increases in earnings observed at ARC/MU and UWash/PCC were not achieved by enrolling treatment-group members in nondemonstration employment-support programs. At ECF, there is some evidence that treatment-group members used nondemonstration transitional-employment services to a greater extent than was true of the treatment groups in the other two most successful projects.

[14]The results of statistical tests presented in this section examined treatment-control differences among subgroups as defined earlier. Alternate specifications of subgroups were possible (for example, a binary designation for IQ level) but were not tested because we had no strong evidence, either theoretical or empirical, that alternate specifications would have yielded substantially different results.

TABLE III.12

ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS FOR KEY SUBGROUPS OF THE SAMPLE DURING MONTHS 1 THROUGH 24 AFTER ENROLLMENT
(Dollars)

Subgroup	Treatment-Group Mean	Control-Group Mean	Treatment-Control Difference	t-Statistic
Total Sample	3,135.06	1,555.68	1,579.38**	7.20
Age			#	
Younger than 22	2,846.83	1,634.69	1,212.14**	2.39
22 or older	3,217.11	1,532.94	1,684.17**	6.55
Gender				
Male	3,092.43	1,430.02	1,662.41**	5.75
Female	3,196.04	1,738.14	1,457.90**	4.14
Race				
Black	3,172.74	1,179.75	1,992.99**	4.60
White and other	3,102.33	1,699.37	1,402.96**	5.18
IQ Score				
Greater than 70	4,107.57	1,296.35	2,811.22**	3.92
55 to 70	3,124.99	1,556.79	1,568.20**	4.80
40 to 54	2,961.20	1,654.88	1,306.32**	3.39
Less than 40	1,939.18	710.40	1,228.78	1.33
Received Social Security in the Month Prior to Enrollment				
Received	3,314.69	1,389.00	1,925.69**	3.66
Did not receive	3,051.97	1,626.73	1,425.24**	4.74
Employment History during the Year Prior to Enrollment				
Had a regular job	4,152.66	3,185.71	966.95	1.38
Had mainstream job training or volunteer job	2,934.09	903.75	2,030.34**	2.59
Worked in a sheltered workshop or enclave	3,757.71	1,988.84	1,768.87**	4.12
Had other type of job	2,983.78	1,109.40	1,874.38**	3.20
No job	2,206.15	905.12	1,301.03**	3.02
Living Arrangement				
In supervised or semi-supervised setting	3,489.24	1,920.37	1,568.87**	2.72
With parents	2,949.07	1,488.58	1,460.49**	5.06
Independently	3,325.14	1,344.43	1,980.71**	3.66
Cohort				
Enrolled 6/85 to 9/85	2,887.70	1,547.32	1,340.38**	3.35
Enrolled 10/85 to 12/85	3,597.67	1,401.44	2,196.23**	4.50
Enrolled 1/86 to 3/86	2,610.77	1,215.72	1,395.05**	2.55
Enrolled 4/86 to 7/86	3,363.15	1,866.69	1,496.46**	3.51
Intake Worker's Opinion of Probability of Success on Competitive Job				
High	4,757.88	2,330.36	2,427.52**	5.94
Medium or low	2,272.90	1,150.21	1,122.69**	3.90
Sample Size	367	358	725	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment-control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Treatment-control differences (and, therefore, treatment-group members) are evaluated at the overall sample mean. Control group means are raw means; they are not regression-adjusted.

#Treatment/control differences do not differ statistically across subgroups defined by any variables at the 95 percent confidence level.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

Despite the lack of statistical significance, some of the general patterns in the impacts by subgroups are of interest. First, the magnitude of the impacts for subgroups defined by IQ score was greater for individuals whose scores were higher. The largest impact occurred for individuals whose IQ was higher than 70: the average earnings of this IQ-score subgroup rose by about $2,800 over the 24-month period due to the demonstration. This impact represents a greater than 200 percent increase in the average earnings for the subgroup, the largest impact among the IQ subgroups. The treatment impacts for the middle two IQ-score subgroups were similar to the average impact for the full sample. All of the treatment impacts on earnings for the top three IQ-score categories were also statistically greater than zero at the 99 percent confidence level. In contrast, the impact of the demonstration on earnings for the lowest IQ-score subgroup (those sample members whose scores were below 40) did not differ statistically from zero at the 95 percent confidence level.[15] This finding suggests that time-limited services such as those fielded in the demonstration may not be as effective for persons whose mental retardation is categorized as severe.

Second, the impact for the subgroup that had held a regular job in the year prior to the demonstration was not statistically significant. This result reflects the apparent success of the control-group members in this subgroup at obtaining jobs, rather than a lack of success at placing treatment-group members with prior job experience. This finding suggests that, in order to generate the largest impacts, projects should target their resources toward persons who had

[15]It should be noted that the small size of this subgroup makes it less likely that we will find that an impact of a given size is statistically significant. Thus, this finding, as with all other conclusions about the subgroups, must be interpreted with caution.

not previously held regular jobs. This general result has been widely observed for employment programs that serve broad groups of welfare recipients (see, for example, Cottingham and Ellwood, 1989).

Third, the impacts appear to differ for the two subgroups defined by the intake worker's opinion of the probability that a client will be successful in a competitive job. Although both subgroups of workers defined along this dimension experienced a significant increase in average earnings over 24 months, the increase was approximately $2,400 for the group whose predicted probability of success was high, compared with about $1,100 for the group whose predicted probability of success was medium or low. This finding suggests that the intake workers, who subjectively weighed a large number of sample member characteristics in making their assessments, were able to identify those persons who could best benefit from the transitional-employment services. While the evidence is far from conclusive, this subgroup result suggests that the overall impacts of the demonstration on earnings could be increased if projects were allowed to target their efforts toward the persons whom they felt had the highest probability of benefiting from time-limited transitional-employment services.

E. THE DECAY OF IMPACTS OVER TIME

As discussed earlier, the key to the success of demonstration services is whether treatment-group members can successfully maintain jobs in the long-term. The evidence on the long-term persistence of impacts cannot be assessed directly, given that the length of the postenrollment period of observations differs across enrollment cohorts. Thus, we examined the impacts on earnings

for the two years following enrollment for subgroups defined by enrollment cohorts.

In a previous section of this chapter, we noted that the interview data indicated that the impacts for the early cohorts were smaller than for the later cohorts. The outcomes for the early cohorts that were taken from the interview were measured at a later point in time relative to enrollment than were the outcomes measured for the later cohorts. Due to these cross-cohort differences in the time between enrollment and the point of observation, we are not able to determine whether cross-cohort differences are caused by true variations in impacts among the cohorts or by the simple decay of the impacts over time. Variation in impacts among the cohorts may occur if unobserved differences in demographic characteristics or service receipt among cohorts cannot be controlled for in the regression equations.

In order to disentangle the possible differences among cohorts from the decay of impacts, we examined the cohort-specific earnings impacts derived from the analysis of subgroups as contained in Table III.12. Because these impacts were estimated with Social Security records, the data were available for the entire 24-month period for all sample members. If the cross-cohort impact differences in the impacts based on the interview were caused by unobserved, time-independent differences among the cohorts, then we would expect to find the same pattern of impacts across cohorts in our analysis of SSI-records-based impacts by subgroup in Table III.12.

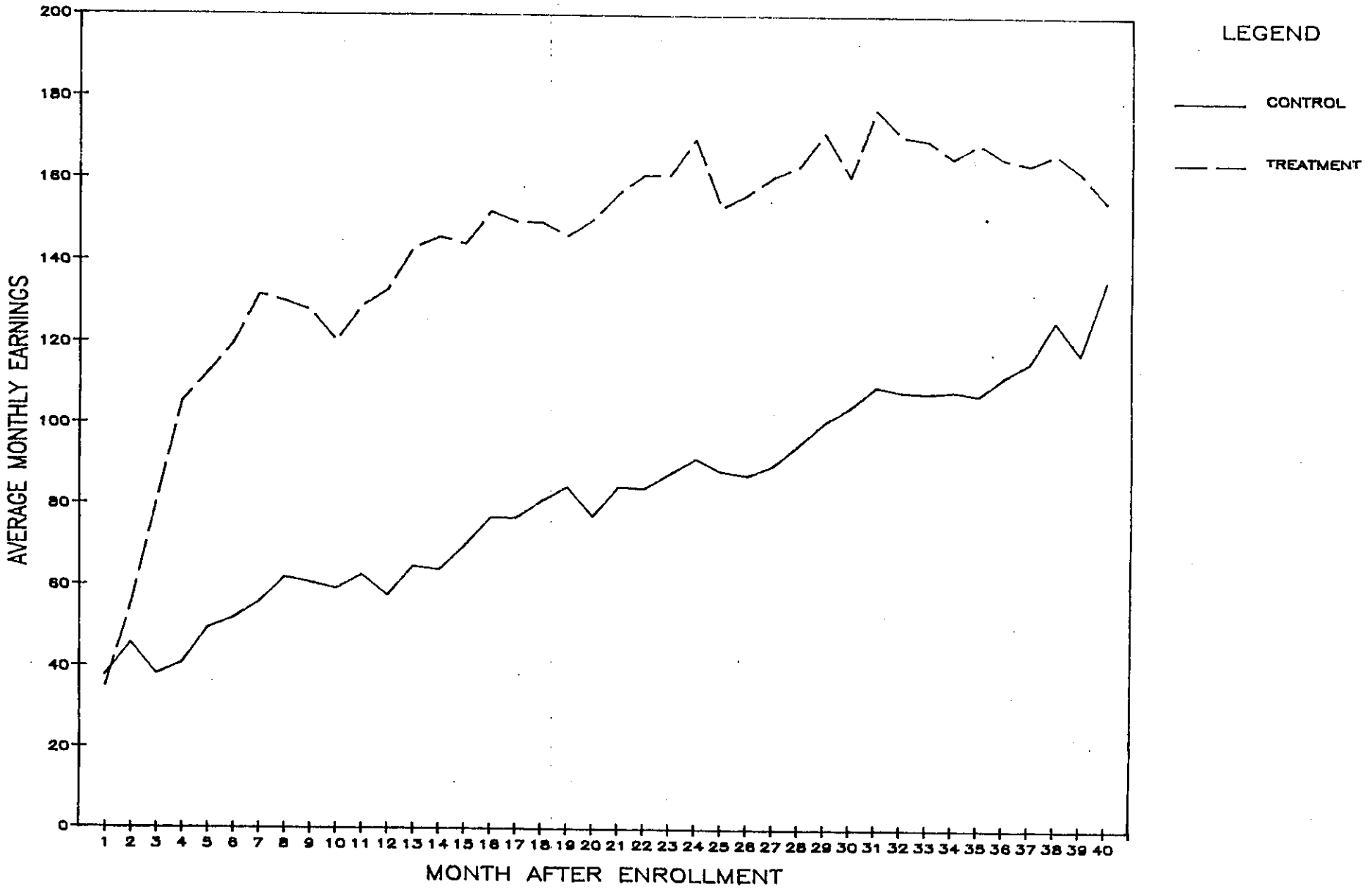
However, Table III.12 indicates that no large differences exist across cohorts, and that, contrary to the findings for the interview outcomes, the early cohorts display slightly larger impacts than do the later cohorts. The impact cited in Table III.12 for the second of the four cohorts was equal to

approximately $2,200. This impact was somewhat greater than the impacts for the other cohorts, which varied between $1,300 and $1,500. In addition, the variation in the impacts of the demonstration on earnings by cohort was not statistically significant at the 95 percent confidence level. The absence of a significant difference in impact estimates across cohorts, combined with the fact that the differences based on SSI records data are not consistent with the pattern of impacts across cohorts from the interview findings, is the basis for our earlier conclusion that it is likely that the variation in impacts by cohort based on the interview responses represents a decay of the impacts of the demonstration on earnings.

Figure III.5 provides further evidence that the impacts of the demonstration on average earnings may have decayed over time. The figure indicates the mean earnings level for the treatment and control groups up to month 40 after enrollment as measured in the Social Security records. Starting at month 31, the treatment-control difference in average earnings declines. Although we have data only on a portion of the full sample beyond month 29, the analysis presented earlier in this report revealed that sample members who enrolled later in the demonstration tended to be similar to earlier enrollees in terms of their characteristics and basic impacts. Thus, we can conclude that the evidence presented in Figure III.5 provides some basis for believing that the impacts of the demonstration decayed over time for the full sample. This issue can be addressed fully only with additional follow-up data.

One final point to be made about decay is that the decay of the demonstration impacts over time varies widely across projects. This assertion is based on the findings presented in Tables III.9, III.10, and III.11. For some of the projects, such as The CENTER, the decay of the impact of

FIGURE III.5
AVERAGE MONTHLY EARNINGS FOR TREATMENT AND CONTROL GROUPS MONTH 1 TO 40 AFTER ENROLLMENT



the demonstration on average earnings is apparent when first-year impacts are compared with second- and third-year impacts. On the other hand, projects such as ECF and UWash display demonstration impacts on earnings that increase over the first three years after enrollment. The long-term impact of the transitional-employment program on the earnings of recipients thus depends largely on the local conditions faced by program operators and the training strategy that they used to serve the local population.

IV. CONCLUSIONS AND IMPLICATIONS

The conclusions from the impact analysis are drawn essentially from four basic observations about the transitional-employment services provided by the demonstration projects to members of the treatment group:

1. The demonstration services increased the employment and earnings of treatment-group members: average earnings for the three years following enrollment were estimated to be 85 percent greater than in the absence of the demonstration, and treatment-group members were more likely to be employed in regular (non-workshop) jobs.

2. The demonstration-induced increase in earnings led to small reductions in the average SSI payments of treatment-group members, although SSI benefits continued to be the major source of income for the treatment group.

3. The demonstration services reduced the use of sheltered-workshop services and increased the use of employment-support services (such as supported employment) by treatment-group members.

4. The costs of providing transitional-employment services in the demonstration were substantial. It is estimated that replicating the service model implemented in the demonstration would cost $5,600 per person enrolled.

As we discuss in the following sections, the implications of these four observations and our assessment of the demonstration overall depend on the analytical perspective that is adopted. In particular, we consider the demonstration from four perspectives that reflect the general views of the major groups that would be involved in the decision to expand transitional employment for SSI recipients with mental retardation:

- The SSI recipients who were enrolled in the demonstration
- The SSI budget (which largely paid for the demonstration services)
- The aggregate government budget
- Society as a whole

A. DEMONSTRATION OUTCOMES FROM THE PERSPECTIVE OF THE TREATMENT-GROUP MEMBERS

From the perspective of the SSI recipients who were assigned to the treatment group, the key outcomes were the increase in earnings and the reduction in SSI payments. Together, these two impacts determined the extent to which treatment-group members benefited financially from their participation in the demonstration. The increased employment in conventional (as opposed to sheltered-workshop) settings was also important, since it represents the integration of the treatment-group members into society. The operating costs of the demonstration programs are essentially irrelevant to the treatment-group members, whose major cost of participation is the time that they devote to training and employment.

Since the estimated increase in earnings far outweighed the reduction in SSI benefits, treatment-group members benefited financially from their participation. The impact estimates presented in Chapter III (in Tables III.4, III.5, and III.6) suggest that the average income of the treatment-group members was more than 10 percent higher than it would have been in the absence of the demonstration. Over the three years following their enrollment in the demonstration, the average earnings of treatment-group members were estimated to have increased by 85 percent (from about $2,700 to just over $5,000), while SSI payments were estimated to have fallen only by 2 percent (approximately $240) over the same three-year period. The net result was an estimated three-year increase in pre-tax income of about $2,000 per treatment-group member.

Even with the increase in average earnings, SSI payments continued to be the major source of income for treatment-group members. Over the three-year period, SSI benefits accounted for just over 50 percent of the income of treatment-group members, while earnings and other unearned income (primarily

benefits from the Social Security Disability Insurance program) each accounted for about one-quarter of total income. The continued receipt of SSI reflects the fact that, while the average earnings gains of treatment-group members were proportionally large, their total earnings remained low relative to the levels that would imply economic self-sufficiency. Consequently, the work-incentive provisions of the SSI program enabled treatment-group members to maintain their eligibility for SSI benefits while they worked.

Given the importance of work in our society, this combination of impacts suggests that treatment-group members will view the offer of transitional-employment services favorably. The demonstration-induced increase in job-holding not only increases the income of treatment-group members, but also provides them with nonpecuniary benefits as they increase their interaction with other members of society and are able to adopt roles that are more in line with those held by their nondisabled peers.[1] At the same time, the continuation of SSI benefits provides basic income support and security that are likely to be important to such persons as the treatment-group members who are seeking to enter an often unsure and volatile labor market. Thus, we conclude that treatment-group members received net pecuniary and nonpecuniary benefits from their enrollment in the demonstration.

B. DEMONSTRATION OUTCOMES FROM THE PERSPECTIVE OF THE SSI BUDGET

The assessment of the transitional-employment services provided in the demonstration is quite different from the perspective of the SSI budget than from the perspective of the treatment-group members. The small reductions in

[1]Matson and Rusch (1986) discuss the preferences of persons with mental retardation for working. Greenblum and Bye (1987) examine evidence of the desire for work by persons who receive disability benefits.

SSI payments represent the only financial benefit to the SSI budget from the investment in transitional-employment services. The reduction in average SSI payments, which was estimated to be approximately $240 per treatment-group member for the entire three-year period following enrollment, would offset only 4 percent of the $5,600 that we estimate it would cost to provide services. This result would not be altered substantially if Social Security disability benefits were considered in addition to SSI payments. As indicated in Chapter III, the demonstration services essentially had no impact on the average level of unearned income (most of which encompassed Social Security disability benefits).

Our analysis of subgroups did not find any evidence that the reduction in SSI payments induced by transitional-employment services for specific subgroups defined according to personal characteristics (such as level of impairment, prior work history, or other demographic characteristics) was sufficient to offset even a major portion of the costs of those service. Essentially, these results mirror the subgroup earnings-impact results shown in Table III.12; for the few cases in which the impact for a specific subgroup was estimated to be larger than the impact for the entire sample, the difference was not sufficiently large (given the precision of the estimates) that we would be confident in asserting that the difference was due to the effects of the demonstration rather than to random chance. Even if the estimates shown in Table III.12 reflect the differences among subgroups, the magnitude of the resulting SSI savings is estimated to be insufficient to offset the operating costs.

In contrast, the project-specific estimates seemed to indicate that the demonstration project operated by ARC/MU may have reduced SSI payments

sufficiently to offset as much as a third of the costs of its delivered services. The evidence on this point is not conclusive, but given that the ARC/MU project appeared to generate above-average increases in earnings (see Tables III.9, III.10, and III.11) we are more willing to conclude that the ARC/MU project also induced above-average reductions in SSI payments. However, even in this case, it seems that the reductions would offset only some of the program costs.

These findings suggest that transitional-employment services are a poor investment for the SSI program to undertake alone. Even if all the projects were able to achieve impacts similar to those estimated for the ARC/MU project, the reductions in SSI would have to persist for at least a year beyond the three-year postenrollment period of our evaluation before the operating costs would be recouped. While such a scenario is possible, it would require that only the experience of the most successful of a carefully selected set of projects be replicated.

Rather than seeking to pay for the services entirely from reductions in SSI payments, it seems more reasonable to seek ways that the SSI program could provide partial program funding in line with the anticipated SSI payment reductions. Such partial funding would provide an incentive for other agencies with responsibility for assisting persons with mental retardation to expand the availability of transitional-employment services, and, if properly designed, could ensure that the SSI budget would not incur net costs. Several possible funding plans deserve consideration--for example, providing state vocational rehabilitation agencies with grants based on the number of SSI recipients served in state-funded transitional employment, or providing funding for ongoing job-retention services to agencies that work with SSI recipients who have been

placed and trained on jobs by transitional-employment programs. In either case, the funding could be based on the estimated SSI savings attributable to the transitional-employment services, so as to keep the funding in line with the expected reduction in SSI payments.

C. DEMONSTRATION OUTCOMES FROM THE PERSPECTIVE OF ALL GOVERNMENT BUDGETS

From the aggregate perspective of all government budgets, the key demonstration effects include those on the use of services other than those provided by the demonstration, as well as the net effect on the SSI budget (that is, the net difference between the costs of the demonstration services and the reduction in SSI benefit payments). Thus, even if the transitional-employment services generated net costs for the SSI budget, it would be possible for those services to generate a net savings from the aggregate budget perspective, if the transitional-employment services prompted treatment-group members to reduce their use of alternative government-funded services.

The alternative services that treatment-group members would have been most likely to use are those provided by sheltered workshops. Based on the activities of the control-group members (as shown in Figure II.1), we estimate that about 25 percent of the treatment-group members would have participated in sheltered workshops in the absence of the demonstration. Based on the cost estimates reported by Noble and Conley (1987, Table 3) for several sheltered workshops, it seems likely that the costs for these services would range from $300 to $450 per month. Thus, it appears that treatment-group members would have used an average of over $1,125 worth of these services per year in the

absence of the transitional-employment services provided by the demonstration.[2] If the transitional-employment services can effect long-term reductions in the use of sheltered workshops, then it would be possible to generate sizable savings.

While the purpose of the demonstration services was to reduce the use of sheltered workshops, these services were also expected to increase the use of long-term job-retention services. Thus, any savings from the reduced use of sheltered workshops may be offset by an increase in the costs for postdemonstration support services. The net impact of the demonstration services on alternative program costs will depend on the overall shifts in the mix of services used and the relative costs of the services.

A variety of support arrangements were made for persons who were terminated from the demonstration projects with a job. Depending on the specific services available in each of the demonstration communities, treatment-group members may have been referred to other programs that provided transitional-employment, supported-employment, or follow-up services. The costs of such services are likely to vary substantially. Services provided by transitional-employment programs would be expected to cost about as much as the services provided by the demonstration projects, for which costs averaged about $550 per month. In contrast, it seems plausible that the costs of long-term follow-up services for persons placed and trained in a transitional-employment program would be approximately $175 per month (see Thornton, Dunstan, and Schore, 1988, pp. 125-

[2]This figure assumes that the average cost of a sheltered workshop is $375 per month (the midpoint of the range reported by Noble and Conley). The average annual cost per treatment-group member is derived by multiplying this monthly cost by 12 months and then multiplying by 25 percent, the estimated proportion of treatment-group members who would have used sheltered workshop services.

126). The costs of supported-employment services for persons enrolled in the demonstration would be expected to fall between those estimated for transitional employment and long-term follow-up and would depend on the mix of placement, training, and job-retention services provided by the supported-employment program. In general, these costs are similar to the $375 estimated for sheltered workshops.

The estimates presented in Tables III.1 and III.2 suggest that the transitional-employment services provided in the demonstration affected the use of alternative services in the expected direction: the use of sheltered workshops declined, while the use of various types of employment-support programs increased. The net cost implications of these changes is essentially to break-even--that is, the costs of the increased job-retention services are approximately equal to the savings from the reduction in the use of sheltered workshops.[3]

One apparent implication of this finding is that the government is unlikely to generate net savings by providing transitional employment to persons who would have used relatively few employment services otherwise. In other words, it seems unrealistic to assume that the government will be able to generate net savings when it offers transitional-employment services to groups of persons who previously did not receive vocational services. We estimate that, in the

[3]As shown in Table III.1, the use of sheltered workshops fell by an average of 1.5 months during the two-year period following enrollment in the demonstration. The estimates for job-retention services indicate an average increase of 1.08 months in the use of supported-employment programs, as well as small increases in the use of nondemonstration transitional-employment and follow-up services. If the costs of these programs are as follows, the net cost of this shift in the mix of services is a reduction of $13 over the two-year period (sheltered workshop: $375/month; transitional employment: $550 per month; follow-up services: $175/month; and supported employment: $363/month).

absence of the demonstration, many of the SSI recipients who were enrolled would not have used vocational services. One-third of the sample members had not participated in any vocational activity in the year prior to their enrollment (see Table II.1), and only about 25 to 30 percent of treatment-group members would have been enrolled in sheltered workshops during the years immediately following their enrollment in the demonstration (see Figure II.1). Thus, for this group of persons, the potential for generating savings by reducing the overall use of alternative services was limited by this modest level of participation.

The situation might be different if transitional-employment services were targeted toward persons currently in sheltered workshops. Such a case would represent a shift in government funding rather than an expansion of services to previously unserved persons. While the demonstration did not directly test this scenario, the subgroup results in Table III.12 suggest that impacts on earnings and employment for SSI recipients who were recruited from sheltered workshops would be essentially the same as those estimated for the demonstration. However, the expected impacts on service use would differ. By recruiting persons from existing programs, a transitional-employment program would be more likely to create a situation in which it was possible to generate savings. In the first year, a transfer of persons from workshops to transitional employment might increase total costs, to the extent that the costs of transitional employment exceeded the costs of the workshops. Savings would likely accrue in subsequent years, since the costs of long-term job-retention services appear to be less than the costs of workshops, and the expectation would be that some persons would be successful in the labor market to the extent that they no longer required any special government-funded services. Clearly, this situation

deserves more attention, particularly if government expenditures for vocational services to persons with mental retardation are relatively fixed, making the expansion of services to new populations less likely.

D. DEMONSTRATION OUTCOMES FROM THE PERSPECTIVE OF SOCIETY AS A WHOLE

When all groups in society are considered together, it appears that transitional employment has the potential of creating impacts that are sufficiently large to justify the costs of this type of service. This conclusion depends on the relative importance attached to the four observations delineated at the beginning of this chapter, and on the ability of the organizations that provide transitional-employment services to achieve impacts similar to those generated by the more successful demonstration projects.

As long as transitional-employment services are targeted toward a mix of SSI recipients with mental retardation, many of whom apparently would not receive vocational services otherwise, then it seems likely that the government will incur a net cost for operating the program. Of course, because the government will realize some savings that will offset the gross operating costs of transitional-employment programs, the net cost is likely to be less than the estimated $5,600 outlay for program operations. Balancing this net expenditure are the earnings gains of the SSI recipients, as well as the nonpecuniary benefits of their increased integration into community life. Indeed, it is the increased self-esteem of persons with mental retardation who are able to enter and participate in the labor market, as well as the satisfaction that we as a society derive from supporting these individuals in their efforts, that represents the major justification for transitional-employment services.

The potential of transitional-employment services is also indicated by the project-specific impact estimates. The projects operated by ARC/MU, ECF, and UWash/PCC appear to have generated greater average impacts on the earnings of treatment-groups members than did the demonstration as a whole. As discussed by Thornton, Dunstan, and Schore (1988), these three projects emphasized placing persons in potentially permanent jobs as soon as possible, matching participants with jobs carefully, and being flexible in response to the individual needs of clients. A closer examination of the training practices of these projects clearly seems warranted. In particular, it would be useful to determine the extent to which client screening, local labor-market and service conditions, and the specific replicable features of the project models were separately responsible for the observed impacts. While the demonstration does not provide a rigorous statistical basis for identifying the separate influences of these various factors, a closer inspection of the project-specific operational and impact information can be expected to yield some insights into the factors that would help program operators improve the overall performance of future transitional-employment programs.

E. SUGGESTIONS FOR FUTURE RESEARCH

In many ways this report constitutes a first look at the data set generated by the Transitional Employment Training Demonstration. The analysis presented herein examines the effect that transitional-employment services can have on SSI recipients with mental retardation, yet it does not begin to exhaust the possible applications of the demonstration data set. In particular, the impact analysis confirms that transitional employment has considerable potential for enabling persons with mental retardation to lead lives that are economically

and socially fulfilling. However, this analysis also indicates that this potential is not realized automatically. Knowing about the overall impacts described in the impact analysis is essentially the first step in the process of examining the transitional-employment model and how it can be made to work more effectively. What remains is using the demonstration database to identify ways that would enable future programs to achieve greater effects at lower costs, or to develop guidelines and performance standards that could be used to monitor transitional-employment programs or to improve recruitment and program-targeting strategies.

One promising avenue for future analysis would be to reconsider the information about project operations in light of the project-specific impact estimates presented in Chapter III. Thornton, Dunstan, and Schore (1988) documented a number of differences among the specific transitional-employment models used by the eight projects and the costs of those models. The project-specific impact estimates provide a basis for assessing the extent to which those operational differences affect the ability of the projects to increase the economic self-sufficiency of SSI recipients with mental retardation. Specific issues that should be addressed in such an analysis include in the following:

- Why the ARC/MU project appears to have generated impacts that were substantially larger than the impacts generated by many of the other demonstration projects, and whether this experience can be replicated
- Whether it would be possible to achieve the larger-than-average impacts observed for the ECF and UWash/PCC projects without incurring the above-average costs observed for those two projects
- The extent to which cross-project differences in observed impacts are due to related differences in local labor markets (this issue is particularly important for assessing the impacts observed for the Children's Hospital project, which operated in the strong Boston economy)

- The extent to which arrangements for postdemonstration job-retention services that were made by the projects contributed to the magnitude and persistence of the observed impacts

These types of issues are exceedingly complex, and it will be difficult to identify the independent contributions of the myriad factors that affected the overall performances of the eight projects. Consequently, the findings of this additional analysis will be more suggestive than definitive. Nevertheless, the demonstration database, which contains consistently measured project-specific information on the services delivered, the structures of the projects, and the activities of treatment- and control-group members, provides an unequalled opportunity for considering the operation of large multi-site transitional employment programs. As states move to fulfill the requirements of the reauthorized Rehabilitation Act of 1973 (particularly the requirements pertaining to the provision of supported employment), issues pertaining to selecting and monitoring projects that provide community-based employment support services will become more important. It is this type of information that the demonstration database can ideally provide.

In this regard, it would also be useful to undertake a more detailed benefit-cost analysis of the transitional-employment projects. While the conclusions presented in Chapter IV indicate the general nature of the findings from a benefit-cost analysis, it would be useful to expand on the work presented in this report. Additional analysis could fruitfully pursue the potential cost and impact implications of a number of alternative program scenarios and provide the information necessary to assess options for funding transitional-employment programs. A more comprehensive analysis would address such issues as:

- The net cost to the government of operating transitional-employment programs

- The extent to which costs and benefits differ across projects and participant subgroups
- The extent to which net benefits would have to persist into the future in order to generate an overall positive benefit-cost finding from the government budget perspective
- The relative extent to which savings accrue to specific government budgets (particularly the SSI and state vocational rehabilitation budgets)

Finally, an analysis of long-term impacts would facilitate determining the extent to which the earnings gains estimated for the first three years persist. This analysis would essentially replicate the procedures developed herein but could be based on an expanded data set that would include additional SSI program records. This analysis would facilitate assessing the relative merits of transitional-employment and alternative program models that emphasize ongoing rather than time-limited services. Clearly, the willingness of taxpayers to fund transitional employment rather than alternative types of employment-support programs will depend to some extent on the persistence of the impacts from transitional employment.

The research and program-implementation efforts of persons interested in fostering the economic and social integration of persons with mental retardation have given considerable momentum to transitional employment and related programs. It is clear that program funding will continue for interventions that help persons with mental retardation obtain and hold jobs in the regular labor market rather than in segregated facility-based settings. What is needed is information on how these efforts can become as efficient as possible and how transitional-employment services should be fit into the overall service system targeted toward persons with mental retardation.

The impact analysis presented in this report addresses the role that transitional-employment services can play relative to the SSI program and the goal of providing economic support to persons with mental retardation. The impact findings confirm that transitional employment can increase the employment and earnings of SSI recipients with mental retardation, but that these impacts are likely to generate insufficient reductions in SSI payments to pay for the considerable costs of the services. The challenge facing researchers and policymakers now is to learn from the experience of the demonstration projects in order to improve the delivery of transitional employment services and to further the goal of integrating persons with mental retardation and other serious impairments into the labor market.

REFERENCES

Betsey, Charles, Robinson Hollister, Jr., and Mary Papageorgiou (eds.). Youth Employment and Training Programs: The YEDPA Years. Washington, D.C.: National Academy Press, 1985.

Bloomenthal, Anne, Russell Jackson, Stuart Kerachsky, Susan Stephens, Craig Thornton, and Kenneth Zeldis. "SW/STETS Evaluation: Analysis of Alternative Data-Collection Strategies." Princeton, NJ: Mathematica Policy Research, Inc., 1982.

Ehrenberg, Ronald G., and Robert S. Smith. Modern Labor Economics. Glenview, IL: Scott, Foresman, and Company, 1985.

Greenberg, David H., and Philip K. Robins. "The Changing Role of Social Experiments in Policy Analysis." Journal of Policy Analysis and Management, vol. 5, no. 2, 1986, pp. 340-362.

Greenblum, Joseph, and Barry Bye. "Work Values of Disabled Beneficiaries." Social Security Bulletin, vol. 50, no. 4, April 1987, pp. 67-74.

Grossman, Herbert J. (ed.). Classification in Mental Retardation. Washington, D.C.: American Association on Mental Deficiency (now American Association on Mental Retardation), 1983.

Kerachsky, Stuart, Craig Thornton, Anne Bloomenthal, Rebecca Maynard, and Susan Stephens. "Impacts of Transitional Employment on Mentally Retarded Young Adults: Results of the STETS Demonstration." Princeton, NJ: Mathematica Policy Research, 1985.

Kerachsky, Stuart, and Craig Thornton. "Findings from the STETS Transitional Employment Demonstration." Exceptional Children, vol. 53, no. 6, 1987, pp. 515-521.

Matson, Johnny L., and Frank R. Rusch. "Quality of Life: Does Competitive Employment Make a Difference?" In Competitive Employment Issues and Strategies, edited by Frank R. Rusch. Baltimore, MD: Paul H. Brookes Publishing Co., 1986.

McCoy, John L., and Kerry Weems. "Disabled-Worker Beneficiaries and Disabled SSI Recipients: A Profile of Demographic and Program Characteristics." Social Security Bulletin, vol. 52, no. 5, May 1989.

Moss, James W. "Postsecondary Vocational Education for Mentally Retarded Adults." Reston, VA: Council for Exceptional Children, 1980.

Noble, J.H., Jr. and R.W. Conley. "Accumulating Evidence on the Benefits and Costs of Supported and Transitional Employment for Persons with Severe Disabilities." Journal of the Association for Persons with Severe Handicaps, vol. 12, no. 3, 1987, pp. 163-174.

Rees, Albert. "An Essay on Youth Joblessness." Journal of Economic Literature, vol. 24, no. 2, June 1986, pp. 613-628.

Rocklin, Sarah G., and David R. Mattson. "The Employment Opportunities for Disabled Americans Act: Legislative History and Summary of Provisions." Social Security Bulletin, vol. 50, no. 3, March 1987.

Social Security Administration. "Implementation and Analysis of Public Law 98-460-Section 1619 (The Social Security Disability Benefits Reform Act of 1984)." Social Security Bulletin, vol. 49, no. 11, November 1986, pp. 11-45.

Social Security Administration. Social Security Bulletin, Annual Statistical Supplement, 1988. Washington, D.C.: Government Printing Office, 1988.

Thornton, Craig, Shari Miller Dunstan, and Jennifer Schore. "The Transitional Employment Training Demonstration: Analysis of Program Operations." Princeton, NJ: Mathematica Policy Research, 1988.

Vogelsberg, R. Timm. "Competitive Employment in Vermont." In Competitive Employment Issues and Strategies, edited by Frank R. Rusch. Baltimore, MD: Paul H. Brookes Publishing Co., 1986.

APPENDIX A:

SURVEY PROCEDURES AND RESULTS

This appendix describes the follow-up survey of Transitional-Employment Training Demonstration (TETD) sample members. The survey was conducted to provide detailed information on the experiences of both treatment and control group members to supplement the SSI records data. This introductory section outlines the rationale and goals of the survey. Following sections discuss the instrument design, field procedures, response rates, and data quality measures for both the interview survey of sample members and the records abstraction effort implemented at the vocational-service agencies that sample members reported using. Copies of the interview instruments can be obtained from Mathematica Policy Research.

A. SURVEY GOALS

Persons enrolled in the demonstration were individuals between the ages of 18 and 40 who were receiving SSI benefits, whose eligibility for benefits had been based on a diagnosis of mental retardation, and who were living in one of the thirteen demonstration sites throughout the United States. At the time of enrollment in the demonstration program, detailed background data were collected by the program staff on each applicant, and applicants were assigned randomly to either the treatment or the control group.[1]

The follow-up survey was designed to provide additional information for the evaluation pertaining to topics not covered by other data source. In particular, more detailed data were desired on employment outcomes--whether reported employment was in supported, sheltered, or competitive settings and the number of hours worked per week--and on the use of nondemonstration

[1]These data collection procedures are described in Thornton, Dunstan, and Schore (1988).

vocational services. These data are not regularly collected by the Social Security Administration and thus could not be obtained from program records. Similarly, it was not possible to obtain these data from the demonstration projects, since the projects could provide information on the experience of treatment-group members only during and immediately after their enrollment in the demonstration. Therefore, research interviews with TETD sample members were determined to be the most efficient way to obtain the needed data.

Previous experience in interviewing mentally retarded persons indicated that these individuals, or family members or friends acting as proxies, were able to provide accurate information on many aspects of their current life experiences, including major characteristics of employment (see Bloomenthal et al., 1982). However, the same experience indicated that determining whether employment was part of a training program or was in a sheltered or supported environment was more precise and reliable if service providers supplied this information. Also, since time concepts are often difficult for persons with mental retardation to understand and report, the length of time that employment-related services had been used was best obtained from provider records. Therefore, the follow-up interview was used as a means to identify service providers and trigger direct contacts with providers to obtain more detailed service information.

Table A.1 summarizes the data required to be collected for the evaluation and the source (sample member/proxy interview, SSA program records, and service agency records) used to provide the necessary data.

B. SURVEY DESIGN

To meet the goal of providing detailed information on employment outcomes and service utilization for members of the treatment and control groups, a

TABLE A.1

DATA NEEDS AND SOURCES USED

Outcome of Interest	Data Source: Sample Member/ Proxy Interview	Data Source: SSA Records	Data Source: Service Agency Records
I. Labor Market Performance			
Employment	C	L	
Earnings	C	L	
Work hours	C		
Wage rates	C		
Health insurance coverage	C		
Occupation	C		
Labor force participation	C		
II. Service Use			
Sheltered workshops	C*		L
Day activity center	C*		L
Job retention service	C*		L
Transitional employment	C*		L
Supported employment	C*		L
Transportation assistance	C*		L
III. Transfer-Program Use			
SSI		L	
Social Security		L	
Medicare/Medicaid			
IV. Living Arrangements			
Arrangements affecting SSI payments	C*		
Residential support	C*		

NOTE: Current data pertaining to the time of the interview are indicated by C. Longitudinal data pertaining to the time since enrollment are indicated by an L. The symbol C* indicates that some data pertaining to the entire period are available in addition to the time-of-the interview data. Capital letters indicate the data sources used.

two-stage survey design was developed. In the first stage, sample members were contacted and asked to participate in a research interview, during which input from proxies was accepted as needed to provide complete and accurate information. In the second stage, service providers identified in the interviews were contacted regarding access to their records on particular individuals, with written permission to do so provided by the sample member. These activities were conducted between September and December of 1988, more than three and one-half years after the first persons were enrolled into the demonstration.

Not all demonstration sites were included in the follow-up survey. Nine of the thirteen original sites were selected for follow-up. Table A.2 lists the original demonstration sites and indicates which ones were included in the follow-up survey. The decision to exclude the four sites reflected an assessment of the costs and benefits of the data collection. Resources for the survey were limited, and it was not possible to survey all sample members. Because there were fixed costs for each site included (e.g., hiring an interviewer, interviewer training, and basic supervision and management costs) it was decided to allocate the available resources to survey the entire sample at a reduced number of sites rather than to reduce the sample at all sites proportionately. The excluded sites were the smallest of the sites, given that, for purposes of the survey, the three central Pennsylvania sites of the AHEDD project could be combined. Thus, all treatment and control group members in each of the nine sites selected for follow-up were included in the survey, for a total follow-up sample of 600 persons (301 from the treatment group and 299 from the control group).

During the interviews, sample members were asked to identify all agencies from which they received services at the time of the interview and

TABLE A.2

ORIGINAL AND FOLLOW-UP TETD SITES AND SAMPLE

TETD Program Site	Sample Enrolled into Demonstration			In Follow-Up Survey?
	Treatment	Control	Total	
AHEDD, Incorporated				
Dover, Delaware	8	7	15	No
Harrisburg, Pennsylvania	10	9	19	Yes
Lancaster, Pennsylvania	11	12	23	Yes
Philadelphia, Pennsylvania	19	18	37	No
Pittsburgh, Pennsylvania	20	20	40	No
York, Pennsylvania	13	11	24	Yes
Association for Retarded Citizens, Monmouth (NJ) Unit--ARC/MU	39	41	80	Yes
The CENTER for the Rehabilitation and Training of the Disabled, Chicago--CENTER	27	26	53	No
The Children's Hospital, Boston	29	28	57	Yes
Exceptional Children's Foundation, Los Angeles--ECF	77	77	154	Yes
Goodwill Industries, Milwaukee (WI) Area--Goodwill	36	35	71	Yes
University of Washington and Portland Community College Portland, OR--UWash/PCC	45	47	92	Yes
University of Wisconsin, Stout, Vocational Development Center--UWis/Stout	41	39	80	Yes
Total in Demonstration	375	370	745	
Total in Follow-Up Sample	301	299	600	

since enrollment. Agency names were recorded on a release form which sample members were asked to sign at the end of the interview, giving permission for research staff to contact the agency for further information on the individual's use of services. The agencies mentioned in the interviews were reviewed and given code numbers by central MPR survey staff, and only those apparently connected with employment-related activities, including training and placement services, were selected for further contact. Table A.3 describes the interview questions which generally triggered the selection of service providers for further contact. Agency contacts and records abstraction began in November 1988 and continued through December 1988.

C. INSTRUMENT DESIGN

Three instruments were developed for the follow-up survey: (1) an interview for the sample member and/or proxy respondent, (2) a questionnaire to be administered at selected service provider agencies, and (3) a records abstraction form for the collection of individual utilization data from service provider agency records. These instruments were developed in the fall of 1987 and were submitted for review by OMB in February 1988. OMB approval was received in August of 1988.

1. Follow-up Survey Instrument

This instrument, designed to be administered in-person by a research interviewer, was modelled after the follow-up survey instrument used in the Structured Training and Employment Transitional Services (STETS) demonstration evaluation, also conducted by Mathematica Policy Research, Inc. (see Kerachsky et al., 1985.) The STETS survey had also been conducted with mentally retarded

TABLE A.3

FOLLOW-UP INTERVIEW QUESTIONS USED TO TRIGGER AGENCY RECORD ABSTRACTION

Question Number	Question Text
1.2	What is the name of the place you work for? PROBE: Who (else) do you work for? PROBE: Where do you go to work? NOTE: Employers were not contacted. This item was used for the records abstraction if the respondent reported a training program or sheltered workshop as a place of employment.
1.11 3.12	Who helps you on your job? IF NAME GIVEN, PROBE: Is NAME a friend or someone you met at EMPLOYER? IF CO-WORKER/FRIEND, ASK: Is it part of NAME's job to train you at work? IF SUPERVISOR, ASK: Does NAME work for EMPLOYER? IF JOB COACH, ASK: What is the name of the place NAME works?
1.26 3.27	Who drives you back and forth to work at EMPLOYER? PROBE: What is that person's name? Is NAME a friend or somebody in your family, or someone's whose job is to drive you ? Is it part of name's job to drive you? IF STAFF MEMBER, ASK: What is the name of the place STAFF MEMBER works?
1.28 3.29	Whose vehicle comes to pick you up to go to work? ALTERNATIVE: Who sends the VEHICLE to pick you up? IF NAME GIVEN, Is NAME a friend, or somebody whose job is to send the VEHICLE to pick you up?

TABLE A.3 (continued)

Question Number	Question Text
	IF FRIEND, ASK: Is it part of NAME's job to send it? IF STAFF MEMBER, ASK: What is the name of the place STAFF MEMBER works?
1.31 3.32	Who calls the taxi to come get you? IF NAME GIVEN, Is NAME a friend or somebody in your family, or somebody whose job is to call a taxi for you? IF STAFF MEMBER, ASK: What is the name of the place STAFF MEMBER works?
2.3	Who is helping you look for a job? PROBE: What is that person's name? IF STAFF MEMBER, ASK: What is the name of the place STAFF MEMBER works?
3.7	What is the name of the program that teaches you about jobs and work? PROBE: Where do you go to be in that program? What is the name of the place where you go?
3.3b	Now I'd like to ask you a few questions about the (other) job you have as part of (school/this training program). What is the name of the place you work for on that job? PROBE: Where do you go for that job--do you work at (school/training program) or do you go to work someplace else?
7.3	Do you have a caseworker, social worker, or case manager, or counselor? PROBE: Is there someone whose job is to talk to you and help you with your problems? What is the name of the place that NAME works?
7.5	Sometimes, do you get together with a group of people to talk to about how things are going or about problems you have?

TABLE A.3 (continued)

Question Number	Question Text
7.6	Where do you go to meet with the group? PROBE: What is the name of the place where the group meets?
7.7-7.9	Is there someone who leads the group or not? Who is that person? Where does NAME work?
7.17	My last question is about any help that you may have received in the past few years from different agencies. I'd like to show you some cards from different places that offer help to people. When I show you these cards, please tell me if you have ever received help from any of these places.

persons, but had used a separate proxy instrument. It seemed likely that proxy respondents would be needed more often with the TETD population, given that all TETD sample members had been judged to be sufficiently disabled to receive SSI benefits, while only one-third of the STETS participants were SSI beneficiaries. Therefore, the TETD instrument was designed to be administered to either the sample member or a proxy, or to include input from a proxy during or after the sample member interview, as determined to be appropriate or necessary by the interviewer. Interviewers were asked to record, at the end of each section of the interview, whether the sample member, a proxy, or both had answered the questions in the section and at the end of the instrument to record all persons present during the interview and the identity of the proxy respondent if any.

The question wording and interviewer probes on the survey questionnaire were designed with the cognitive skills of the sample members in mind. Given the particular cognitive weaknesses of persons with mental retardation, the instrument did not ask respondents to recall past events or to report a history or sequence of activities or services. Rather, it asked for information on current activities and experiences, including services used. Thus, the data that were obtained through the sample member interview represent a "snapshot" as opposed to a "longitudinal" or "historical" perspective. In addition, quantitative concepts such as hours worked per week and earnings were broken down into subquestions to help respondents answer accurately. For example, hours worked per week was obtained by asking respondents the following series of questions:

- The days of the week that he/she usually works
- The days of the week that he/she worked last week

- The days of the week that he/she plans to work during the next week
- Whether he/she starts work at the same time each day
- Whether he/she stops work at the same time each day
- If starts and stops work at the same time each day, the start and stop times and the amount of time allowed for lunch breaks
- If start and stop times are irregular, usual hours worked per day, excluding lunch breaks, and the amount of time allowed for lunch breaks
- If start and stop times are irregular and usual hours worked per day are unknown or variable, the start and stop times for each day worked in the previous week and planned for the next week and the amount of time usually allowed for lunch breaks

Table A.4 provides an overview of the interview instrument.

2. Service-Agency Records Abstraction Form

To facilitate the collection of detailed information on the utilization of employment-related services during the period since enrollment in the demonstration, the follow-up interview instrument was designed to identify the agencies from whom individual sample members received services. The records abstraction form was then intended to collect data from agency records on the history of an individual's participation in the employment services provided by the agency. The data to be collected from program records included:

- Type of service or program (work or adult day activity center, sheltered workshop, supported employment, job placement and training, classroom job training, job search assistance, long term job retention services or follow-up, and other)
- Dates of participation in each type of service or program, including length of any gaps in participation
- Participation in jobs as part of program participation, not including participation in work activities centers, vocational assessment, classroom training, or job search assistance

TABLE A.4

OVERVIEW OF SAMPLE MEMBER INSTRUMENT

Section 1 - Employment

This section covers independent and supported employment (obtaining the name of the program to initiate an agency records abstract), wage rates, earnings, hours worked, health insurance coverage, occupation, and transportation. This section will collect data for up to two current jobs.

Section 2 - Job Search

The section collects data on the respondents' participation in the labor force. We ask whether they are looking for work and, if so, whether anyone is helping them look for work, again obtaining the agency name for records abstracts.

Section 3 - School/Training Programs

This section asks questions about going to school, participation in a job-training program, type of transportation used, and whether they have any jobs as part of school or training that were not in Section 1. The questions asked about this job are the same questions asked in the employment section, allowing us to collect data on two current school or training jobs, if applicable.

Section 4 - Other Activities

Section 4 asks respondents whether they go to any type of classes or programs to learn about other things, such as cooking and money management.

Section 5 - Household

This section asks respondents about their marital status, type of living arrangements, and whether there is any type of residential support where they live.

Section 6 - Government Support

This section asks respondents about Medicaid coverage.

Section 7 - Other Services

In this section, we ask the respondents whether they have any other type of help (besides any counselor mentioned in Section 5), such as a social worker or case worker, and, if so, where they go for this help. We also ask them about different places they may go for organized social or recreational activities.

This section also reviews with the respondent those local agencies that were mentioned in the interview, as well as those that might have served the respondent. This discussion is the basis for completing the consent form to obtain access to agency and employer data.

Section 8 - Interviewer Observations

This section gives the interviewer an opportunity to record his or her opinions about how the interview was administered, the respondent's level of communication and understanding, and the general atmosphere of the surroundings.

- For jobs reported as part of program participation, occupation, full- or part-time status, and type of job (volunteer, competitive, supported, sheltered, or subsidized)
- Frequency, type, duration, and intensity of specific job retention or follow-up support services provided, if any
- Reasons for loss of any jobs in which the sample member was placed by the agency
- Current participation status of sample member (terminated without a referral, referral to another agency, continuing in program, placed in another program at the same agency).

The specific data items to be collected from program records were intended to supplement both SSA administrative data and the information that could be collected in a reliable manner from sample members and their proxies. The form and procedures were adapted from similar studies of utilization of community-based services conducted by MPR.

D. FIELD PROCEDURES FOR FOLLOW-UP INTERVIEW WITH SAMPLE MEMBERS AND PROXIES

As discussed, resource constraints dictated that the follow-up survey concentrate on nine of the thirteen demonstration sites. These sites included 600 of the 745 demonstration sample members. In addition, limited resources also made it necessary to concentrate the follow-up data collection in a three-month period, between September and December of 1988. It would have been preferable to obtain follow-up data at the same point relative to the date of enrollment in the demonstration for all sample members. However, since enrollment took place over a thirteen-month period beginning in May of 1985, this would have required a much longer follow-up period, involving greater costs to staff for managing such an effort. Therefore, depending upon the date of enrollment, follow-up data are available for individual sample members at a

point from two and a half to three years after random assignment into the treatment or control group. Interviewing assignments were made in the order in which individual sample members had been enrolled into the demonstration in an attempt to minimize the variation in the length of the follow-up period.

1. Recruiting and Training Interviewers

Ten interviewers were recruited to conduct the follow-up survey, one in each survey site except in Los Angeles, where two interviewers were needed due to the sample size. Interviewers were recruited from persons known through previous work with MPR or with other survey research organizations. Applicants were screened for their sensitivity to issues involved in conducting interviews with cognitively impaired persons, their general interviewing experience, their ability to work approximately full-time (including evenings and weekends during the field period), and their willingness to conduct interviews in the neighborhoods in which sample members resided.

Interviewer training was conducted in a two-and-one-half-day session held at MPR's offices in Plainsboro, New Jersey, in September 1988. Interviewers were provided with an Interviewers' Procedures Manual, which provided an introduction to the demonstration and the follow-up survey, described basic interviewing concepts and techniques, discussed field procedures for the follow-up survey (including how to contact sample members, proxies, and service agencies), and provided information on administrative procedures for making and reporting on assignments and for submitting time and expense reports. These same topics were covered in detail in presentations made by MPR project staff during the training session. In addition, interviewers viewed a videotape on general interviewing principles and good interviewing practices, produced by

MPR and covering such items as asking questions, recording responses, probing, and controlling interviewer-introduced bias. The training session included practice interviews conducted among the interviewers as a group and one-on-one with MPR project staff.

The initial training session held in September provided detailed instructions on using the follow-up interview to identify agencies and obtaining signed releases from sample members for contacting agencies and collecting service-use data from agency records. It also reviewed the basic procedures for contacting service agencies. However, since it was more efficient to assign the service agency record abstractions after review by MPR central survey staff and after a sufficient caseload had been built up, further interviewer training was provided on this aspect of the follow-up survey in telephone conferences between MPR project staff and the interviewers.

2. Informing Sample Members and Gaining Cooperation in the Follow-up Survey

Interviewers received lists of sample members to be contacted every two weeks. Each assignment list included a set of contact sheets for recording the date of each attempt to contact individual sample members to schedule an interview, as well as information on the results of each attempt. Before contact was to be attempted with sample members, however, interviewers mailed an advance letter to each sample member.

Current address information for sample members and for their representative payees was obtained from Social Security records and used to prepare contact sheets and advance letters for the sample. The advance letter, printed on MPR stationary and signed by the interviewer, provided basic information on the follow-up survey in simple language. The letter covered the purpose of the

study, the reason that the individual was in the survey, the voluntary nature of participation in the interview, the fact that the decision about participation in the survey would not affect sample members' benefits or services, and the confidentiality of any information provided in the survey. A similar letter was sent to the individual, if any, identified on Social Security records as the representative payee for the sample member.

Within several days after mailing the advance letter, interviewers called sample members to set up an appointment to conduct the interview. Interviewers were trained to answer questions that sample members might have about the interview, to ask the sample member to repeat the scheduled date and time for the interview, and to probe whether the scheduled appointment would interfere with the sample members' regular activities, such as work or program participation.

After contact with the sample member, interviewers were to contact the representative payee, very often the parent or relative of the sample member, to inform them about the scheduled interview with the sample member and to indicate that it may be desirable for them to participate in the interview as a proxy or that there may be a need to ask them some questions afterwards to clarify information from the sample member interview. In cases where there was no representative payee, interviewers were instructed to review the contact sheet which contained information on whether any individual had accompanied the sample member during the application to the TETD program and provided information during the intake interview as a proxy. If there had been such a person, he or she was to be to contacted regarding the scheduled interview if

no representative payee was indicated (very often the intake proxy and the representative payee were the same individual).

3. Identifying and Interviewing Proxies

The contact sheet provided to the follow-up survey interviewers indicated whether a proxy respondent had been used during the intake process when the sample member applied to the demonstration. Whether a proxy had been required at that time was considered to indicate the need for a proxy during the follow-up interview.

Interviewers were instructed to attempt to use the intake proxy during the follow-up interview if a proxy respondent was needed. If that person was not available, interviewers were to identify the person most familiar with the sample member's regular daily activities, particularly with participation in job-related programs and in employment.

Proxy respondents were used to "fill in" answers that the sample member was unable to answer fully and those which appeared to be inconsistent or unreliable. In all cases the interviewer was first to ask the question of the sample member and to record the sample member's answer. If the responses of a sample member and the proxy differed, the interviewer was to record both answers, indicating the source of each, and to provide marginal notes about the nature of the discrepancy and any indications of the relative validity of one response over the other.

Parents or guardians named on the contact sheet were also to participate in the process of obtaining releases for the agency record abstraction. Both the sample member and the parent or guardian, who in most cases would be acting as the proxy respondent as well, were to sign the release form after the

interviewer had filled in the names and addresses of service providers from the follow-up questionnaire.

E. SAMPLE MEMBER INTERVIEW SURVEY RESULTS

This section reviews the results of the follow-up survey, including response rates, interviewer observations regarding the interview process, the use of proxy respondents, and indications of data quality. Overall, the survey achieved a response rate of almost 92 percent for an interview that averaged 28 minutes to administer. Proxy respondents were used in just over 73 percent of the completed cases, but in only about 8 percent did the proxy answer all the questions during the interview. Interviewers judged the combination of sample member and proxy responses to be reliable on most or all items, and there was very little missing data, even on variables known to be difficult for this population (such as reports of earnings).

1. Response Rates

Table A.5 presents the distribution of final statuses and response rates by research status (treatment versus control group) and demonstration site. Response rate was defined as the number of completed follow-up interviews divided by the number of sample members assigned minus those who had moved out of the study area, had been institutionalized, or had died.

A total of 524 sample members completed the follow-up interview, 301 from the treatment group and 299 from the control group, for a total response rate of 91.9 percent (91.7 for the treatment group and 92.2 for the control group). There was some variation in response rate by site, with a high of 98.6 percent response in the Goodwill site to a low of 81.8 percent response in the

TABLE A.5

DISTRIBUTION OF FINAL STATUS AND RESPONSE RATES FOR SAMPLE MEMBER FOLLOW-UP SURVEY, BY RESEARCH STATUS AND PROGRAM SITE

		Research Status		Site								
	Total	Treatment	Control	AHEDD-Harrisburg	AHEDD-Lancaster	AHEDD-York	Children's Hospital Boston	UWash/PCC, Portland	ECF, Los Angeles	ARC/MU, New Jersey	Goodwill, Milwaukee	UWis/Stout Wisconsin
FINAL STATUS												
Complete (1)	524	264	260	17	20	18	51	76	126	73	69	74
Refusal by Sample Member (3)	9	5	4	1	0	0	1	0	5	0	0	2
Refusal by Parent/Guardian (4)	18	10	8	0	0	1	0	6	6	4	1	0
Unavailable to Locate, Contact, or Unavailable (5,6,10)	17	8	9	1	2	3	2	4	4	0	0	1
Barrier to Interviewing (Physical Mental, Language) (7,8)	2	1	1	0	0	0	0	0	2	0	0	0
Moved Out of Area (9)	26	11	15	0	1	2	3	4	11	2	1	2
Deceased (1)	4	2	2	0	0	0	0	2	0	1	0	1
Institutionalized (12)	0	0	0	0	0	0	0	0	0	0	0	0
Total	600	301	299	19	23	24	57	92	154	80	71	80
RESPONSE RATE[a]	91.9	91.7	92.2	89.5	90.9	81.8	94.4	88.4	88.1	94.8	98.6	96.1

[a]Defined as the number of completed interviews divided by the number of cases assigned minus cases found to have moved out of the interviewing area or to be institutionalized or deceased at the time of the follow-up contact.

AHEDD-York site. However, when all the AHEDD sites are combined (as was done for the impact analysis), the overall AHEDD response rate was 87.3 percent.

2. Interviewer Observations Regarding Interview Process

Table A.6 presents information on the length of the interview, arrayed by research status and site. Interviews lasted, on average, 28 minutes, with a median length of 26 minutes. The interview length was approximately the same for both treatment and control group members, but varied by site, with interviews conducted with sample members from the Goodwill project being particularly short (averaging 17 minutes).

Table A.7 provides information on the interview setting--whether others were present during the interview and the location of the interview. In the majority (83 percent) of cases, the interview was completed in the sample member's home, although almost 12 percent of the interviews were conducted in an agency office or other public place. In about three-quarters of the interviews, other persons were present, most often a parent or a counselor.

Table A.8 presents the distribution of observations noted by interviewers after completing the follow-up interview about the sample member's ability to communicate and provide reliable information. All but a very few sample members spoke English as their primary language and for most (about 85 percent) their communication ability was unimpaired or only partially impaired. Interviewers generally rated sample members as very attentive and cooperative during the interview and as being fairly self-confident.

TABLE A.6

LENGTH OF INTERVIEW, BY RESEARCH STATUS AND PROGRAM SITE

(In Minutes)

		Research Status		Site								
	Total	Treatment	Control	AHEDD-Harrisburg	AHEDD-Lancaster	AHEDD-York	Children's Hospital, Boston	U Wash/PCC, Portland	ECF, Los Angeles	ARC/MU, New Jersey	Goodwill, Milwaukee	UWis/Stout
Mean	28.0	28.0	28.0	32.5	21.5	32.2	27.5	30.8	32.1	24.0	17.2	32.4
Median	26.0	25.0	27.0	30.0	20.0	31.0	26.0	30.0	30.0	22.0	14.0	30.0

TABLE A.7

INTERVIEWER OBSERVATIONS ON INTERVIEW SETTING

	Percent of Completed Interviews[a]	
LOCATION		
Sample Member's Home	83.1	
Home of Relative or Friend	3.4	
Program/Agency Office	9.2	
Other Public Place	2.5	
Other	1.7	
PRESENCE OF OTHERS DURING INTERVIEW		
Others Present, including[b]	74.8	
Parent, Foster Parent or Guardian		63.5
Sibling		9.2
Formal Caregiver, Residential Manager, or Counselor		22.2
Roommate, Friend or Spouse		9.2
Other Relative		5.9
Some Other Person		5.4

[a]Number of completed interviews = 524.

[b]More than one response was coded.

TABLE A.8

INTERVIEWER OBSERVATIONS ON SAMPLE MEMBER COMMUNICATION AND ATTITUDE

	Percent of Completed Interviews[a]
SAMPLE MEMBER COMMUNICATION	
Primary Language:	
English	97.3
Spanish	0.8
Other	0.6
Unknown or Not Recorded	1.3
Used Means of Communication Other than Speech	2.7
Level of Impairment in Communication:	
Completely Unimpaired	55.2
Partially Impaired	29.9
Severely Impaired	11.2
Completely Impaired	3.7

	Mean/Median Value
SAMPLE MEMBER ATTITUDE DURING INTERVIEW[b]	
Attentiveness	1.7/1.0
Cooperativeness	1.4/1.0
Self-Confidence	2.0/2.0

[a]Number of completed interviews = 524.

[b]Coded on a scale from 1 to 5, where point 1 on the scale was labeled with the positive value of the attribute (e.g., mentally alert and attentive) and point 5 on the scale was labeled with the negative value (e.g., inattentive).

3. Use of Proxies

Table A.9 provides information on the use of proxy respondents during the follow-up interviews, by section of the questionnaire, and gives the interviewers' estimates of the proportion of questions in total answered by the proxy. This information is also arrayed by research status and site.

Overall, proxy respondents contributed to 73 percent of the completed interviews, but there was considerable variation in proxy use depending upon the nature of the questions in each module. Sample members were most likely to answer questions unaided on such topics as job-search activities and support, participation in school or training programs, participation in other regular activities (other than employment), and household characteristics. Proxies were frequently relied on (in at least one-quarter of the cases) to provide information on hours worked and earnings if employed and receipt of Medicare or Medicaid benefits. Proxies also frequently assisted the sample member in responding to other questions about employment.

Proxy use did not differ substantially by the research status of the sample member, although control group member interviews were somewhat more likely to involve a proxy respondent. However, interviews conducted in certain sites were much more likely to involve proxies than those in other sites. Interviews in the AHEDD sites and the ARC site in New Jersey almost all included the use of a proxy respondent, while the UWIS/Stout, ECF, and Children's Hospital sites had relatively low rates of proxy use (between 55 and 69 percent). Interviewers reported that a large proportion of cases (more than one-third) had more than half of all the questions on the interview answered by a proxy respondent in several sites: AHEDD-York (44.5 percent), UWash/PCC (54.0 percent), and ARC/MU

TABLE A.9

USE OF PROXY RESPONDENTS, BY SECTION OF QUESTIONNAIRE

		Research Status		Site								
	Total	Treatment	Control	AHEDD-Harrisburg	AHEDD-Lancaster	AHEDD-York	Children's Hospital Boston	UWash/PCC, Portland	ECF, Los Angeles	ARC/MU, New Jersey	Goodwill, Milwaukee	UWis/Stout Wisconsin
Section 1: Employment (n=341)												
Sample Member Only	40.5	39.9	41.1	20.0	25.0	20.0	66.7	32.1	46.8	10.9	57.1	52.7
Proxy Only	6.5	7.3	5.5	0	0	0	6.1	14.3	3.8	16.4	0	0
Proxy and Sample Member	53.1	52.8	53.4	80.0	75.0	80.0	27.3	53.6	49.4	72.7	42.9	47.3
Section 1: Hours and Dollars Responses (n=339)												
Sample Member Only	45.1	45.8	44.4	30.0	12.5	20.0	69.7	32.1	48.7	29.1	67.7	52.7
Proxy Only	27.1	24.9	29.6	10.0	25.0	20.0	12.1	42.9	21.8	67.3	5.9	5.5
Proxy and Sample Member	27.7	29.4	25.9	60.0	62.5	60.0	18.2	25.0	29.5	3.6	26.5	41.8
Section 2: Job Search (n=524)												
Sample Member Only	71.9	74.2	69.6	52.9	70.0	55.6	82.3	52.6	76.2	56.2	88.4	86.5
Proxy Only	12.4	10.6	14.2	5.9	5.0	27.8	3.9	21.1	11.9	32.9	0	1.3
Proxy and Sample Member	15.7	15.1	16.1	41.2	25.0	16.7	13.7	26.3	11.9	11.0	11.6	12.2
Section 3: School/Training Program (n=524)												
Sample Member Only	68.7	70.5	66.9	70.6	85.0	33.3	88.2	44.7	69.1	42.5	88.4	90.5
Proxy Only	11.6	9.9	13.5	5.9	5.0	27.8	2.0	21.1	13.5	26.0	1.5	0
Proxy and Sample Member	19.7	19.7	19.6	23.5	10.0	38.9	9.8	34.2	17.5	31.5	10.1	9.5
Section 4: Other Activities (n=522)												
Sample Member Only	66.3	68.6	63.9	58.5	70.0	33.3	92.2	46.7	64.8	45.2	87.0	81.0
Proxy Only	16.9	14.0	19.8	5.9	10.0	33.3	3.9	24.0	17.6	46.6	2.9	1.3
Proxy and Sample Member	16.9	17.4	16.3	35.3	20.0	33.3	3.9	29.3	17.6	8.2	10.1	17.6
Section 5: Household Characteristics (n=524)												
Sample Member Only	62.8	62.5	63.1	41.2	70.0	33.3	78.4	47.4	63.5	42.5	79.7	81.1
Proxy Only	14.1	14.4	13.9	5.9	10.0	22.2	7.8	18.4	15.1	38.4	2.9	0
Proxy and Sample Member	23.1	23.1	23.1	52.9	20.0	44.4	13.7	34.2	21.4	19.2	17.4	18.9
Section 6: Government Support (n=524)												
Sample Member Only	58.6	59.1	58.1	29.4	40.0	27.8	82.3	46.1	66.7	31.5	75.4	71.6
Proxy Only	25.4	25.8	25.0	29.4	15.0	38.9	13.7	30.3	23.0	67.1	11.6	2.7
Proxy and Sample Member	16.0	15.1	16.9	41.2	45.0	33.3	3.9	23.7	10.3	1.4	13.0	25.7

TABLE A.9 (continued)

	Total	Research Status		Site								
		Treatment	Control	AHEDD-Harrisburg	AHEDD-Lancaster	AHEDD-York	Children's Hospital Boston	UWash/PCC, Portland	ECF, Los Angeles	ARC/MU, New Jersey	Goodwill, Milwaukee	UWis/Stout Wisconsin
Section 7: Other Services (n=523)												
Sample Member Only	55.5	57.9	52.9	35.3	50.0	27.8	74.0	36.8	61.9	23.3	81.2	71.6
Proxy Only	13.0	12.1	13.9	0	10.0	22.2	8.0	17.1	14.3	34.3	2.9	0
Proxy and Sample Member	31.5	29.9	33.2	64.7	40.0	50.0	18.0	46.1	23.8	42.5	15.9	28.4
Any Use of Proxy During Interview (n=524)	73.1	71.6	74.6	94.1	85.0	94.4	68.6	72.4	64.3	93.1	76.8	55.4
Interviewer Estimate of Proportion of Questions Answered by Proxy (n=524)												
None	34.7	36.4	33.1	11.8	30.0	5.6	56.9	32.9	42.1	6.9	34.8	50.0
Very Few	16.2	15.9	16.5	17.7	20.0	16.7	17.7	5.7	15.1	12.3	29.0	18.9
Some	14.9	15.5	14.2	52.9	25.0	33.3	15.7	7.9	11.9	19.2	4.3	16.2
About Half to Most	26.3	24.6	28.1	17.7	25.0	27.8	7.8	30.3	23.8	49.3	30.4	14.9
All	7.8	7.6	8.1	0	0	16.7	2.0	23.7	7.1	12.3	1.5	0

(61.6 percent). On the other hand, over half the interviews in the Children's Hospital (74.6 percent), ECF (57.2 percent), Goodwill (63.8 percent), and UWis/Stout (68.9) sites obtained all or most of the information from sample members themselves.

Table A.10 presents the distribution of proxy respondents by their relationship to the sample member. Of the 383 completed interviews in which a proxy respondent participated, almost two-thirds of the proxies were a parent, foster parent, or guardian of the sample member. Another 29 percent of proxies were not related to the sample member and were generally a counselor or residential staff member from an agency.

4. Indications of Data Quality

Table A.11 provides information on the interviewers' assessment of the overall reliability of responses to the follow-up survey, sample member and proxy responses combined, by research status and site. Overall, interviewers judged about two-thirds of the interviews to provide very reliable information, and only a very small fraction (about 5 percent) of interviews were judged to be reliable only on some items or to be very unreliable. Interviewer assessment of reliability did not vary by the research status of the sample member. In some sites, however, overall reliability was judged to be much higher than in others; in the Portland site, in particular, 25 percent of the interviews were judged to yield unreliable data on some items or to be very unreliable. On the other hand, with only one interviewer assigned to a site, it is impossible to disentangle differences in interviewers' criteria for reliability and ability to assess reliability from differences in the ability and willingness of sample members and proxies to provide reliable data.

TABLE A.10

PROXY RESPONDENTS BY RELATIONSHIP TO SAMPLE MEMBER

Relationship to Sample Member	Percent of Completed Interviews in which Proxy Participated[a]
Parent, Foster Parent, or Guardian	63.4
Sibling	3.7
Roommate, Friend, or Spouse	2.1
Other Relative	2.1
Other Non-Relative	28.7

[a]Number of completed interviews in which a proxy participated = 383 or 73.1 percent.

TABLE A.11

INTERVIEWER ASSESSMENT OF OVERALL RELIABILITY OF RESPONSES
BY SAMPLE MEMBER AND/OR PROXY

Variable	Total	Research Status		Program Site								
		Treatment	Control	AHEDD-Harrisburg	AHEDD-Lancaster	AHEDD-York	Children's Hospital, Boston	U Wash/PCC, Portland	ECF, Los Angeles	ARC/MU, New Jersey	Goodwill, Milwaukee	UWis/Stout
Reliability												
Very Reliable	65.6	65.0	66.3	88.2	85.0	61.1	95.9	43.4	60.0	50.7	85.5	64.9
Reliable on Most Items	29.2	29.7	28.7	11.8	15.0	38.9	4.1	31.6	34.4	49.3	13.0	35.1
Reliable on Some Items	4.0	3.4	4.7	0	0	0	0	18.4	4.8	0	1.5	0
Very Unreliable	1.1	1.9	0.4	0	0	0	0	6.6	.8	0	0	0

Table A.12 presents the proportion of missing data on several key variables from the follow-up study, by research status and site. On this measure of data quality, it is clear that, in general, sample members and/or proxy respondents were able to provide answers to key variables, including those (such as receipt of benefits and earnings) that are sometimes difficult for nonhandicapped populations to report completely. Earnings was the only key variable with missing data for more than one percent of the cases. The incidence of missing data, where it existed at all, was not differentially distributed by research status, but it did vary by site. Three sites had missing data for the earnings variable on 10 percent or more of the cases: PCC in Portland (10.5 percent), AHEDD-Harrisburg (11.8 percent), and AHEDD-York (16.7 percent). However, of the remaining six sites, all but one had no missing data or less than 3 percent missing data on this variable.

Overall, based on these two indicators of data quality, the follow-up survey was able to provide complete data on the vast majority of cases, and the data provided was generally assessed by the interviewers to be quite reliable.

F. ABSTRACTION OF DATA FROM AGENCY RECORDS

This section describes the procedures by which data on individual sample member participation in job-related services provided by service agencies other than the TETD demonstration program were abstracted from agency records. It also includes a summary of the success of the records abstraction process in obtaining complete data on the sample.

TABLE A.12

PERCENTAGE OF MISSING DATA ON KEY VARIABLES

Variable	Total	Research Status		Program Site								
		Treatment	Control	AHEDD-Harrisburg	AHEDD-Lancaster	AHEDD-York	Children's Hospital, Boston	U Wash/PCC, Portland	ECF, Los Angeles	ARC/MU, New Jersey	Goodwill, Milwaukee	UWis/Stout
Receive Medicare or Medicaid	0.8	0.8	0.8	5.9	0	0	0	1.3	0	0	0	2.7
Whether Employed	0	0	0	0	0	0	0	0	0	0	0	0
Type of Job	0.9	0.8	1.1	0	0	0	2.0	1.3	1.6	0	0	1.3
Hours Worked	0	0	0	0	0	0	0	0	0	0	0	0
Earnings	5.3	5.3	5.4	11.8	0	16.7	2.0	10.5	7.9	2.7	0	2.7
Type of Living Arrangement	0	0	0	0	0	0	0	0	0	0	0	0

1. Records Abstraction Procedures

Several key questions during the sample member interview were intended to trigger the records abstraction process (see Table A.3 for the list of these questions). Interviewers were instructed to obtain as complete information as possible on the name and address of any agency mentioned during the interview and to complete an agency log form with this information for each agency mentioned. The log forms were then sent to MPR for review along with the completed follow-up interview.

MPR staff reviewed the follow-up interview instruments to make sure that all agencies mentioned were properly identified and that all agencies were listed on the signed sample member release form giving permission to contact the agencies for further information. MPR staff also prepared a complete list of all sample members who reported receiving services at a particular agency, so that contact with the agency and the records abstraction process would be efficient and create the least amount of burden for the agency.

MPR staff also reviewed the complete list of agencies developed in each site with the TETD program director to verify that all agencies in fact provided some type of employment-related service. Agencies known not to provide services related to employment--training, placement or referral, sheltered or supported work, and/or follow-up or support after placement on a job--were excluded from the records abstraction process.

Once the list of agencies had been prepared, interviewers contacted the agency director or other key staff member to conduct a brief interview about agency services and patterns of client participation (e.g., phases or types of client activities). This information was used to interpret notations on the

records and to clarify the types of services provided.[2] In addition, if this agency interview indicated that the agency did not provide vocational services, no further efforts were made to abstract records data from that agency.

After the interview with the agency director, interviewers obtained whatever records the agency maintained on the participation of individual sample members in employment-related programs and services. Information from the records was entered onto an abstract form for each sample member's participation in each program or service. In most cases, the abstract forms were completed by the site interviewer in-person at the agency offices. However, the on-site interviewers in California were unable to complete the records abstraction process, and many of the abstract forms in that site were completed by an MPR staff person, who obtained the necessary information by telephone from agency staff. This modified records abstraction procedure worked very well and did not pose any greater burden on the agencies than did the in-person process.[3]

2. Results of the Records Abstraction Process

A total of 1,372 cases were identified from the sample member interviews, in which a case is defined as an agency mentioned by an individual sample member. Based on information provided by the staff at demonstration projects and other sources, 783 cases were eliminated as not associated with agencies

[2]The agency interview was quite lengthy and complicated to administer in a standardized fashion. Interviewers used the instrument to obtain the information necessary to conduct the records abstraction, and data from the agency interview instrument were not entered into the research data base.

[3]When required, copies of the signed consent forms were mailed to the agency prior to completing the records abstraction over the telephone.

providing vocational services. The remaining 589 cases were assigned to the interviewing staff for the records abstraction data collection.

Of the assigned cases, 420 (71 percent) resulted in completed records abstraction forms, that is, the sample member was confirmed to receive services at the agency, the information on services requested on the abstraction form was available, and the form was completed. An additional 107 cases were closed because the agency reported that the sample member had not been served by or participated in vocational-service programs at the agency. (This disposition of the cases may have been due to any of several reasons: the respondent may not have remembered the agency name correctly, the interviewer may have failed to properly identify the agency from the information provided during the sample member interview, or agency records may have been in error.) The remaining 62 cases were not completed, and it was not possible to verify whether the sample member had received vocational services. In some of these cases, the agency was no longer in operation or there was insufficient information to identify a particular agency; in most, however, the agency did not maintain records of program participation in sufficient detail to definitely state whether the sample member had or had not received services.

APPENDIX B

STATISTICAL METHODOLOGY

The impact analysis, the results of which appear in Chapter III, is one component of the overall evaluation of the Transitional-Employment Training Demonstration. It relies heavily on statistical models and tests and is based on a number of underlying statistical assumptions. In order to simplify the presentation of the results of the impact analysis, issues associated with statistical methodology were not discussed in detail in Chapter III. They are the focus of this appendix.

A. ESTIMATING THE OVERALL IMPACTS

Given the random assignment of sample members to treatment status, unbiased estimates of the impact of transitional employment could be obtained simply by comparing mean values of outcomes for the treatment and control groups. However, the approach used in our analysis was ordinary least squares regression, since, to the extent that outcomes are associated with characteristics at enrollment, regression analysis can explain some of the variation among individuals, thus leading to more precise impact estimates than would be obtained by simply comparing means. In addition, regression analysis offers a convenient framework for testing hypotheses about the comparison of impacts across specific subgroups of the sample.

The econometric model used to estimate the overall impact of transitional employment was:

$$(1) \quad Y = a_0 + a_tT + a_cC + a_pP + a_xX + e,$$

where Y is an outcome variable that is hypothesized to be affected by transitional employment, such as earnings; T is a binary variable equal to one for treatment-group members and to zero for control-group members; C is a set

of variables that represent personal characteristics;[1] P is a set of binary variables that represent the projects; X is a set of variables that represent other factors that could influence the outcome;[2] and e is a disturbance term. (Regression models that controlled for unemployment rate were also estimated. Because the unemployment rate was correlated to some degree with project, and because the unemployment rate varied relatively little over time within projects, the impact estimates from these models did not differ markedly from those presented in this report.)

The a's are coefficients estimated with ordinary least squares. In particular, a_t measures the treatment/control difference in outcome, Y, controlling for any differences that exist between the treatment and control groups in preenrollment variables C, P, and X. Hence, a_t is our estimate of the impact of transitional employment. Even if transitional employment had no impact, outcomes for the treatment and control groups could have differed by chance. Therefore, we used statistical tests to determine whether the estimated

---

[1]These characteristics were used later to define subgroups for an analysis of differential impacts across subgroups. They include IQ score, age, gender, race/ethnicity, living arrangement, work experience in the year prior to enrollment, eligibility for Social Security benefits at enrollment, intake worker's opinion of the probability of program completion, and cohort membership.

[2]These characteristics were believed to affect the outcomes of interest but were not used to define subgroups. Included in X are whether the sample member was in school at enrollment, whether the sample member had used public transportation regularly, whether the sample member had ever been institutionalized, whether the sample member was currently receiving psychiatric treatment, whether the sample member had physical, emotional, social, or speech problems, the number of months on SSI prior to enrollment, the level of SSI receipt during the year prior to enrollment, whether the sample member received Food Stamps, welfare, or Medicaid at enrollment, and earnings in the year prior to enrollment.

difference between the two groups, a_t, was sufficiently large and the variation of the estimated difference sufficiently small that they were unlikely to have occurred by chance.

2. Estimating the Impacts by Subgroup

In addition to determining whether transitional employment had an impact on the overall sample, tests were also performed to determine whether impacts on earnings differed across projects and across various subsets of the sample defined by the characteristics of sample members. This was an attempt to answer such questions as whether transitional employment differentially benefited persons with different IQ scores. The stratifications of the sample were defined by the following: IQ score, age, gender, race/ethnicity, living arrangement, work experience in the year prior to enrollment, eligibility for Social Security benefits at enrollment, intake worker's opinion of the probability of program completion, and cohort membership.

To obtain estimated impacts for the subgroups formed by each of the classifying variables, we modified the standard regression, equation (1), as follows:

$$(2) \quad Y = a_0 + a_tT + a_cC + a_pP + a_{tc}T*C + a_{tp}T*P + a_xX + e \ ,$$

where T, C, P, X, and e are defined as they were for equation (1), and the interaction variables, T*C and T*P, were formed by multiplying the treatment binary times the binaries that represented personal characteristics and project, respectively.

The estimated impact of transitional employment obtained from this model is:

$a_t + a_{tc}C + a_{tp}P$.

Estimated impacts for a particular subgroup were calculated by setting the variables in C that represented the classifying characteristics of interest at one for the category for which impact estimates were desired and zero for the other categories of this characteristic, and setting all of the other characteristics in C and P at the means for the entire sample. For example, when a project-specific impact was estimated, the binary for the project of interest was set to one, the other project binaries set to zero, and all the remaining variables in C set at their sample means.[3] Impacts were estimated in this way for each subgroup defined by each of the classifying variables. Standard errors of these estimated impacts were computed and used to form t-statistics to test whether impacts were significantly different from zero.

The primary tests conducted were of whether the estimated impacts differed from each other across the subgroups defined by each of the classifying variables. The hypothesis that no such difference occurred was tested by performing for each classifying characteristic an F-test of whether the coefficients in a_{tc} on the binary variables that represented that characteristic

[3]Setting the remaining variables to their sample mean values assumed that subgroup inhabitants within a given stratification exhibited identical characteristics except for their subgroup membership. Alternatively, subgroup-level, regression-adjusted treatment-group means were also computed under the assumption that sample members exhibited the characteristics of the particular subgroup in the analysis sample. If a strong correlation existed among the characteristics used to define the subgroups and if one (or more) of the characteristics had a large influence on the outcome of interest, then the results of tests based on this assumption could differ markedly from those based on the assumption described in the text. However, this was not the case for a small subset of the demographic categories for which we investigated these impacts.

were equal to zero. Given the large number of such tests, however, we first jointly tested all of the coefficients in a_{tc} to determine whether they were equal to zero. If the impacts did not differ, apparently "significant" results from subsequent, less general tests were determined to be spurious. The rejection of this hypothesis indicated that the impacts of transitional employment on a given outcome did vary with at least one of the classifying characteristics. In such cases, the F-tests for each characteristic were then examined to determine with which of the characteristics the impacts of transitional employment varied.

APPENDIX C:

SUPPLEMENTAL TABLES

TABLE C.1

PERCENTAGE OF THE CONTROL GROUP RECEIVING
EMPLOYMENT-RELATED SERVICES, MONTHS 1 TO 36 AFTER ENROLLMENT

Month After Enrollment	Transitional Employment/ Supported Employment	Work Activity Center/ Shelter Workshop	Other
1	1.5	23.7	2.2
2	2.3	23.7	2.2
3	2.3	23.7	2.2
4	2.3	23.3	2.6
5	2.7	23.0	2.6
6	4.2	24.2	2.6
7	3.9	24.2	2.2
8	3.5	24.6	3.0
9	3.5	24.2	3.4
10	3.9	24.6	3.9
11	3.9	24.6	4.3
12	4.3	23.9	3.8
13	4.6	23.5	3.8
14	5.4	23.5	3.0
15	5.4	23.5	2.6
16	5.4	23.9	2.6
17	5.4	24.0	2.6
18	5.8	25.2	2.2
19	6.2	25.5	2.1
20	6.2	25.1	1.7
21	6.6	25.1	2.1
22	6.6	25.9	1.7
23	5.8	25.5	2.1
24	5.0	25.9	2.1
25	5.4	25.9	2.1
26	5.8	25.9	2.1
27	5.8	25.9	2.1
28	5.8	25.9	2.1
29	6.2	25.1	1.7
30	5.8	23.5	0.9
31	7.3	26.0	0.9
32	8.4	27.1	0.5
33	7.8	27.9	0.6
34	7.7	24.6	1.2
35	7.9	27.3	1.4
36	6.7	29.5	1.5

TABLE C.2

AVERAGE MONTHLY INCOME BY SOURCE, MONTHS 1 TO 36
AFTER ENROLLMENT, CONTROL GROUP

Month After Enrollment	SSI Benefits	Total Unearned Income	Total Income
1	308	401	439
2	342	427	473
3	313	399	437
4	310	398	439
5	308	401	451
6	310	400	452
7	307	410	466
8	304	395	457
9	310	405	466
10	309	399	459
11	317	410	473
12	310	405	463
13	309	413	478
14	308	403	467
15	308	407	477
16	305	402	479
17	304	402	478
18	304	406	487
19	306	405	490
20	309	423	500
21	302	402	487
22	305	405	489
23	306	413	501
24	302	406	498
25	303	418	506
26	309	415	502
27	300	405	495
28	299	408	503
29	296	405	505
30	305	412	517
31	286	392	501
32	286	392	500
33	288	399	506
34	285	394	502
35	278	390	497
36	277	419	531

TABLE C.3

ANALYSIS SAMPLE SIZES OF THE TREATMENT AND CONTROL GROUPS, MONTHS 1 TO 40 AFTER ENROLLMENT

Month After Enrollment	Treatment Analysis	Control Group	Total Sample
1	375	370	745
2	375	370	745
3	375	370	745
4	375	370	745
5	375	370	745
6	375	370	745
7	375	370	745
8	375	370	745
9	375	370	745
10	375	370	745
11	375	370	745
12	375	370	745
13	375	370	745
14	375	370	745
15	375	370	745
16	375	370	745
17	375	370	745
18	375	370	745
19	375	370	745
20	375	370	745
21	375	370	745
22	375	370	745
23	375	370	745
24	375	370	745
25	375	370	745
26	375	370	745
27	375	370	745
28	375	370	745
29	375	370	745
30	340	335	675
31	304	298	602
32	290	284	574
33	270	258	528
34	247	237	484
35	221	213	434
36	201	193	394
37	181	171	352
38	150	143	293
39	119	116	235
40	93	89	182

TABLE C.4

AVERAGE MONTHLY INCOME BY SOURCE, MONTHS 1 TO 36
AFTER ENROLLMENT, TREATMENT AND CONTROL GROUPS

Month After Enrollment	Earnings		SSI Benefits		Total Income		Computed SSI Benefits	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
1	35	38	318	308	440	439	314	301
2	55	46	315	342	456	473	312	302
3	80	38	314	313	482	437	311	303
4	105	41	312	310	504	439	304	300
5	113	50	309	308	510	451	296	299
6	120	52	302	310	521	452	288	300
7	132	56	294	307	513	466	289	296
8	130	62	296	304	526	457	291	298
9	128	61	293	310	509	466	290	299
10	120	59	299	309	510	459	292	298
11	129	63	298	317	518	473	293	297
12	133	58	300	310	524	463	292	299
13	143	65	290	309	530	478	291	298
14	146	64	302	308	556	467	288	301
15	144	70	290	308	531	477	286	299
16	152	77	298	305	549	479	284	297
17	149	77	292	304	539	478	282	294
18	149	81	288	304	539	487	284	291
19	146	85	303	306	548	490	292	292
20	150	77	311	309	561	500	290	290
21	157	85	301	302	554	487	289	291
22	161	84	303	305	557	489	289	295
23	161	88	306	306	561	501	289	291
24	170	92	299	302	566	498	288	292
25	153	88	294	303	544	506	289	293
26	156	87	296	309	561	502	281	291
27	161	90	284	300	545	495	281	291
28	163	95	288	299	552	503	280	291
29	172	101	284	296	563	505	276	290

TABLE C.4(continued)

Month After Enrollment	Earnings		SSI Benefits		Total Income		Computed SSI Benefits	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
30	161	104	279	305	550	517	272	288
31	177	109	268	286	563	501	268	282
32	171	108	288	286	564	500	272	278
33	170	108	288	288	566	506	272	282
34	165	108	276	285	546	502	274	279
35	169	107	307	278	582	497	285	276
36	165	112	291	277	594	531	285	268

TABLE C.5

MEANS AND STANDARD DEVIATIONS OF INDEPENDENT VARIABLES
USED IN REGRESSION MODEL FOR OVERALL IMPACTS
(Proportion with Characteristic Unless Otherwise Specified)

	Mean	Standard Deviation	Size of Subgroup Defined by Variable
Treatment Status			
Treatments	0.51	0.50	367
Project			
ARC/MU	0.11	0.31	77
The CENTER	0.07	0.25	50
Children's Hospital	0.08	0.27	57
ECF	0.21	0.40	149
Goodwill	0.10	0.30	70
UWash/PCC	0.12	0.33	89
UWis/Stout	0.11	0.31	80
Age			
22 or older	0.78	0.42	564
Gender			
Male	0.59	0.49	431
Race			
Black	0.30	0.46	217
IQ Score[a]			
55 to 70	0.49	0.50	355
40 to 54	0.35	0.48	252
Less than 40	0.06	0.24	44
Receipt of Transfers[b]			
Food stamps	0.18	0.39	132
Welfare[c,d]	0.14	0.34	101
Medicaid[e]	0.93	0.25	675
Eligibility for Social Security in the Month prior to Enrollment[f]			
Was eligible	0.31	0.46	223
Number of Months on SSI	78.0	43.7	n.a.

TABLE C.5 (continued)

	Mean	Standard Deviation	Size of Subgroup Defined by Variable
Employment History during the Year prior to Enrollment			
Had a regular job	0.10	0.31	76
Had mainstream job training or volunteer job	0.09	0.28	62
Worked in sheltered workshop or enclave	0.33	0.47	239
Had other type of job	0.17	0.37	122
Total Earned Income during Year prior to Enrollment (Dollars)	472	878	n.a.
School			
In school	0.15	0.35	107
Unassisted Use of Public Transportation			
Has used regularly	0.77	0.42	559
Living Arrangement			
In a supervised or semi-supervised setting	0.18	0.39	134
With parents	0.62	0.48	453
Federal SSI Computed Amount in the Year prior to Enrollment[g]			
In the lower 25th percentile	0.25	0.43	181
Physical, Social, and Emotional Disabilities[b]			
Has been institutionalized	0.18	0.38	128
Is receiving psychiatric treatment	0.14	0.35	104
Has 1 or more physical disabilities that limit employment[h]	0.40	0.49	292
Has 1 or more emotional problems that limit employment[i]	0.24	0.43	176

TABLE C.5 (continued)

	Mean	Standard Deviation	Size of Subgroup Defined by Variable
Exhibits social behavior inadequate for job interview situation[j]	0.46	0.50	334
Cannot speak clearly in sentences	0.26	0.44	191
Cohort			
Enrolled 6/85 to 9/85	0.32	0.47	230
Enrolled 10/85 to 12/85	0.22	0.41	156
Enrolled 1/86 to 3/86	0.18	0.38	129
Intake Worker's Opinion of Probability of Success in Competitive Job			
High	0.35	0.48	254
Sample Size[k]			725

[a]An imputation for 29 sample members was based on average IQ scores in sample member's project.

[b]Categories are not mutually exclusive.

[c]Welfare includes Aid to Families with Dependent Children (AFDC) and General Assistance.

[d]The proportion receiving welfare was imputed for 18 cases for which this item was missing on the intake form.

[e]The proportion eligible for Medicaid was imputed for 8 cases for which this item was missing on the intake form.

[f]If the sample member received a Social Security benefit payment in the month prior to enrollment in the demonstration, then he or she was categorized as "eligible" for Social Security in that month.

[g]The lower 25th percentile of computed federal SSI receipt includes all those sample members receiving $1,413.55 or less (approximately $118 per month) in the year prior to enrollment in the demonstration. The computed federal amount was used rather than the actual federal payment because it better reflects the amount to which the sample member was entitled in a particular month.

TABLE C.5 (continued)

[h]Physical disabilities include severe visual or hearing impairment, seizure disorders, cerebral palsy, general health problems, arm/head mobility problems, whole body range-of-motion limitations, and ambulatory limitations.

[i]Emotional problems include emotional impairment, mental illness, chemical or drug dependency or abuse, and maladaptive, anti-social, or disruptive behavior.

[j]Inadequate social behavior includes inattention to interviewer, inability to respond appropriately to questions and conversation, inability to make eye contact with interviewer, inability to display appropriate greetings, postures, and gestures, inadequate grooming or attire, physical appearance and characteristics not "normal," and exhibition of unusual behavior or gestures.

[k]Sample size of 725 differs from the demonstration sample size of 745 due to item nonresponse for independent variables.

TABLE C.6

ESTIMATED REGRESSION COEFFICIENTS FOR MODELS USED TO ESTIMATE IMPACTS ON EARNINGS, SSI, AND TOTAL INCOME DURING THE FIRST 24 MONTHS AFTER ENROLLMENT
(t-statistics in parentheses)

	Earnings		Total SSI Payment		Total Income	
Treatment Status						
Treatment	1,574**	(7.06)	-226*	(-1.90)	1,281**	(5.75)
Project						
ARC/MU	2,208**	(4.85)	-836**	(-2.91)	1,568**	(3.45)
The CENTER	284	(0.55)	914**	(2.82)	849	(1.64)
Children's Hospital	2,129**	(4.36)	-105**	(-0.35)	2,372**	(4.86)
ECF	-5	(-0.01)	4,468**	(18.53)	4,429**	(11.52)
Goodwill	-319	(-0.71)	1,677**	(-5.97)	1,696**	(3.79)
UWash/PCC	102	(0.22)	1,299**	(-4.56)	-772*	(-1.70)
UWis/Stout	-448	(-0.92)	2,123**	(6.92)	1,582**	(3.23)
Age						
22 or older	-317	(-0.94)	-210	(-0.99)	-270	(-0.80)
Gender						
Male	51	(0.22)	32	(0.22)	114	(0.49)
Race						
Black	274	(1.00)	354*	(2.06)	510*	(1.87)
IQ Score[a]						
55 to 70	-461	(-1.19)	78	(0.32)	-329	(-0.85)
40 to 54	-361	(-0.87)	160	(0.62)	-320	(-0.77)
Less than 40	-1,165*	(-1.92)	171	(0.45)	-1,477**	(-2.43)
Receipt of Transfers						
Food stamps	-138	(-0.38)	265	(1.16)	-124	(-0.34)
Welfare	71	(0.19)	258	(1.06)	312	(0.81)
Medicaid	-575	(-1.22)	107	(0.36)	-749	(-1.58)
Eligibility for Social Security in the Month prior to Enrollment						
Was eligible	-688*	(-2.05)	-2,756**	(-13.07)	839**	(2.50)
Number of Months on SSI	-3.1	(-1.05)	4.6**	(2.49)	-0.02	(-0.01)
Employment History during the Year prior to Enrollment						
Had a regular job	328	(0.74)	176	(0.63)	694	(1.56)
Had mainstream job training or volunteer job	150	(0.34)	-330	(-1.20)	8	(0.02)
Worked in sheltered workshop or enclave	517	(1.54)	-210	(-1.00)	618*	(1.84)
Had other type of job	81	(0.22)	-94	(-0.40)	-60	(-0.16)
Total Earned Income during Year prior to Enrollment (Dollars)	1.07**	(7.33)	-0.29**	(-3.12)	0.56**	(3.82)
School						
In school	64	(0.17)	130	(0.54)	-6	(-0.02)
Unassisted Use of Public Transportation						
Has used regularly	260	(0.88)	-58	(-0.31)	194	(0.66)

TABLE C.6 (continued)

	Earnings		Total SSI Payment		Total Payment	
Living Arrangement						
In a supervised or semi-supervised setting	-416	(-1.00)	1,519**	(5.82)	946*	(2.27)
With parents	-525	(1.58)	197	(0.95)	-731*	(-2.21)
Federal SSI Computed Amount in the Year prior to Enrollment						
In the lower 25th percentile	969**	(2.70)	-3,425**	(-15.24)	-69	(-0.19)
Physical, Social, and Emotional Disabilities						
Has been institutionalized	487	(1.47)	-26	(-0.12)	551*	(1.67)
Is receiving psychiatric treatment	100	(0.27)	90	(0.39)	40	(0.11)
Has 1 or more physical disabilities that limit employment	51	(0.21)	80	(0.52)	303	(1.24)
Has 1 or more emotional problems that limit employment	-590*	(-1.91)	-166	(-0.86)	-900**	(-2.92)
Exhibits social behavior inadequate for job interview situation	-84	(-0.33)	36	(0.22)	0	(0)
Cannot speak clearly in sentences	154	(0.54)	-211	(-1.18)	8	(0.03)
Cohort						
Enrolled 6/85 to 9/85	-70	(-0.23)	78	(0.32)	-195	(-0.65)
Enrolled 10/85 to 12/85	40	(0.12)	160	(0.62)	-254	(-0.76)
Enrolled 1/86 to 3/86	-317	(-0.93)	171	(0.45)	-277	(-0.81)
Intake Worker's Opinion of Probability of Success in Competitive Job						
High	1,067**	(3.85)	-488**	(-2.81)	597*	(2.16)

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE C.7

ESTIMATED IN-PROGRAM IMPACTS ON EARNINGS BY PROJECT
DURING THE FIRST 24 MONTHS AFTER ENROLLMENT
(Dollars)

	Treatment-Group Mean	Control-Group Mean	Treatment/Control Difference	t-Statistic
Total Sample	3,135.06	1,555.68	1,579.38**	7.20
Project			#	
AHEDD	2,193.30	1,534.61	658.69	1.30
ARC/MU	6,906.69	2,614.75	4,291.94**	6.15
The CENTER	3,240.37	595.59	2,644.78**	3.00
Children's Hospital	5,056.22	3,093.32	1,962.90**	2.43
ECF	2,322.46	734.15	1,588.31**	3.03
Goodwill	2,543.18	1,065.97	1,477.21*	2.05
UWash/PCC	2,861.15	1,340.98	1,520.17*	2.17
UWis/Stout	2,164.34	2,245.34	-81.00	0.10
Sample Size	367	358	752	

SOURCE: Social Security Records

NOTE: Treatment-group means and treatment/control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Treatment/control differences (and therefore treatment-group members) are evaluated at the overall sample mean. Control group means are raw means; they are not regression-adjusted.

#Treatment/control differences differ statistically across subgroups defined by this variable at the 95 percent confidence level.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE C.8

ESTIMATED IN-PROGRAM IMPACTS ON SERVICE USE OF TRANSITIONAL-EMPLOYMENT/SUPPORTED-EMPLOYMENT SERVICE PROJECTS DURING MONTHS 1 TO 24 AFTER ENROLLMENT

(Months)

	Treatment-Group Mean	Control-Group Mean	Treatment/Control Difference	t-Statistic
Total Sample	2.54	1.05	1.49**	2.98
Project			#	
AHEDD	2.51	0.00	2.51*	1.83
ARC/MU	-0.46	0.48	-0.94	0.75
Children's Hospital	3.39	2.98	0.41	0.32
ECF	2.31	0.21	2.10*	2.05
Goodwill	6.06	4.53	1.53	1.12
UWash/PCC	2.38	0.68	1.70	1.31
UWis/Stout	1.18	0.61	0.57	0.46
Sample Size	205	211	416	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment/control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Treatment/control differences (and therefore treatment-group members) are evaluated at the overall sample mean. Control group means are raw means; they are not regression-adjusted.

#Treatment/control differences do not differ statistically across subgroups defined by this variable at the 95 percent confidence level.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE C.9

ESTIMATED IN-PROGRAM IMPACTS ON MONTHS OF OTHER SERVICES BY PROJECT
DURING MONTHS 1 TO 24 AFTER ENROLLMENT

	Treatment-Group Mean	Control-Group Mean	Treatment/Control Difference	t-Statistic
Total Sample	0.44	0.62	-0.18	0.49
Project			#	
AHEDD	0.14	0.55	-0.41	0.41
ARC/MU	0.48	1.22	-0.74	0.80
Children's Hospital	2.16	1.92	0.24	0.25
ECF	0.37	0.00	0.37	0.50
Goodwill	0.48	1.31	-0.83	0.83
UWash/PCC	-0.25	0.46	-0.71	0.75
UWis/Stout	0.78	0.00	0.78	0.86
Sample Size	205	211	416	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment/control differences were estimated with multiple regression to control for project and individual pre-enrollment characteristics. Treatment/control differences (and therefore treatment-group members) are evaluated at the overall sample mean. Control group means are raw means; they are not regression-adjusted.

#Treatment/control differences do not differ statistically across subgroups defined by this variable at the 95 percent confidence level.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE C.10

ESTIMATED IN-PROGRAM IMPACTS ON SERVICE USE OF WORK ACTIVITY CENTER OR SHELTERED WORKSHOP DURING MONTHS 1 TO 24 AFTER ENROLLMENT

(Months)

	Treatment-Group Mean	Control-Group Mean	Treatment/Control Difference	t-Statistic
Total Sample	4.80	6.02	-1.22	1.34
Project			#	
AHEDD	3.55	4.40	-0.85	0.34
ARC/MU	1.91	5.34	-3.43	1.50
Children's Hospital	4.72	7.40	-2.68	1.15
ECF	0.99	0.78	0.21	0.11
Goodwill	5.66	12.14	-6.48**	2.59
UWash/PCC	4.48	4.29	0.19	0.08
UWis/Stout	14.17	12.71	1.46	0.65
Sample Size	205	211	416	

SOURCE: Social Security Administration records.

NOTE: Treatment-group means and treatment/control differences are estimated using multiple regression to control for project and individual pre-enrollment characteristics. Treatment/control differences (and therefore treatment-group members) are evaluated at the overall sample mean. Control group means are raw means; they are not regression-adjusted.

#Treatment/control differences do not differ statistically across subgroups defined by this variable at the 95 percent confidence level.

*Statistically significant at the 95 percent confidence level in a one-tailed test.
**Statistically significant at the 99 percent confidence level in a one-tailed test.