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JOBSTART, a federally funded demonstration program, offered basic education, occupational skills training, support services, and job placement assistance to young school dropouts. A study of the impacts of JOBSTART was designed to separate effects of the program from events attributable to other factors. A total of 2,312 people eligible for JOBSTART were randomly assigned: 1,163 to the experimental group and 1,149 to the control group. Thirteen sites conducted random assignments over varying periods of time from August 1985 to November 1987. Follow-up surveys at 12 and 24 months after assignment gathered data on outcomes such as participation in education and training programs, educational attainment, employment, earnings, and use of public benefit programs. An examination of JOBSTART services and participation at the 13 sites found great diversity within the general framework of the guidelines; longer and more substantial participation than that in other programs for young school dropouts; and similar participation hours among different groups and types of sites. JOBSTART had strong impacts on educational attainment--completion of high school or receipt of a General Educational Development certificate. The impacts on employment and earnings were encouraging for mothers; small, but slightly positive, for other women; and generally negative for men. JOBSTART was sometimes effective and sometimes ineffective in yielding second-year labor market gains in brokered programs and in-house programs; it also showed varying success in both concurrent programs and sequential programs. (An executive summary, 64 tables, and 10 figures are provided. Appendixes include additional data tables and 55 references.) (YLB)
ASSESSING JOBSTART

Interim Impacts of a Program for School Dropouts

George Cave
Fred Doolittle

October 1991

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MDRC
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INTERIM IMPACTS OF A PROGRAM
FOR SCHOOL DROPOUTS

George Cave
Fred Doolittle

Manpower Demonstration
Research Corporation

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Our first debt is to the staff at the demonstration sites, especially the JOBSTART counselor/coordina tors. Their efforts turned the idea of JOBSTART into a reality for young people in their communities.

At MDRC, many people played a crucial role in preparing this report. Robert Ivry has been active from the very beginning of the project, helping to conceive the program model, raising funds for the demonstration, and reviewing all reports. Gordon Berlin, Barbara Goldman, Judith Gueron, Milton Little, and Edward Pauly reviewed and provided comments on earlier drafts of this report. Marilyn Price and Sharon Rowser, members of MDRC's Operations Department, helped prepare ratings of JOBSTART's implementation at the various sites.

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The JOBSTART Demonstration was guided by members of MDRC's Board of Directors and its JOBSTART Advisory Committee, chaired by Bernard Anderson. For their thoughtful comments on an earlier draft, the authors would especially like to thank Bernard Anderson, Eli Ginzberg, and Alan Kistler from our Board; Burt Barnow, Richard El'more, Robinson Hollister, and Margaret Simms, who are members of the Advisory Committee; and Orley Ashenfelter and Andy Sum, who served as outside reviewers.

The Authors
PREFACE

One of the major challenges on the domestic policy agenda is to improve the skills levels and employment prospects of disadvantaged young people. For those without a post-secondary education, the labor market situation continues its downward spiral. For youths who have dropped out of high school, the problem is particularly acute. While many ideas have been put forth for alleviating this problem — most recently in a rash of reports on school reform and labor market trends — there is no compelling evidence about what works.

The underlying hypothesis of the JOBSTART Demonstration was that a comprehensive program incorporating many of the key features of the widely respected, primarily residential Job Corps — basic education, occupational skills training, support services, and job placement assistance — could succeed for high school dropouts in nonresidential settings. Success was to be defined as increases in educational attainment, employment, and earnings. Thirteen organizations were selected to implement JOBSTART and to cooperate with the multifaceted evaluation that was central to the project. Since special demonstration funding was not available, the bulk of program operating support came from the nation's employment and training system authorized by the Job Training Partnership Act (JTPA). Thus, the JOBSTART model received a "real-world" test by operating within the existing mainstream delivery system. MDRC — a private, nonprofit corporation experienced in designing, overseeing, and evaluating innovative programs — conceived the demonstration and had overall responsibility for managing it and for conducting the evaluation.

This report, the third of four on the JOBSTART Demonstration, summarizes the key implementation lessons, including recruitment and participation patterns, and features the impact findings from a two-year follow-up survey. In this survey, the educational and labor market experiences of youths who were referred to JOBSTART were compared to those of a randomly selected control group. Overall, the results were mixed. The sites succeeded in implementing JOBSTART, and JOBSTART substantially increased educational attainment levels, as measured by receipt of a GED (high school equivalency certificate). However, the educational investment has not yet translated into employment and earnings gains, although the results are somewhat more encouraging for young women than for young men.

The report also provides insight into different ways of structuring education and training
services — either sequentially or concurrently — but there were no clear patterns of impact differences based on how sites organized these services.

A final report, scheduled for 1993, will include the results of a four-year follow-up survey and thus clarify whether, over this longer period, the educational investment led to greater labor market success.

MDRC and the policy community at large owe a special debt of gratitude to the consortium of 12 demonstration funders, the program staff at the 13 participating sites, and the sites' state and local funding agents. They shared a commitment both to enhance the employment prospects of disadvantaged young people and to build a knowledge base so that future policies can be guided by facts and evidence rather than speculation. Learning what works in the social policy arena hinges on the formation of collaboratives such as this, and it is our hope that the structure of the JOBSTART Demonstration becomes a model for future endeavors aimed at alleviating poverty and increasing the self-sufficiency of disadvantaged populations.

Robert J. Ivry  
Senior Vice President
EXECUTIVE SUMMARY

Although most young people move fairly smoothly from adolescence to employment and self-sufficiency, high school dropouts with poor skills are increasingly unable to make this transition. The statistics are stark: In 1990, only about one-half of all 16- to 24-year-olds who had not completed high school and were not enrolled in some type of education program were working. The figures for blacks are even more discouraging, showing less than one-third working.

The consequences of these employment problems reach well beyond the lives of the young people themselves. Employers and the business community as a whole suffer because job applicants lack the basic skills needed to perform productively. Furthermore, demographic trends suggest a possible worsening of this problem: Over the next decade, the number of young entry-level workers will remain basically stable while the economy continues to grow, and an increasing proportion of these young people will come from groups with historically higher-than-average school dropout rates and basic skills weaknesses. From the perspective of government budgets, these nonworking young people are using services but not paying taxes. Government and nonprofit agencies must contend with long-term welfare dependence, crime, and drug abuse, which are tied to lack of employment success in many direct and indirect ways.

The JOBSTART Demonstration addressed these issues by testing a program of basic education, occupational skills training, support services, and job placement assistance for young school dropouts who read below the eighth-grade level. The demonstration, which was developed and is being evaluated by the Manpower Demonstration Research Corporation (MDRC), was implemented between 1985 and 1988 in 13 diverse sites. Operating funds were provided primarily under the Job Training Partnership Act (JTPA), the nation’s principal employment and training program for economically disadvantaged people.

This report, the third of four on the JOBSTART Demonstration, summarizes findings on the program’s implementation (covered in detail in past reports) and presents early findings, based on two years of follow-up, on the difference that the program made in young people’s educational attainment, employment, welfare receipt, and other important outcomes. A final report, based on approximately four years of follow-up, will present a more complete picture of program impacts and summarize findings on JOBSTART’s cost-effectiveness.
The Goals of the JOBSTART Demonstration and a Summary of Interim Findings

Few programs have been shown to be effective in increasing the educational attainment, employment, and earnings of young people with poor skills. At the time JOBSTART was conceived, the evidence pointed toward more comprehensive programs, which attempt to remedy basic and occupational skills weaknesses, provide support services such as transportation and child care assistance, and substantially help participants find a job. JOBSTART was modeled after one of the few programs recognized to be a success story: the residential Job Corps. That program provides basic education, occupational training, job placement assistance, and an extensive array of support services to the participating youths, who live at Job Corps Centers.

However, the residential Job Corps cannot be offered to most young dropouts. It is relatively expensive, operates in specialized centers, requires the development of work experience positions with employers, and attracts only young people willing and able to live away from home. JOBSTART drew on the Job Corps' experience by offering most of the same basic components in a nonresidential program, although clearly the support services available in most demonstration sites were less extensive than those provided by the Job Corps. Also, there was no work experience component in JOBSTART.

The JOBSTART Demonstration sought to answer four key policy questions relating to this general programmatic approach.

- Recruitment. Could local agencies recruit young, economically disadvantaged, poorly skilled school dropouts?

Many program operators have discovered how difficult it is to reach alienated young people and provide them with the hope and support they need to participate in an intensive program such as JOBSTART. Young school dropouts are often reluctant to return to a school setting, require extensive support services to participate, or seek immediate employment to meet pressing financial needs. In addition, the lengthy eligibility determination process characteristic of many programs (including JTPA's) may discourage some of those who are initially interested.
Implementation. Could sites, working within the funding and administrative constraints of JTPA, put in place a package of services that would address the needs of these youths?

Sites participating in JOBSTART had to raise operating funds from existing programs, and most relied on Title IIA of JTPA. When the demonstration began (and to a considerable extent at the present time), federal and state regulations and prevailing administrative practices encouraged local JTPA administrative agencies (called service delivery areas) to emphasize shorter-term, lower-cost programs and to enroll participants who were more employable than the JOBSTART target group. More specifically, JTPA's performance standards (which emphasized the proportion of participants placed in a job, their wages, and the cost per "success story") created incentives to choose people who were more likely than the JOBSTART target group to achieve these successes at a relatively modest cost. All these factors led to a clear result: At the time JOBSTART began, only about one in four young people served under Title IIA of JTPA were school dropouts, and only about one in four of these school dropouts received basic education. Thus, the successful implementation of JOBSTART (with its relatively lengthy and intensive program components and its disadvantaged target group) could not be taken for granted within JTPA.

Participation. Would the young people respond favorably to this opportunity and make an investment of their time and effort by participating in the services?

Many youths need a substantial amount of education to improve their basic skills, and occupational training to instill job-related competencies, before they can be competitive in the job market. Yet the conditions that make it difficult to recruit them into education and training programs often preclude their completing the coursework. Financial pressures are severe, including the need for immediate cash to pay the rent and buy food. Since many disadvantaged young people work already — albeit sporadically and for very low wages — it is difficult to attract them into programs that cannot pay them stipends, as is the case under JTPA rules. Other barriers to participation — even in programs offering "free" training — include child care responsibilities, unstable housing arrangements, and peer pressure against participation. Programs must find ways to help "stabilize" the young people's lives so they can
move beyond their immediate problems and commit themselves to invest in learning new skills with a longer-term payoff.

- **Impacts.** Would the program lead to an increase in educational attainment, and would this in turn have an impact on subsequent employment, earnings, and other outcomes?

The JOBSTART Demonstration provided a rigorous test of the difference that JOBSTART's combination of nonresidential services made in the lives of young people. Youths who applied for the program were randomly assigned to a group given access to JOBSTART (the experimental group) or to a group not given that access but free to seek other services in the community (the control group). Since the two groups were created by chance, using a lottery-like process, there was only one systematic difference between them: Only those in the experimental group could receive JOBSTART services. Thus, the control group provides information on what those in the experimental group would have done if there had been no JOBSTART program: Some would have found alternative services, some would have worked, and so forth. Therefore, a comparison of the two groups' behavior over time provides an estimate of the difference that the added services the experimental group received (that is, services above the level the control group received on its own) had on their subsequent employment, earnings, welfare receipt, and other outcomes.

Findings available at this half-way point in the follow-up period provide answers to the first three questions and a partial answer to the fourth:

- **Recruitment.** Through intensive outreach efforts, sites were able to recruit the target population of poorly skilled, economically disadvantaged young people, although several sites were unable to meet their recruitment goals.

- **Implementation.** With considerable special effort, sites were able to fund and operate the program components within the JTPA system, and the program model received a fair test in most sites.

- **Participation.** Participation by the experimental group in education and training was substantial and was much above that of the control group youths who sought similar types of services elsewhere. However, most youths fell short of the hours-of-attendance targets for the demonstration.

- **Impacts (on educational attainment).** During the 24 months of follow-up available for this report, JOBSTART led to a doubling of the rate of
receipt of a high school diploma or GED (high school equivalency credential) relative to the control group. This was true for both the full sample and most subgroups.

Information on longer-run labor market outcomes is not yet available. The early findings may be summarized as follows:

- **Early impacts on employment.** As expected, more youths in the control group worked during the first year of follow-up than did those in the experimental group, for whom participation in JOBSTART was a major activity. In the second year of follow-up, approximately equal proportions of youths in the two groups worked.

- **Early impacts on earnings.** During the first year of follow-up, when a large portion of the experimental group were active in JOBSTART, controls' earnings, not surprisingly, exceeded experimentalists' earnings by a significant amount. The average earnings of the experimental group remained below those of the control group during the second year. However, the gap in annual earnings showed an encouraging trend, narrowing from $585 in the first year to $205 in the second, and this second-year difference was not statistically significant.

- **Early impacts for key subgroups.** There were early differences among key subgroups, although smaller sample sizes make conclusions less certain. Among men, earnings of experimentalists were significantly less than those of controls in both years of follow-up, although the difference narrowed. For women living with their own children and for other women, the trend was more favorable: After earning less than controls in the first year, experimentalists in both categories of women earned slightly more than controls in the second year.

- **Impacts for individual sites.** Individual sites' impacts varied widely but, except for impacts on educational attainment, the differences across sites were not statistically significant. There was no clear relationship between individual sites' impacts and whether sites offered education before training or both services concurrently, or whether they provided all services themselves or arranged for some services to be provided by other agencies.

Later sections of this Executive Summary present these results in more detail, again grouping them under the topics of program recruitment, implementation, participation, and impacts.
The Structure of the JOBSTART Demonstration

The JOBSTART Demonstration guidelines specified the target group and the character of the core service components. The program was to target 17- to 21-year-old, economically disadvantaged school dropouts who read below the eighth-grade level and were eligible for JTPA Title IIA programs or the Job Corps (which is funded under Title IVB of JTPA). The four central program components were to be implemented as follows:

- **Instruction in basic academic skills** was to be based on individualized curricula chosen by the sites to allow youths to proceed at their own pace toward competency goals in reading, communication, and basic computational skills.

- **Occupational skills training** was to be given in a classroom setting combining theory and hands-on experience to prepare participants for jobs in high-demand occupations.

- **Training-related support services** were to include assistance with transportation and child care, counseling, and, where possible, additional support such as work-readiness and life skills (practical everyday knowledge) training and needs-based or incentive payments tied to program performance.

- **Job placement assistance** was to be provided to help JOBSTART youths find training-related jobs.

Sites were required to offer at least 200 hours of basic education and at least 500 hours of occupational training. These minimums were set after balancing two factors: (1) Youths in the JOBSTART target group were likely to need a substantial amount of education and training if they were to have real opportunities to become competitive in the job market, and (2) the administrative practices of JTPA made higher service targets unrealistic.

Within this general framework, the 13 local JOBSTART programs did vary, reflecting their diverse operating experiences, funding sources, clientele, and local service networks. Among the important types of local variation were: additional program entry requirements in some sites, the extent of integration of education and training instruction, whether the young people were taught in separate classes or by being "mainstreamed" in classes with adults, the number and duration of occupational training courses (small community-based organizations typically offered far fewer courses than did other sites), and the strength of the implementation
of the core JOBSTART components, especially training and job placement assistance.

The sites are listed in Table 1. They included six community-based organizations, three adult vocational schools, a community college, and three Job Corps Centers that already operated nonresidential Job Corps programs.

**Findings on Recruitment of Youths**

Analysis of the characteristics of the 1,839 youths who made up the research sample for this report focused on three key questions: Did sites succeed in recruiting the demonstration's intended target group? Were these JOBSTART youths more or less disadvantaged than those served in other programs? Were there observed educational, employment, or other differences in the backgrounds of subgroups of youths (such as men, women living with their children, and other women) that might help explain differences in their participation and impacts?

- **With considerable effort, the sites recruited the poorly skilled, economically disadvantaged young people making up the intended target group for the demonstration.**

The youths in the demonstration were all eligible for JTPA Title IIA programs or the Job Corps; they included slightly more women than men. Most of the sample were members of minority groups and unmarried; nearly three-fourths were under 20 years of age; almost one-half did not work during the year prior to random assignment; and about three-fifths left school before the eleventh grade.

JOBSTART youths appear to have been more disadvantaged than the majority of young people – or even young dropouts – served nationwide by JTPA Title IIA programs during the period JOBSTART was in operation. However, those in JOBSTART, especially the men, were not among the most disadvantaged youths (young people who rarely participate in any program) and were probably slightly more skilled than the typical residential Job Corps member during the same period.

- **Among JOBSTART youths, women living with their own children had noticeably weaker ties to employment than did other women or men.**

Prior work experience could affect both the likely behavior of the youths in the control group and the service needs of participants in the program. Women living with their own
<table>
<thead>
<tr>
<th>Agency Name and Location</th>
<th>Type of Organization</th>
<th>Prior Service Emphasis(^a)</th>
<th>JOBSTART Program Structure(^b)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Allentown Youth Services Consortium, Buffalo, NY(^c)</td>
<td>Community-based</td>
<td>Education</td>
<td>Sequential/brokered</td>
</tr>
<tr>
<td>Atlanta Job Corps, Atlanta, GA</td>
<td>Job Corps Center</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>Basic Skills Academy (BSA), New York, NY</td>
<td>Community-based</td>
<td>Education</td>
<td>Sequential/brokered</td>
</tr>
<tr>
<td>Capitol Region Education Council (CREC), Hartford, CT</td>
<td>Community-based</td>
<td>Education</td>
<td>Sequential/brokered</td>
</tr>
<tr>
<td>Center for Employment Training (CET), San Jose, CA</td>
<td>Community-based</td>
<td>Training with some education</td>
<td>Concurrent</td>
</tr>
<tr>
<td>Chicago Commons Association's Industrial and Business Training Programs, Chicago, IL</td>
<td>Community-based</td>
<td>Training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>Connelley Skill Learning Center, Pittsburgh, PA</td>
<td>Adult vocational school</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>East Los Angeles Skills Center, Monterey Park, CA</td>
<td>Adult vocational school</td>
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<tr>
<td>El Centro Community College Job Training Center, Dallas, TX(^d)</td>
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<td>Education and training</td>
<td>Sequential/in-house</td>
</tr>
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<td>Adult vocational school</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
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<td>Los Angeles Job Corps, Los Angeles, CA</td>
<td>Job Corps Center</td>
<td>Education and training</td>
<td>Sequential/in-house</td>
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<td>Phoenix Job Corps, Phoenix, AZ</td>
<td>Job Corps Center</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>SER/Jobs for Progress, Corpus Christi, TX</td>
<td>Community-based</td>
<td>Training</td>
<td>Concurrent</td>
</tr>
</tbody>
</table>

NOTES:
\(^a\)Education refers to basic education, often as preparation for the GED examination. Training refers to instruction in occupational skills needed for specific jobs.
\(^b\)Concurrent programs offer basic education and occupational training concurrently from the beginning of participation. Sequential/in-house programs offer basic education followed by occupational training, with both components provided in-house by the agency. Sequential/brokered programs provide basic education and then serve as a broker for occupational training, referring participants to other agencies.
\(^c\)In October 1990 this site was renamed The Clarkson Center, Inc.
\(^d\)In September 1988 this site was renamed the Edmund J. Kahn Job Training Center.
children were least likely to have worked during the year before random assignment and most likely to have received Aid to Families with Dependent Children (AFDC) and Food Stamps. Other women worked much more, and the men in the sample worked most of all. Because of clear differences in initial characteristics between mothers caring for their children and other women, the gender-based subgroup analyses that follow divide the female sample into these two groups. However, in most of the analysis, all men are grouped together because only a small number of men reported at the outset that they were parents.

Findings on Program Implementation

- In general, the JOBSTART program model received a fair test in the demonstration. Most sites were able to put the program model in place, although the sites varied considerably in the intensity of their services and the way they were offered.

Basic education, occupational training, support services, and job placement assistance were available to participants in each site, but the varying ways these services were linked reflected the sites' past operational experience and current capacity to provide services in-house. As shown in Table 1, eight of the 13 sites provided basic education and occupational skills training concurrently ("concurrent" sites); two provided a sequence of education followed by training ("sequential/in-house" sites); and three provided education and then referred participants to other agencies for training ("sequential/brokered" sites). In sites offering education and training concurrently, participants usually attended two hours of education classes and four hours of vocational training a day. In sites operating a sequential program, participants generally attended three hours a day of basic skills classes during the education phase, with the remaining three hours a day being devoted to life skills classes and employability workshops. As discussed later, participation rates and hours in the components of the model differed among these three types of sites.

Basic Education Activities

- Sites successfully implemented the JOBSTART basic education component, although they varied in educational emphasis.

The education component typically consisted of individualized instruction, which allowed students to move at their own pace learning reading, mathematics, and other subjects needed
to pass the General Educational Development (GED) examination.

In sites where funding for education services was based on students passing the GED examination, staff made GED certification an important short-term goal and emphasized the skills tested on the GED examination in their education components. Two concurrent sites—CET/San Jose and Chicago Commons—provided some or all of their basic skills instruction as part of vocational training, with a focus on improving those skills needed to successfully complete the training. The remaining sites fell between these extremes.

**Occupational Skills Training**

- Despite great variation in course offerings among the sites, JOBSTART youths generally studied occupations with skills requirements comparable to those for adults served within JTPA nationwide.

Participants at large vocational schools could choose courses in more than 20 occupations, and the Job Corps Centers and large community-based organizations also offered a wide range of training. In contrast, small community-based organizations providing training in-house typically offered fewer than five courses. In theory, youths at sequential/brokered sites could choose courses from a variety of agencies, but in practice their choices were limited because they could not meet entrance requirements or encountered other administrative difficulties in gaining admission. Occupational choices for men and women followed traditional patterns, with about three-fourths of the women in clerical fields and slightly more than one-half of the men in machine trades, metal fabrication, or construction.

JOBSTART training was intended to prepare participants primarily for jobs requiring moderate skills (about one-half of participants) or higher skills (about one-fourth of participants). This distribution of skills ratings for training occupations was similar to what the U.S. General Accounting Office found for JTPA adult programs, an unexpected result since JOBSTART participants faced more barriers to employment than did the typical JTPA adult client.

**Training-Related Support Services**

- All sites provided transportation and child care assistance, but the availability of other services varied greatly.

Clearly, the support services and other activities available at the Job Corps Centers
surpassed those at the other sites in both breadth and intensity. However, many sites were able to provide instruction in life skills and some type of needs-based payments.

Job Placement Assistance

- The job placement component of the program was the least developed in many sites. In particular, participants leaving JOBSTART before completing the curriculum received relatively little aid in finding a job.

In sites with strong job placement assistance, instruction in proper work behavior, employer expectations, and job search techniques began while students were still in training; placement specialists provided leads and assistance in finding jobs; and staff had especially strong ties to local employers. Job placement assistance was noticeably weaker in the three sequential/brokered sites, where it was the responsibility of the training agency. Unfortunately, most participants never reached the training components, so only informal assistance was available from the JOBSTART agency.

Findings on Participation in JOBSTART

- Young people in the experimental group attended an average of more than 400 hours of group activities, which is impressive when compared to the experience of many other JTPA-funded programs. Despite this relative success, most young people did not acquire the skills needed to pass the GED examination and did not participate enough to complete an occupational training course.

Experimentals (including the 11 percent who did not participate at all) averaged 415 hours of attendance in JOBSTART classes, as shown in Table 2. The young people spent most of this time in education and occupational training, as opposed to other activities such as life skills training. Slightly more than one-third of all experimentals participated for more than 500 hours, and another one-quarter were active for 201 to 500 hours. These findings show that JOBSTART succeeded in engaging more than one-half of the youths in the experimental group in the program and its activities, but for about two-fifths of those in the experimental group, participation was neither lengthy nor intensive.

The average length of stay in JOBSTART was 6.8 months, with 16 percent of experimentals still active in the program 12 months after random assignment and nearly 10 percent active 15 months after random assignment. This means that for most experimentals
<table>
<thead>
<tr>
<th>Activity Measure</th>
<th>Experimentals</th>
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<tr>
<td>Percent participating in</td>
<td></td>
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<tr>
<td>Any activity</td>
<td>88.7</td>
</tr>
<tr>
<td>Education</td>
<td>85.9</td>
</tr>
<tr>
<td>Training</td>
<td>66.6</td>
</tr>
<tr>
<td>Education and training</td>
<td>64.4</td>
</tr>
<tr>
<td>Other activities</td>
<td>40.0</td>
</tr>
<tr>
<td>Average hours in</td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>128.1</td>
</tr>
<tr>
<td>Training</td>
<td>248.9</td>
</tr>
<tr>
<td>Education and training</td>
<td>377.0</td>
</tr>
<tr>
<td>Other activities</td>
<td>37.3</td>
</tr>
<tr>
<td>All activities</td>
<td>414.8</td>
</tr>
<tr>
<td>Percentage distribution of hours in</td>
<td></td>
</tr>
<tr>
<td>education and training</td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>11.9</td>
</tr>
<tr>
<td>Up to 200</td>
<td>33.2</td>
</tr>
<tr>
<td>201 to 500</td>
<td>22.4</td>
</tr>
<tr>
<td>501 to 700</td>
<td>15.5</td>
</tr>
<tr>
<td>701 or more</td>
<td>17.0</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
</tr>
<tr>
<td>Percentage distribution of hours in</td>
<td></td>
</tr>
<tr>
<td>all activities</td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>11.3</td>
</tr>
<tr>
<td>Up to 200</td>
<td>28.6</td>
</tr>
<tr>
<td>201 to 500</td>
<td>25.5</td>
</tr>
<tr>
<td>501 to 700</td>
<td>15.2</td>
</tr>
<tr>
<td>701 or more</td>
<td>19.5</td>
</tr>
<tr>
<td>Total</td>
<td>100.1</td>
</tr>
<tr>
<td>Length of stay (months)</td>
<td></td>
</tr>
<tr>
<td>Average</td>
<td>6.8</td>
</tr>
<tr>
<td>Median</td>
<td>6.0</td>
</tr>
<tr>
<td>Percent still participating in month</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>78.0</td>
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<tr>
<td>6</td>
<td>53.6</td>
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<tr>
<td>9</td>
<td>30.6</td>
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<tr>
<td>12</td>
<td>16.4</td>
</tr>
<tr>
<td>15</td>
<td>9.6</td>
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<tr>
<td>18</td>
<td>4.8</td>
</tr>
<tr>
<td>19 or later</td>
<td>3.7</td>
</tr>
<tr>
<td>Number of experimentals</td>
<td>949</td>
</tr>
</tbody>
</table>

**NOTES:** Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.
the first year of follow-up was primarily a period of program participation and that for 16 percent the second year also included months with program activity. JOBSTART experimentals stayed in the program considerably longer than the typical young dropout served in JTPA Title IIA programs and nearly as long as the average Job Corpsmember.

- Differences in participation were associated with choices made at the site level (1) to offer concurrent versus sequential education and training and (2) to house all services on-site rather than refer youths elsewhere for training.

Youths in sequential/in-house sites averaged the highest total hours, followed by youths in concurrent programs. Those in sequential/brokered sites averaged noticeably fewer total hours, primarily because only about 25 percent of the young people successfully made the transition to the off-site occupational training. This occurred because of difficulties arranging linkages with another organization for training within JTPA and because youths at these education-oriented sites may have been more interested in studying for their GED credential than in getting occupational training. (Young people in sequential/brokered sites averaged the most education hours.) As would be expected, sequential sites showed a higher proportion of experimentals still active in the program a year after random assignment.

- Participation was surprisingly similar among key demographic groups.

Men, women living with their own children, and other women averaged 410, 408, and 429 total hours, respectively. The groups' average lengths of stay in the program were quite similar, although a somewhat higher proportion of women than men were still active in JOBSTART 12 months after random assignment. Youths who had not been arrested between their sixteenth birthday and the time of random assignment did have significantly higher average hours of participation. However, other subgroup analysis (such as by race, age, grade level at school dropout, marital status, and receipt of public assistance) showed no statistically significant differences in average total hours.

Findings on Program Impacts

Young people in the JOBSTART sample could have improved their skills by several means: through participating in JOBSTART (for those in the experimental group) or in other
education and training programs, or by learning on the job. Program impacts are measured by comparing the experiences of the experimental and control groups; in essence, this compares the payoff of the investment made by experimentals with that made by controls.

**A Framework for Analyzing JOBSTART's Labor Market Impacts**

As is true of most investments, many of the costs of participating in a program such as JOBSTART are incurred in the short run. The benefits will accrue over the young people's lifetime if they learn new skills that pay off in the labor market. Figure 1 presents a theoretical framework for analyzing the labor market aspects of this investment and the alternative investments made by young people in the control group. Those in JOBSTART committed their time and effort to improving their skills in the expectation of a future payoff. While participating, they gave up the chance to work and earn, so any forgone income was an in-program opportunity cost of the program, represented in the figure by the shaded area between the start and end of JOBSTART participation.

The figure also shows the control group's earnings rising over time. This reflects their growing employment rate and, for some, the acquisition of new skills on the job. For young people with poor skills, work experience can be an important source of new skills, which can translate into increased productivity and earnings and more stable employment. Once their participation in JOBSTART ended, young people in the experimental group will have looked for jobs, but their employment rates and earnings may not immediately exceed (or even reach) those of controls who were already working rather than attending a program. This post-program opportunity cost is the area in the figure between the end of program participation and the hoped-for point at which the earnings of the experimental group exceed those of the control group.

For a program such as JOBSTART to pay off for young people, the long-term benefits of increased education and training (represented in the figure by the shaded area on the right) must exceed the forgone — more immediate — rewards of possible earnings and enhanced skills through work experience. Even in successful programs, it will take time for participants to overcome the head start of those who have been working throughout the program period.
FIGURE 1

A THEORETICAL VIEW OF THE PAYOFF
OF A PERSONAL INVESTMENT IN EDUCATION AND TRAINING
The Two-Year Impact Sample

The 2,312 youths who applied for JOBSTART and were judged eligible were randomly assigned to either the experimental or control group. Follow-up surveys attempted to reach all members of both groups 12 and 24 months after they were randomly assigned. This analysis of program impacts uses a sample of 1,839 (80 percent of all those who were randomly assigned). These are the youths who provided information for this two-year period. Impacts reported in the following sections were statistically significant (that is, unlikely to have arisen by chance) unless otherwise noted.

Experimental-Control Differences in Participation in Education and Training

For JOBSTART to make a difference in the lives of the young people, those in the experimental group must have participated in substantially more education and training activities than did those in the control group, who had access to other services in the community.

- During the follow-up period, experimentals were nearly twice as likely as controls to have participated in some type of education or training. However, nearly one-half of all controls also got some education or training.

Table 3 shows experimental-control differences in the percentage of the youths who ever participated in education and training; the differences are shown for both the full sample and key subgroups. In the two years following random assignment, 93 percent of experimentals versus 44 percent of controls received some type of education or training. Experimentals averaged 619 hours in these activities, whereas controls averaged 250 hours. The differences were largest during the early months of the follow-up period, when most experimentals were active in JOBSTART, and gradually disappeared by the end of the two years. All the subgroups that were analyzed showed large differences between experimentals and controls in service receipt. There was a greater experimental-control difference in the percentage of youths participating in education than in the percentage participating in training, because many young people at sequential (and especially sequential/brokered) sites did not participate in training.
### TABLE 3

**IMPACTS OF JOBSTART ON SERVICE RECEIPT AND EDUCATIONAL ATTAINMENT DURING THE 24 MONTHS FOLLOWING RANDOM ASSIGNMENT**

<table>
<thead>
<tr>
<th>Outcome and Subgroups</th>
<th>Sample Size</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ever received any education or training</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Full sample</td>
<td>1,839</td>
<td>92.7%</td>
<td>44.2%</td>
<td>48.4***</td>
</tr>
<tr>
<td>Men</td>
<td>871</td>
<td>92.8</td>
<td>36.6</td>
<td>56.1***</td>
</tr>
<tr>
<td>Women living with own child(ren)</td>
<td>484</td>
<td>93.6</td>
<td>47.9</td>
<td>45.7***</td>
</tr>
<tr>
<td>Women not living with own child(ren)^a</td>
<td>484</td>
<td>92.5</td>
<td>54.1</td>
<td>38.4***</td>
</tr>
<tr>
<td>Received GED or high school diploma</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Full sample</td>
<td>1,839</td>
<td>33.2</td>
<td>16.4</td>
<td>16.7***</td>
</tr>
<tr>
<td>Men</td>
<td>871</td>
<td>32.1</td>
<td>16.9</td>
<td>15.2***</td>
</tr>
<tr>
<td>Women living with own child(ren)</td>
<td>484</td>
<td>35.5</td>
<td>14.2</td>
<td>21.3***</td>
</tr>
<tr>
<td>Women not living with own child(ren)^a</td>
<td>484</td>
<td>32.4</td>
<td>18.2</td>
<td>14.2***</td>
</tr>
</tbody>
</table>

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

^aIncludes women who did not have children.
Impacts on Educational Attainment

- JOBSTART led to a doubling of the rate of GED certification or receipt of a high school diploma, from 16.5 percent of the control group to 33.1 percent of the experimental group.

Table 3 presents JOBSTART's impacts on educational attainment during the 24 months of follow-up and includes both the full sample and key subgroups. The full sample impact on attainment of a GED or high school diploma was 16.7 percentage points, similar to the results found in an evaluation of the residential Job Corps.

- These large educational attainment impacts were present for many different subgroups in the overall sample.

Among men, 32.1 percent of experimentals versus 16.9 percent of controls completed high school or passed the GED examination during the follow-up period, for an impact of 15.2 percentage points. For women living with their own children, the figures were 35.5 percent of experimentals and 14.2 percent of controls, for an impact of 21.3 percentage points; and for other women, the figures were 32.4 percent of experimentals and 18.2 percent of controls, for an impact of 14.2 percentage points. Numerous other subgroups based on work experience, welfare receipt, prior education, initial reading level, and age all showed similar large impacts.

Impacts on Employment and Earnings for the Full Sample

- As expected, more youths in the control group than in the experimental group worked during the first year of follow-up. In the second year of follow-up, the proportions were not significantly different.

As shown in Table 4, 57.6 percent of experimentals and 61.6 percent of controls worked at some time during the first year of follow-up, for a 4.0 percentage point decrease in employment among experimentals relative to controls. At some point during the second year, 69.5 percent of controls and 72.0 percent of experimentals worked. This 2.5 percentage point impact was not statistically significant. The control group's employment rate exceeded the experimental group's by the largest amount in month 5, after which the difference narrowed. In the second year of follow-up, the experimentals' employment rate exceeded the controls' rate in each of six months and was below it in the other six months; however, in no month was this difference statistically significant.
## TABLE 4
IMPACTS OF JOBSTART ON EMPLOYMENT AND EARNINGS
DURING THE 24 MONTHS FOLLOWING RANDOM ASSIGNMENT

<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Full sample</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>79.2</td>
<td>77.9</td>
<td>1.3</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>57.6</td>
<td>61.6</td>
<td>-4.0*</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>72.0</td>
<td>69.5</td>
<td>2.5</td>
</tr>
<tr>
<td>Total earnings ($)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>5,859.56</td>
<td>6,649.07</td>
<td>-789.52***</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>1,965.47</td>
<td>2,550.26</td>
<td>-584.78***</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>3,894.08</td>
<td>4,098.81</td>
<td>-204.73</td>
</tr>
<tr>
<td>Sample size</td>
<td>949</td>
<td>890</td>
<td></td>
</tr>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>89.0</td>
<td>90.1</td>
<td>-1.1</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>66.7</td>
<td>74.8</td>
<td>-8.1***</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>84.3</td>
<td>84.3</td>
<td>0.0</td>
</tr>
<tr>
<td>Total earnings ($)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>7,797.22</td>
<td>9,492.61</td>
<td>-1,695.39***</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>2,648.50</td>
<td>3,676.88</td>
<td>-1,028.38***</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>5,148.72</td>
<td>5,815.73</td>
<td>-667.01*</td>
</tr>
<tr>
<td>Sample size</td>
<td>438</td>
<td>433</td>
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</tr>
<tr>
<td><strong>Women living with own child(ren)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>62.1</td>
<td>57.2</td>
<td>4.9</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>41.0</td>
<td>38.3</td>
<td>2.7</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>53.3</td>
<td>46.1</td>
<td>7.2</td>
</tr>
<tr>
<td>Total earnings ($)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>3,035.55</td>
<td>2,952.31</td>
<td>83.24</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>1,011.83</td>
<td>1,100.96</td>
<td>-89.13</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>2,023.73</td>
<td>1,851.35</td>
<td>172.38</td>
</tr>
<tr>
<td>Sample size</td>
<td>250</td>
<td>234</td>
<td></td>
</tr>
<tr>
<td><strong>Women not living with own child(ren)(a)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>79.3</td>
<td>75.8</td>
<td>3.4</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>57.0</td>
<td>61.5</td>
<td>-4.5</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>69.5</td>
<td>65.3</td>
<td>4.2</td>
</tr>
<tr>
<td>Total earnings ($)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>5,070.23</td>
<td>5,290.80</td>
<td>-220.58</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>1,607.50</td>
<td>2,030.02</td>
<td>-422.53*</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>3,462.73</td>
<td>3,260.78</td>
<td>201.95</td>
</tr>
<tr>
<td>Sample size</td>
<td>261</td>
<td>223</td>
<td></td>
</tr>
</tbody>
</table>

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

\(a\)Includes women who did not have children.
Experiments earned significantly less than controls in the first year of follow-up. In the second year, the gap between experimental and controls narrowed, and the difference was no longer statistically significant.

As expected, experimental earned less than controls during the first year of follow-up (see Table 4); this $585 difference was a clear opportunity cost of participating in the program. In the second year, although the proportion of experimental working drew even with the proportion of control, experimental continued to lag slightly (but not significantly in the statistical sense) behind controls in hours worked per week and weeks worked per month. As a result, the earnings of experimental remained below those of control during the second year; however, the difference ($205) was no longer statistically significant. The cumulative opportunity cost in the form of forgone earnings was, therefore, $790.

**Impacts on Employment and Earnings for Key Subgroups**

Many past studies of nonresidential education and training programs have found starkly different results for men, women living with their children, and other women. Thus, it is important to move behind the findings for the full sample and examine subgroups that are of special policy interest. In this analysis, sample sizes are smaller and the conclusions are therefore less certain.

Table 4 shows employment rate and earnings impacts for these subgroups. Among men, a significantly lower percentage of experimental than controls worked at some point in the first year, but in the second year employment rates were equal. Earnings for experimental were significantly below those of control in both years, although the gap did narrow. For the two groups of women, the pattern over time was more positive, but sample sizes were smaller, so almost all of the impacts were not statistically significant. Among women living with their own children at the time of random assignment, a higher percentage of experimental than controls worked in each of the two years, with the second year showing a somewhat larger employment rate impact. For this subgroup, controls earned slightly more than experimental during the first year, while experimental earned more during the second, resulting in a small
positive earnings impact for the two-year period. For other women, a higher percentage of controls than experimentals worked at some point in the first year, while in the second year, a higher percentage of experimentals worked. Reflecting this, the earnings of controls exceeded those of experimentals (by a statistically significant amount) in the first year; in the second year, experimentals' earnings were higher than controls' earnings, but the two-year total for earnings was slightly negative.

- The employment experience of the controls in the three groups provides much of the explanation for the pattern of impacts among the groups.

One likely explanation for better employment results for women is that it is easier to improve the employment and earnings of those who do not spend much time in the world of work (for example, young mothers) than of those who are already in the labor force but fail to find and keep steady, well-paying jobs (for example, poorly skilled young men). Thus, from this perspective, women have greater potential to improve their labor market outcomes than do men, and less to lose (in terms of forgone employment and earnings) by investing in education and training.

The JOBSTART sample followed this pattern, as shown in Table 4. During the first year after random assignment, 74.8 percent of control men worked at some point, compared to 38.3 percent of women living with their own children and 61.5 percent of other women. During this period, which, for many experimentals, consisted mostly of program participation, the impact of JOBSTART on employment rates was 2.7 percentage points for the young mothers, -4.5 percentage points for other women, and -8.1 percentage points for men. Men, and women who were not caring for children, thus paid an opportunity cost for JOBSTART in terms of forgone employment, while young mothers did not.

- The occupational distribution of training for men and women in JOBSTART may have also contributed to these differences in impacts.

A second possible explanation for the poorer impacts observed for men in JOBSTART is the greater difficulty of placing men in jobs that reward a GED. Many women in JOBSTART, for example, were trained in clerical occupations and sought that type of work, a domain in which educational credentials are important. However, young men were more likely to find work in occupations that did not reward the GED, at least not in the early stages
of employment. Many types of blue-collar work, especially physically demanding work, may well fall into this category. The problem for men could be further exacerbated by the greater difficulty of finding training-related employment for men who do study occupations in which having a GED matters.

**Impacts on Other Outcomes for the Full Sample**

- During the first 24 months of follow-up, JOBSTART had no statistically significant impacts on a variety of other outcomes.

This general conclusion was true for the full sample and both women and men, and applies to a range of outcomes including receipt of most public benefits, childbearing, fathering of children, provision of child support (by noncustodial parents), and criminal arrests. The lack of impacts on criminal arrests differs from the finding in a study of the residential Job Corps program, but there is an obvious reason to expect much less of an early impact on criminal behavior for JOBSTART. In the residential Job Corps program, young people moved from their own communities to special centers that provided the education and training services. Many of these centers are in isolated areas or in communities without large gang populations or heavy involvement of youths in the drug trade. Much of the Job Corps' impact on criminal behavior came during the in-program period because of this "isolation" effect. In JOBSTART, no such change took place in the young people's lives; they continued to live in their own neighborhoods and to spend time outside the program with their existing circle of friends.

**Site Differences in Impacts**

There is strong policy interest in the influence of program characteristics on impacts. However, it is extremely difficult to draw clear lessons from differences in the impacts observed for different types of JOBSTART sites. This study was not designed to analyze how variations in the way the basic JOBSTART model was implemented may have affected impacts. Youths were randomly assigned to the experimental or control group in each site, rather than to different types of programs, which would be necessary for a rigorous experimental test of the effectiveness of alternative approaches.

Attempts to use differences in impacts among the sites to understand the influence of program characteristics face serious problems. The number of sites was limited. Sample sizes
in the individual sites were small, so most experimental-control differences within a site were not statistically significant. Furthermore, when the impacts in each site are compared, the differences among the site-specific impacts are not significant on most outcomes. Finally, the sites differed in many characteristics including the background of the youths in the sample, labor market conditions, the level of alternative services received by controls, and many dimensions of their JOBSTART programs.

Despite these difficulties, if a clear pattern of impacts across the sites emerged, it might still be possible to draw some tentative conclusions. For example, if most sites operating sequential programs of education followed by training had impacts that differed markedly from those of sites offering education and training concurrently from the start of program participation, one might conclude that this difference in program structure was an important factor in understanding the differences in impacts.

There was no clear pattern of impacts among the 13 sites that supports conclusions about the effect of program characteristics on program impacts.

This general point can be illustrated using site-specific impacts on second-year earnings. One aspect of program design that is of special interest is the choice of sequential programs of education followed by training versus concurrent programs. When the JOBSTART sites are divided into two groups based on this program feature, within each group there are sites with both positive and negative earnings impacts. When the sequential programs are further subdivided into those offering all services themselves and those linking up with other agencies for some program components, within each of these groups there are both positive and negative impacts. To sum up the site story on this outcome: Very different types of programs had positive impacts. One program with positive earnings impacts was oriented toward training and offered education primarily in the context of occupational training. This program also had a strong job placement effort, little initial screening of applicants, and a relatively short length of participation. A second, in contrast, placed a heavy emphasis on basic education, and only one-third of its participants were active in any occupational training. It had a relatively weak job placement effort and lengthy participation. A third program, between these two extremes, also had positive impacts on second-year earnings.
Next Steps in the Research

The JOBSTART Demonstration tackled a difficult issue, the employment problems of poorly skilled, economically disadvantaged young people. This report provides early follow-up on how a program of education and training, support services, and job placement assistance will affect their employment and earnings. When these young people entered the demonstration, most were teenagers. The four-year follow-up survey, now in progress, will carry the JOBSTART story to the youths’ early twenties, the time when most young people begin to apply their skills in a more serious and committed way to employment.

The program impacts reported here compared two different kinds of skills-enhancing investments. The experimental group, most of whom invested in JOBSTART, paid a clear initial cost in forgone employment in the hope of future payoffs in employment and earnings. Although some members of the control group were in education and training programs, controls were more likely than experimentals to be working; many were, in the process, learning new skills on the job. For these controls, the initial cost in forgone income was small or nonexistent. A long follow-up period is needed for a valid comparison of these two very different types of investments; if the follow-up is too short, the large initial costs of JOBSTART will be included, but not the chance to see if the payoff appears over time. Of course, as the long search for effective programs for disadvantaged young people illustrates, there is no guarantee that the payoff will occur.

Longer follow-up could be especially important in addressing two issues. Sites differed in the extent to which they emphasized education versus occupational training within JOBSTART. Training is more likely than education to have an immediate payoff if placement in a training-related job can be arranged. But education might have longer-run payoffs if it produces the basic skills needed to learn new skills in the future. The longer follow-up will provide an opportunity to understand the terms of this likely trade-off in program emphasis.

Longer follow-up may also prove useful in helping to explain the clear difference in short-term impacts for men and women. Young men, especially minority men, appear to have a more difficult time than women in moving into jobs that reward better basic skills and that relate to their occupational training. Longer-term follow-up will provide a chance to see whether, over time, they can make this transition.
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<tr>
<td>AFDC</td>
<td>Aid to Families with Dependent Children</td>
</tr>
<tr>
<td>BSA</td>
<td>Basic Skills Academy (New York, New York)</td>
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<tr>
<td>CBO</td>
<td>Community-Based Organization</td>
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<tr>
<td>CCP</td>
<td>Comprehensive Competencies Program</td>
</tr>
<tr>
<td>CET</td>
<td>Center for Employment Training (San Jose, California)</td>
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<tr>
<td>CETA</td>
<td>Comprehensive Employment and Training Act (1973)</td>
</tr>
<tr>
<td>CREC</td>
<td>Capitol Region Education Council (Hartford, Connecticut)</td>
</tr>
<tr>
<td>EGOS</td>
<td>Emily Griffith Opportunity School (Denver, Colorado)</td>
</tr>
<tr>
<td>FSA</td>
<td>Family Support Act (1988)</td>
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<tr>
<td>GAO</td>
<td>U.S. General Accounting Office</td>
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<td>GED</td>
<td>General Educational Development Certificate</td>
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<tr>
<td>JOBS</td>
<td>Job Opportunities and Basic Skills Training Program</td>
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<tr>
<td>JTPA</td>
<td>Job Training Partnership Act (1982)</td>
</tr>
<tr>
<td>MDRC</td>
<td>Manpower Demonstration Research Corporation</td>
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<tr>
<td>MFSP</td>
<td>Minority Female Single Parent Demonstration</td>
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<tr>
<td>MIS</td>
<td>Management Information System</td>
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<tr>
<td>PIC</td>
<td>Private Industry Council</td>
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<td>SDA</td>
<td>Service Delivery Area</td>
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<td>SSI</td>
<td>Supplemental Security Income</td>
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<td>YIEPP</td>
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<td>YOU</td>
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CHAPTER 1

AN OVERVIEW OF THE JOBSTART DEMONSTRATION

Most young people make the transition from adolescence to employment and self-sufficiency between the ages of 16 and 24. However, many others — especially high school dropouts with poor skills — fail to do so, even in a period of strong economic growth such as the mid to late 1980s. The negative consequences extend well beyond the lives of the young people, affecting both the general public and the business community. There is strong evidence that the incidence of poverty, welfare receipt, criminal activity, and unwed parenthood is significantly higher for those with poor basic skills than for the population as a whole.\textsuperscript{1} Society bears the cost of this in the form of social disruption and increased public services.

Employers and the business community also suffer from young people’s lack of job-related skills, and they are likely to find this an even greater problem in the future. The U.S. Department of Labor projects that in the year 2000 the number of young people in the labor force — a major source of entry-level workers — will be basically unchanged, even though the economy as a whole will have expanded considerably.\textsuperscript{2} Further, a growing proportion of these young people will come from groups with traditionally higher-than-average school dropout rates and basic skills deficiencies — minorities, recent immigrants, youths from single-parent families, and the poor. Basic skills shortages could translate into jobs going unfilled, employers having to pay higher wages to attract skilled workers, and some jobs needing restructuring to accommodate the available workforce.

What will help disadvantaged young school dropouts lead more productive lives? A number of experts have called for programs of education and training.\textsuperscript{3} The JOBSTART Demonstration tested such a program by combining basic education, training in occupational skills, limited support services (primarily assistance with child care and transportation), and job placement assistance. Developed and overseen by the Manpower Demonstration Research Corporation (MDRC), JOBSTART was implemented in 13 sites: four adult schools (three adult

\textsuperscript{1}Berlin and Sum, 1988.
\textsuperscript{2}Fullerton, 1989.
vocational schools and one community college), six community-based organizations (CBOs), and three nonresidential Job Corps programs. The demonstration ran from 1985 to 1989, with operating support consisting primarily of funds provided under the Job Training Partnership Act (JTPA), the nation's largest funder of employment and training programs for economically disadvantaged persons.  

For an employment program like JOBSTART to improve the lives of young people, certain things must happen:

- The sites must recruit young, economically disadvantaged school dropouts with poor skills (a group that is typically not served even in intensive programs).
- The sites must put in place a package of services that addresses the needs of the youths.
- The young people must respond favorably to this opportunity and make an investment of their time and effort by participating in education and training activities.
- Their efforts must yield them new skills, as measured, for example, by their completing high school or passing the General Educational Development (GED) Test for high school equivalency certification.
- Over time, these new skills must translate into greater employment and earnings than these youths otherwise would have had, and less need to rely on public assistance.

Understanding whether these changes did occur is the goal of the JOBSTART evaluation.

It is an open question whether helping young disadvantaged people increase their educational attainment will lead to increased earnings, especially in the short run. Numerous studies have found that people with higher levels of education earn more than those

---

4Such funds came from Title IIA of JTPA, the largest portion of the JTPA program, or Title IVB, which funds the Job Corps Centers.

5The General Educational Development Test is a national examination produced and administered by the GED Testing Service of the American Council on Education in Washington, D.C. States have different criteria for who may take the examination, different passing scores, and different credentials awarded to those passing (for example, a state high school equivalency certificate or a state high school diploma). In accordance with common usage, the credential is referred to in this report as a GED certificate or, simply, a GED.

6The program could also affect other aspects of the young people's lives. They might be better able to live on their own instead of with their parents, more likely to postpone childbearing (because they see opportunities in the labor market), and less likely to engage in criminal or other antisocial behavior.
without a high school diploma. For example, one estimate, based on the experiences of young men and women during the late 1970s, found that the payoff of an additional year of secondary school was approximately $700 in increased annual income, while a high school diploma had a "credential effect" of about $925 per year. But direct rigorous tests of the impact of increasing the educational attainment of a group of disadvantaged young people who did not complete high school are rare.

Although, in general, increasing the skills of young people does increase their earning capacity, there are two important countervailing effects of a program like JOBSTART, at least in the short run. Participation in an intensive program "pulls" people out of the labor force. Not only do they give up earnings while they participate in the program, but they also have less chance to gain skills and seniority through their on-the-job experience. For young people, especially those with low levels of educational attainment, work experience is an important source of new skills and increased job stability and wages.

Further, little is known about how employers assess receipt of a GED, the primary educational outcome in "second chance" programs like JOBSTART. The assumption behind such programs is that employers will view a GED as evidence of increased skills, but virtually nothing is known about how long it takes for a GED to pay off and how this might vary among subgroups of youths and types of occupations. Alternatively, and less optimistically, a GED may do little to counter the negative impression created by the fact that the young person did not finish high school.

Earlier reports on the JOBSTART Demonstration, summarized and updated in this document, found that the first four conditions for program impacts listed above were generally met: The programs recruited the target group of youths; they generally offered the intended

---

7 Berlin and Sum, 1988. These estimates attempted to control for the level of basic skills of individuals, by including youths' scores on the Armed Forces Qualifying Test, administered as part of a special survey of youths, as an independent variable in a regression. The estimated effect on annual earnings of an additional grade-equivalent of basic skills (for example, progressing from a seventh- to an eighth-grade reading level) was $185.

8 An unpublished analysis by Andrew Sum, using the Current Population Survey, finds that the average earnings of high school dropouts increase with age noticeably during the late teenage years and early and mid 20s. In 1986, for example, the annual average earnings of 19-year-olds were about $2,000 higher than for 18-year-olds.

9 A disproportionately higher labor market reward for completing high school than for dropping out part-way through may indicate that employers rely on high schools more for screening workers than for teaching them basic skills (see Layard and Psacharopoulos, 1974). If this is true, attaining a GED after dropping out may add little to a young person's employability.
services; the youths participated in education and training; and this led to increased GED receipt. Thus, the JOBSTART Demonstration presents a unique opportunity to see whether and how enhanced educational attainment and vocational skills training for disadvantaged youths are "processed" by employers in the labor market.

This report, covering two years in the lives of the young people in the demonstration, is about the JOBSTART program's early effects. As with most investments, JOBSTART had up-front costs. For the young people, the costs were (1) the time and effort devoted to participating in the program, (2) the income forgone by not working, and (3) the lost opportunity to learn new skills on the job. For the public, the cost was financial support for the program activities. As with many other investments, the costs were incurred quickly, while the benefits may accrue over an extended period.

With only two years of follow-up on the experiences of the young people, this is an interim report rather than the final story on JOBSTART. It summarizes the project's policy goals and research design; describes the nature and intensity of the education, training, and other services the young people participated in; and presents 24-month program impacts on educational attainment, employment, earnings, receipt of public assistance, and other outcomes. Throughout, the analysis examines whether the program has worked differently for subgroups of the young population it served.

This chapter discusses aspects of the youth employment problem that shaped the program model and its evaluation. It then examines the policy and programmatic context in which JOBSTART emerged, highlighting how the rules and reality of the JTPA system strongly affected the demonstration guidelines. It also describes how events have made the findings relevant to a larger policy community seeking ways to serve young dropouts, including young mothers receiving public assistance. The chapter ends by summarizing the service guidelines for the program, important differences among the 13 sites, and the topics addressed in the chapters that follow.

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10See Auspos, 1987; Auspos et al., 1989.
11Understanding more about the longer-term labor market value of a GED is a central goal of the four-year follow-up to be analyzed in the final report on the JOBSTART Demonstration. That report will examine longer-term program impacts.
I. The Youth Employment Problem and the Challenge of Serving Young School Dropouts

At the heart of the youth employment problem is "a small group of young people who remain out of work a large portion of the time." Overwhelmingly, they are from poor families, have dropped out of school, and lack the basic skills needed to succeed in the job market. Many are members of minority groups, some of whom confront the continuing existence of job discrimination. Young people who have dropped out of school and are unemployed — or only sporadically employed — may face lingering negative consequences.

A. A Closer Look at the Employment Problems of Young School Dropouts

In an economy increasingly reliant on service sector employment, it is virtually impossible for young school dropouts to find stable, well-paying jobs. Numerous national studies have found that dropouts are more likely than other young people to be poor academic achievers who had trouble fitting into a structured environment (the schools) and who lack self-esteem and — often — the habit of persevering in complex and challenging tasks. Employers value the very skills and qualities dropouts often lack: They want employees who have the basic academic skills and a readiness to learn and to set goals, communication skills, self-esteem, and the motivation to follow through. Many black and Hispanic youths face a further obstacle to employment. They are more likely than whites to drop out, but less likely to have access to informal networks that are useful for finding well-paying, steady work.

The resulting employment statistics highlight graphically the circumstances of young school dropouts. Those young people without four years of high school are much less likely to be working and more likely to be out of the labor force or (if in the labor force) unemployed. For example, as Table 1.1 shows, in 1990, 76 percent of all 16- to 24-year-olds without a high school diploma were unemployed.

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12 Clark and Summers, 1982, p. 200. See also Ellwood, 1982; Rees, 1986; Hahn and Lerman, 1985, p. 6. Using data from the late 1970s, the Congressional Budget Office estimated that about 10 percent of all youths accounted for 61 percent of all youth unemployment (U.S. Congress, 1982, p. 12).
13 Nationally, about 15 percent of all people aged 20 to 24 have not completed four years of high school. See U.S. Department of Education, 1988.
14 The research on which this section is based is summarized in Ekstrom et al., 1986.
16 The labor force is defined in official statistics as individuals who are employed or actively seeking employment. Unemployment statistics exclude people who are not in the labor force. Thus, many people who are not working are not counted as "unemployed" because they are not actively seeking work. Figures in this section are from U.S. Department of Labor, 1991.
TABLE 1.1

LABOR FORCE STATUS AND EMPLOYMENT RATES FOR 16- TO 24-YEAR-OLDS NOT ENROLLED IN SCHOOL IN 1990

<table>
<thead>
<tr>
<th>Subgroup</th>
<th>Percent Employed</th>
<th>Percent in Labor Force and Unemployed</th>
<th>Percent Not in Labor Force</th>
</tr>
</thead>
<tbody>
<tr>
<td>All 16- to 24-year-olds</td>
<td>71.2</td>
<td>9.0</td>
<td>19.8</td>
</tr>
<tr>
<td>High school graduates with some subsequent education</td>
<td>85.6</td>
<td>5.6</td>
<td>8.8</td>
</tr>
<tr>
<td>High school graduates with no subsequent education</td>
<td>75.6</td>
<td>8.7</td>
<td>15.7</td>
</tr>
<tr>
<td>School dropouts&lt;sup&gt;a&lt;/sup&gt;</td>
<td>50.6</td>
<td>12.6</td>
<td>36.8</td>
</tr>
<tr>
<td>White school dropouts</td>
<td>55.3</td>
<td>11.3</td>
<td>33.4</td>
</tr>
<tr>
<td>Black school dropouts</td>
<td>29.0</td>
<td>19.7</td>
<td>51.3</td>
</tr>
<tr>
<td>Hispanic school dropouts&lt;sup&gt;b&lt;/sup&gt;</td>
<td>57.1</td>
<td>9.3</td>
<td>33.6</td>
</tr>
</tbody>
</table>


NOTES: The labor force is defined as individuals who are employed or actively seeking employment.

- Rows may not total 100.0 percent because of rounding.
- Those who did not complete four years of high school but received a GED are defined as school dropouts.
- Persons of Hispanic origin may be of any race and thus are included in both the white and black school dropout subgroups.
olds with four years of high school and no subsequent schooling were working, while 16 percent were not in the labor force, and 9 percent were in the labor force but were unemployed. Among 16- to 24-year-olds without four years of high school, only 51 percent were working; 37 percent were outside the labor force; and 13 percent were in the labor force and unemployed. In this same age group, only 29 percent of blacks were working, while 51 percent were outside the labor force, and 20 percent were in the labor force and unemployed. Among the same-aged whites and Hispanics, about 55 to 57 percent were working, while 33 to 34 percent were not in the labor force, and about 9 to 11 percent were in the labor force and unemployed.

B. The Different Circumstances of Young Women and Young Men

In many obvious and important ways, the lives of young men and young women differ. This carries through to the experiences of young people who drop out of school. For young men, the last two decades have brought a deterioration in the ability to earn a living, with the decline in earnings being most serious for those with poor skills. For young women, single parenthood — and scant financial resources for handling it — is a common predicament.

During the 1970s and 1980s, young men without a high school diploma experienced a sharp decline in average earnings (after adjustments for inflation). For example, between 1973 and 1986, the average real earnings of 20- to 24-year-old men who had not graduated from high school and were not enrolled in school declined 42 percent. The picture for black men in this group was especially bleak, with their average real earnings dropping 61 percent. During the same period (which included serious recessions), the earnings of male high school graduates declined 28 percent, while the earnings of male college graduates declined only 6 percent.

The incidence of unwed teenage parenthood increased during the last two decades. While the number of births to American teenagers actually declined between 1970 and the late 1980s, the proportion of those births that were outside marriage rose dramatically, from about

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17 This employment-to-population ratio is calculated by dividing all those who were employed in 1990 by the total civilian noninstitutional population.
19 This bleak picture for black male school dropouts also appears in labor force statistics of the type cited earlier. Unpublished U.S. Department of Labor data for 1989 show that only 42 percent of 18- to 24-year-old black men without a high school diploma and not enrolled in school were employed, compared to 69 percent of comparable whites and 75 percent of comparable Hispanics.
30 percent (200,000) in 1970 to about 65 percent (300,000) in the late 1980s. Giving birth as a teenager typically coincides with a disruption in a mother's education and greatly increases her labor market difficulties. Unmarried women who become mothers in their teens and do not complete high school are at great risk of long-term welfare dependence and unemployment. In addition, recent evidence indicates that while teenage mothers can often recover partially from the detrimental effects of early childbearing, their children are more likely to suffer long-term social and educational disadvantages than are children born to older women.

C. Research on Effective Programs for Young School Dropouts

At the time the JOBSTART Demonstration began, program designers seeking insights from the previous research on youth programs found few solid success stories on which to base new efforts. Many programs had been tried, but nearly all evaluations found unfavorable results, were inconclusive, or were seriously flawed.

The one influential exception to this pattern was the residential Job Corps, which a study found to be effective in increasing the educational attainment and earnings of young dropouts. The residential Job Corps provides basic skills education, occupational training, life skills instruction, work experience, job placement assistance, health care, counseling, and other support services to youths who live at centers (often outside urban areas) and participate in

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20 In a national survey of high school students, almost one-fourth of all young women who dropped out of school reported that pregnancy was an important factor in their decision. See Ekstrom et al., 1986, for a discussion of this study.

21 See Ellwood, 1986. In 1989, only 42 percent of 18- to 24-year-old white women without four years of high school and not currently enrolled in school were employed; for blacks, the comparable figure was 22 percent and for Hispanics, 32 percent. The percentages for young school dropouts who were also mothers would be even lower, but were not calculated owing to sample size limitations in the Current Population Survey.


23 A common methodological problem was the absence of an appropriate group (one that was similar to participants but not served by the program) against which the experiences of the group that was served could be compared. Without such a comparison, evaluators frequently confused outcomes that followed a program with the real difference a program made — in the language of evaluation, its "impacts." For example, the outcomes of a program might include a post-program job placement rate of 50 percent. However, the employment rate for the appropriate comparison group might also be 50 percent, suggesting that the program had no impact on employment rates. See Betsey et al., 1985, and the discussion in Chapter 2 of this report for more on this issue.

24 See Mallar et al., 1982.
the program for up to two years. About 80 percent of Job Corps participants have not completed high school. The residential Job Corps, however, could not be offered to all dropouts: It was a relatively expensive program, accessible only to those willing and able to live away from home, requiring development of work experience positions with employers, and clearly not the answer for all disadvantaged youths.

Other efforts to directly connect young people with work — by either helping them look for work more effectively or providing subsidized work experience — were tested in demonstrations in the early 1980s. The evaluation of job search assistance for youths found that the program produced short-term increases in employment and earnings, but that in the long run participants were no better off than a comparison group.

As for the most common youth employment strategy — subsidized work experience — two evaluations failed to find any long-term impacts on educational attainment, employment, or earnings for dropouts. The National Supported Work Demonstration, managed by MDRC in the late 1970s, enrolled very disadvantaged young dropouts (many with a criminal record) in a 12- to 18-month program of paid work experience with gradually increasing job responsibilities. Program impacts for this group were not positive, even though the program proved successful for long-term welfare recipients. The Youth Incentive Entitlement Pilot Projects (YIEPP), which offered subsidized minimum-wage jobs to high school students and dropouts who returned to school, also was ineffective for dropouts. While the program did increase the employment and earnings of young people still in school, evaluators found that the offer did not induce dropouts to return to and remain in regular high school. Many of those who did return dropped out a second time, and there were no effects on educational attainment, employment, or earnings for dropouts.

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25 Some Job Corps Centers also operate a nonresidential program. These were not included in this earlier study. As previously noted, three nonresidential Job Corps programs were included in the JOBSTART Demonstration.

26 The demonstration assessed the effectiveness of a program providing job search assistance through simulated interviews, seminars on job-seeking techniques, and help in making contact with potential employers. See Public/Private Ventures, 1983. This finding differs from that of research on job search assistance programs for women receiving Aid to Families with Dependent Children (AFDC), which did find long-term employment and earnings impacts. See Gueron and Pauly, 1991.

27 See Maynard, 1980, on the findings for young dropouts and Manpower Demonstration Research Corporation, 1980, for the results for AFDC recipients.

Thus, the research record of the mid 1980s put the Job Corps in a special category as an effective program for raising the employment and earnings of young school dropouts.\textsuperscript{29} As would be expected in an intensive program of skills enhancement, Job Corps participants initially earned less than their comparison group counterparts.\textsuperscript{30} This pattern of earnings continued for a time after people left the program because those in the comparison group had accrued more work experience (an important source of skills-building), seniority and protection against layoffs, and promotions.\textsuperscript{31} The positive effects of enhanced human capital did not outweigh the negative effects of lost work experience until roughly 6 to 12 months after participation in the program, the first semi-annual period in which the average earnings of program participants exceeded those of their comparison group counterparts.

Not only did the research record find positive impacts for the residential Job Corps program, but it also found program benefits to be greater than program costs. Although program costs per participant were much higher than for most other programs (averaging about $5,000 in 1977), the estimated benefits to society exceeded the costs. Especially encouraging was the program's positive findings for young male dropouts, a group that had proven especially hard to serve in many previous programs.

Among the questions left open by the existing research, however, was whether the Job Corps approach could be successfully adapted to a new setting: operation as a nonresidential program by other agencies, which could not offer comprehensive support services and probably would not have the Job Corps' level of resources to devote to staff training, facilities, and curriculum. This shift to a nonresidential program is an important one, since some of the benefits of the traditional Job Corps program seem to stem from its residential nature. For example, in a residential program, it is much easier to provide an intensive program of support services (including counseling outside class time, positive peer support, recreational activities, and health care) than when young people are active in the program for at most eight hours a day. Furthermore, the decline in criminal activity and substance abuse observed for Job

\textsuperscript{29}The National Academy of Sciences, in its review of research on employment programs for young people, pointed out the distinction between the failure of research to provide adequate evidence of program effectiveness and the finding that a program is ineffective. Betsey et al., 1985.

\textsuperscript{30}This would occur because of participants' forgone earnings and lost opportunities for on-the-job skills enhancement while they were in the program, as discussed earlier in this chapter.

\textsuperscript{31}Evaluations of the Job Corps discussed the problem of the post-program transition back into the labor market that led to these initial negative impacts. See Mallar et al., 1978, 1980.

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Corps participants (especially during program participation) was partly attributable to their isolation in residential centers outside urban areas, or at least outside their previous neighborhood. While the residential nature of the program may have been a factor in its success, it did pose problems for some young people. For young mothers with child care responsibilities, the program demanded too much time away from home, and it did not prove effective for them. Also, many young men and women did not wish to leave their communities.

In 1983, the National Academy of Sciences convened a panel of experts on youth programs. Their assessment—summing up research findings—recommended further testing of the Job Corps approach in a nonresidential setting using random assignment to produce the most reliable findings. The JOBSTART Demonstration was, in part, a response to this call.

D. Growing Operational Experience in Programs for Young Dropouts

Program operators serving young, disadvantaged dropouts have identified a number of lessons that also informed the development of theJOBSTART Demonstration. When serving these youths, who often look back on past educational experiences with dissatisfaction, programs have to actively seek out participants rather than passively wait for volunteers to come forward. Program operators have also learned that achieving continued participation is not easy: Counseling and peer support have often proved useful in improving young people’s self-esteem and motivation, but even with these efforts, participation levels can be disappointing.

Finally, program operators have increasingly become sensitive to the multiple needs of clients. For some economically disadvantaged young people, a low level of basic skills prevents them from taking advantage of occupational training. In addition, as is the case for many

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32 Random assignment is generally recognized to be a reliable method of measuring the effectiveness of new programs of employment and training. As discussed later in this report, it was used in the JOBSTART Demonstration.

33 As the later discussion of the JOBSTART program model and its implementation will make clear, JOBSTART did not offer the same comprehensive list of support services available in the Job Corps. Nor did it use the same curriculum in education or training, except in the three sites that already operated a Job Corps nonresidential program. Nationally, about 10 percent of Job Corps participants are in nonresidential programs.

34 Many of these lessons are summarized in 70001 Training and Employment Institute, 1988, and Public/Private Ventures, 1990.
young people, economically disadvantaged youths may not be experienced in setting goals, making plans to achieve them, and following through with effective action. And finally, many young people have a pressing need for immediate income, for themselves or their families, so programs must help them find a means of financial support while they invest in their future by enhancing their skills.

II. The Changing Programmatic Context of the JOBSTART Demonstration

Because the JOBSTART Demonstration did not have special program funding to support site operations, it was shaped in important ways by the need to find a funder for local JOBSTART sites within existing programs. Since JTPA was the most likely source of local operational funds, its provisions and the local interpretation of them were central to the structure of the project. As the demonstration unfolded, JTPA changed in ways that make the JOBSTART findings even more relevant to the JTPA system. And a new audience for the demonstration findings has emerged with the passage of the Family Support Act of 1988, calling for expanded programs of education and employment services for young women who are school dropouts and receive welfare.

A. JTPA as a Crucial Funder of Program Services

The Job Training Partnership Act (JTPA) of 1982 is the federal government's major program for funding employment and training for economically disadvantaged adults and youths. JTPA distributes the majority of its funds to states which, in turn, pass along most of what they get to local administrative entities called service delivery areas (SDAs). The federal JTPA statute sets general rules for program eligibility and allowable types of activities. An SDA's staff and private industry council (PIC) — often operating like a board of directors for the agency — determine what specific types of services are to be offered, which groups will get priority for services, and how service providers under contract to the SDA are to be evaluated and paid.

The manner in which JTPA was initially implemented during the mid 1980s presented operational constraints that had to be taken account of in the design and implementation of the JOBSTART Demonstration:

35Most JTPA funds under Title IIA, the largest part of JTPA, are distributed to states, using a formula based on the states' number of unemployed and economically disadvantaged people.
Performance standards that made SDAs hesitant to serve youths with very poor skills. The incentives embedded in Title IIA, the largest part of JTPA and the one that finances most youth programs, made SDAs and JTPA-funded education and training agencies hesitant to enroll youths with very poor basic skills who were in need of intensive programs of education and training and support services. In designing and applying the performance measures used during the first five years of JTPA, federal, state, and local administrators focused on the proportion of participants placed in a job, their wages, and the cost per "success story." This encouraged SDAs and service providers to choose people who were more likely to achieve these successes at relatively modest costs. In seeking to serve school dropouts with poor skills in an intensive program, JOBSTART had to confront these incentives.

Performance contracts that inhibited combined education and training. Many SDAs wrote contracts with service providers that linked payment to the achievement of the events measured in performance standards. This made it difficult for those service providers that wished to serve youths with poor skills to be paid and complicated the administration of program models that required both education and training when a single agency could not provide both.

Severe restrictions on paid work experience. Experience with public service employment under JTPA's predecessor, the Comprehensive Employment and Training Act (CETA), led Congress to eliminate most forms of paid work experience when enacting JTPA.

Tight limitations on support services. The statute limited spending on support services (such as transportation and child care assistance) and

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36From the early 1980s until program year 1987 (ending in June 1988), the performance of SDAs serving adults was judged by the following standards: the percentage of adults who found a job; the percentage of adults who were receiving welfare when they enrolled in JTPA and who found a job; the average wage at placement in a job; and the program cost per person entering employment. For youths, the standards included the percentage who found employment and the "positive termination rate," defined as entering employment or other quantifiable measures of program success. These included attainment of employment competencies recognized by local private industry councils, completion of a level of schooling, enrollment in further non-Title IIA training, enlistment in the armed forces, return to school full time, or (for 14- and 15-year-olds) completion of specified program objectives. The youth standards included the cost per "positive termination." For each measure, the U.S. Department of Labor set national levels, which -- at state option -- could be adjusted to reflect the characteristics of those served and the conditions in the local labor market.

37Data from the mid 1980s illustrate the effects of these program priorities. During program years 1984 to 1986, when the JOBSTART Demonstration was beginning, young dropouts constituted only .4 percent of all Title IIA participants and 27 percent of all young participants. Among young dropouts who were served under Title IIA nationally in 1986, only 23 percent received basic education, a service likely to promote their long-term employability but unlikely to lead to immediate placement in a job.
needs-based cash payments and completely eliminated the payment of stipends to participants.

The early experience under JTPA prompted allegations that the program was making little real difference in participants' lives because service providers tended to enroll more job-ready applicants (a practice known as "creaming"). Further, sharp declines in the unemployment rate during the 1980s, which allowed many more job-ready individuals to find work, caused a rethinking about whether JTPA should continue to emphasize quick placement of participants in a job. Over time, Congress, the U.S. Department of Labor, and program operators have all expressed renewed interest in intensive programs of education and training targeted on more disadvantaged youths.

Responding to the early pattern of program operation, the U.S. Department of Labor changed its administrative practices and regulations and encouraged greater provision of intensive services for youths through a formal demonstration of such programs. In late 1987, the Department stated that "more emphasis must be placed on intensive investments in youth within JTPA" and recommended that "a significant portion of youths who participate... should receive competency-based instruction in either basic education or occupational skills." Soon thereafter, amendments to the regulations (effective in program year 1988) encouraged states to choose as the key standard for youth programs one that includes measures of increased educational and skills competencies. This increased the opportunities to include young dropouts with poor skills in JTPA. In addition, the Department in 1989 issued a request for proposals for the Youth Opportunities Unlimited (YOU) Demonstration and in 1990 selected seven sites to operate innovative programs.

38The U.S. Department of Labor has contracted for an independent study of JTPA to determine program impacts in a sample of 16 SDAs across the country. The study is being conducted by Abt Associates Inc., MDRC, NORC, and Lewin-ICF. See Doolittle and Traeger, 1990. The first impact results will be available in early 1992.
40Sites applying to participate were required to operate one of three programs: a work experience program modeled on Ventures in Community Improvement (VICI), which operated from 1978 to 1980; an alternative high school program modeled on High School Redirection in Brooklyn, New York; and a program of education and training modeled after JOBSTART. Seven sites were chosen for the three-year demonstration; some of them chose to operate a program modeled after JOBSTART. An implementation study is part of that demonstration, which is separate from the JOBSTART Demonstration reported on here.
Debate over further changes in JTPA also signaled greater interest in more intensive programs for young dropouts. An advisory committee to the U.S. Department of Labor recommended shifting more JTPA resources to harder-to-serve youths and ending restrictions on the support services these youths are likely to need. Although JTPA was not amended in the 1989-90 session of Congress, nearly all of the proposals would have abolished or downplayed cost standards for youth programs and required greater targeting of program resources on youths with multiple barriers to employment, a group likely to need more intensive services.

While this interest in hard-to-serve youths came too late to affect the implementation of the demonstration, it has heightened the importance of the project as an early test of a new direction for JTPA and has increased the chances that the JOBSTART program will be successfully replicated if the research findings are positive.

B. The Family Support Act

Passage of the Family Support Act of 1988 (FSA) signaled a growing emphasis on programs of education and employment services for AFDC recipients, making the JOBSTART findings relevant for organizations serving this group. The Job Opportunities and Basic Skills Training (JOBS) title of the Act expands the obligations of AFDC mothers to participate in activities intended to increase their employability, and of states and counties administering the AFDC program to offer more education and training than were typically offered under the predecessor Work Incentive (WIN) program. Especially relevant to JOBSTART is the fact that the JOBS legislation allows states to impose a participation obligation on AFDC parents under 20 years of age who lack a high school diploma or GED regardless of the age of their child. For this group, education is normally presumed to be the appropriate first activity. States and local service providers seeking to expand their offerings of education and training for young mothers are currently grappling with many of the issues addressed in JOBSTART.

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42While single parents with children under age three are normally exempt from participation in JOBS programs, this is not the case for custodial parents under age 20 who have not graduated from high school or received a GED.
III. The JOBSTART Demonstration

MDRC began the JOBSTART Demonstration in 1985 with two purposes: (1) to determine the operational feasibility within JTPA of an intensive program incorporating several of the key elements of the residential Job Corps, and (2) to rigorously test its effectiveness. Local and state JTPA agencies provided most of the operational funding for the JOBSTART sites, but the MDRC evaluation was funded by an unusual consortium consisting of the U.S. Department of Labor, the Rockefeller Foundation, the Ford Foundation, Charles Stewart Mott Foundation, the William and Flora Hewlett Foundation, the National Commission for Employment Policy, AT&T Foundation, Exxon Corporation, ARCO Foundation, Etna Foundation, the Chase Manhattan Bank, and Stuart Foundations. Funding from this consortium also enabled MDRC to award a modest $25,000 grant to each site.

This funding structure shaped the character of the demonstration at the local level in two important ways. First, the JOBSTART program operated within existing agencies and programs under the rules and performance standards of Title IIA of JTPA or, for the nonresidential Job Corps Centers, under Title IVB of JTPA. It proved a serious challenge for the non-Job Corps sites simultaneously to follow the demonstration guidelines, the rules of Title IIA of JTPA, and the provisions in their contracts with SDAs.

Second, without special funding, sites could not be expected to make major changes in their existing programs, limiting the extent to which the JOBSTART curriculum and instructional methods could be standardized. Instead, MDRC gave sites general guidelines for program operation specifying the type and duration of required components of the program (education, occupational training, job placement, and support services). Even within this flexible framework, some program operators faced major implementation challenges. Some of the sites normally offered only basic skills education or vocational training; the demonstration called for both, requiring them either to add a whole new kind of activity or to link up with other local agencies providing it. Some sites also had to adapt to a younger and less skilled student body than they normally served. The lack of special program funding also limited the extent to which non-Job Corps sites could offer the array of support services that were a part of the Job Corps program.

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43See Auspos, 1987, for a discussion of the origins of JOBSTART and its early implementation.
The demonstration was thus a hybrid: part evaluation of existing programs and part test of a new program. The basic program differed from site to site in myriad details, but the variety did permit a test of how a scaled-down Job Corps-type program could operate under existing rules in different kinds of established agencies. If the demonstration showed positive results, it would be easier to replicate the program widely.

A. The JOBSTART Sites

MDRC staff recruited sites each of which they thought could (1) meet the JOBSTART program guidelines (discussed below) with little or no technical assistance except on techniques of client outreach and retention, (2) assemble sufficient operational funding for the full array of JOBSTART services (a significant barrier, as discussed above), and (3) yield a target of 200 sample members. A total of 13 sites across the country, listed in Table 1.2, participated in the demonstration. All had experience running programs that included some or all of the components of the JOBSTART model or working with young dropouts. As noted earlier, most of the operating funds for the demonstration sites were provided through the regular JTPA system under Title IIA of the legislation. (The Job Corps, as also noted earlier, is separately funded and administered under Title IVB of JTPA, so the three Job Corps Centers in JOBSTART received funding through that title.)

While all agreed to implement the JOBSTART model, the sites brought to the demonstration varying operating experiences:

- **Sponsoring organizations.** The participating organizations included adult vocational schools, a community college, community-based organizations that focus on literacy development and GED preparation, community-based organizations that focus on occupational skills training, and the nonresidential components of three Job Corps Centers.

- **Prior service emphasis.** Some sites previously had offered only basic education and no skills training, while others had offered both but had emphasized skills training. The education-focused sites may have attracted youths who were primarily interested in basic education rather than skills training. Similarly, some sites with strong histories of skills training may have attracted youths who were primarily interested in learning the skills needed for a particular occupation rather than attaining a GED.

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44See Auspos, 1987, and Auspos et al., 1989, for a detailed discussion of the characteristics of the sites in the demonstration.

45The Job Corps Centers operated their usual nonresidential programs. Thus, they offered all JOBSTART services plus other Job Corps services that are not part of the JOBSTART model.
<table>
<thead>
<tr>
<th>Agency Name</th>
<th>Location</th>
<th>Type of Organization</th>
<th>Prior Service Emphasis (^a)</th>
<th>JOBSTART Program Structure (^b)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Allentown Youth Services Consortium (^c)</td>
<td>Buffalo, NY</td>
<td>Community-based</td>
<td>Education</td>
<td>Sequential/brokered</td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>Atlanta, GA</td>
<td>Job Corps Center</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>Basic Skills Academy (BSA)</td>
<td>New York, NY</td>
<td>Community-based</td>
<td>Education</td>
<td>Sequential/brokered</td>
</tr>
<tr>
<td>Capitol Region Education Council (CREC)</td>
<td>Hartford, CT</td>
<td>Community-based</td>
<td>Education</td>
<td>Sequential/brokered</td>
</tr>
<tr>
<td>Center for Employment Training (CET)</td>
<td>Sar Jose, CA</td>
<td>Community-based</td>
<td>Training with some education</td>
<td>Concurrent</td>
</tr>
<tr>
<td>Chicago Commons Association's Industrial and Business Training Programs</td>
<td>Chicago, IL</td>
<td>Community-based</td>
<td>Training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>Connelley Skill Learning Center</td>
<td>Pittsburgh, PA</td>
<td>Adult vocational school</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>East Los Angeles Skills Center</td>
<td>Monterey Park, CA</td>
<td>Adult vocational school</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>El Centro Community College Job Training Center (^d)</td>
<td>Dallas, TX</td>
<td>Community college</td>
<td>Education and training</td>
<td>Sequential/in-house</td>
</tr>
<tr>
<td>Emily Griffith Opportunity School (EGOS)</td>
<td>Denver, CO</td>
<td>Adult vocational school</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>Los Angeles Job Corps</td>
<td>Los Angeles, CA</td>
<td>Job Corps Center</td>
<td>Education and training</td>
<td>Sequential/in-house</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>Phoenix, AZ</td>
<td>Job Corps Center</td>
<td>Education and training</td>
<td>Concurrent</td>
</tr>
<tr>
<td>SER/Jobs for Progress</td>
<td>Corpus Christi, TX</td>
<td>Community-based</td>
<td>Training</td>
<td>Concurrent</td>
</tr>
</tbody>
</table>

(continued)
TABLE 1.2 (continued)

NOTES:  

a "Education" refers to basic education, often as preparation for the GED examination. "Training" refers to instruction in occupational skills needed for specific jobs.

b Concurrent programs offer basic education and occupational training concurrently from the beginning of participation. Sequential/in-house programs offer basic education followed by occupational training, with both components provided in-house by the agency. Sequential/brokered programs provide basic education and then serve as a broker for occupational training, referring participants to other agencies.

c In October 1990 this site was renamed The Clarkson Center, Inc.

d In September 1988 this site was renamed the Edmund J. Kahn Job Training Center.
This diverse background led the sites to implement the basic JOBSTART program components in several ways. Eight sites were able to offer both education and training in-house and chose to provide them concurrently, with participants active in both activities from the start. Two sites provided both activities in-house, but offered them in sequence, with skills training following education. The remaining three sites did not have the capacity to offer skills training and chose to provide basic education themselves and work with other agencies to place their participants in subsequent occupational training elsewhere.

B. The JOBSTART Program Guidelines

Drawing on the lessons of the Job Corps and applying them within the constraints of JTPA, the demonstration developed a new alternative program offered in a nonresidential setting with fewer support services available to participants. The key elements, shown in Table 1.3, included the core components of the Job Corps (basic education, occupational training, and job search) but a less extensive system of support services. In some respects (the definition of the target population and the requirement that certain activities be included), the program model was quite specific, while in others it allowed for considerable variation. The model set requirements as to the type and intensity of education and training services that were to be offered to participants, and it placed strong emphasis on the need for strategies to increase program retention. However, as mentioned earlier, sites were given a great deal of flexibility in implementing these core requirements.

1. Target group. Since the program was designed to reach a population largely unserved by existing programs, eligibility requirements were quite specific. Participation was limited to school dropouts who were between 17 and 21 years of age, did not have a high school diploma or GED, read below the eighth-grade level, and satisfied the JTPA definition of economically disadvantaged (defined primarily by household income or receipt of public assistance). Recognizing that program operators needed to meet enrollment and performance standard targets, however, the guidelines allowed for up to 20 percent of participants to read at or above the eighth-grade level.

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46Chapter 3 of this report provides more detail on the JOBSTART program model as implemented by the sites in the demonstration. A fuller description is given in Auspos et al., 1989.
### TABLE 1.3
**THE JOBSTART PROGRAM GUIDELINES**

<table>
<thead>
<tr>
<th>Target Population</th>
<th>To be eligible for JOBSTART, individuals had to be:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>• 17 to 21 years old</td>
</tr>
<tr>
<td></td>
<td>• school dropouts without a high school diploma or GED</td>
</tr>
<tr>
<td></td>
<td>• reading below the eighth-grade level on a standardized test(^a)</td>
</tr>
<tr>
<td></td>
<td>• economically disadvantaged(^b)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Basic Education</th>
<th>Sites were to implement a curriculum that:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>• was self-paced and competency-based</td>
</tr>
<tr>
<td></td>
<td>• was computer-managed and -assisted, if possible</td>
</tr>
<tr>
<td></td>
<td>• was a minimum of 200 hours in length</td>
</tr>
<tr>
<td></td>
<td>• focused on reading, communication, and basic computation skills</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Occupational Skills Training</th>
<th>Sites were to implement a curriculum that:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>• was in a classroom setting</td>
</tr>
<tr>
<td></td>
<td>• combined theory and hands-on experience</td>
</tr>
<tr>
<td></td>
<td>• prepared enrollees for jobs in high-demand occupations</td>
</tr>
<tr>
<td></td>
<td>• provided at least 500 hours of training</td>
</tr>
<tr>
<td></td>
<td>• had been developed with the assistance of the private sector to ensure that graduates would meet the entry-level requirements of local employers</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Training-Related Support Services</th>
<th>Services were to be tailored to individual needs and were to include, in addition to transportation and child care, some combination of the following:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>• work-readiness and life skills training</td>
</tr>
<tr>
<td></td>
<td>• personal and vocational counseling, mentoring, tutorial assistance, and referral to external support systems</td>
</tr>
<tr>
<td></td>
<td>• needs-based payments or incentive payments tied to length of stay, program attendance, or performance</td>
</tr>
</tbody>
</table>

| Job Development and Placement Assistance | JOBSTART operators and/or their subcontractors were to be responsible for assisting participants in finding training-related jobs |

**SOURCE:** Manpower Demonstration Research Corporation, 1985.

**NOTES:**

\(^a\)To help meet enrollment targets, each site was allowed to enroll individuals - up to 20 percent of its total JOBSTART enrollment - who read at or above the eighth-grade level.

\(^b\)To be eligible for JTPA services - economically disadvantaged by JOBSTART standards - a person must be receiving public assistance; have family income at or below the poverty line or 70 percent of the lowest living standard income level; be homeless, under the definition of federal statutes; or, in some cases, be a handicapped adult whose own income fits within the guidelines but whose family income exceeds it.
2. Education and training. The demonstration sought to test an intervention that would be relatively intensive and lengthy compared to the usual JTPA activities and that would address the multiple deficits in participants' skills. As a result, the program model required sites to offer a specified minimum amount of both basic education and occupational training to provide the young people with a real opportunity to enhance their skills. This combination of services, as noted earlier, differed from the usual situation under Title IIA of JTPA. The 200-hour minimum of education was based on an estimate of what would be needed to bring the basic skills of most participants reading below the eighth-grade level up to the point where they could qualify for a GED or enter occupational skills training. The 500 hours of training was a compromise between the very lengthy training that research suggested was useful and what was practical in most JTPA environments. Given the difficulty of keeping young people engaged in a program for an extended period and the competing demands on their time (including their need for income and their child care responsibilities), staff recognized that not all participants would complete these activities and that the total time in the program would be a year or less.

The two instructional components were structured in ways intended to make them appealing and accessible to young people who entered the program with widely varying levels of skills. For basic education, the guidelines required sites to offer instruction in reading, communication, and basic computational skills, using individualized curricula that allowed youths to proceed at their own pace toward required competency goals. The program model did not specify any particular curricula, though it did encourage — but not require — sites to offer computer-assisted instruction.

The occupational skills component required classroom rather than on-the-job training, in the belief that participants would benefit from the intensive, closely supervised instruction possible in a classroom setting. Again, no specific curriculum was required. Recognizing the advantages of applying learning to practical problems, however, the program model required that the training include a combination of theory and hands-on experience. Seeking to increase the chances of placement following training, the program model required that the training prepare participants for jobs in high-demand occupations and be developed in cooperation with local representatives of the private sector.

47Sites were also expected to supplement this individualized instruction with group activities.
3. **Support services.** Attracting and keeping disadvantaged youths in education and training programs is a challenging problem, and the sites were expected to assist participants with transportation and child care. They were also encouraged to develop a package of other support services to facilitate program participation; the Job Corps sites offered considerably more support services than did the others.⁴⁸

4. **Job placement assistance.** The guidelines required sites to identify possible training-related jobs for participants and to assist them in securing employment, but were not specific about how this should be done. All sites instructed the youths on work disciplines, employer expectations, and job search techniques, but the intensity of this effort ranged from informal guidance by counselors and other staff to more than 50 class hours in one site. Seven sites offered some form of work experience or internships (both paid and unpaid) to improve job skills. All sites provided assistance in seeking employment when the youths left the program, although in two of the three sequential/brokered sites (CREC in Hartford and BSA in New York City), the responsibility fell solely on the training provider. This arrangement for job search assistance proved a serious limitation since, as will be discussed in Chapter 3, many young people did not reach the training phase in sequential/brokered sites.

C. **Key Dimensions of Local Variation**

The previous report on the implementation of JOBSTART highlighted two dimensions of local variation as important influences on the program experience of the JOBSTART youths:

- **Concurrent versus sequential education and training.** Programs could offer youths basic education classes and vocational skills instruction at the same time (a concurrent model) or basic education before skills training (a sequential model).

- **In-house versus brokered services.** Programs could offer youths education and training at the same agency, or the agency providing basic education could serve as a broker, helping participants who were completing the education phase to find appropriate training at other institutions (sequential/brokered sites).

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⁴⁸Job Corps Centers offered health services, recreational activities, and on-site food service, and more intensive counseling and peer support than did most other sites.
In the period following the last JOBSTART report, research on other programs has also called attention to variations among sites offering concurrent programs. The Minority Female Single Parent (MFSP) Demonstration, funded by the Rockefeller Foundation, tested different models of education and training in four local agencies. The early evaluation reports based on one year of follow-up argue that the one program among the four in the study with positive impacts on employment and earnings (CET, operating in the San Francisco Bay area) achieved this result because of programmatic and organizational features that distinguished it from the remaining three. Specifically, researchers highlighted "the training design of the CET program—which emphasized training for all regardless of educational skill levels, offered remedial education within the context of job skill training, and accommodated trainees with diverse levels of educational skills." The hallmark of such an integrated program is a focus on vocational training, aided by the teaching of literacy and mathematics skills needed in the chosen occupational area (rather than general literacy and mathematics training). The U.S. armed forces have experimented with this approach in teaching military occupations to recruits with poor basic skills.

In practice, there is no clear distinction between integrated concurrent programs and other concurrent programs; they form a spectrum rather than falling into two neat categories. Among the JOBSTART sites, for example, CET/San Jose operated the most integrated program. Basic skills instruction was largely incorporated into the training curriculum, with the goal of supporting the learning of occupational skills. The challenge for this approach is to serve participants with very low levels of basic skills. Some people in this group will initially be unable to participate in serious vocational training, so program operators face a choice of

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49 See Gordon and Burghardt, 1990.
50 The CET site in the MFSP Demonstration enrolled minority female single parents, whose average age was 28, and served them in San Jose and several other East Bay communities. The San Jose program was also a site in JOBSTART, but only 10 mothers became part of the JOBSTART sample at CET/San Jose, and they were considerably younger than the MFSP sample.
51 Gordon and Burghardt, 1990, p. xxvi. The authors also cited a number of other factors that were unique to CET among the four sites and that they believe contributed to its large impacts, including its financial stability and experienced staff; integration of the MFSP program into an ongoing training operation with a similar mission; large scale, which allowed for training in a variety of occupations in demand in the local economy; attention paid to job placement; and availability of on-site child care. The remaining three sites in the MFSP Demonstration emphasized "the acquisition of basic skills before entry into job skill training" (p. xxvi) — that is, sequential programs in the terminology used in the present report.
52 This approach is sometimes called "functional context training." See Sticht, 1987.
offering them special help in literacy and mathematics or routing them to less demanding training courses. This trade-off is illustrated by the experiences of a second site, which operated a partially integrated program. Chicago Commons offered several training courses requiring technical knowledge and mathematics skills. Even after imposing entrance requirements among the most stringent in any JOBSTART site, Chicago Commons found that the skills deficiencies of some participants were too severe to be addressed within the integrated training context. Thus, the site also offered a separate basic education class. Other concurrent sites such as SER/Corpus Christi and Connelley in Pittsburgh operated separate education classes aimed at preparing people to pass the GED examination. These sites tried to coordinate the activities in education and training classes via conferences among the instructors and inclusion of basic skills instruction in some training classes.

The strong policy interest in these issues has led to a lively debate about the arguments for and against each approach.

1. Sequential versus concurrent versus integrated education and training. Proponents of sequential (as opposed to concurrent) programs argue that youths who are reading and computing at low levels get more out of training if their basic skills are improved before they enter occupational coursework. The youths, they maintain, will have more choices of training and can get more out of the instruction. Sequential programming also eases the burden of scheduling classes since students are freed from the pressure of simultaneously participating in two types of intensive coursework. Furthermore, their daily schedule can allow time for activities designed to address a variety of needs, such as life skills training, recreational activities, or part-time jobs.

Advocates of "nonintegrated" programs (both sequential and concurrent) believe that employers value workers with a broad range of basic skills, not just those needed to master a specific occupation. In this view, workers with these more general skills will learn new skills more quickly on the job, be more flexible and productive than other workers in the long run, and be able more easily to shift to a new employer or even a new occupation when the economy changes and their original job no longer exists.

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53Hahn and Lerman, 1985.
54Many existing occupational curricula require that entrants have tenth-grade reading and mathematics skills.
55See, for example, National Academy of Sciences, 1984; Johnson and Packer, 1987; National Association of Manufacturers, 1982.
Advocates of concurrent — and especially integrated — programs focus on a different issue: the difficulty of keeping disadvantaged young school dropouts in programs. Students may find the education phase of a sequential program too much like their past high school experience, which many did not like, and may leave the program before they get to occupational skills training. In this view, being able to combine basic education with skills training — which has a more obvious connection to the job market — makes the education component more appealing.\textsuperscript{56} It is argued, for example, that if students see that they need basic mathematics in order to make measurements for carpentry, they will be more motivated to solve addition and division problems.

Another argument for concurrent programming — and especially integrated programs — rests on a narrower view of the purpose of basic skills education for young dropouts. Its proponents hold that instruction in basic skills should focus on the particular skills needed for a job (the goal of most young people in the program) rather than on imparting general knowledge that is less directly relevant to the lives of the youths.\textsuperscript{57}

2. \textbf{Brokered versus in-house services}. Practically speaking, brokering may be the only way that small agencies specializing in one type of service can provide multi-component, comprehensive programs. None of the small, community-based education providers participating in the demonstration, for example, had the capability to develop on-site training facilities offering a variety of training options. Agencies with a limited number of training courses might also choose to broker training for some participants in order to increase the range or quality of training available to them.

Brokered programs increase the operational challenges for the program operator, however. There are potential difficulties, for example, in ensuring that participants in education will be accepted for training by other agencies, in scheduling the end of the education phase to coincide with a variety of different training schedules, and in monitoring

\textsuperscript{56}Mathematica Policy Research, 1988.

\textsuperscript{57}A final argument for concurrent programs rests on the particular features of most JTPA programs. Programs offering concurrent education and training tend to be shorter and do not involve a transition from education to training. If this transition also involves a shift from an education service provider to a second provider (for training), JTPA performance contracts may typically not allow final payment of the first provider unless participants receive a GED certificate, a milestone recognized under the JTPA performance standards. Since many youths with poor skills will not pass the GED test, this is a practical obstacle to sequential/brokered programs.
the progress of students referred to other agencies and the quality of the services provided to
them.

3. **A further key dimension of local variation.** JOBSTART sites operated in very
different labor markets. The unemployment rates in the sites' metropolitan areas varied from
a low of 3 percent in 1987 in Hartford, where CREC is located, to 12 percent in 1986 in
Corpus Christi, where SER operated. Youth unemployment rates varied from 6 percent in
1986 in Hartford to 27 percent in 1985 in New York City, where BSA was located.58

Later chapters of this report present a more in-depth look at JOBSTART implementation
and present generalizations about how local variation affected the way the program operated.
However, as discussed in more detail in Chapter 2, caution must be exercised in making cross-
site comparisons of program implementation and impacts. Since many features of programs
differ among the sites, it is very difficult to isolate the influence of a difference in one factor.59

**IV. The JOBSTART Evaluation and the Organization of This Report**

The evaluation of JOBSTART is divided into three main parts. The first dealt with the
sites' implementation of the program. *Launching JOBSTART*, the initial report on the
demonstration, discussed site selection and characteristics, the operation of the program within
JTPA, and early implementation experiences.60 A second report, *Implementing JOBSTART*,
completed the implementation analysis by describing the content of JOBSTART activities, the
participation patterns of the young people in the program, and operational lessons to be drawn
from the demonstration.61 These findings are summarized and updated in this report.

The second part of the evaluation is an analysis of program impacts; the early findings
on impacts form the core of this report. The research was designed to separate out the effects
of JOBSTART itself from events attributable to other factors (such as other services
participants were receiving and events in their lives outside the program). To accomplish this,

59The previous report (Auspos et al., 1989) also highlighted variations among the sites: whether
they were serving JOBSTART youths in mainstream adult classes or in separate classes for youths; whether
they offered computer-assisted instruction; and how they differed in scheduling (that is, the number of
hours a day devoted to various activities) and in the length of their courses. Since these did not appear
to have a major influence on program implementation or on participation by the youths, they were unlikely
to have affected impacts and are not emphasized in this report.
61Auspos et al., 1989.
all people who applied for JOBSTART and were found to be eligible were randomly assigned to either an experimental or a control group. Those in the experimental group were given access to the JOBSTART program services; those in the control group were not, although they could receive other services offered in their community. Since the youths were assigned at random to the two groups, they were similar except for the fact that only the experimental group could receive JOBSTART services.

Individuals in both groups were scheduled to be surveyed 12, 24, and approximately 48 months after being randomly assigned to their group. (The time frame for applying to JOBSTART varied from site to site but ranged overall from August 1985 through November 1987. Hence the fielding of each wave of the survey also extended over many months.) Using these surveys, the experiences of the two groups can be compared to estimate the effect of the program on educational attainment, employment, earnings, use of public benefits, and other outcomes.

The third part of the evaluation will assess the cost-effectiveness of the program. While the present report includes a discussion of the costs of JOBSTART, the final report, based on four years of follow-up, will summarize the findings on program costs and benefits.

As discussed above, this research design uses random assignment to provide reliable information on the central question — the impact of JOBSTART services — by comparing the experiences of two groups that were equivalent except for the experimental group's access to the program. It is important to understand that it was not designed to answer certain other questions with equal rigor. Most important, the study was not designed to rigorously compare the effectiveness of sequential versus concurrent or integrated programs. The young people were not randomly assigned to these different programmatic approaches. Instead, the sites offering these three approaches operated in different kinds of settings and local labor markets and served different types of youths with varying backgrounds and interests. Thus, the structure of the program was not the only difference among sites. Given these many differences, it is impossible to isolate the influence of one factor — such as concurrent versus sequential program structure — on program effectiveness.62

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62 To rigorously compare the impacts of different programmatic approaches, more than one approach would have to be operated in each site and youths would have to be randomly assigned to one of them. Even with this design, if the programs differed on several dimensions, it would still be impossible to isolate the effect of any one dimension. This type of research has rarely been undertaken. Examples (continued...)
Chapter 2 of this interim report on JOBSTART presents the key research questions and the research design used to address them. It also includes information showing that the sites did succeed in recruiting the young, disadvantaged target group of the demonstration. Chapter 3 discusses the implementation of the JOBSTART program, concluding that most youths in JOBSTART participated in education and training activities more than the typical participant in JTPA-funded programs and nearly as much as the typical Job Corps member. Here, as in all subsequent chapters, the report seeks to understand the overall results by examining whether and how JOBSTART operated differently for key subgroups of youths in the sample, with a particular focus on young men and young mothers. Chapter 4 examines educational outcomes, especially the degree of JOBSTART youths' participation in education and training compared to that of the control group, and whether JOBSTART led to increased attainment of a high school diploma or GED during the two years of follow-up. Chapter 5 examines the early indications of how this investment in "human capital" affected youths' employment, earnings, welfare receipt, and other outcomes. In Chapter 6, the analysis compares the experiences of youths in the 13 sites in the demonstration and — to the extent possible given the research design of the study — explores possible explanations for differences among sites.

62(...continued)

include MDRC's study of the impacts of job search alone versus job search plus community work experience in San Diego (Goldman et al., 1986) and Mathematica Policy Research's study of alternative reforms of the unemployment insurance system.
CHAPTER 2

STUDY DESIGN AND SAMPLE CHARACTERISTICS

This chapter presents the study design used to address the research questions posed in Chapter 1, and the characteristics of the research sample used in the analysis. Part I describes the research approach, with special emphasis on the random assignment research approach for assessing the difference the program makes in the lives of young people. Part II describes the research sample for this report.

I. An Overview of the Study Design

Although education and training services for young school dropouts are limited, some youths who entered JOBSTART would have gotten GEDs or high school diplomas, found jobs, increased their earnings, or gotten off welfare on their own even if they had not been in the program. As noted in Chapter 1, to isolate the impact of JOBSTART from other factors that may produce such outcomes, MDRC randomly assigned applicants to experimental and control groups. The two groups were similar except that only the experimental group could receive JOBSTART services. Comparison of the two groups' experiences during the two years after random assignment (the follow-up information available for this report) provided a reliable estimate of the difference the program made during an early post-program period.¹

¹Sources of the data for the evaluation are discussed in detail in Appendix A. They include enrollment forms completed just prior to random assignment; a management information system (MIS) that provided data on participation in the program; results from the Test of Adult Basic Education (TABE) administered to members of the experimental group; follow-up surveys (for this report) conducted 12 and 24 months after random assignment and (for the final report) approximately 42 months after random assignment; program cost data from a variety of sources; and qualitative data based on interviews with the program staff, field observations of program operations, and focus group discussions with participants.
A. How Random Assignment Was Carried Out

Figure 2.1 shows the steps in the intake and random assignment process. Youths who expressed an interest in program services entered the program through a process that took from one day to one month (10 days on average), depending on the site. Most of the steps were part of the usual JTPA Title IIA (or, in Atlanta, Los Angeles, and Phoenix, the Job Corps) intake procedure; in most sites, only the reading test and random assignment were added for the JOBSTART Demonstration.

The order of steps varied from site to site, as did the division of responsibility between the program operator and the local service delivery area (SDA). The process included:

- **Client recruitment.** JOBSTART was voluntary, so the program operator and, in some cases, the SDA actively recruited youths to apply, using a variety of techniques to meet their enrollment goals. Program staff approached potential recruits through media announcements; mailings to dropouts and welfare recipients; and outreach visits to schools, parks, and other youth gathering places. They distributed posters and fliers advertising program services and sought referrals of eligible youths from JTPA, community organizations, schools, and social service agencies. Recruitment activities frequently took staff beyond the boundaries of the office and the nine-to-five workday. Recruitment through public school referrals or outreach was productive in school-based JOBSTART programs.

- **Informational interview.** In a brief interview, JOBSTART staff explained to potential applicants the program's services and obligations and, often, the random assignment procedures. Some sites also regularly included a tour of their facilities to help recruits understand program services, opportunities, and demands.

- **Assessment.** Program staff assessed whether applicants met the age (17 to 21), educational status (school dropout), and income requirements for JOBSTART. They also ascertained the youths' support service needs and appropriateness for the program, screening out those with problems the program was not equipped to handle. The assessment process was relatively extensive at the Job Corps sites, which had the broadest array of support services. Job Corps staff assessed recruits for emotional problems, drug and alcohol abuse, trouble with the law, unstable living situations, health problems, and motivation. Other sites screened mostly to identify

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2See Auspos et al., 1989, for the details.
3Sites varied greatly in the amount of initial assessment they conducted before allowing entry into the program. There was also wide variation in state and local interpretation of the documentation needed to establish eligibility for JTPA Title IIA programs.
FIGURE 2.1
THE JOBSTART EVALUATION DESIGN
AND SAMPLE FLOW

Client Recruitment

Informational Interview

Assessment

Reading Test

JTPA/Job Corps Certification

Informed Consent and Enrollment Forms

Random Assignment

Drop-off

Experiments

Concurrent Sites

Basic Education and Occupational Skills Training

Job Development and Placement Assistance

Sequential Sites

Basic Education

Occupational Skills Training

Job Development and Placement Assistance

Controls

No JOBSTART Services but Eligible for Other Services

SOURCE: Adapted from Auspos et al., 1989.

NOTE: Support services such as child care and transportation were offered at both concurrent and sequential sites.
youths who were likely to prove dangerous or disruptive, such as those with evident drug or alcohol problems.

- **Reading test.** Most program operators tested the reading level of recruits early in the intake process to determine that applicants read below the eighth-grade level, as required by JOBSTART eligibility criteria. Four sites (the three Job Corps Centers and CET/San Jose) delayed testing until later in the program, limiting their testing to participants. As noted earlier, sites were permitted to enroll up to 20 percent of their recruits with higher reading scores to help meet enrollment goals. Some sites also set a lower limit or floor—a fourth-, fifth-, or sixth-grade reading level. These program operators felt that the youths would need to read at least at these levels in order to benefit from the education and training services that were available locally.

- **JTPA Title IIA/Job Corps certification.** Recruits had to prove that they fulfilled eligibility criteria for JTPA Title IIA-funded services. At the Job Corps sites, recruits also had to meet Job Corps eligibility criteria. At all the sites, certification of eligibility required proof of residency, age, and economic disadvantage. SDAs at most sites required applicants to provide supporting documentation of all aspects of JTPA Title IIA eligibility for approval of enrollment into JOBSTART. Local regulations and practices affecting the certification process strongly influenced the speed and ease of certification. JTPA Title IIA certification procedures were cited by program operators in six sites as a major bottleneck in the intake and enrollment process.

- **Informed consent form, enrollment form, and random assignment.** After staff described the random assignment process, the applicant signed an informed consent form, agreeing to accept the results of random assignment and to cooperate in follow-up survey interviews. Program or SDA staff then filled out the enrollment form, using information provided by the applicant. Staff then telephoned MDRC, where random assignment was made. Youths entering the experimental group were told to report to classes or, in some sites, to an orientation session. Program staff contacted experimentalists who did not appear for program activities, encouraging them to participate and assisting them with needed support services. Applicants assigned to the control group were reminded that they were part of the research project and would be contacted later. They were also told that they could seek services elsewhere on their own.
A total of 2,312 people were randomly assigned: 1,163 to the experimental group and 1,149 to the control group. Sites conducted random assignment over varying periods of time. Connelley Skill Learning Center enrolled the first sample members in August 1985, and the Los Angeles Job Corps enrolled the last sample members in November 1987. Open-entry/open-exit sites continuously recruited applicants to maintain enrollment levels, while sites operating fixed-cycle programs—such as Connelley in Pittsburgh, Chicago Commons, and SER/Corpus Christi—intensified recruitment efforts before the start of classes.

Overall, sites reported that about 89 percent of the youths in the experimental group participated to some extent in JOBSTART. The percentage participating did vary among the sites, from a high of 100 percent at Allentown in Buffalo and El Centro in Dallas to a low of 64 percent at CET/San Jose. (Site-specific participation information is presented in Chapter 3.) Four factors influenced the percentage of experimentals reported to be active in the program:

- **Length of the intake process.** The process of selection into the JOBSTART Demonstration took a relatively short time in many sites, often less than a week. However, at a few sites, the extended checks of eligibility (most important in the Job Corps sites) meant that intake lasted much longer, and in the process some youths who were eventually assigned to the experimental group found other program options or lost interest.

- **Open-entry/open-exit versus fixed-cycle scheduling.** Open-entry programs allow young people to enter and finish at any time, while other programs operate on fixed schedules of class cycles. Youths assigned to the experimental group in fixed-cycle sites might face delays in program start-up, resulting in lower participation rates.

- **Start-up or scheduling problems.** Some sites had unexpected problems getting youths into services. The most notable example was the experience of the early entrants at CET/San Jose, where program slots were not available for up to a month after random assignment because of funding cuts. This delay contributed to this site's experimentals having the lowest rate of participation in JOBSTART services.

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4All but one of the 2,312 youths who were randomly assigned filled out enrollment forms providing pre-program baseline data on age, sex, prior employment, extent of schooling, and other characteristics used in impact calculations and to define key groups within the full sample.

5Sites operating open-entry/open-exit programs included Allentown in Buffalo, the Atlanta Job Corps, CET/San Jose, the East Los Angeles Skills Center, El Centro in Dallas, the Los Angeles Job Corps, and the Phoenix Job Corps. EGOS in Denver offered classes on a semester schedule but allowed entry whenever classes were in session.
• Differences in sites' attendance reporting. The program elements counted in participation in all sites included education, training, and other activities such as life skills training, work experience, and — in the Job Corps sites — a lengthy orientation. Participation in an extended assessment of training interests at CET/San Jose was not included in reported hours. Therefore, if youths attended this assessment and nothing else, their reported hours were zero and they were counted as nonparticipants. This could have affected CET/San Jose's participation rate and reported hours in activities.

B. The Research Samples Used in This Report

Follow-up surveys at 12 and 24 months after random assignment gathered data on outcomes such as participation in education and training programs, educational attainment, employment, earnings, and use of public benefit programs. Of the 2,311 youths in the full research sample, 1,839, or 80 percent, provided 24 months of survey follow-up data and constitute the "impact sample" analyzed in this report in Chapters 4 through 6. The 949 experimentals in this sample are used to examine the implementation of JOBSTART, in Chapter 3, where issues such as participation rates in JOBSTART and its components, and hours and duration of participation, are examined.

C. Key Methodological Issues for the Impact Analysis

For this study to produce unbiased estimates of program impacts, several conditions must be met. These are addressed in the following questions:

1. Did random assignment lead to a group of experimentals with the same measured pre-program characteristics as the controls? Random assignment — properly implemented — creates a group of JOBSTART controls with the same pre-program characteristics as JOBSTART experimentals, so that observed differences between

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6 As noted in Appendix A, information on participation in JOBSTART was provided by the sites as part of a special management information system (MIS) created for the demonstration. The follow-up surveys collected information on participation in all other education, training, and employment programs for both experimentals and controls.

7 Most responded to both the 12- and 24-month follow-up surveys (1,604, or 87 percent of responders), while the remainder (235 people) responded to a special combination survey covering the entire 24 months, which was fielded for youths who did not respond to the 12-month survey but were located at 24 months.

8 For a fuller discussion, see Appendix B.
experimentals and controls in post-random assignment behavior provide unbiased, accurate estimates of program impacts. The information presented in Appendix B (Table B.1) for the 2,311 people randomly assigned shows that there were virtually no measured differences in characteristics between experimentals and controls.

2. Do experimentals and controls in the impact sample for this report (that is, those with 24 months of survey follow-up) have the same measured pre-program characteristics? Appendix Table B.2 shows that the 1,839 experimentals and controls in the impact sample are virtually identical in average measured characteristics.

3. Are those 1,839 sample members with 24 months of survey data representative of the entire JOBSTART sample of 2,311? Twenty-four months of survey data are available for nearly 80 percent of all the youths who were randomly assigned. Appendix Table B.3 shows that there are some statistically significant differences between those who responded to the surveys and nonresponders. Responders were more likely to be experimentals: 82 percent of experimentals provided 24 months of survey follow-up information compared to 78 percent of controls. Responders were also more likely to have entered the sample at the Allentown in Buffalo site, to have had no criminal convictions between age 16 and baseline, and to have lived with both parents at age 14.

When nonresponse is randomly distributed among members of both the experimental and control groups, it is troublesome only because it reduces the sample size and thus the statistical power to find impacts of a given size. However, when nonresponse is greater among one research group (such as controls) or among members of either research group who have certain characteristics (such as those who, at age 14, lived with their parents), impacts may be biased slightly unless they are corrected for nonresponse.

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9 This condition is known as the "internal validity" of the estimate.
10 The only difference that was statistically significant at the 5 percent level was that experimentals in the sample were slightly more likely than controls to be a part of an AFDC case headed by another member of their household. On a site-by-site basis, the 24-month JOBSTART impact sample consists of 26 separate groups of experimentals and controls. If experimentals in any site are compared with controls in that site, the internal validity of site impacts may be assessed. As would be expected in 13 relatively small subsamples of the full 24-month impact sample of 1,839, there are a few experimental-control differences in demographic characteristics within individual sites.
11 Randomly distributed nonresponse does not alter the expected values of adjusted mean outcomes, and thus does not bias impacts.
The impacts presented in this report do not include any corrections for the differences in survey responders and nonresponders just described. The success of attempts to implement such corrections is uncertain, and the differential response rates found do not seem large enough to warrant such measures, which could introduce biases of their own. The high overall response rate of 80 percent makes findings from the 24-month impact sample representative of a very broad group of the full sample.

4. Did most experimentals receive JOBSTART services, and did relatively few controls receive them or any equivalent services? For this condition to be met, experimentals must participate in JOBSTART, and controls must be excluded from JOBSTART and not find equivalent services elsewhere in their community. As discussed above, nearly 90 percent of experimentals were active in JOBSTART. For controls, random assignment procedures were followed, and virtually no controls were served in JOBSTART programs. In addition, Chapter 4’s analysis of the receipt of education and training services from all sources indicates that controls did not find an equivalent level of services elsewhere. For example, in the first year after random assignment, 91 percent of experimentals and 29 percent of controls participated in some type of education or training activity. As this and the other measures used in Chapter 4 indicate, experimentals did receive a noticeably greater total amount of employment and training services. But it is important to keep in mind that controls were not an unserved group; many received substantial services from sources other than the JOBSTART programs. The impact findings presented in Chapters 4 through 6 of this report, therefore, should be interpreted as measuring the incremental impact of the services received by experimentals above the level of services received by controls.

5. Do the impacts per person assigned to the experimental group differ greatly from the impacts per person participating in JOBSTART? Some of those who were randomly assigned to the experimental group (the group given access to the JOBSTART program) never participated. However, they were still included as part of the experimental

The most flexible correction for nonresponse is incorporation of an additional equation for survey response into a two-equation system with the impact equation.
group when average impacts were calculated, somewhat "watering down" the impacts.\textsuperscript{13} Fortunately, the percentage of nonparticipants was small (only 11 percent of the 949 experimentals in the impact sample), so including them "diluted" the impacts only slightly. In other words, while the impacts refer to all surveyed experimentals (nonparticipants as well as participants), they would be only slightly higher if they were adjusted to apply to surveyed participants only.\textsuperscript{14}

II. The Characteristics of the JOBSTART Youths

Examining the pre-program experiences and characteristics of the young people in the JOBSTART sample is important for three reasons. First, it shows whether the sites in the demonstration succeeded in enrolling economically disadvantaged young people with poor skills who were the target group for the demonstration. Second, it permits a comparison of the JOBSTART youths with those served by other important employment and training programs. Third, much of the analysis in this report moves beyond results for the full sample of JOBSTART youths to examine whether and how the program worked differently for subgroups of young people (especially young males, mothers, and other women), and understanding the pre-program characteristics of these groups is the first step in this analysis.

The third point is important because groups defined by a single characteristic (such as gender, age, prior employment, or the type of site to which they applied) may vary on other characteristics as well. Young women in the sample, for example, may have had less prior employment experience and more prior public assistance receipt than did the men in the sample. Sites offering education followed by training at another agency may attract very different applicants than those known for their training courses. Understanding the combination of characteristics associated with subgroups such as men or women or types of sites helps prevent misinterpretations of any observed differences in program participation and

\textsuperscript{13}If the nonparticipants had not been counted, the experimental group would no longer have been truly comparable to the control group. Including them in the impact calculations was designed to avoid a form of "selection bias" – in this case, caused by those who had "selected themselves" out of their chance to join the JOBSTART program or were discouraged by program staff.

\textsuperscript{14}See Appendix B for details on such adjustments. In some sites, nonparticipation rates were considerably higher than the 11 percent for the entire impact sample, so the difference between impacts per experimental and per participant is greater.
effectiveness. With this goal of the report in mind, Part II of this chapter summarizes the characteristics of the youths at each site and of key subgroups of young people that will be examined throughout the remaining chapters.15

A. The Characteristics of the Impact Sample of JOBSTART Youths

Table 2.1 provides detailed background information on the impact sample of JOBSTART youths and shows that the sites in the demonstration succeeded in recruiting the intended target group. The column labeled "all sites" shows the characteristics of the entire impact sample; the remaining columns are discussed in Part IIB of this chapter. The sample is made up of slightly more women than men; most of the sample are members of minority groups and are unmarried; nearly three-fourths are under 20 years of age; slightly fewer than half did not work during the year prior to random assignment; and about 60 percent left school before the eleventh grade.16

JOBSTART participants appear to have been more disadvantaged than the majority of youths served nationwide by JTPA Title IIA programs during the period JOBSTART was in operation. In the effort to serve those youths at risk of chronic unemployment, JOBSTART worked exclusively with dropouts, a segment of the youth population that makes up a relatively small part of JTPA Title IIA enrollees. Even when the comparison of participants is limited to young dropouts, it appears that JOBSTART reached a more disadvantaged population than did most JTPA Title IIA-funded programs.17

15More detailed comparisons are included in Appendix C.
16The only real divergence from the intended target group occurred because a slightly higher than planned percentage of the youths read at the eighth-grade level or above. This happened because of the educational testing practices of some sites and is not shown in Table 2.1 because of inconsistent data among the sites. Some sites did not administer the reading test as part of the initial assessment of suitability for JOBSTART, which occurred before random assignment.
17Approximately 56 percent of JOBSTART participants were receiving some form of public assistance at the time they entered the program, compared to 39 percent of young dropouts served by JTPA Title IIA programs. Moreover, the proportion of JOBSTART participants who received AFDC (38 percent) was much higher than that of young dropouts in other JTPA Title IIA programs (21 percent). This higher rate of welfare receipt partly reflects the fact that a greater proportion of JOBSTART participants were young women (53 percent) compared to the dropout group participating in other JTPA Title IIA programs (45 percent females). Also, minorities were much more heavily represented in JOBSTART than in JTPA Title IIA-funded services for young dropouts nationally. Hispanic dropouts constituted 44 percent of JOBSTART participants but only 14 percent of JTPA Title IIA dropouts, and JOBSTART served proportionally more black dropouts (46 percent) than did other JTPA Title IIA programs (34 percent).
### TABLE 2.1
CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY SITE

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>All 13 Sites</th>
<th>Atlanta Job Corps</th>
<th>CET/ San Jose</th>
<th>Chicago Commons</th>
<th>Connelley (Pittsburgh)</th>
<th>East LA Skills Center</th>
<th>EGOS (Denver)</th>
<th>Phoenix Job Corps</th>
<th>SER/ Corpus Christi</th>
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<tbody>
<tr>
<td>Gender</td>
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<td>43.2%</td>
<td>53.3%</td>
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<td>64.5%</td>
<td>51.5%</td>
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<tr>
<td><strong>Employed within past year</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>47.3***</td>
<td>45.2</td>
<td>75.2</td>
</tr>
<tr>
<td>Yes</td>
<td>52.7</td>
<td>54.8</td>
<td>24.8</td>
</tr>
<tr>
<td><strong>Prior employment, by gender</strong></td>
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<tr>
<td>Women employed within past year</td>
<td></td>
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</tr>
<tr>
<td>No</td>
<td>29.7***</td>
<td>27.7</td>
<td>47.2</td>
</tr>
<tr>
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<td>12.4</td>
</tr>
<tr>
<td>Men employed within past year</td>
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<tr>
<td>No</td>
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<td>17.4</td>
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<td>29.8</td>
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<tr>
<td><strong>Left school in grade 11 or 12</strong></td>
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<tr>
<td>No</td>
<td>58.6***</td>
<td>67.7</td>
<td>41.3</td>
</tr>
<tr>
<td>Yes</td>
<td>41.4</td>
<td>32.3</td>
<td>58.7</td>
</tr>
<tr>
<td><strong>Sample size</strong></td>
<td>1,839</td>
<td>155</td>
<td>218</td>
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</tbody>
</table>

(continued)
| Characteristic and Subgroups | Concurrent |  |  |  |  |  |  |  |
|-----------------------------|------------|---|---|---|---|---|---|
|                             | All Sites  | Atlanta Job Corps | CET/ San Jose | Chicago Commons | Connelley (Pittsburgh) | LA Skills Center | EGOS (Denver) |
| Received occupational training within past year |  |  |  |  |  |  |  |
| No                          | 83.1***    | 65.6% | 90.1% | 90.5% | 66.8% | 86.0% | 91.0% |
| Yes                         | 16.9       | 34.4% | 9.9%  | 9.5%  | 33.2% | 14.0% | 8.2%  |
| Age                         |  |  |  |  |  |  |  |
| 16-19                       | 73.9***    | 77.0% | 78.9% | 47.3% | 54.9% | 78.0% | 76.5% |
| 20 or 21                    | 26.1       | 23.0% | 21.1% | 52.7% | 45.1% | 22.0% | 23.5% |
| Marital status              |  |  |  |  |  |  |  |
| Ever married                | 9.5***     | 6.6%  | 12.5% | 1.4%  | 3.8%  | 4.0%  | 7.1%  |
| Never married               | 90.5       | 93.4% | 87.5% | 98.6% | 96.2% | 96.0% | 92.9% |
| Living in own household or with boy/girlfriend |  |  |  |  |  |  |  |
| No                          | 81.6***    | 88.5% | 87.5% | 78.4% | 75.5% | 90.0% | 77.0% |
| Yes                         | 18.4       | 11.5% | 12.5% | 21.6% | 24.5% | 10.0% | 23.0% |
| Own AFDC case or receiving General Assistance |  |  |  |  |  |  |  |
| No                          | 73.1***    | 67.2% | 90.8% | 47.3% | 56.5% | 76.0% | 73.2% |
| Yes                         | 26.9       | 32.8% | 9.2%  | 52.7% | 43.5% | 24.0% | 26.8% |
| Own AFDC case               |  |  |  |  |  |  |  |
| No                          | 78.6***    | 78.7% | 94.7% | 73.0% | 68.5% | 79.0% | 74.9% |
| Yes                         | 21.4       | 21.3% | 5.3%  | 27.0% | 31.5% | 21.0% | 25.1% |
| Receiving Food Stamps       |  |  |  |  |  |  |  |
| No                          | 62.2***    | 63.9% | 90.1% | 39.2% | 25.5% | 68.0% | 50.8% |
| Yes                         | 37.8       | 36.1% | 9.9%  | 60.8% | 74.5% | 32.0% | 41.5% |
| Arrested since age 16       |  |  |  |  |  |  |  |
| No                          | 85.2***    | 88.5% | 76.3% | 81.1% | 89.7% | 84.0% | 83.1% |
| Yes                         | 14.8       | 11.5% | 23.7% | 10.9% | 10.3% | 16.0% | 16.9% |
| Lived with both parents at age 14 |  |  |  |  |  |  |  |
| No                          | 65.1***    | 75.4% | 52.0% | 74.3% | 79.9% | 54.0% | 59.6% |
| Yes                         | 34.9       | 24.6% | 48.0% | 25.7% | 20.1% | 46.0% | 40.4% |
| Sample size                 | 1,839      | 61   | 152  | 74   | 184  | 100  | 183  | 130  | 236 |
### TABLE 2.1 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>All 13 Sites</th>
<th>Sequential/In-House EL Centro (Dallas)</th>
<th>Sequential/In-House LA Job Corps</th>
<th>Sequential/Brokered Allen-town BSA (NYC)</th>
<th>Sequential/Brokered CREC (Hartford)</th>
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<td>Received occupational training within past year</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>83.1***</td>
<td>89.0%</td>
<td>91.7%</td>
<td>82.9%</td>
<td>79.8%</td>
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<tr>
<td>Yes</td>
<td>16.9</td>
<td>11.0</td>
<td>8.3</td>
<td>17.1</td>
<td>20.2</td>
</tr>
<tr>
<td>Age</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>16-19</td>
<td>73.9***</td>
<td>83.9</td>
<td>77.5</td>
<td>73.6</td>
<td>73.9</td>
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<tr>
<td>20 or 21</td>
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<td>26.1</td>
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<td>Ever married</td>
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<td>11.6</td>
<td>5.0</td>
<td>4.3</td>
<td>1.7</td>
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<tr>
<td>Never married</td>
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<td>95.0</td>
<td>95.7</td>
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<tr>
<td>No</td>
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<td>86.7</td>
<td>59.3</td>
<td>95.0</td>
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<tr>
<td>No</td>
<td>73.1***</td>
<td>84.5</td>
<td>67.0</td>
<td>52.1</td>
<td>77.3</td>
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<tr>
<td>Yes</td>
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<td>15.5</td>
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<tr>
<td>Own AFDC case</td>
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<td></td>
</tr>
<tr>
<td>No</td>
<td>78.6***</td>
<td>84.5</td>
<td>69.3</td>
<td>67.9</td>
<td>83.2</td>
</tr>
<tr>
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<td>15.5</td>
<td>30.7</td>
<td>32.1</td>
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<td>Receiving Food Stamps</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>62.2***</td>
<td>85.8</td>
<td>69.7</td>
<td>30.0</td>
<td>62.2</td>
</tr>
<tr>
<td>Yes</td>
<td>37.8</td>
<td>14.2</td>
<td>30.3</td>
<td>70.0</td>
<td>37.8</td>
</tr>
<tr>
<td>Arrested since age 16</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>85.2***</td>
<td>90.3</td>
<td>90.4</td>
<td>87.1</td>
<td>89.9</td>
</tr>
<tr>
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<td>14.8</td>
<td>9.7</td>
<td>9.6</td>
<td>12.9</td>
<td>10.1</td>
</tr>
<tr>
<td>Lived with both parents at age 14</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>65.1***</td>
<td>71.6</td>
<td>75.2</td>
<td>80.7</td>
<td>70.6</td>
</tr>
<tr>
<td>Yes</td>
<td>34.9</td>
<td>28.4</td>
<td>24.8</td>
<td>19.3</td>
<td>29.4</td>
</tr>
<tr>
<td>Sample size</td>
<td>1,839</td>
<td>155</td>
<td>218</td>
<td>140</td>
<td>119</td>
</tr>
</tbody>
</table>
TABLE 2.1 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires. Distributions may not total 100.0 percent because of rounding. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
Nationwide, 80 percent of Job Corpsmembers were school dropouts in program year 1986, when the JOBSTART Demonstration was in operation, but their other characteristics suggest greater barriers to employment than the JOBSTART youths faced. Job Corpsmembers tended to be younger than JOBSTART participants: 42 percent were age 16 or under in 1986 compared to 29 percent in JOBSTART. Sixty-one percent read at the sixth-grade level or below at entry into the Job Corps compared to 52 percent in JOBSTART. On the other hand, a higher proportion of JOBSTART participants were receiving public assistance and were members of minority groups than were Job Corpsmembers. The residential character of the Job Corps program also introduces another difference: All residential Corpsmembers are willing and able to live away from home, but an unknown — though probably large — portion of JOBSTART members would not fall into this category.

These comparisons suggest that JOBSTART sites did succeed in attracting disadvantaged, young school dropouts, as intended in the demonstration. However, these young people were not among the most disadvantaged youths: Relatively few reported at program intake that they had criminal arrests; most were not teenage parents; and about half had worked during the year before random assignment. In summary, the JOBSTART youths probably fell between the typical JTPA and Job Corps participant in initial skills levels and job readiness.

B. Site Differences in Sample Characteristics

When individuals with certain characteristics are largely concentrated in one or a few sites, the influences of their individual characteristics on program implementation and impacts are "confounded" with the influence of site characteristics. This is virtually a non-issue with

18 In JOBSTART, JTPA performance standards and practices led some JTPA Title IIA-funded sites to exclude youths with very low reading scores; the Job Corps sites in JOBSTART appeared to include a higher proportion of youths with very low reading scores than did other sites.

19 The difficulties encountered in implementing an unsuccessful random assignment study of the residential versus nonresidential Job Corps illustrate the importance of this difference. The study originally assumed that a substantial portion of the Job Corps applicant pool would be indifferent as to whether they got into a residential or nonresidential program, and the study proposed to randomly assign members of this group to the two program types. This "indifferent" group turned out to be too small a portion of applicants for the study to proceed.

20 It is very likely that youths underreported past arrests, since they were asked about this at program intake and may have assumed that a positive response would lower their chances of getting into the program. Also, they may not have wanted to provide this information to staff, whom they did not yet know. In addition, only 106 men (or 12 percent of men) report being a father.
regard to the proportion of experimentals and controls at the sites: All sites had approximately equal proportions of the two groups. Not so with ethnicity, in particular: The proportion of black sample members ranged from zero at the East Los Angeles Skills Center to 97 percent at the Atlanta Job Corps, as shown in the individual site columns of Table 2.1. Thus, while the proportion of experimentals and controls is almost independent of site, the influence of ethnicity is much more confounded with the influence of site characteristics. Because of this, simple experimental-control comparisons of post-program outcomes for the full sample can confidently be interpreted as resulting from experimentals' access to JOBSTART, but there is not the same confidence about comparisons of experimental-control experiences for ethnic subgroups (which could be heavily influenced by different program structures, labor market conditions, or other important background factors completely external to JOBSTART). Most characteristics lie somewhere between independence and confounding. Thus, unless special techniques are used to remove associations between site and other characteristics, impact comparisons for many subgroups and site groupings may be misleading.

Fortunately, in view of the heavy emphasis this report places on comparisons of outcomes by gender, there is much less cause for concern in making comparisons of impacts for gender-defined subgroups than for those based on ethnicity. The sample was 47 percent male overall, and men were distributed across sites much more evenly than were blacks or Hispanics. A more refined analysis of gender, appearing throughout this report, splits the sample further, based on parenting status. This creates categories that can signal differences in barriers to employment or willingness to sacrifice in order to obtain a steady source of earned income. Overall, half of the women lived with children of their own, and half did not; among men, about one-seventh reported that they already were parents at baseline, and six-sevenths reported having no children at that time. The proportions of parents among women and men

21 Overall, most sample members were black (46 percent) or Hispanic (43 percent). In six sites, more than two-thirds of the participants were black, while in three, more than two-thirds were Hispanic. The 8 percent overall proportion of white non-Hispanic sample members was disproportionately concentrated at the Phoenix Job Corps and, to a lesser extent, at Allentown in Buffalo, CET/San Jose, and EGOS in Denver.

22 The proportion of males in each site ranged from a high of 61 percent at SER/Corpus Christi to a low of 36 percent at EGOS in Denver. In addition to SER/Corpus Christi, four other sites had male majorities: Chicago Commons, the East Los Angeles Skills Center, BSA in New York City, and CET/San Jose.
in the sample did vary among the sites, but the variation was much less than was the case for ethnic groups.23

Site differences were also large for several other subgroups of the research sample:

- **The amount of prior schooling varied among the sites more than gender and parenting status.** Large differences in baseline educational attainment are important to bear in mind when examining rates of post-program GED attainment. Other factors aside, those who were closer to finishing high school at baseline are more likely to have received a GED at follow-up.

- **Employment during the year before random assignment varied among the sites even more than prior schooling.** Holding all other observed factors constant, not having worked recently may signal either greater barriers to employment or more interest in schooling than in employment.24

- **Public assistance receipt varied greatly.** The percentage of a site's sample receiving public assistance may be a good indication of the relative income and job-readiness of the young people there.25

In subsequent chapters, program impacts for subgroups of the impact sample are presented. These subgroups are defined based on pre-random assignment (that is, pre-program) characteristics, and two types of analyses are presented.26 One approach splits the

---

23The proportion of men acknowledging fatherhood ranged from almost none at the Los Angeles Job Corps, BSA in New York City, and the Atlanta Job Corps, to 18 percent at Chicago Commons. At CET/San Jose, the East Los Angeles Skills Center, and BSA in New York City, JOBSTART women were more likely not to be custodial mothers than to be living with their own children, while at Connelley in Pittsburgh, El Centro in Dallas, SEP/Corpus Christi, Chicago Commons, EGOS in Denver, the Phoenix Job Corps, and Allentown in Buffalo, the opposite was true.

24More men than women worked in the year prior to random assignment, but for each group the proportion working also varied greatly among the sites. In most sites, the majority of women had not worked in the year prior to random assignment, with the ratio of nonworking women to working women going above two-to-one at the Phoenix Job Corps. However, Connelley in Pittsburgh and CREC in Hartford were notable exceptions, with substantial majorities of women having had prior-year work experience in those sites. Men's prior employment profiles by site were quite the opposite, with ratios of employed to nonemployed as high as four-to-one at SER/Corpus Christi, CREC in Hartford, and the Atlanta Job Corps. Only among Los Angeles Job Corps men and men at BSA in New York City did nonworkers outnumber workers, with the former's ratio of nonworkers to workers exceeding two-to-one.

25The average initial reading level on entering the program also varied among the sites. This is not discussed in detail because initial test scores are not available in all sites.

26Defining subgroups based on pre-random assignment characteristics is necessary in order to maintain the legitimacy of comparisons of experimentals and controls. For example, those who had not worked in the year prior to random assignment were just as likely to be randomly assigned to the experimental group as to the control group, making comparisons of experimentals and controls with this characteristic appropriate.
entire sample into groups defined by a characteristic such as gender. This "split file" subgroup analysis does not control for other measured differences among the groups, such as their site location or prior work experience. If a subgroup is concentrated in a few sites, as is the case for Hispanics, then the "split file" results may be reflecting site differences as much as subgroup differences. Since men and women are not concentrated in particular sites, the split file analysis presented for these groups in later chapters is appropriate. The second type of subgroup analysis presents results for designated subgroups that are statistically adjusted to account for other measured pre-program differences in the groups besides the characteristic used in defining them. For example, it presents results for ethnic subgroups controlling for differences in measured characteristics other than ethnicity. This analysis does control for site differences and thus can be used for subgroups that are relatively concentrated among a few sites.

C. Sample Differences for Key Site Groupings

In view of this report's special emphasis on sites' delivery systems for employment and training — whether concurrent, sequential/in-house, or sequential/brokered — Table 2.2 collapses the 13 site columns of Table 2.1 into three columns, one for each type of delivery system.27 Averaging data for sites in broad categories destroys much of the observed site variation — particularly regarding ethnicity, receipt of welfare and Food Stamps, parenting status, amount of schooling, and prior-year employment.

Some of the observed site variation remains, however. Sample members at sequential/in-house sites were more likely to be custodial mothers, less likely to have acknowledged fatherhood on the enrollment form, less likely to have worked during the prior year, and more likely to have quit school during junior or senior year than were sample members in other sites. Sample members in concurrent sites were older, more likely to have worked during the prior year, less likely never to have been married, less likely to be receiving

27The first eight columns of Table 2.1 are collapsed into the first column of Table 2.2, for concurrent sites. The next two columns of Table 2.1 — for El Centro in Dallas and the Los Angeles Job Corps — are collapsed into the "sequential/in-house" column of Table 2.2. Finally, the last three columns of Table 2.1 — for Allentown in Buffalo, BSA in New York City, and CREC in Hartford — are collapsed into the "sequential/brokered" column of Table 2.2.
<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Concurrent</th>
<th>Sequential/ In-House</th>
<th>Sequential/ Brokered</th>
<th>All Categories</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gender</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td>968</td>
<td>50.3%</td>
<td>57.4%</td>
<td>55.2%</td>
<td>52.6%**</td>
<td>0.034</td>
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<tr>
<td>Men</td>
<td>871</td>
<td>49.7%</td>
<td>42.6%</td>
<td>44.8%</td>
<td>47.4</td>
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</tr>
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<td>Ethnicity</td>
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<td></td>
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<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
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<td>9.8%</td>
<td>4.6%</td>
<td>8.1%</td>
<td>8.4***</td>
<td>0.000</td>
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<tr>
<td>Black, non-Hispanic</td>
<td>840</td>
<td>34.4%</td>
<td>58.4%</td>
<td>68.5%</td>
<td>45.7</td>
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</tr>
<tr>
<td>Hispanic</td>
<td>783</td>
<td>53.1%</td>
<td>29.2%</td>
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</tr>
<tr>
<td>Other</td>
<td>61</td>
<td>2.7%</td>
<td>7.8%</td>
<td>0.6%</td>
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<td>Ethnicity, by gender</td>
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<td></td>
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</tr>
<tr>
<td>White, non-Hispanic</td>
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<td>4.9%</td>
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<td>White, non-Hispanic</td>
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<td>106</td>
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<td>6.4%</td>
<td>5.8</td>
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</tr>
<tr>
<td>Employed within past year</td>
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<td>No</td>
<td>870</td>
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<td>47.3***</td>
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<tr>
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<td>969</td>
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</tr>
<tr>
<td>Prior employment, by gender</td>
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<td></td>
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<td>373</td>
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(continued)
<table>
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<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
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<th>Sequential/ In-House</th>
<th>Sequential/ Brokered</th>
<th>All Categories</th>
<th>p²</th>
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<tbody>
<tr>
<td>Left school in grade 11 or 12</td>
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<td></td>
<td></td>
</tr>
<tr>
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<td>1,078</td>
<td>59.9%</td>
<td>52.3%</td>
<td>61.3%</td>
<td>58.6%**</td>
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<td>Yes</td>
<td>761</td>
<td>40.1%</td>
<td>47.7%</td>
<td>38.7%</td>
<td>41.4</td>
<td></td>
</tr>
<tr>
<td>Received occupational training within past year</td>
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<td></td>
<td></td>
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<tr>
<td>No</td>
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<td>81.1%</td>
<td>90.6%</td>
<td>81.8%</td>
<td>83.1***</td>
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<td>310</td>
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<td>9.4%</td>
<td>18.2%</td>
<td>16.9</td>
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</tr>
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<td>Age</td>
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</tr>
<tr>
<td>16-19</td>
<td>1,359</td>
<td>71.3%</td>
<td>80.2%</td>
<td>75.4%</td>
<td>73.9***</td>
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</tr>
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<td>20 or 21</td>
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<td>Marital status</td>
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<td>Ever married</td>
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<td>7.8%</td>
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<td>92.2%</td>
<td>96.2%</td>
<td>90.5</td>
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<tr>
<td>Living in own household or with boy/girlfriend</td>
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</tr>
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<td>11.3%</td>
<td>24.0%</td>
<td>18.4</td>
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</tr>
<tr>
<td>Own AFDC case or receiving General Assistance</td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
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<td>75.1%</td>
<td>74.3%</td>
<td>65.3%</td>
<td>73.1***</td>
<td>0.001</td>
</tr>
<tr>
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<td>24.9%</td>
<td>25.7%</td>
<td>34.7%</td>
<td>26.9</td>
<td></td>
</tr>
<tr>
<td>Own AFDC case</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,446</td>
<td>80.9%</td>
<td>75.6%</td>
<td>74.6%</td>
<td>78.6**</td>
<td>0.012</td>
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<tr>
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<td>393</td>
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<td>24.4%</td>
<td>25.4%</td>
<td>21.4</td>
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</tr>
<tr>
<td>Receiving Food Stamps</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,143</td>
<td>61.4%</td>
<td>76.4%</td>
<td>49.1%</td>
<td>62.2***</td>
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<td>696</td>
<td>38.6%</td>
<td>23.6%</td>
<td>50.9%</td>
<td>37.8</td>
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<td>Arrested since age 16</td>
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<td>272</td>
<td>16.9%</td>
<td>9.7%</td>
<td>13.6%</td>
<td>14.8</td>
<td></td>
</tr>
<tr>
<td>Lived with both parents at age 14</td>
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</tr>
<tr>
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<td>73.7%</td>
<td>76.9%</td>
<td>65.1***</td>
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</tr>
<tr>
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<td>26.3%</td>
<td>23.1%</td>
<td>31.9</td>
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</tr>
<tr>
<td>Sample size</td>
<td>1,839</td>
<td>1,120</td>
<td>373</td>
<td>346</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
TABLE 2.2 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Concurrent</th>
<th>Sequential/ In-House</th>
<th>Sequential/ Brokered</th>
<th>All Categories</th>
<th>p(^a)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Site</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
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<td></td>
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</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>61</td>
<td>5.4%</td>
<td>0.0%</td>
<td>0.0%</td>
<td>3.3%***</td>
<td>0.000</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>13.6%</td>
<td>0.0%</td>
<td>0.0%</td>
<td></td>
<td>8.3</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>/</td>
<td>6.6%</td>
<td>0.0%</td>
<td>0.0%</td>
<td></td>
<td>4.0</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>18</td>
<td>16.4%</td>
<td>0.0%</td>
<td>0.0%</td>
<td></td>
<td>10.0</td>
</tr>
<tr>
<td>East LA Skills Center</td>
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<td>8.9%</td>
<td>0.0%</td>
<td>0.0%</td>
<td></td>
<td>5.4</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
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<td>0.0%</td>
<td>0.0%</td>
<td></td>
<td>10.0</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>130</td>
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<td>0.0%</td>
<td>0.0%</td>
<td></td>
<td>7.1</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>236</td>
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<td>0.0%</td>
<td>0.0%</td>
<td></td>
<td>12.8</td>
</tr>
<tr>
<td>Sequential/in-house</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>155</td>
<td>0.0%</td>
<td>41.6%</td>
<td>0.0%</td>
<td></td>
<td>8.4</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>218</td>
<td>0.0%</td>
<td>58.4%</td>
<td>0.0%</td>
<td></td>
<td>11.9</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>140</td>
<td>0.0%</td>
<td>0.0%</td>
<td>40.5%</td>
<td></td>
<td>7.6</td>
</tr>
<tr>
<td>BSA (NYC)</td>
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<td>0.0%</td>
<td>34.4%</td>
<td></td>
<td>6.5</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>87</td>
<td>0.0%</td>
<td>0.0%</td>
<td>25.1%</td>
<td></td>
<td>4.7</td>
</tr>
</tbody>
</table>

Sample size 1,839 1,120 373 346

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

\(^a\)The column labeled "p" is the statistical significance level of differences among groups in distributions of characteristics: that is, p is the probability that observed proportions in each subgroup differ from one column to another only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
AFDC, more likely to have lived with two parents at age 14, less likely to be black, and more likely to be Hispanic than were sample members in other sites.\textsuperscript{28}

D. Gender Differences in Baseline Characteristics

An important question in evaluating JOBSTART is whether participation and program impacts varied by gender. A first step toward understanding these gender differences is to examine the other characteristics of the various gender-defined groups. Table 2.3 shows that men and women in the impact sample were similar in many characteristics, including age, ethnic background, educational attainment, and initial reading levels. However, men were more likely to have had recent work experience and vocational training and to have been arrested since age 16. They were less likely to have been married, to be a parent, and to be receiving public assistance.

Most of the differences between men and women just enumerated are due mainly to differences between mothers and other members of the sample. It is the mothers who are least likely to have worked in the year before random assignment, most likely to have lived on their own, most likely to have received AFDC and Food Stamps, and least likely to have lived with both parents at age 14. Because of these clear differences in initial characteristics between young mothers and other young women, the gender-based subgroup analysis that follows divides the female sample into these two groups. However, in most of the analysis, all men are grouped together because of the small number of men reporting that they were parents at baseline.

\textsuperscript{28}There may also be unobserved differences. For example, youths attracted to sequential/brokered programs run by community-based educational institutions may be more interested in passing the GED examination than are youths at concurrent sites run by training agencies.
TABLE 2.3
CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY GENDER AND PARENTAL STATUS

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Women</th>
<th>Men</th>
<th>All</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Living with Own Child(ren)</td>
<td>All Others</td>
<td>Do Not Have Own Child(ren)</td>
</tr>
<tr>
<td>Ethnicity</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>155</td>
<td>7.2%</td>
<td>10.1%</td>
<td>9.0%</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>840</td>
<td>54.1%</td>
<td>37.8%</td>
<td>43.5%</td>
</tr>
<tr>
<td>Hispanic</td>
<td>763</td>
<td>37.8%</td>
<td>47.5%</td>
<td>43.0%</td>
</tr>
<tr>
<td>Other</td>
<td>61</td>
<td>0.8%</td>
<td>4.5%</td>
<td>4.4%</td>
</tr>
<tr>
<td>Ethnicity, by gender</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>84</td>
<td>7.2%</td>
<td>10.1%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>445</td>
<td>54.1%</td>
<td>37.8%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Hispanic</td>
<td>413</td>
<td>37.8%</td>
<td>47.5%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Other</td>
<td>26</td>
<td>0.8%</td>
<td>4.5%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Men</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>71</td>
<td>0.0%</td>
<td>0.0%</td>
<td>9.0%</td>
</tr>
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<td>0.0%</td>
<td>43.5%</td>
</tr>
<tr>
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<td>43.0%</td>
</tr>
<tr>
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<td>0.0%</td>
<td>4.4%</td>
</tr>
<tr>
<td>Employed within past year</td>
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<td></td>
<td></td>
<td></td>
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<tr>
<td>No</td>
<td>870</td>
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<td>50.6%</td>
<td>38.2%</td>
</tr>
<tr>
<td>Yes</td>
<td>969</td>
<td>37.6%</td>
<td>49.4%</td>
<td>61.8%</td>
</tr>
<tr>
<td>Prior employment, by gender</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women employed within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>547</td>
<td>62.4%</td>
<td>50.6%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Yes</td>
<td>421</td>
<td>37.6%</td>
<td>49.4%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Men employed within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>323</td>
<td>0.0%</td>
<td>0.0%</td>
<td>38.2%</td>
</tr>
<tr>
<td>Yes</td>
<td>548</td>
<td>0.0%</td>
<td>0.0%</td>
<td>61.8%</td>
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</table>

Sample size 1,839 484 484 765 106
(continued)
### TABLE 2.3 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Living with Own Child(ren)</th>
<th>All Others</th>
<th>Women</th>
<th>Men</th>
<th>p^a</th>
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</thead>
<tbody>
<tr>
<td>Left school in grade 11 or 12</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,078</td>
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<td>57.5%</td>
<td>58.6%</td>
</tr>
<tr>
<td>Yes</td>
<td>761</td>
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<td>41.9%</td>
<td>42.7%</td>
<td>42.5%</td>
<td>41.4%</td>
</tr>
<tr>
<td>Received occupational training within past year</td>
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<td></td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,529</td>
<td>88.0%</td>
<td>86.2%</td>
<td>79.1%</td>
<td>76.4%</td>
<td>83.1***</td>
</tr>
<tr>
<td>Yes</td>
<td>310</td>
<td>12.0%</td>
<td>13.8%</td>
<td>20.9%</td>
<td>23.6%</td>
<td>16.9%</td>
</tr>
<tr>
<td>Age</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>16-19</td>
<td>1,359</td>
<td>61.6%</td>
<td>85.1%</td>
<td>77.9%</td>
<td>50.0%</td>
<td>73.9***</td>
</tr>
<tr>
<td>20 or 21</td>
<td>480</td>
<td>38.4%</td>
<td>14.9%</td>
<td>22.1%</td>
<td>50.0%</td>
<td>26.1%</td>
</tr>
<tr>
<td>Marital status</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever married</td>
<td>174</td>
<td>19.6%</td>
<td>6.0%</td>
<td>3.1%</td>
<td>24.5%</td>
<td>9.5***</td>
</tr>
<tr>
<td>Never married</td>
<td>1,665</td>
<td>80.4%</td>
<td>94.0%</td>
<td>96.9%</td>
<td>75.5%</td>
<td>90.5%</td>
</tr>
<tr>
<td>Living in own household or with boy/girlfriend</td>
<td></td>
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<tr>
<td>No</td>
<td>1,500</td>
<td>55.8%</td>
<td>86.8%</td>
<td>94.9%</td>
<td>79.2%</td>
<td>81.6***</td>
</tr>
<tr>
<td>Yes</td>
<td>339</td>
<td>44.2%</td>
<td>13.2%</td>
<td>5.1%</td>
<td>20.8%</td>
<td>18.4%</td>
</tr>
<tr>
<td>Own AFDC case or receiving General Assistance</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>No</td>
<td>1,344</td>
<td>37.6%</td>
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<td>87.8%</td>
<td>79.2%</td>
<td>73.1***</td>
</tr>
<tr>
<td>Yes</td>
<td>495</td>
<td>62.4%</td>
<td>16.1%</td>
<td>12.2%</td>
<td>20.8%</td>
<td>26.9%</td>
</tr>
<tr>
<td>Own AFDC case</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,446</td>
<td>39.9%</td>
<td>89.9%</td>
<td>94.5%</td>
<td>89.6%</td>
<td>78.6***</td>
</tr>
<tr>
<td>Yes</td>
<td>393</td>
<td>60.1%</td>
<td>10.1%</td>
<td>5.5%</td>
<td>10.4%</td>
<td>21.4%</td>
</tr>
<tr>
<td>Sample size</td>
<td>1,839</td>
<td>484</td>
<td>484</td>
<td>765</td>
<td>106</td>
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</table>

(continued)
<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Living with Own Child(ren)</th>
<th>All Others</th>
<th>Do Not Have Own Child(ren)</th>
<th>Have Own Child(ren)</th>
<th>All Categories</th>
<th>p&lt;sup&gt;*&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Receiving Food Stamps</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,143</td>
<td>41.1%</td>
<td>67.6%</td>
<td>72.8%</td>
<td>56.6%</td>
<td>62.2%***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>696</td>
<td>58.9%</td>
<td>32.4%</td>
<td>27.2%</td>
<td>43.4%</td>
<td>37.8</td>
<td></td>
</tr>
<tr>
<td><strong>Arrested since age 16</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,567</td>
<td>96.1%</td>
<td>93.8%</td>
<td>75.3%</td>
<td>67.9%</td>
<td>85.2%***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>272</td>
<td>3.9%</td>
<td>6.2%</td>
<td>24.7%</td>
<td>32.1%</td>
<td>14.8</td>
<td></td>
</tr>
<tr>
<td><strong>Lived with both parents at age 14</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>No</td>
<td>1,198</td>
<td>73.8%</td>
<td>64.7%</td>
<td>59.1%</td>
<td>71.7%</td>
<td>65.1%***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>641</td>
<td>26.2%</td>
<td>35.3%</td>
<td>40.9%</td>
<td>28.3%</td>
<td>34.9</td>
<td></td>
</tr>
<tr>
<td><strong>Site</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>61</td>
<td>3.7%</td>
<td>3.7%</td>
<td>3.1%</td>
<td>0.9%</td>
<td>3.3%***</td>
<td>0.000</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>2.1%</td>
<td>13.4%</td>
<td>9.0%</td>
<td>7.5%</td>
<td>8.3</td>
<td></td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>74</td>
<td>3.7%</td>
<td>2.9%</td>
<td>3.8%</td>
<td>12.3%</td>
<td>4.0</td>
<td></td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>184</td>
<td>13.0%</td>
<td>7.2%</td>
<td>8.9%</td>
<td>17.0%</td>
<td>10.0</td>
<td></td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>100</td>
<td>2.5%</td>
<td>6.6%</td>
<td>6.8%</td>
<td>3.8%</td>
<td>5.4</td>
<td></td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>183</td>
<td>13.8%</td>
<td>10.5%</td>
<td>8.0%</td>
<td>3.8%</td>
<td>10.0</td>
<td></td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>130</td>
<td>7.6%</td>
<td>6.2%</td>
<td>7.3%</td>
<td>6.6%</td>
<td>7.1</td>
<td></td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>236</td>
<td>11.0%</td>
<td>8.3%</td>
<td>15.9%</td>
<td>19.8%</td>
<td>12.8</td>
<td></td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>155</td>
<td>10.5%</td>
<td>6.8%</td>
<td>8.5%</td>
<td>5.7%</td>
<td>8.4</td>
<td></td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>218</td>
<td>13.6%</td>
<td>13.2%</td>
<td>11.2%</td>
<td>1.9%</td>
<td>11.9</td>
<td></td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>140</td>
<td>9.5%</td>
<td>7.4%</td>
<td>5.5%</td>
<td>15.1%</td>
<td>7.6</td>
<td></td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>119</td>
<td>3.9%</td>
<td>7.0%</td>
<td>8.4%</td>
<td>1.9%</td>
<td>6.5</td>
<td></td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>87</td>
<td>5.0%</td>
<td>6.6%</td>
<td>3.5%</td>
<td>3.8%</td>
<td>4.7</td>
<td></td>
</tr>
</tbody>
</table>

Sample size: 1,839

(continued)
TABLE 2.3 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires. Distributions may not total 100.0 percent because of rounding.

The column labeled "p" is the statistical significance level of differences among groups in distributions of characteristics: that is, p is the probability that observed proportions in each subgroup differ from one column to another only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
CHAPTER 3

AN OVERVIEW OF JOBSTART SERVICES AND PARTICIPATION

The JOBSTART model requires sites to operate basic education and occupational skills training classes that are interesting and accessible, effective in improving the skills of young people, and of relatively long duration. It also requires young people to take advantage of these opportunities. Historically, education and training programs have had problems retaining young, economically disadvantaged dropouts (or even high school graduates).\(^1\) Thus, a key question for the evaluation is whether youths offered JOBSTART services do actually participate in lengthy, intensive services.

This chapter looks at the JOBSTART experience from three perspectives.\(^2\) First, it briefly summarizes the nature of program services (highlighting key aspects of site variation) and reports youths' subjective reactions to the services.\(^3\) Second, it describes participation patterns of youths who were active in the JOBSTART Demonstration and compares that experience to other programs for young school dropouts. The analysis shows that participation was, in general, longer and more substantial than in most other JTPA Title IIA-funded activities for young dropouts, and that it was roughly comparable to participation in intensive programs such as the nonresidential Job Corps and the National Supported Work Demonstration (generally referred to simply as Supported Work).

Third, the chapter analyzes the extent to which participation varied among different groups of youths and types of sites. This analysis finds that participation hours were similar for many groups: males and females, various ethnic groups, older and younger participants, youths with relatively higher and lower levels of reading skills, and recipients and nonrecipients of public assistance. Participation hours tended to be higher in labor markets with poorer employment opportunities. Average total participation hours were highest in sites that operated


\(^2\) The chapter summarizes and updates information in Chapters 2 and 4 through 8 of Auspos et al., 1989. See that report for more details.

\(^3\) These reactions were captured in the Initial follow-up survey, which was conducted 12 months after random assignment, and in focus groups with participants. This section presents information on the JOBSTART components in specific sites. Chapter 6, which discusses program impacts by site, includes a summary of each site's program characteristics.
sequential programs with all services provided in-house (536 hours) followed by sites with concurrent programs (405 hours) and sequential sites that referred participants to another agency for training (316 hours). Average hours in education were highest in sequential sites, while average training hours were highest at concurrent sites.

The discussion of site experiences reinforces three basic themes of this report. First, the variation in the details of the programs highlights the diversity of JOBSTART experiences among the sites within the general framework of the JOBSTART guidelines. Second, the experience of the sites shows that the basic program model can be implemented in a variety of administrative and labor market settings and using different basic program structures, though there were clearly stronger and weaker programs among the sites in the demonstration. Finally, this summary of how sites varied along many different dimensions provides background needed for interpreting the differences in site impacts that are presented in Chapter 6.

I. The Nature of JOBSTART Services

Basic education, occupational training, support services, and job placement assistance were available to participants in each site. To operate JOBSTART, two of the six community-based organizations (SER/Corpus Christi and Chicago Commons) added education to their regular service offerings, and three of the others (Allentown in Buffalo, BSA in New York City, and CREC in Hartford) developed or strengthened relationships with outside training programs so that they could serve as brokers, arranging training elsewhere for JOBSTART participants. The one community college (El Centro in Dallas) and three adult vocational schools (Connelley in Pittsburgh, the East Los Angeles Skills Center, and EGOS in Denver) had previously offered education and training but had to strengthen support services and job placement assistance. The three Job Corps Centers (in Atlanta, Los Angeles, and Phoenix) already had all four kinds of services in place. CET/San Jose already operated a program of integrated training and education, with support services and job placement assistance.

Table 3.1 (which groups the sites by whether they operated concurrent, sequential/in-house, or sequential/brokered programs) describes the entry and exit rules, availability of separate classes for youths, expected duration of occupational training, and scheduled hours per
### TABLE 3.1
CHARACTERISTICS OF JOBSTART ACTIVITIES, BY SITE

<table>
<thead>
<tr>
<th>Site</th>
<th>Fixed Cycle or Open Entry and Exit</th>
<th>Separate Classes for Youths</th>
<th>Expected Duration of Occupational Training</th>
<th>Scheduled Hours per Day</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Education</td>
<td>Training</td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>Open entry and exit</td>
<td>Yes</td>
<td>1 year maximum(^b)</td>
<td>Individualized, usually 2 hours</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>Open entry and exit</td>
<td>In education only</td>
<td>600-1,000 hours during 23-37 weeks</td>
<td>2 hours, may vary</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>Fixed cycle</td>
<td>In education only</td>
<td>500-1,380 hours during 22-42 weeks per week</td>
<td>1-2 hours, -5 Jays</td>
</tr>
<tr>
<td>Conneiley (Pittsburgh)</td>
<td>Fixed cycle with semesters</td>
<td>Sometimes in education hours</td>
<td>700-1,000 hours</td>
<td>2 hours</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>Open entry and exit</td>
<td>No</td>
<td>600-840 hours during 20-28 weeks</td>
<td>2 hours, may vary</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>Open entry and exit with semesters</td>
<td>In education only</td>
<td>600-1,000 hours</td>
<td>2 hours, may vary</td>
</tr>
</tbody>
</table>

\(^a\) Individualized, usually 2 hours.
\(^b\) Individualized, usually 2.5 hours at start, more in subsequent weeks.
\(^c\) Usual 2 hours in life skills and avocational activities at start, less in subsequent weeks.
\(^d\) Usual 2 hours in life skills and avocational activities at start, less in subsequent weeks.

(continued)
<table>
<thead>
<tr>
<th>Site</th>
<th>Fixed Cycle or Open Entry and Exit</th>
<th>Separate Classes for Youths</th>
<th>Expected Duration of Occupational Training</th>
<th>Scheduled Hours per Day</th>
</tr>
</thead>
<tbody>
<tr>
<td>Phoenix Job Corps</td>
<td>Open entry and exit</td>
<td>Yes</td>
<td>1 year maximumb</td>
<td>Individualized, usually 2 hours</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Individualized, usually 2.5 hours at start, more in subsequent weeks</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Usually 2 hours in life skills and avocational activities at start, less in subsequent weeks</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>6.5 hours</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>Fixed cycle</td>
<td>Yes</td>
<td>500-660 hours during 22-23 weeks</td>
<td>3.5 hours for first 12-16 weeks</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>6 hours</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>None</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>Open entry and exit</td>
<td>In education only</td>
<td>720 hours during 24 weeks</td>
<td>3-4 hours</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>6 hours</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>2-3 hours in life skills activities during education phase</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>6 hours</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>Open entry and exit</td>
<td>Yes</td>
<td>1 year maximumb</td>
<td>3 hours for first 10-12 weeks, then individualized</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>6 hours, may vary</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>3 hours in life skills or avocational activities during education phase</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>6 hours</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>Open entry and exit</td>
<td>In education only</td>
<td>Varied by training provider</td>
<td>Varied by training provider</td>
</tr>
<tr>
<td></td>
<td>for education, only varied in training</td>
<td></td>
<td></td>
<td>3 hours in life skills activities during education phase</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>6 hours during education phase</td>
</tr>
</tbody>
</table>

(continued)
<table>
<thead>
<tr>
<th>Site</th>
<th>Fixed Cycle or Open Entry and Exit</th>
<th>Separate Classes for Youths</th>
<th>Expected Duration of Occupational Training</th>
<th>Scheduled Hours per Day</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td><strong>Education</strong>&lt;sup&gt;a&lt;/sup&gt;</td>
<td>Training</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Other Activities</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td><strong>Total</strong></td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>Open entry and exit for education, varied in training</td>
<td>In education only</td>
<td>Varied by training provider</td>
<td>3 hours, 4 days per week</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>Open entry and exit for education, varied in training</td>
<td>No</td>
<td>Varied by training provider</td>
<td>3 hours</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**SOURCE:** Adapted from Auspos et al., 1989.

**NOTES:**

<sup>a</sup>Education hours refer to time spent in a basic education or GED-preparation class and do not include education provided as part of an occupational training course.

<sup>b</sup>Job Corps Centers offer a maximum of two years of training, but JOBSTART participants were supposed to be enrolled in courses that could be completed in one year.

<sup>c</sup>Some participants worked in paid or unpaid work experience positions for limited periods.

<sup>d</sup>Life skills classes typically provided instruction in work behaviors, goal setting, personal budgeting, health, and interpersonal relations. Avocational activities included physical education and driver education.

<sup>e</sup>Additional hours were available on an individualized basis after the course ended.
day in each site. In some sites, participants could enter courses at any time (open entry) and leave them when they had achieved a certain competency level (open exit), while in others, they had to adhere to a fixed cycle, with entry on specified dates and exit after a set period of time. Some sites held classes for youths only, while others mixed youths and adults. Sites also varied in their expected duration of training, daily scheduling, and support services.

A. Basic Education

The education component typically consisted of individualized instruction, which allowed students to move at their own pace learning reading, mathematics, and other subjects needed to pass the GED examination. Mostly they worked on their own, doing workbook exercises or, less commonly, using computer-assisted instruction. In sites offering education and training concurrently, participants usually attended two hours of education classes and four hours of vocational training a day. In sites operating a sequential program, participants generally attended three hours a day of basic skills classes during the education phase, with the remaining three hours a day being devoted to life skills classes.

The payment provisions of the contracts between service providers and funding agencies (especially local SDAs) were an important source of variation in the emphasis of the education component. In four sites (Connelley in Pittsburgh, EGOS in Denver, El Centro in Dallas, and SER/Corriente Christi), payment for education services was based on students passing the GED examination. This led these sites to make GED certification an important short-term goal of the program and to emphasize the skills tested on the GED examination in their education component. Other sites—CET/San Jose, the East Los Angeles Skills Center, and especially Chicago Commons—saw GED attainment as a long-term goal and did not stress it in their JOBSTART programs, focusing more on improving basic skills as an aid to vocational training and job placement.

The actual curricula and instructional materials were not specified by the JOBSTART guidelines. The three Job Corps sites used the standard Job Corps materials (workbooks, textbooks, and audiovisual materials), though two centers (Atlanta and Phoenix) also had supplementary computer-assisted instruction. The three sequential/brokered sites used the Comprehensive Competencies Program (CCP) developed by U.S. Basic Skills Investment

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4This grouping was chosen because, as discussed later, participation rates by component, participation hours, and program emphasis differed among these three types of sites.
Corporation. CCP is an instructional management system integrating textbooks, workbooks, computer software, audiovisual materials, and progress tests. In the seven other sites, teachers developed their own instructional materials using a variety of sources, such as GED preparation courses and reading and mathematics textbooks that used the "mastery learning" approach, which focuses on the step-by-step acquisition of specific competencies. In four of these sites, staff supplemented pencil and paper exercises with computer-assisted instruction.\(^5\)

Teachers in most sites felt that the individualized, self-paced instruction provided a better learning environment than participants had typically found in high school. The competency-based courses allowed the youths to see themselves making incremental progress as they advanced toward what was, for many, a remote goal of mastering basic skills and receiving a GED. Most students preferred this instructional approach because they felt that it made them active participants in the process of learning and allowed them to master one topic before beginning another. In the follow-up survey, about three-fourths of JOBSTART participants found self-paced instruction "very helpful," while virtually no one found it "not helpful at all."

Yet students also valued interaction with instructors, as much for the personal attention and motivation it provided as for instruction in specific skills. About 75 percent of JOBSTART participants rated support from teachers and fellow students in the education component "very helpful."

Despite the overall favorable assessment, three concerns emerged. First, with a few important exceptions, the basic education and skills training activities operated separately, with little integration of material. As discussed in Chapter 1, only at CET/San Jose, and to a lesser extent Chicago Commons, were basic skills and occupational training instruction truly integrated. Though several other sites did attempt to coordinate the two activities to a limited extent (creating a distribution of sites rather than two clear-cut categories), these sites fell short of the integration observed at CET/San Jose and Chicago Commons. Second, some instructors thought the curriculum should include more material on critical thinking and general knowledge, in contrast to the functional literacy and mathematics emphasis of many integrated programs. Third, some instructors said that students with very poor skills or low motivation found the work boring and, as a remedy, suggested more group activities. One site, El Centro

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\(^5\)See Auspos et al., 1989, for the details of these programs.
in Dallas, shifted to this approach, relying more heavily than other sites on class exercises and lectures.

B. Occupational Skills Training

The choices of occupational training available to participants varied among the sites. Participants at large vocational schools could choose courses in more than 20 occupational areas. The Job Corps Centers and larger community-based organizations (CBOs) also offered a wide range of vocational training. In contrast, SER/Corpus Christi, which provided training in-house, offered only a few courses.

In theory, youths in sequential/brokered sites could choose courses from a variety of local agencies. However, in practice, some courses were unavailable to them because they could not satisfy entrance requirements, or other difficulties prevented them from gaining entry.\(^6\) As discussed later in this chapter, the resulting low rate of participation in training in sequential/brokered sites was the major operational issue concerning the training component.

As a group, JOBSTART participants were enrolled in training for a broad range of occupations — clerical and service jobs, machine trades, benchwork occupations, and structural work such as welding. Occupational choices for men and women followed traditional patterns, as shown in Table 2, with about three-fourths of the women participants training for clerical jobs.\(^7\)

Using categories employed by the U.S. General Accounting Office (GAO) in a recent analysis of JTPA Title IIA adult training, MDRC classified the JOBSTART training provided to participants as leading to jobs requiring low or low/moderate skills (slightly less than one-fourth of participants), moderate skills (about one-half of participants), and higher skills (about one-fourth of participants).\(^8\) This distribution of skills ratings for training occupations was similar to what the GAO found for JTPA Title IIA adult programs. This was unexpected.

\(^6\)As mentioned in Chapter 1, the JTPA performance standards created an incentive for SDAs to emphasize lower-cost, short-term programs. Some SDAs in study sites were reluctant to provide a single individual with both education and training, and many JTPA Title IIA-funded service providers operated under performance-based contracts linking payment to placement in a job. Both practices hindered the efforts of JOBSTART youths in sequential/brokered sites to find a training agency willing to accept them.

\(^7\)This table, taken from an earlier report on JOBSTART (Auspos et al., 1989), is based on a similar, but slightly smaller, sample than that used for this report.

\(^8\)See U.S. General Accounting Office, 1988, for the definitions of categories of training. The percentage distribution reported in the text for JOBSTART was calculated in Auspos et al., 1989, based on a slightly different sample of participants than that used in this report.
TABLE 3.2
PERCENTAGE DISTRIBUTION OF OCCUPATIONS
FOR PARTICIPANTS IN TRAINING, BY GENDER

<table>
<thead>
<tr>
<th>Training Categorya</th>
<th>Men</th>
<th>Women</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Clerical and sales occupation</strong>:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Stenography, typing, filing, and related occupations</td>
<td>5.0%</td>
<td>51.3%</td>
<td>29.2%</td>
</tr>
<tr>
<td>Computing and account-recording</td>
<td>7.0</td>
<td>20.3</td>
<td>13.9</td>
</tr>
<tr>
<td>Production and stock clerks, and related occupations</td>
<td>0.3</td>
<td>0.0</td>
<td>0.1</td>
</tr>
<tr>
<td>Information and message distribution</td>
<td>1.4</td>
<td>0.0</td>
<td>0.7</td>
</tr>
<tr>
<td>Miscellaneous clerical</td>
<td>0.0</td>
<td>0.5</td>
<td>0.3</td>
</tr>
<tr>
<td>Sales and consumable commodities</td>
<td>0.8</td>
<td>1.0</td>
<td>0.9</td>
</tr>
<tr>
<td>Total</td>
<td>14.6</td>
<td>73.1</td>
<td>45.1</td>
</tr>
<tr>
<td><strong>Service occupations</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Food and beverage preparation and services</td>
<td>3.1</td>
<td>4.6</td>
<td>3.9</td>
</tr>
<tr>
<td>Miscellaneous personal services</td>
<td>0.6</td>
<td>11.5</td>
<td>6.3</td>
</tr>
<tr>
<td>Building and related services</td>
<td>8.4</td>
<td>2.1</td>
<td>5.1</td>
</tr>
<tr>
<td>Total</td>
<td>12.0</td>
<td>18.2</td>
<td>15.3</td>
</tr>
<tr>
<td><strong>Machine trades occupations</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Metal machining</td>
<td>5.0</td>
<td>0.8</td>
<td>2.8</td>
</tr>
<tr>
<td>Mechanics and machinery repair</td>
<td>22.4</td>
<td>1.3</td>
<td>11.4</td>
</tr>
<tr>
<td>Printing</td>
<td>0.6</td>
<td>1.3</td>
<td>0.9</td>
</tr>
<tr>
<td>Wood machining</td>
<td>0.6</td>
<td>0.5</td>
<td>0.5</td>
</tr>
<tr>
<td>Total</td>
<td>28.6</td>
<td>3.8</td>
<td>15.7</td>
</tr>
<tr>
<td><strong>Benchwork occupations</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Assembly and repair of electrical equipment</td>
<td>11.5</td>
<td>1.3</td>
<td>6.2</td>
</tr>
<tr>
<td>Painting, decorating, and related occupations</td>
<td>0.8</td>
<td>0.3</td>
<td>0.5</td>
</tr>
<tr>
<td>Fabrication and repair of plastics, synthetics, rubber, and related products</td>
<td>2.2</td>
<td>0.3</td>
<td>1.2</td>
</tr>
<tr>
<td>Fabrication and repair of textile, leather, and related products</td>
<td>1.7</td>
<td>0.3</td>
<td>0.9</td>
</tr>
<tr>
<td>Total</td>
<td>16.2</td>
<td>2.1</td>
<td>8.8</td>
</tr>
<tr>
<td><strong>Structural work occupations</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Metal fabricating</td>
<td>9.8</td>
<td>0.5</td>
<td>5.0</td>
</tr>
<tr>
<td>Welders, cutters, and related occupations</td>
<td>0.8</td>
<td>0.0</td>
<td>0.4</td>
</tr>
<tr>
<td>Electrical assembling, installing, and repairing</td>
<td>5.9</td>
<td>0.5</td>
<td>3.1</td>
</tr>
<tr>
<td>Painting, plastering, waterproofing, cementing, and related occupations</td>
<td>1.7</td>
<td>0.0</td>
<td>0.8</td>
</tr>
<tr>
<td>Construction</td>
<td>8.4</td>
<td>1.3</td>
<td>4.7</td>
</tr>
<tr>
<td>Total</td>
<td>26.6</td>
<td>2.3</td>
<td>13.9</td>
</tr>
<tr>
<td><strong>Miscellaneous occupations</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Transportation</td>
<td>0.0</td>
<td>0.3</td>
<td>0.1</td>
</tr>
<tr>
<td>Graphic art work</td>
<td>2.0</td>
<td>0.3</td>
<td>1.1</td>
</tr>
<tr>
<td>Total</td>
<td>2.0</td>
<td>0.5</td>
<td>1.2</td>
</tr>
<tr>
<td><strong>All training categories</strong></td>
<td>100.0</td>
<td>100.0</td>
<td>100.0</td>
</tr>
</tbody>
</table>

Number of participants in training                       | 357   | 390   | 747   

(continued)
TABLE 3.2 (continued)


NOTES: Calculations for this table used data for all youths who were active for at least one hour in a JOBSTART training component within 12 months of random assignment and responded to the 12-month follow-up survey. Distributions may not total 100.0 percent because of rounding. Individual category totals may not equal the general category totals because of rounding. Tests of statistical significance were not run. *Individuals participating in more than one training category are included in the category in which they attended the most hours.
since JOBSTART participants faced more barriers to employment than did the typical JTPA Title IIA adult client.

One argument for sequential programs is that the up-front education allows participants to enter more advanced training. In the JOBSTART Demonstration, this did not appear to occur. In terms of the same GAO categories, the jobs that youths trained for in sequential sites did not appear to require higher skills than those in concurrent sites.

C. Support Services To Facilitate Participation

All sites provided basic support services such as assistance with transportation and child care, which helped participants attend the program, as shown in Table 3.3. All sites provided bus passes or small allowances to cover the costs of commuting to the program. JOBSTART counselor/coordinators placed a high priority on adequate child care arrangements. In most sites, staff referred JOBSTART participants to other agencies for child care, with the expenses being covered by JTPA or the Work Incentive (WIN) program. The Atlanta Job Corps, two CBOs (SER/Corpus Christi and CET/San Jose), and one adult school (Connelley Jr. Pittsburgh) had on-site day care facilities, but staff reported that students frequently preferred to make their own arrangements in their own neighborhoods.

Many sites also found ways to provide small payments to meet other costs of participating, though the Job Corps Centers were consistently able to provide more support than the other sites. Ten of the 13 sites were able to provide some type of small needs-based payment, while nine of the sites provided on-site meals or food to take home or special allowances for clothing or to meet rent emergencies. Seven of the sites (including all three Job Corps Centers) provided some form of incentive payments to participants who reached milestones in the program. The Job Corps Centers also provided on-site medical and dental care.

In addition, to increase participants' motivation and commitment to the program, site staff used a variety of strategies: personal counseling, peer support, time management training, and group recreational activities. Finally, staff at most sites provided training in life skills—covering topics such as health, personal finances, and workplace routines—to help the young people function more responsibly and productively in a variety of roles and situations. Six of the sites (the three Job Corps programs, El Centro in Dallas, Allentown in Buffalo, and BSA in New York City) incorporated two to three hours of formal life skills classes into the regular
<table>
<thead>
<tr>
<th>Site</th>
<th>Needs-Based Payments</th>
<th>Transportation</th>
<th>Child Care</th>
<th>Other</th>
<th>Incentive Payments</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Job Corps Centers</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months</td>
<td>Bus passes</td>
<td>On-site</td>
<td>Free meals; clothing</td>
<td>Merit raises can increase basic allowance to $100 per month after 6 months; $75 per month is placed in sixth and tenth months, escrow for enrollees who stay 6 months, which increases to $100 per month after 6 months; $150 bonus in tenth month</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months</td>
<td>Bus passes</td>
<td>By referral</td>
<td>Free meals; clothing</td>
<td>Merit raises can increase basic allowance to $100 per month after 6 months; $75 per month is placed in sixth and tenth months, escrow for enrollees who stay 6 months, which increases to $100 per month after 6 months; $150 bonus in tenth month</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months</td>
<td>Bus passes</td>
<td>By referral</td>
<td>Free meals; clothing</td>
<td>Merit raises can increase basic allowance to $100 per month after 6 months; $75 per month is placed in sixth and tenth months, escrow for enrollees who stay 6 months, which increases to $100 per month after 6 months; $150 bonus in tenth month</td>
</tr>
<tr>
<td>Schools</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>$5 per day$^a</td>
<td>$2 per day or bus passes$^a</td>
<td>On-site and by referral</td>
<td>$50 one-time clothing grant</td>
<td>$50 for passing GED; $50 for each month of perfect attendance; quarterly payment of $50 for &quot;A&quot; average, $25 for &quot;B&quot; average, $10 for &quot;C&quot; average</td>
</tr>
</tbody>
</table>

(continued)
### TABLE 3.3 (continued)

<table>
<thead>
<tr>
<th>Site</th>
<th>Needs-Based Payments</th>
<th>Transportation</th>
<th>Child Care</th>
<th>Other</th>
<th>Incentive Payments</th>
</tr>
</thead>
<tbody>
<tr>
<td>East LA Skills Center</td>
<td>None</td>
<td>Bus passes, gasoline vouchers</td>
<td>By referral</td>
<td>Emergency funds, lunch money during a brief period</td>
<td>None</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>None</td>
<td>Bus passes, gasoline vouchers</td>
<td>By referral</td>
<td>Lunch money during a brief period</td>
<td>None</td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>$5 per day</td>
<td>Bus passes</td>
<td>By referral</td>
<td>Emergency rent funds</td>
<td>$5 per week for perfect attendance</td>
</tr>
<tr>
<td><strong>Community-based organizations</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>$1 per hour if on AFDC, otherwise $2 per hour, during education and training</td>
<td>Included in needs-based payment</td>
<td>By referral</td>
<td>None</td>
<td>None</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>$23-$30 per week during education, $30 per week during JTPA training</td>
<td>Included in needs-based payment; tokens available otherwise</td>
<td>By referral, $15 per week for expenses</td>
<td>Free breakfasts</td>
<td>$5 for weekly academic progress; $5 for perfect weekly attendance^d</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>$1 per hour, for farm-workers only</td>
<td>Bus passes for farm-workers and others who demonstrate need</td>
<td>On-site and by referral</td>
<td>Weekly food bank to provide free groceries</td>
<td>None</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>$6 per day</td>
<td>Included in needs-based payment</td>
<td>By referral</td>
<td>None</td>
<td>None</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>None</td>
<td>Bus passes</td>
<td>By referral</td>
<td>None</td>
<td>None</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>$8 per day</td>
<td>Included in needs-based payment</td>
<td>On-site for children over 18 months and by referral</td>
<td>None</td>
<td>$20 for each grade-level gain in reading; $20 for passing GED pre-test; $40 for passing GED test; $45 for &quot;A&quot; average throughout occupational training, $25 for &quot;B&quot; average</td>
</tr>
</tbody>
</table>

**SOURCE:** Adapted from Auspos et al., 1989.

**NOTES:**

^a At intervals, this site combined transportation and needs-based payments into one $7 per day payment.

^b Available during 1986-87 school year.


^d Available after October 1987.
The remaining seven sites did not focus as systematically on life skills, instead incorporating these topics into the training curriculum, counseling or group discussion sessions, or occasional lectures.

Youths cited personal attention from staff as a crucial aid in helping them move toward self-sufficiency. While agencies that traditionally served disadvantaged youths typically offered these support services from the beginning of the demonstration, a number of sites accustomed to serving adults increased this type of activity as their programs evolved.

Clearly, the support services and other activities available at the Job Corps Centers surpassed those at the other sites in both breadth and intensity. To a large extent, the JOBSTART participants in these sites were able to partake of the full array of Job Corps activities, including recreational, health, and food services. However, Allentown in Buffalo, Connelley in Pittsburgh, and SER/Corpus Christi also offered high levels of these services.

D. Job Placement Services

Sites were required to assist youths in finding training-related employment, but this phase of the program typically received less attention than others. Nearly all the sites did provide instruction about employers' expectations as well as job search techniques. About one-half of the sites arranged paid or unpaid part-time work experience positions for some participants during the program. Approximately one-fourth of a sample of participants worked at some point—in program-arranged or self-initiated jobs—while they were active in the program. Those in the sample who were employed worked an average of about 50 percent of the weeks they were in the JOBSTART program and were employed an average of about 30 hours per week during the weeks they worked. During the months they worked, their hours of classes in JOBSTART were lower than were those of nonworking participants.

Efforts to find participants permanent employment typically began near the end of training, with instructor contacts serving as an important source of information about job openings. Since many youths left the program without reaching this stage, it is not surprising that only about one-fourth of participants reported that program staff referred them to a job or told them about openings.

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9The life skills curricula in these sites was oriented around daily living, with units on health education, substance abuse prevention, sexuality and family planning, personal finances, civics, communication skills, goal-setting and planning, and improving one's self-esteem.
Job placement assistance was especially strong at CET/San Jose, Chicago Commons, and the Job Corps program in Phoenix. In all these sites, instruction in proper work behavior, employer expectations, and job search techniques began while students were still in training; placement specialists provided leads and assistance in finding a job; and CET/San Jose and Chicago Commons had especially strong ties to local employers.

Job placement assistance was noticeably weaker at Allentown in Buffalo, BSA in New York City, and CREC in Hartford (the three sequential/brokered sites), and at the Atlanta Job Corps, the East Los Angeles Skills Center, EGOS in Denver, and SER/Corpus Christi (among the concurrent sites). At the first three of these sites, job placement was intended to be the responsibility of the training agency, but most participants never were active in that component so only informal assistance was available from the JOBSTART agency. Those concurrent sites with weak job placement typically lacked any or sufficient job development specialists on staff, were larger agencies with no special emphasis on placing JOBSTART youths, or contracted out job placement to another organization that did not see the JOBSTART youths as a high-priority group.

E. Scheduling, Daily Service Mix, and Planned Program Duration

Sites also varied in the way they scheduled classes and the expected duration of their programs. The demonstration sites scheduled JOBSTART classes in three basic ways. The majority of sites that operated both the education and training components themselves scheduled the classes on an "open-entry/open-exit" basis. This means that participants could enter the program at any time, progress through the material at their own pace, and complete the course whenever they reached the specified competency levels. The duration of training was open-ended, but sites anticipated that participants would typically be able to complete the prescribed training curriculum in many fields in approximately 600 to 800 hours. Individuals who needed additional time to complete competencies could stay longer, however.

In a second program variation, some concurrent sites operated JOBSTART as a series of "fixed cycles," meaning that all participants started and completed training together on

10 These sites included concurrent sites (the Atlanta Job Corps, CET/San Jose, the East Los Angeles Skills Center, and the Phoenix Jobs Corps) and sequential/in-house sites (El Centro in Dallas and the Los Angeles Job Corps).
specified dates and the maximum length of training was prescribed. In a third variation, the three sequential/brokered sites operated the education component on an open-entry/open-exit schedule, but the training schedule was determined by the variety of training organizations at which JOBSTART participants were enrolled.

Sites also showed great variety in the number of hours scheduled for activities each day. The usual schedule ranged from a low of three hours per day at CREC in Hartford to seven to eight hours per day in some courses at Chicago Commons. A typical day can be described in terms of three basic models:

- **Concurrent sites that were CBOs or schools.** Students typically had six hours of classes per day, five days a week. In general, two hours were spent in education classes, with training classes scheduled for the remaining four hours.

- **Concurrent sites that were Job Corps Centers.** These sites had six and a half class hours per day. Schedules were highly individualized and changed frequently, but commonly included two hours of education, two and a half hours of vocational training, and two hours devoted to life skills, health education, or avocational activities such as sports.

- **Sequential sites.** These also scheduled a six-hour day during the education phase, but the daily distribution of activities was quite different. Typically, three hours were spent in education classes and another three hours were spent in life skills training. The training schedules were set by the training providers at the brokered sites, but typically involved five to six hours of classes per day. Training classes ran for six hours a day at the sequential/in-house sites.

The duration of the occupational training component also varied among the sites, ranging from 22 to 23 weeks at SER/Corpus Christi to a year at the Job Corps sites. Even within a site, there could be significant variation among the different training options. At Chicago Commons, for example, scheduled training ranged from 500 hours in industrial inspection to 1,380 hours in packaging-machine repair.

This diversity in scheduled daily hours and program duration meant that the planned participation hours for youths varied greatly across the sites, with the greatest variation showing in the training component. At SER/Corpus Christi, a participant completing education and

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11These sites included Chicago Commons, Connelley in Pittsburgh, EGOS in Denver, and SER/Corpus Christi.
training in about six months, as planned, would have had no more than 660 hours of occupational training. In contrast, one training course at Chicago Commons totaled nearly 1,400 hours, and a sequential program such as the one operated by the Los Angeles Job Corps could last for as long as a year.¹²

F. Summary of Program Implementation by Site

Table 3.4 rates the implementation of the four central JOBSTART components in each site. (See Appendix Table D.1 for the details behind these ratings.) The information in Table 3.4 and the material already presented in this section suggest that the four key components of the JOBSTART program were implemented most successfully at CET/San Jose,¹³ Chicago Commons, Connelley in Pittsburgh, El Centro in Dallas, the Los Angeles Job Corps, and the Phoenix Job Corps and least successfully at the Atlanta Job Corps, BSA in New York City, CREC in Hartford, and EGOS in Denver. To summarize the ratings by component:

- **Education.** Most sites that chose to operate a separate education component were able to offer an activity meeting the JOBSTART guidelines. The two sites with noticeably weak education activities were the Atlanta Job Corps (where unclear objectives for education and staff turnover hampered implementation) and CREC in Hartford (where computer facilities were under-utilized and poor attendance was a serious problem).

- **Training.** The training component showed the most variation – primarily because in sequential/brokered sites most youths never participated in training. In addition, the limited training offerings, less experienced staff, and older equipment of SER/Corpus Christi (reflecting the common problems of a community-based organization) hampered its ability to implement the training component.

- **Support services.** Although the Job Corps Centers did offer substantially more services than other sites, all programs were able to provide the limited types of support services called for in the JOBSTART guidelines: assistance in arranging and/or financing child care and transportation to and from coursework. Allentown in Buffalo, Connelley in Pittsburgh, and SER/Corpus Christi, in addition to the three Job Corps Centers, provided a noticeably longer list of services, including better needs-based

¹²Job Corps Centers offer a maximum of two years of training, but JOBSTART participants were only to be enrolled in courses that could be completed in one year.

¹³CET/San Jose provided most of its basic education services within training activities, so this overall characterization reflects a judgment about the other three components and the way in which education was incorporated into the training component.
<table>
<thead>
<tr>
<th>Site</th>
<th>Concurrent</th>
<th>Sequential/in-house</th>
<th>Sequential/brokered</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Education</td>
<td>Training</td>
<td>Support Services</td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>Medium</td>
<td>Medium</td>
<td>High</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>No rating</td>
<td>High</td>
<td>Medium</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>Medium</td>
<td>High</td>
<td>High</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>High</td>
<td>High</td>
<td>High</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>Medium</td>
<td>Medium</td>
<td>Medium</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>Medium</td>
<td>Medium</td>
<td>Medium</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>Medium</td>
<td>High</td>
<td>High</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>High</td>
<td>Low</td>
<td>High</td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC operations staff.

**NOTES:** See Appendix Table D.1 for details of the implementation of components in each site.

*aIn this site, a separate rating of the education component was inappropriate because education and training were more integrated than in other sites and staff strongly emphasized training over passing the GED examination.*
payments, life skills training and counseling, and a method of identifying service needs and making referrals of youths to other agencies providing the required services.

- **Job placement assistance.** Most sites offered job placement assistance that fell short of the JOBSTART guidelines, either because many youths never received the service (especially in sequential/brokered sites) or too few staff with a specialty in job search assistance were assigned to work with the JOBSTART youths. As mentioned earlier, job placement assistance was especially strong at CET/San Jose, Chicago Commons, and the Phoenix Job Corps.

II. **The Intensity of JOBSTART Participation**

Participation in JOBSTART among experimentals was measured by participation rates in each activity, hours of participation in each activity, and overall length of stay. Table 3.5 shows these summary measures for all experimentals in the impact sample:

- **Participation rates.** Nearly 90 percent of all experimentals in the impact sample participated in JOBSTART to some extent. Eighty-six percent of all experimentals (and nearly all of those who were active in JOBSTART) attended basic skills education classes, while 67 percent participated in training, and 40 percent participated in other activities, which were optional for sites.

- **Participation hours.** Average hours were 128 in education, 249 in training, and 37 in other activities, for a total of 415 hours.14 Forty percent of all experimentals spent fewer than 201 hours in all JOBSTART activities; 26 percent spent 201 to 500 hours, and 35 percent spent more than 500 hours.

- **Length of stay.** The average length of stay was 6.8 months, with the median length being slightly less, 6 months; 78 percent of experimentals were active for 3 months or more, while 54 percent stayed in the program for 6 months or more. This was measured from the time of random assignment through the last month that included any hours of participation.15 Sixteen percent of the experimental sample were still

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14These averages and those cited in the next paragraph include the 11 percent of the sample with zero hours of JOBSTART activities.

15The period of participation could include months of inactivity if a person stopped attending classes and then returned to the program within the 12-month follow-up period. However, this does not appear to have been a common pattern. Among a sample of participants, about 85 percent did not have any months of inactivity within the period they were counted as active, and among those with inactivity, the average period of inactivity was about two months. Youths who attended JOBSTART were counted as (continued...)
<table>
<thead>
<tr>
<th>Activity Measure</th>
<th>Experimentals</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percent participating in</td>
<td></td>
</tr>
<tr>
<td>Any activity</td>
<td>88.7</td>
</tr>
<tr>
<td>Education</td>
<td>85.9</td>
</tr>
<tr>
<td>Training</td>
<td>66.6</td>
</tr>
<tr>
<td>Education and training</td>
<td>64.4</td>
</tr>
<tr>
<td>Other activities</td>
<td>40.0</td>
</tr>
<tr>
<td>Average hours in</td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>128.1</td>
</tr>
<tr>
<td>Training</td>
<td>248.9</td>
</tr>
<tr>
<td>Education and training</td>
<td>377.0</td>
</tr>
<tr>
<td>Other activities</td>
<td>37.3</td>
</tr>
<tr>
<td>All activities</td>
<td>414.8</td>
</tr>
<tr>
<td>Percentage distribution of hours in education and training</td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>11.9</td>
</tr>
<tr>
<td>Up to 200</td>
<td>33.2</td>
</tr>
<tr>
<td>201 to 500</td>
<td>22.4</td>
</tr>
<tr>
<td>501 to 700</td>
<td>15.5</td>
</tr>
<tr>
<td>701 or more</td>
<td>17.0</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
</tr>
<tr>
<td>Percentage distribution of hours in all activities</td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>11.3</td>
</tr>
<tr>
<td>Up to 200</td>
<td>28.6</td>
</tr>
<tr>
<td>201 to 500</td>
<td>25.5</td>
</tr>
<tr>
<td>501 to 700</td>
<td>15.2</td>
</tr>
<tr>
<td>701 or more</td>
<td>19.5</td>
</tr>
<tr>
<td>Total</td>
<td>100.1</td>
</tr>
<tr>
<td>Length of stay (months)</td>
<td></td>
</tr>
<tr>
<td>Average</td>
<td>6.8</td>
</tr>
<tr>
<td>Median</td>
<td>6.0</td>
</tr>
<tr>
<td>Percent still participating in month</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>78.0</td>
</tr>
<tr>
<td>6</td>
<td>53.6</td>
</tr>
<tr>
<td>9</td>
<td>30.6</td>
</tr>
<tr>
<td>12</td>
<td>16.4</td>
</tr>
<tr>
<td>15</td>
<td>9.6</td>
</tr>
<tr>
<td>18</td>
<td>4.8</td>
</tr>
<tr>
<td>19 or later</td>
<td>3.7</td>
</tr>
<tr>
<td>Number of experimentals</td>
<td>949</td>
</tr>
</tbody>
</table>

(continued)
TABLE 3.5 (continued)

**SOURCE:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

**NOTES:** Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. Distributions may not total 100.0 percent because of rounding.
active in the program in the twelfth month after random assignment, while 10 percent were still active in the fifteenth month.

These findings show that JOBSTART succeeded in engaging more than half of the youths in the experimental group in the program and its activities, but that for about 40 percent of them, participation was quite low and JOBSTART did not constitute an intensive or lengthy program. Because of this wide range of participation levels, with some people having very few hours, the average total hours for the sample as a whole is the equivalent of slightly less than three and one-half months of regular attendance for six hours per day. Most people in the sample did not participate long enough to get a GED or complete a training course.

To place these results in context, JOBSTART participation may be compared to reported participation in other programs for young, disadvantaged school dropouts. Length of participation is a simple measure that permits comparisons with three types of youth programs: JTPA Title IIA programs for young dropouts, the Job Corps, and Supported Work.\(^\text{16}\) JTPA Title IIA typically provides relatively short-term activities, while the Job Corps and Supported Work have been among the most intensive employment and training programs for disadvantaged youths. In these comparisons, either the average or median length of participation is used, depending on the available data.

Overall, JOBSTART participants stayed in the program considerably longer than did young dropouts in JTPA Title IIA activities, as shown in Table 3.6. During program year 1986, when the demonstration was in operation, the median length of participation for all young dropouts in JTPA Title IIA programs was 3.4 months compared to 6 months for JOBSTART.\(^\text{17}\) JOBSTART's median length of participation exceeded that of young dropouts participating for the entire month in which they were randomly assigned and all months in which they showed any JOBSTART hours. The measure might have overestimated the length of participation somewhat when a youth was randomly assigned late in a month or ended participation early in a month.

\(^{16}\)For information on the Job Corps, see Richardson and Burghardt, 1985, and U.S. Department of Labor, 1987. On Supported Work, see Maynard, 1980. For JTPA Title IIA, see U.S. Department of Labor, 1988. Hours of attendance are not reported for all programs, hence this comparison uses length of stay, for which the data are available.

\(^{17}\)The average length of participation in JTPA Title IIA programs is not available from published sources. The figure for JTPA Title IIA includes only persons who actually participated, while the JOBSTART figure includes the 11 percent of the sample made up of nonparticipants with 0 months of activity.
### TABLE 3.6
PARTICIPATION AND LENGTH OF STAY FOR YOUNG DROPOUTS IN JTPA TITLE IIA, BY ACTIVITY

<table>
<thead>
<tr>
<th>Activity</th>
<th>Percentage Distribution of Youths in JTPA</th>
<th>Median Length of Stay (Months)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Classroom activities</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Basic education</td>
<td>22.8</td>
<td>3.71</td>
</tr>
<tr>
<td>Occupational skills training</td>
<td>15.6</td>
<td>3.98</td>
</tr>
<tr>
<td>Combined basic education and occupational skills training&lt;sup&gt;a&lt;/sup&gt;</td>
<td>4.6</td>
<td>6.97</td>
</tr>
<tr>
<td>Total</td>
<td>42.9</td>
<td>3.97</td>
</tr>
<tr>
<td>On-the-job training</td>
<td>12.2</td>
<td>3.14</td>
</tr>
<tr>
<td>Job search assistance</td>
<td>15.3</td>
<td>0.81</td>
</tr>
<tr>
<td>Work experience</td>
<td>7.8</td>
<td>3.67</td>
</tr>
<tr>
<td>Other services</td>
<td>21.8</td>
<td>3.59</td>
</tr>
<tr>
<td>Any activity</td>
<td>100.0</td>
<td>3.40</td>
</tr>
</tbody>
</table>


**NOTES:** This table includes data for young dropouts served under JTPA Title IIA during program year 1986.

<sup>a</sup>JTPA data (as recorded by the U.S. Department of Labor, Division of Performance Management and Evaluation, 1988) combined basic education and occupational skills training under the label CT-Other.
in all JTPA components except one. The exception was a program combining basic education and occupational skills training, a mix similar to JOBSTART's, which had a median length of 7 months but was offered to only 5 percent of all young dropouts in JTPA Title IIA activities. For JOBSTART participants active in both education and skills training, the median length of stay in the program was also approximately 7 months. These findings support the conclusion that JOBSTART achieved its goal of operating a program more intensive than that typically offered in JTPA Title IIA programs for young dropouts.

JOBSTART's average length of participation was similar to those of the Job Corps and Supported Work. During program year 1986, the average stay in the Job Corps was 6.9 months, compared to JOBSTART's average of 6.8 months. Supported Work was an experimental program of paid work experience under conditions of gradually increasing responsibility on the job, close supervision, and work in association with a crew of peers. It operated from 1975 to 1979 and included young school dropouts, many with a criminal record, as one of its target groups. While precise comparisons are impossible, the length of participation in the two programs appears to have been similar. The average length of participation in Supported Work was 6.7 months (close to that in JOBSTART) and the median was approximately 6 months (the same as in JOBSTART), but 25 percent of Supported Work participants were still active in the program at 12 months after random assignment, as opposed to 16 percent for JOBSTART.

In summary, while only approximate comparisons can be made, it appears that JOBSTART achieved its goal of providing young school dropouts with more intensive education and training than is usual within the JTPA system. The data also suggest that JOBSTART offered an intensity of activity close to that of the Job Corps and Supported Work, which operated through special agencies and had the sole mission of providing services to very disadvantaged individuals. However, for the 40 percent of the JOBSTART sample who did not participate or had very few hours of activity, the treatment was unlikely to be intense and lengthy enough to move them above the threshold of skills needed to secure significantly better jobs than they could before the program.

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18The median for the Job Corps is not available.
19The JOBSTART measure of length of participation included some periods of inactivity in the midst of participation, while the Supported Work measure factored these out. As discussed above, however, this problem does not appear to have been serious in the JOBSTART data.
III. Moving Behind the Aggregate Participation Measures

Aggregate measures, however, tell only part of the story. Table 3.5 makes clear that JOBSTART was not the same experience for all youths: 40 percent participated for 200 or fewer total hours, while nearly 20 percent exceeded 700 hours, the required offering under the demonstration (200 hours of education and 500 hours of training). Clear differences in average participation also existed among the sites, as discussed later in this chapter. Understanding the sources of these variations in participation is the first step in developing ways to improve the design and implementation of the program.

The following analysis begins with subgroups of JOBSTART participants. It shows that while there were differences among subgroups, they did not seem to account for all the variation in participation. This implies that factors such as unmeasured differences among youths, local employment opportunities, and program characteristics associated with particular sites may also have affected participation.

The key finding on program characteristics is that youths in sites operating sequential/brokered programs tended to have lower rates of participation in occupational skills training, although they tended to receive more intensive instruction in basic skills.

A. Differences in Participation Among Subgroups

Although JOBSTART participants all satisfied the program's eligibility requirements, they varied in gender, age, marital and parental status, criminal records, and educational attainment, among other characteristics. Research and operational experience suggest that these types of factors can influence participation in programs.20

Among JOBSTART participants, two groups are of special concern: males (who have often been hard to recruit and retain in programs) and young mothers (a group at risk of long-term welfare receipt). As Table 3.7 shows, average total hours and other measures of participation were similar for all males and females, although a higher percentage of females were active in the twelfth month after random assignment. There were some differences, however, for females living with their children, compared to other women: Mothers averaged

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### TABLE 3.7
PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY, BY GENDER AND PARENTAL STATUS

<table>
<thead>
<tr>
<th>Activity Measure</th>
<th>Men</th>
<th>Women</th>
<th>Men</th>
<th>Women</th>
<th>Men</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percent participating in</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Any activity</td>
<td>89.0</td>
<td>89.6</td>
<td>87.4</td>
<td>88.5</td>
<td>88.7</td>
</tr>
<tr>
<td>Education</td>
<td>85.8</td>
<td>87.6</td>
<td>84.3</td>
<td>85.9</td>
<td>85.9</td>
</tr>
<tr>
<td>Training</td>
<td>67.4</td>
<td>69.6</td>
<td>62.5</td>
<td>65.9</td>
<td>66.6</td>
</tr>
<tr>
<td>Education and training</td>
<td>64.6</td>
<td>68.4</td>
<td>60.2</td>
<td>64.2*</td>
<td>64.4</td>
</tr>
<tr>
<td>Other activities</td>
<td>36.1</td>
<td>41.6</td>
<td>45.2</td>
<td>43.4</td>
<td>40.0**</td>
</tr>
<tr>
<td>Average hours in</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>120.2</td>
<td>119.6</td>
<td>149.4</td>
<td>134.8**</td>
<td>128.1</td>
</tr>
<tr>
<td>Training</td>
<td>258.5</td>
<td>250.5</td>
<td>231.4</td>
<td>240.7</td>
<td>248.9</td>
</tr>
<tr>
<td>Education and training</td>
<td>378.7</td>
<td>370.1</td>
<td>380.8</td>
<td>375.6</td>
<td>377.0</td>
</tr>
<tr>
<td>Other activities</td>
<td>31.6</td>
<td>36.2</td>
<td>47.8</td>
<td>42.1**</td>
<td>37.3***</td>
</tr>
<tr>
<td>All activities</td>
<td>410.3</td>
<td>408.1</td>
<td>428.5</td>
<td>418.5</td>
<td>414.8</td>
</tr>
<tr>
<td>Percentage distribution of hours in</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>education and training</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>11.4</td>
<td>11.2</td>
<td>13.4</td>
<td>12.3</td>
<td>11.9</td>
</tr>
<tr>
<td>Up to 200</td>
<td>31.7</td>
<td>36.8</td>
<td>32.2</td>
<td>34.4</td>
<td>33.2</td>
</tr>
<tr>
<td>201 to 500</td>
<td>24.0</td>
<td>19.2</td>
<td>23.0</td>
<td>21.1</td>
<td>22.4</td>
</tr>
<tr>
<td>501 to 700</td>
<td>16.7</td>
<td>16.0</td>
<td>13.0</td>
<td>14.5</td>
<td>15.5</td>
</tr>
<tr>
<td>701 or more</td>
<td>16.2</td>
<td>16.8</td>
<td>18.4</td>
<td>17.6</td>
<td>17.0</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
<td>100.0</td>
<td>100.0</td>
<td>99.9</td>
<td>100.0</td>
</tr>
<tr>
<td>Percentage distribution of hours in</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>all activities</td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>11.0</td>
<td>10.4</td>
<td>12.6</td>
<td>11.5</td>
<td>11.3</td>
</tr>
<tr>
<td>Up to 200</td>
<td>27.6</td>
<td>30.8</td>
<td>28.0</td>
<td>29.4</td>
<td>28.6</td>
</tr>
<tr>
<td>201 to 500</td>
<td>26.7</td>
<td>24.8</td>
<td>24.1</td>
<td>24.5</td>
<td>25.5</td>
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<tr>
<td>501 to 700</td>
<td>16.2</td>
<td>15.6</td>
<td>13.0</td>
<td>14.3</td>
<td>15.2</td>
</tr>
<tr>
<td>701 or more</td>
<td>18.5</td>
<td>18.4</td>
<td>22.2</td>
<td>20.4</td>
<td>19.5</td>
</tr>
<tr>
<td>Total</td>
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<td>100.0</td>
<td>99.9</td>
<td>100.1</td>
<td>100.1</td>
</tr>
<tr>
<td>Average length of stay (months)</td>
<td>6.4</td>
<td>7.2</td>
<td>7.1</td>
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<td>6.8**</td>
</tr>
<tr>
<td>Percent still participating in month</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>77.6</td>
<td>79.2</td>
<td>77.4</td>
<td>78.3</td>
<td>78.0***</td>
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<tr>
<td>6</td>
<td>52.5</td>
<td>55.6</td>
<td>53.6</td>
<td>54.6</td>
<td>53.6</td>
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<tr>
<td>9</td>
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<td>12</td>
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<td>7.5</td>
<td>11.6</td>
<td>11.1</td>
<td>11.4</td>
<td>9.6</td>
</tr>
<tr>
<td>18</td>
<td>3.4</td>
<td>6.8</td>
<td>5.4</td>
<td>6.1</td>
<td>4.8</td>
</tr>
<tr>
<td>19 or later</td>
<td>2.7</td>
<td>4.4</td>
<td>4.6</td>
<td>4.5</td>
<td>3.7</td>
</tr>
</tbody>
</table>

Number of experimentals: 438, 250, 261, 511, 949

(continued)
TABLE 3.7 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Among all women, the distributions compared are those for women who were living with their own child(ren) and those for women not living with their own child(ren), including those who were childless, at the time of random assignment. An F-statistic was used to test the hypothesis of equal column means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

*Includes women who did not have children.
somewhat fewer hours of participation, and a slightly higher percentage received fewer than 200 hours of services.

Table 3.8 presents average total hours of participation in JOBSTART for several other subgroups. Although past research and experience suggest that the characteristics listed in the table might affect participation, many of the comparisons do not show significant differences in average hours for the groups under review. Youths who had never been arrested since age 16 participated for significantly more hours.21 But other groupings did not show differences in hours.

B. Differences in Participation Among Sites

Hours of participation in the sites in the demonstration varied considerably, as shown in Table 3.9. Average total hours ranged from a high of 631 for experimentalists at the Los Angeles Job Corps to a low of 166 at CREC in Hartford, a spread of 465 hours. Another important aspect of variation was the percentage of experimentalists still participating in JOBSTART at 12 months after random assignment. This proportion varied from a low of zero percent at Chicago Commons and SER/Corpus Christi to a high of 42 percent at Allentown. The proportion still active at 18 months ranged from zero percent to 17 percent at the Los Angeles Job Corps. Thus, the 24 months of follow-up do not represent the same length of post-program follow-up at all sites.

As noted earlier, this variation could have had several possible sources, such as characteristics of the youths, local employment opportunities, and program characteristics.22 With only 13 sites in the demonstration, it is very difficult to isolate the effects on participation of the many differences among programs. If, for example, the sites with the most support services were also Job Corps sites and also operated a youths-only program, it would be impossible to separate out the effects of these individual factors on participation hours.

21 The mix of activities did differ by initial reading score. Those testing in the low group averaged slightly more hours in education, and had somewhat fewer hours in training, than those in the highest group.

22 Differences in the way random assignment interacted with site recruitment efforts, and also in attendance reporting, led to variations in participation rates as well. At CET/San Jose, for example, services were not available for the first part of the sample for up to one month. Furthermore, as discussed in Chapter 2, attendance at a multi-day assessment of occupational training interests was not included in reported hours. As a result, 36 percent of the experimentalists at that site had no reported hours in program services. At other sites, the gap between random assignment and reported program start-up was shorter and participation rates were higher.
TABLE 3.8
AVERAGE TOTAL PARTICIPATION HOURS, BY SELECTED CHARACTERISTICS
OF EXPERIMENTALS AT THE TIME OF RANDOM ASSIGNMENT

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Average Total Hours</th>
<th>Number of Experimentals</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Age</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-19</td>
<td>405.1</td>
<td>703</td>
</tr>
<tr>
<td>20 or 21</td>
<td>442.3</td>
<td>246</td>
</tr>
<tr>
<td><strong>Ethnicity</strong>a</td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>401.7</td>
<td>72</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>403.7</td>
<td>439</td>
</tr>
<tr>
<td>Hispanic</td>
<td>393.5</td>
<td>411</td>
</tr>
<tr>
<td><strong>School grade at time of dropout</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Grade 10 or below</td>
<td>423.6</td>
<td>548</td>
</tr>
<tr>
<td>Grade 11 or 12</td>
<td>402.6</td>
<td>401</td>
</tr>
<tr>
<td><strong>Reading grade level</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1-6</td>
<td>364.1</td>
<td>258</td>
</tr>
<tr>
<td>7-8</td>
<td>370.7</td>
<td>195</td>
</tr>
<tr>
<td>9 or above</td>
<td>302.3</td>
<td>18</td>
</tr>
<tr>
<td><strong>Gender</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td>418.5</td>
<td>511</td>
</tr>
<tr>
<td>Men</td>
<td>410.3</td>
<td>438</td>
</tr>
<tr>
<td><strong>Marital status</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever married</td>
<td>439.8</td>
<td>88</td>
</tr>
<tr>
<td>Never married</td>
<td>413.2</td>
<td>857</td>
</tr>
<tr>
<td><strong>Parental status</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women living with own child(ren)</td>
<td>408.1</td>
<td>250</td>
</tr>
<tr>
<td>Women not living with own child(ren)b</td>
<td>428.5</td>
<td>261</td>
</tr>
<tr>
<td><strong>AFDC benefits received</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>409.9</td>
<td>438</td>
</tr>
<tr>
<td>Own AFDC case</td>
<td>448.8</td>
<td>197</td>
</tr>
<tr>
<td>Household AFDC case</td>
<td>431.7</td>
<td>186</td>
</tr>
<tr>
<td><strong>Received occupational training within past year</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>420.0</td>
<td>797</td>
</tr>
<tr>
<td>Yes</td>
<td>387.3</td>
<td>152</td>
</tr>
<tr>
<td><strong>Criminal record</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No arrest since age 16</td>
<td>435.7</td>
<td>805</td>
</tr>
<tr>
<td>Arrested since age 16</td>
<td>297.8***</td>
<td>144</td>
</tr>
<tr>
<td><strong>Sample size</strong></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Sample size

(continued)

---

-85- 137
TABLE 3.8 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all 949 experimentals for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

An F-statistic was used to test the hypothesis of equal means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

a The sample also included 27 experimentals who were members of other ethnic groups.

b Includes women who did not have children.
### TABLE 3.9

**PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS, BY SITE**

<table>
<thead>
<tr>
<th>Activity Measure</th>
<th>Concurrent</th>
<th>Sequential/In-House</th>
<th>Sequential/Brokered</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Atlanta Job Corps</td>
<td>CEI/San Jose Commons (Pittsburgh)</td>
<td>East LA Skills Center (Denver)</td>
</tr>
<tr>
<td></td>
<td>81.8</td>
<td>44.0</td>
<td>81.1</td>
</tr>
<tr>
<td></td>
<td>78.8</td>
<td>60.0</td>
<td>91.9</td>
</tr>
<tr>
<td></td>
<td>78.8</td>
<td>40.0</td>
<td>81.1</td>
</tr>
<tr>
<td></td>
<td>84.8</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>Percent participating in</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>95.1</td>
<td>28.8</td>
<td>72.1</td>
</tr>
<tr>
<td>Training</td>
<td>149.8</td>
<td>336.9</td>
<td>372.8</td>
</tr>
<tr>
<td>Education and training</td>
<td>244.9</td>
<td>365.7</td>
<td>444.9</td>
</tr>
<tr>
<td>Other activities</td>
<td>50.7</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>All activities</td>
<td>295.7</td>
<td>365.7</td>
<td>444.9</td>
</tr>
<tr>
<td>Average hours in</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>18.2</td>
<td>36.0</td>
<td>8.1</td>
</tr>
<tr>
<td>Training</td>
<td>42.4</td>
<td>16.0</td>
<td>29.7</td>
</tr>
<tr>
<td>Education and training</td>
<td>24.2</td>
<td>13.3</td>
<td>18.9</td>
</tr>
<tr>
<td>Other activities</td>
<td>9.1</td>
<td>9.3</td>
<td>13.5</td>
</tr>
<tr>
<td>All activities</td>
<td>6.1</td>
<td>25.3</td>
<td>29.7</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
<td>99.9</td>
<td>99.9</td>
</tr>
</tbody>
</table>

| Percentage distribution of hours in education and training | | | | | | | | | | | | | | |
| None | 15.2 | 36.0 | 8.1 | 1.1 | 17.6 | 6.5 | 13.4 | 1.7 | 0.0 | 20.9 | 0.0 | 24.6 | 11.1 | 11.3 |
| Up to 200 | 33.3 | 16.0 | 29.7 | 19.1 | 27.5 | 48.4 | 29.9 | 15.1 | 34.6 | 16.5 | 37.8 | 27.7 | 64.4 | 28.6 |
| 201 to 500 | 30.3 | 13.3 | 18.9 | 22.3 | 21.6 | 24.7 | 20.9 | 40.3 | 35.8 | 21.7 | 24.3 | 29.2 | 15.6 | 25.5 |
| 501 to 700 | 9.1 | 9.3 | 13.5 | 21.3 | 15.7 | 11.8 | 13.4 | 42.9 | 8.6 | 7.6 | 14.9 | 3.1 | 4.4 | 15.2 |
| 701 or more | 12.1 | 75.3 | 29.7 | 36.2 | 17.6 | 8.6 | 22.4 | 0.0 | 21.0 | 33.9 | 23.0 | 15.4 | 4.4 | 19.5 |
| Total | 100.0 | 99.9 | 99.9 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.1*** |

| Percentage distribution of hours in all activities | | | | | | | | | | | | | | |
| None | 15.2 | 36.0 | 8.1 | 1.1 | 17.6 | 6.5 | 13.4 | 1.7 | 0.0 | 20.9 | 0.0 | 24.6 | 11.1 | 11.3 |
| Up to 200 | 33.3 | 16.0 | 29.7 | 19.1 | 27.5 | 48.4 | 29.9 | 15.1 | 34.6 | 16.5 | 37.8 | 27.7 | 64.4 | 28.6 |
| 201 to 500 | 30.3 | 13.3 | 18.9 | 22.3 | 21.6 | 24.7 | 20.9 | 40.3 | 35.8 | 21.7 | 24.3 | 29.2 | 15.6 | 25.5 |
| 501 to 700 | 9.1 | 9.3 | 13.5 | 21.3 | 15.7 | 11.8 | 13.4 | 42.9 | 8.6 | 7.6 | 14.9 | 3.1 | 4.4 | 15.2 |
| 701 or more | 12.1 | 75.3 | 29.7 | 36.2 | 17.6 | 8.6 | 22.4 | 0.0 | 21.0 | 33.9 | 23.0 | 15.4 | 4.4 | 19.5 |
| Total | 100.0 | 99.9 | 99.9 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.1*** |

| Average length of stay (months) | 5.5 | 4.4 | 4.5 | 10.1 | 5.4 | 7.1 | 6.5 | 5.7 | 5.8 | 8.1 | 10.7 | 5.5 | 6.3 | 6.8*** |

(continued)
| Activity Measure | Concurrent | | | | | | Sequential/ In-House | | | | | | Sequential/ Brokered | | | | | |
| | Atlanta Job Corps | CET/ San Jose | Chicago | Connelley | East LA | EGOS | Phoenix | SER/ LA Job Corps | El Centro | La (Dallas) Job Corps | Allentown | BSA CREC | Total |
| Percent still participating in month | | | | | | | | | | | | | | |
| 3 | 69.7 | 57.3 | 64.9 | 97.9 | 66.7 | 79.6 | 79.1 | 89.1 | 85.2 | 72.2 | 91.9 | 63.1 | 66.7 | 78.0*** |
| 6 | 36.4 | 40.0 | 45.9 | 76.6 | 51.0 | 57.0 | 50.7 | 60.5 | 44.4 | 48.7 | 78.4 | 38.5 | 40.0 | 53.6 |
| 9 | 21.2 | 21.3 | 8.1 | 64.9 | 27.5 | 34.4 | 32.8 | 0.0 | 24.7 | 38.3 | 55.4 | 76.2 | 28.9 | 30.6 |
| 12 | 12.1 | 8.0 | 0.0 | 24.5 | 7.8 | 21.5 | 19.4 | 0.0 | 6.2 | 26.1 | 41.9 | 15.4 | 22.2 | 16.4 |
| 15 | 6.1 | 1.3 | 0.0 | 17.0 | 0.0 | 7.5 | 6.0 | 0.0 | 0.0 | 23.5 | 29.7 | 10.8 | 11.1 | 9.6 |
| 18 | 3.0 | 0.0 | 0.0 | 11.7 | 0.0 | 2.2 | 1.5 | 0.0 | 0.0 | 16.5 | 10.8 | 3.1 | 4.4 | 4.8 |
| 19 or later | 3.0 | 0.0 | 0.0 | 7.4 | 0.0 | 0.0 | 1.5 | 0.0 | 0.0 | 13.0 | 10.8 | 1.5 | 4.4 | 3.7 |
| Number of experimentals | 33 | 75 | 37 | 94 | 51 | 93 | 67 | 119 | 81 | 115 | 74 | 65 | 45 | 949 |

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. An F-statistic was used to test the hypothesis of equal column means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

a The Phoenix Job Corps did not report hours spent by participants in life skills or avocational activities.
Further, the demonstration was not designed to address this type of question with the same rigor provided for comparisons of experimentals and controls. Applicants were randomly assigned to the experimental or control group, but there was no random assignment to various types of sites, and within each labor market there was usually only one site. This means that the power of the random assignment research design applies to differences between experimentals and controls (at a site or in the aggregate) and for differences among subgroups (as defined by pre-random assignment characteristics). Other types of comparisons, such as between types of sites, are inherently less reliable, and the strength of conclusions depends on the consistency of results across sites.23

Analysis presented in a previous report found that differences in experimentals' characteristics among the sites explained only a small part of the differences in average total hours.24 The analysis also found that labor market conditions affected participation: Sites with better employment opportunities, other things being equal, had lower average participation hours.25

The strongest influence on participation in JOBSTART appeared to be program structure: whether a site was concurrent, sequential/in-house, or sequential/brokered. Participation rates by component, participation hours, and percentage of time in education or training all differed among the three types of sites, as detailed in Table 3.9 and summarized in Table 3.10.26 Four conclusions about program structure can be drawn:

- **Average hours of participation varied by type of site.** Experimentals in sequential/in-house sites had the highest average participation hours, while those in sequential/brokered sites had by far the lowest because of very low average hours in training.

- **The mix of education, training, and other activities varied by type of site.** The concurrent sites, other than two Job Corps sites, did not offer the optional "other activities" and emphasized occupational training; as

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23See Chapter 6 for a more detailed discussion of the difficulty of making cross-site comparisons.
24See Auspos et al., 1989.
25This could have been because those participating in JOBSTART found a job more easily and left the program after fewer hours. Alternatively, sites in labor markets with low unemployment may recruit youths who have more unmeasured barriers to employment, are harder to work with in a program, and end up with fewer hours of participation.
26As discussed earlier, eight sites provided concurrent basic education and occupational skills training ("concurrent" sites); two provided education followed by training ("sequential/in-house" sites); and three provided education and then referred participants to other agencies for training ("sequential/brokered" sites).
### TABLE 3.10

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY, BY PROGRAM STRUCTURE

<table>
<thead>
<tr>
<th>Activity Measure</th>
<th>Concurrent</th>
<th>Sequential/In-House</th>
<th>Sequential/Brokered</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percent participating in</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>84.9</td>
<td>86.7</td>
<td>88.0</td>
<td>85.9</td>
</tr>
<tr>
<td>Training</td>
<td>85.2</td>
<td>51.0</td>
<td>25.5</td>
<td>66.6***</td>
</tr>
<tr>
<td>Education and training</td>
<td>81.5</td>
<td>51.0</td>
<td>25.5</td>
<td>64.4***</td>
</tr>
<tr>
<td>Other activities</td>
<td>14.2</td>
<td>87.8</td>
<td>69.0</td>
<td>40.0***</td>
</tr>
<tr>
<td>Average hours in</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>103.7</td>
<td>151.7</td>
<td>178.4</td>
<td>128.1***</td>
</tr>
<tr>
<td>Training</td>
<td>291.3</td>
<td>286.6</td>
<td>77.7</td>
<td>248.9***</td>
</tr>
<tr>
<td>Education and training</td>
<td>395.0</td>
<td>438.3</td>
<td>256.1</td>
<td>377.0***</td>
</tr>
<tr>
<td>Other activities</td>
<td>10.0</td>
<td>95.1</td>
<td>60.0</td>
<td>37.3***</td>
</tr>
<tr>
<td>All activities</td>
<td>405.0</td>
<td>535.6</td>
<td>316.2</td>
<td>414.8***</td>
</tr>
<tr>
<td>Percentage distribution of hours</td>
<td></td>
<td></td>
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<td></td>
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<tr>
<td>Percentage distribution of hours</td>
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<td></td>
</tr>
<tr>
<td>in education and training</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>11.4</td>
<td>13.3</td>
<td>12.0</td>
<td>11.9</td>
</tr>
<tr>
<td>Up to 200</td>
<td>26.9</td>
<td>37.8</td>
<td>47.8</td>
<td>33.2</td>
</tr>
<tr>
<td>201 to 500</td>
<td>24.8</td>
<td>17.3</td>
<td>20.7</td>
<td>22.4</td>
</tr>
<tr>
<td>501 to 700</td>
<td>19.9</td>
<td>8.7</td>
<td>9.2</td>
<td>15.5</td>
</tr>
<tr>
<td>701 or more</td>
<td>17.0</td>
<td>23.0</td>
<td>10.3</td>
<td>17.0</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
<td>100.1</td>
<td>100.0</td>
<td>100.0***</td>
</tr>
<tr>
<td>Percentage distribution of hours</td>
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<tr>
<td>Percentage distribution of hours</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>in all activities</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>None</td>
<td>20.9</td>
<td>13.4</td>
<td>1.7</td>
<td>11.3</td>
</tr>
<tr>
<td>Up to 200</td>
<td>16.5</td>
<td>29.9</td>
<td>15.1</td>
<td>28.6</td>
</tr>
<tr>
<td>201 to 500</td>
<td>21.7</td>
<td>20.9</td>
<td>40.3</td>
<td>25.5</td>
</tr>
<tr>
<td>501 to 700</td>
<td>7.0</td>
<td>13.4</td>
<td>42.9</td>
<td>15.2</td>
</tr>
<tr>
<td>701 or more</td>
<td>33.9</td>
<td>22.4</td>
<td>0.0</td>
<td>19.5</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
<td>100.0</td>
<td>100.0</td>
<td>100.1***</td>
</tr>
<tr>
<td>Average length of stay (months)</td>
<td>6.4</td>
<td>7.1</td>
<td>7.8</td>
<td>6.8***</td>
</tr>
<tr>
<td>Percent still participating in month</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>78.9</td>
<td>77.6</td>
<td>75.5</td>
<td>78.0***</td>
</tr>
<tr>
<td>6</td>
<td>55.5</td>
<td>46.9</td>
<td>54.9</td>
<td>53.6</td>
</tr>
<tr>
<td>9</td>
<td>27.2</td>
<td>32.7</td>
<td>38.6</td>
<td>30.6</td>
</tr>
<tr>
<td>12</td>
<td>12.3</td>
<td>17.9</td>
<td>27.7</td>
<td>16.4</td>
</tr>
<tr>
<td>15</td>
<td>5.3</td>
<td>13.8</td>
<td>18.5</td>
<td>9.6</td>
</tr>
<tr>
<td>18</td>
<td>2.6</td>
<td>9.7</td>
<td>6.5</td>
<td>4.8</td>
</tr>
<tr>
<td>19 or later</td>
<td>1.6</td>
<td>7.7</td>
<td>6.0</td>
<td>3.7</td>
</tr>
<tr>
<td>Number of experimentals</td>
<td>569</td>
<td>196</td>
<td>184</td>
<td>949</td>
</tr>
</tbody>
</table>
TABLE 3.10 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. An F-statistic was used to test the hypothesis of equal column means.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
a result, average training hours for experimentals amounted to 72 percent of average total hours. The sequential/brokered sites emphasized education and other non-training services, all of which were provided in-house. They had the highest average hours in education; training hours were only about 25 percent of average total hours. The sequential/in-house sites had the highest total average hours, hours in education and other non-training activities approximating those of the sequential/brokered sites, and hours in training like those of the concurrent sites.

- Sequential/brokered sites had difficulty moving participants from education to training. Only 26 percent of participants at sequential/brokered sites made the transition to occupational training, although those who made the transition did receive substantial training. This low rate of participation in training occurred because of the difficulty of linking participants with other organizations, in part because of the nature of typical JTPA Title IIA contracts. Possibly, it also arose because participants in these sites (which were primarily basic education organizations) were more interested in receiving a GED than occupational training.

- These relationships do not appear to have been the result of measured differences in participant characteristics or local employment opportunities. Even after adjustments for measured differences in participant characteristics and local employment opportunities, these patterns of participation among sites with different program structures still appear.

While these three categories of sites do clarify patterns of participation, the sites within each category were clearly not identical. Among the concurrent sites, EGOS in Denver stood out with especially low hours — possibly because of its very large size, which could have left the JOBSTART youths feeling isolated and disconnected from the program. CREC in Hartford, among the sequential/brokered sites, had very low hours because it scheduled only three hours of education per day and very few experimentals participated in training. Furthermore, CREC offered limited support services and moved several times during the demonstration, which disrupted program operations. The high total hours for sequential/in-house sites were primarily owing to the Los Angeles Job Corps, which had the highest

---

27The percentage of average total hours is calculated by dividing average training hours by average total hours. Percentages that follow are similarly calculated.

28When dummy variables for type of site were added as independent variables to a regression equation with individual demographic characteristics and a measure of local employment opportunities, the relationships still held.
average hours among all sites. El Centro in Dallas, the other site in this category, ranked slightly below the average for all sites in total hours.

IV. **Summary of the Features of the Sites**

The diversity of the sites within the general framework of the JOBSTART guidelines has been a theme of this chapter. But before moving on to consider the impacts of these programs, it is useful to summarize in one place the key features of each local JOBSTART program. Table 3.11 groups the sites by program structure (concurrent, sequential/in-house, sequential/brokered) and uses three types of measures to summarize JOBSTART implementation. First, it describes participation in JOBSTART for experimentals by presenting average total hours; average length of stay in the program; average hours per month in the program; and average hours of education and training. (Note that each of these measures includes the 11 percent of experimentals who did not participate in the program.) All of these items have been included in previous tables except for average hours per month, which is a measure of the extent to which program services were concentrated or spread out over time.

Table 3.11 also includes more subjective ratings of implementation, including a rating of the level of initial screening done by each site at intake. Initial screening was greatest at the three Job Corps Centers (which had special entrance criteria) and Chicago Commons (which had special requirements for entering its vocational training program). The table also includes ratings of job placement and support services (drawn from Table 3.4) and an overall assessment of JOBSTART implementation.29

Finally, the cost per experimental in each site's research sample, listed in the right column of this table, adds to the description of program implementation already presented in this chapter.30 Most programs tend to fall in the range of $4,500 to $6,500, but several fall

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29 Chapter 6, which discusses differences in impacts among the sites, contains a further summary table of each site's program.
30 See Appendix E for the details of these cost data, which include items not normally part of program budgets, such as the value of donated goods and services. These cost figures are not the ones that would be produced as part of a benefit-cost analysis that compares the impact of the program to the incremental cost of the services received by experimentals above the level of services received by controls. As is discussed in Chapter 4, the controls received substantial non-JOBSTART services. Thus the impact figures estimate the effect of the services received by experimentals above this base of services. Costs used to calculate a benefit-cost ratio would measure the resource cost of the extra services received by experimentals. The cost figures reported here are gross program costs, before subtracting the cost of services received by controls.
<table>
<thead>
<tr>
<th>Site</th>
<th>Total Hours</th>
<th>Length of Stay (Months)</th>
<th>Average Hours per Month</th>
<th>Hours of Education</th>
<th>Hours of Training</th>
<th>Level of Initial Screening</th>
<th>Rating of Job Placement</th>
<th>Rating of Support Services</th>
<th>Rating of Implementation</th>
<th>Operating Costs per Experimental ($)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>405</td>
<td>6.4</td>
<td>60</td>
<td>104</td>
<td>291</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>4,100\textsuperscript{b}</td>
</tr>
<tr>
<td>CET/San Jose\textsuperscript{c}</td>
<td>366</td>
<td>4.4</td>
<td>74</td>
<td>29\textsuperscript{d}</td>
<td>337</td>
<td>Low</td>
<td>High</td>
<td>Low</td>
<td>High</td>
<td>2,000</td>
</tr>
<tr>
<td>Chicago Commons\textsuperscript{c}</td>
<td>445</td>
<td>4.5</td>
<td>83</td>
<td>72\textsuperscript{d}</td>
<td>373</td>
<td>High</td>
<td>High</td>
<td>Medium</td>
<td>High</td>
<td>6,400</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>579</td>
<td>10.1</td>
<td>54</td>
<td>105</td>
<td>473</td>
<td>Medium</td>
<td>Medium</td>
<td>Medium</td>
<td>High</td>
<td>5,200</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>345</td>
<td>5.4</td>
<td>55</td>
<td>73</td>
<td>272</td>
<td>Medium</td>
<td>Low</td>
<td>Medium</td>
<td>Medium</td>
<td>4,900</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>274</td>
<td>7.1</td>
<td>33</td>
<td>126</td>
<td>148</td>
<td>Low</td>
<td>Low</td>
<td>Medium</td>
<td>Low</td>
<td>2,000\textsuperscript{b}</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>441</td>
<td>6.5</td>
<td>60</td>
<td>163</td>
<td>218</td>
<td>High</td>
<td>High</td>
<td>High</td>
<td>High</td>
<td>4,700\textsuperscript{b}</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>418</td>
<td>5.2</td>
<td>76</td>
<td>124</td>
<td>294</td>
<td>Medium</td>
<td>Low</td>
<td>High</td>
<td>Medium</td>
<td>2,100</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td>536</td>
<td>7.1</td>
<td>66</td>
<td>152</td>
<td>287</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>401</td>
<td>5.8</td>
<td>60</td>
<td>142</td>
<td>179</td>
<td>Medium</td>
<td>Medium</td>
<td>Medium</td>
<td>Medium</td>
<td>5,100</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>631</td>
<td>8.1</td>
<td>71</td>
<td>159</td>
<td>362</td>
<td>High</td>
<td>Medium</td>
<td>High</td>
<td>High</td>
<td>5,700\textsuperscript{b}</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>316</td>
<td>7.8</td>
<td>37</td>
<td>178</td>
<td>78</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td></td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>440</td>
<td>10.7</td>
<td>39</td>
<td>244</td>
<td>113</td>
<td>Medium</td>
<td>Low</td>
<td>Medium</td>
<td>High</td>
<td>5,900</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>280</td>
<td>5.5</td>
<td>45</td>
<td>145</td>
<td>66</td>
<td>Low</td>
<td>Low</td>
<td>Medium</td>
<td>Low</td>
<td>7,500</td>
</tr>
<tr>
<td>All sites</td>
<td>415</td>
<td>6.8</td>
<td>57</td>
<td>128</td>
<td>249</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td></td>
</tr>
</tbody>
</table>

\textbf{Sources:} MDRC calculations from JOBSTART enrollment form, MIS, and survey data (participation figures); MDRC operations staff (implementation ratings); Appendix E (costs).

\textbf{Notes:} \textsuperscript{a} These cost estimates are preliminary and therefore rounded to the nearest \\textcent. All costs are in 1986 dollars.

\textsuperscript{b} The estimates do not include the cost of providing on-site medical and dental services. The value of these services per JOBSTART experimental was approximately $400 in the Atlanta site, $400 in Phoenix, $600 in Los Angeles, and $24 in Denver.

\textsuperscript{c} In this site, education and training were more integrated than in other sites, and staff strongly emphasized training over passing the GED examination.

\textsuperscript{d} In this site, some education hours are included in the training component hours.
well outside this range. The variation in program costs is substantial, reflecting several factors:

- **Enrollment levels.** Programs serving a higher volume of participants relative to their institutional capacity can spread the fixed costs of operation over many people, thereby lessening the average cost. EGOS in Denver, a large adult vocational school with more than 15,000 students, thus had very low costs per experimental. In contrast, BSA in New York City had difficulty enrolling enough students to fill all of its available slots and as a result had very high costs per experimental.

- **Intensity of the planned components.** More ambitious training or support services cost more per month of operation. This was the case at Chicago Commons, where the training options were often quite intensive.

- **Average length of participation.** Since costs per experimental depend on the cost per unit of service and the number of units used, sites with longer participation, such as the Los Angeles Job Corps, have higher costs.

- **Staff salary levels.** Personnel costs are the most important part of costs, so an agency’s salary structure and level are major determinants of cost. The low average cost of SER/Corpus Christi is partly explained by that site’s relatively low personnel expenses.

The following two chapters present program impacts, for the sample as a whole and for key subgroups defined by individual characteristics. The final chapter of the report returns to the issue of site variation, in reviewing the pattern of program impacts among the 13 sites.
CHAPTER 4

RECEIPT OF EDUCATION AND SKILLS TRAINING
AND JOBSTART'S IMPACTS ON EDUCATIONAL ATTAINMENT

The goals of the JOBSTART program included increasing participation in education and training activities by a group who otherwise would be little served, and thereby enhancing their educational attainment, employability, and long-term earnings. Chapter 3 described the experimental group's participation in JOBSTART activities. This chapter takes the story a step farther by comparing the participation of these experimentals in JOBSTART (and other programs) with the activities of those in the control group, to determine whether the offer of JOBSTART services actually led to an increase in participation over what would have occurred otherwise. As will be shown, the youths in the experimental group did participate in more education and training than did those in the control group, but the control group was not unserved. Chapters 5 and 6 examine the extent to which this increase in services has so far led to improvements in employment, earnings, welfare receipt, and other longer-term outcomes.

This and succeeding chapters rest on the random assignment research design, described briefly in earlier chapters, to estimate the difference the JOBSTART program made in the lives of those young people given access to it. To evaluate these differences (often called program "impacts"), it is necessary to answer two basic questions. First, on average, what happened to those who were offered the program – in this context, the "experimental" group? Second, on average, what would have happened to them had they not been offered the program, here represented by the experience of the "control" group? The average effect, or "impact," of a program is the difference between the two groups in the many outcomes of interest.

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1 This was based on attendance information reported by the JOBSTART program operators for those in the experimental group.
2 In addition to using the JOBSTART attendance data from the program MIS, this chapter relies on survey data for both experimentals and controls gathered 12 and 24 months after random assignment. The survey data include participation in other programs as well as in JOBSTART.
3 Chapter 5 focuses on the story for the full impact sample and for subgroups defined by individual characteristics such as gender, while Chapter 6 treats cross-site differences in impacts.
4 Since, as shown in Chapter 2, assignment to JOBSTART was random, there were no systematic differences between experimentals and controls at enrollment, and outcomes for controls could be used to measure what would have happened to experimentals without the program.
This chapter addresses several key evaluation questions:

- Did the planned differential between experimentals and controls in the receipt of education and occupational skills training materialize? Was this differential maintained during the second year of follow-up, or had controls begun to catch up to experimentals?

- How did service receipt and the differential in service receipt compare for important subgroups such as men, young mothers, and other women?

- Did JOBSTART produce gains in educational attainment, as measured by receipt of high school diplomas and passing of the GED examination, during the first two years after random assignment? Were controls catching up to the experimentals in educational attainment during the second year?

- How did the educational attainment differential compare for important subgroups such as men and women?

To summarize the basic findings in this chapter, access to JOBSTART did substantially increase the experimentals' participation in education and training activities, raising their rates and average hours of participation well above those of controls, almost half of whom were also active in these types of programs. This greater service receipt among experimentals occurred for all important subgroups in the sample; it led to substantially higher rates of receipt of a GED or high school diploma among experimentals than among controls for the full impact sample and for most subgroups.

I. Receipt of Education and Training by Experimentals and Controls

The JOBSTART control group was used as a benchmark for measuring program impacts, but if most controls received services similar to those received by experimentals, the benchmark would have been useless, and it would be very difficult to evaluate JOBSTART. Because JOBSTART targeted disadvantaged dropouts with poor reading skills (a group thought

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5The service receipt differences reported here are calculated by comparing the experiences of experimentals and controls as is done for analysis of the impact of JOBSTART on outcomes such as educational attainment and employment. These service receipt differences are not normally thought of as program impacts because they are an intermediate step in reaching the final program goals. This section uses the terminology of program impacts at times when it simplifies the presentation of the results. This terminology is useful here because the chapter discusses differences among subgroups in the size of these experimental-control differences; the use of the term impact for the experimental-control difference simplifies sentences.
hard to serve within the JTPA system), demonstration planners anticipated that JOBSTART controls would not be served to any great extent.

However, JOBSTART controls were expected to receive some services. Even though many performance-driven programs seem to screen out people with low reading levels, JOBSTART recruits (including those who ended up in the control group) were more determined to receive help than was the average school dropout. As a result, at some point within the first 24 months following random assignment, 44 percent of controls found remedial or occupational instruction elsewhere. The program impacts on educational attainment, employment, and earnings presented here, therefore, are the incremental impacts of JOBSTART over the mix of services available throughout the community to a group of poor readers, on their own initiative, without special referral from JOBSTART site operators.

A. In-Program and Post-Program Outcomes

All the events tracked by the JOBSTART program attendance reporting system and the follow-up survey (including program participation, GED receipt, employment, and other important outcomes) were reckoned from the date of random assignment, not the date of termination from the program.6 There was a great deal of variation in lengths of stay in JOBSTART. However, an approximate dividing line between predominantly in-program and predominantly post-program periods is no earlier than about the end of the twelfth month after random assignment. About 84 percent of experimentals had stopped participating in JOBSTART by then. (See Table 3.5.)

Table 4.1 shows that, over the two-year period as a whole, 92.7 percent of experimentals and 44.2 percent of controls received some education or training, for an impact of 48.4 percentage points. This impact was statistically significant, that is, too big to have arisen entirely by chance. As shown in Figure 4.1, the proportion of experimentals in programs, mainly JOBSTART, was highest during the first three months and fell rather steadily over time, to 11.2 percent during month 24. The proportion of controls in programs was much smaller at the beginning of the follow-up period, peaked at about 15 percent during months

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6This is a different approach from that used in the Job Corps study, in which follow-up began at termination from the program. As will become clear in Chapter 5, starting follow-up with entry into the program allows a careful analysis of the forgone earnings caused by participation in the program. But this difference in approach complicates the comparison of JOBSTART and Job Corps impact findings.
### TABLE 4.1

**IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 24**

<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ever received any education or training, months 1-24</td>
<td>92.7%</td>
<td>44.2%</td>
<td>48.4***</td>
<td>0.000</td>
</tr>
<tr>
<td>Ever received any education or training, months 1-12</td>
<td>90.7</td>
<td>29.0</td>
<td>61.6***</td>
<td>0.000</td>
</tr>
<tr>
<td>Ever received any education or training, months 13-24</td>
<td>34.3</td>
<td>30.7</td>
<td>3.6*</td>
<td>0.089</td>
</tr>
</tbody>
</table>

Sample size

949 890

---

### FIGURE 4.1

**MONTHLY RECEIPT OF EDUCATION OR TRAINING, BY RESEARCH STATUS**

![Graph showing monthly receipt of education or training by research status]
TABLE AND FIGURE 4.1 (continued)

SOURCES FOR TABLE AND FIGURE 4.1: MDRC calculations from JOBSTART enrollment form, MIS, and survey data (Table 4.1); Appendix Table F.1 (Figure 4.1).

NOTES FOR TABLE AND FIGURE 4.1: Calculations for this table and figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
10 through 13, and then soon reached a plateau at about 10 to 12 percent, about even with the level to which experimentals had fallen.

Breaking the follow-up period into a predominantly in-program period (months one through 12) and a predominantly post-program period (months 13 through 24) reveals a more dramatic pattern of experimental and control differences. Table 4.1 shows that during the first 12 months of follow-up, 90.7 percent of experimentals participated in education or training compared to 29 percent of controls, for an impact of 61.6 percentage points. During the second 12 months, 34.3 percent of experimentals and 30.7 percent of controls were in education or training, for an impact of 3.6 percentage points. Thus, there is no evidence that controls are catching up with experimentals in education and training. On the contrary, experimentals had not fallen below controls in education and training even in the second year of follow-up; both groups had stabilized at about the same rate of service receipt by the end of the second year.

Hours of education or training followed a very similar pattern. Table 4.2 shows that over the two-year period, experimentals received an average of 619 hours, while controls received an average of 250 hours, for an impact of 369 hours. It is important to remember that these average figures for the two groups include those who did not participate and, therefore, had zero hours. As Figure 4.2 shows, average experimental hours peaked at about 70 hours per month during month two and then fell steadily, while control hours, always much lower, peaked during month nine and then stabilized at about 10 hours per month.

Among those 92.7 percent of experimentals and 44.2 percent of controls who received any services during the two-year period—that is, excluding those who received no services—experimentals averaged 668 hours, and controls averaged 566 hours. Because so many fewer controls received services, and those who did find services on their own were probably quite motivated, this difference in duration for those who received services may understate the true advantage in intensity for served experimentals. The served experimentals who were directly comparable in motivation to the served controls might well have been those who received many more than the average hours for all served experimentals.

Thus, the planned service differential between experimentals and controls materialized. Not only did experimentals receive education and training at vastly higher rates than did
TABLE 4.2
IMPACTS ON TOTAL HOURS OF EDUCATION OR TRAINING RECEIVED THROUGH MONTH 24

<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total hours of education or training received, months 1-24</td>
<td>619.16</td>
<td>250.14</td>
<td>369.02***</td>
<td>0.000</td>
</tr>
<tr>
<td>Total hours of education or training received, months 1-12</td>
<td>439.02</td>
<td>130.06</td>
<td>308.96***</td>
<td>0.000</td>
</tr>
<tr>
<td>Total hours of education or training received, months 13-24</td>
<td>180.14</td>
<td>120.08</td>
<td>60.06***</td>
<td>0.001</td>
</tr>
</tbody>
</table>

Sample size
949          890

FIGURE 4.2
MONTHLY AVERAGE HOURS OF EDUCATION OR TRAINING RECEIVED, BY RESEARCH STATUS

(continued)
TABLE AND FIGURE 4.2 (continued)

SOURCES FOR TABLE AND FIGURE 4.2: MDRC calculations from JOBSTART enrollment form, MIS, and survey data (Table 4.2); Appendix Table F.2 (Figure 4.2).

NOTES FOR TABLE AND FIGURE 4.2: Calculations for this table and figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

For experimentals, "hours of education or training" include JOBSTART hours from MIS data and non-JOBSTART hours from survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
controls throughout much of the 24-month follow-up period, but they also on average received many more hours. However, as noted earlier, the control group did receive some other services in the community. Thus, the findings presented in Chapters 4 through 6 represent the incremental impacts of JOBSTART above the existing level of service.

B. Differences in Receipt of Education and Training by Men and Women

Table 4.3 results from splitting the sample into three groups (men, women living with their own children, and other women) and repeating the calculations made for the full sample in Tables 4.1 and 4.2. In general, Table 4.3 shows that among the experimental group, men, women living with their own children, and other women tended to be active in all types of education and training activities in approximately similar amounts in the first year. However, there were clear differences in participation among the control groups for the three subgroups: Men in the control group participated in education and training less than did women living with their own children, who in turn participated less than other women. The larger differences between experimentals and controls for men among those who ever participated and in average total hours, therefore, result largely from the lower level of activity among men in the control group.

To summarize the service receipt differences shown in Table 4.3: During the two-year follow-up period, 92.8 percent of men in the experimental group and 36.6 percent in the control group were active in education or training, for a difference of 56.1 percentage points. This resulted in a difference of 424 average total hours between men in the experimental and control groups. For women living with their own children, 45.7 percentage points more experimentals were active during the two-year follow-up period, and experimentals averaged 344 more hours than did controls. For other women, the comparable figures were 38.4 percentage points and 303 hours.

---

8This finding is based on analysis of the proportion of experimentals in each group who "ever participated" in an activity during the period in question. The finding holds during the first year of follow-up and over the entire 24-month follow-up period. In the second year of follow-up (and over the entire two-year follow-up period), more "other women" participated in an activity (registering the highest average total hours in the second year). Women living with their own children had the next highest average total hours, and men had the lowest. These differences were largely attributable to differences in participation in non-JOBSTART activities. As reported in Chapter 3, participation in JOBSTART activities was similar among the three groups.
<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever received any education or training (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>92.8</td>
<td>36.6</td>
<td>56.1***</td>
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<tr>
<td>Ever received any education or training (%)</td>
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<tr>
<td>Months 1-24</td>
<td>92.5</td>
<td>54.1</td>
<td>38.4***</td>
<td>0.000</td>
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<tr>
<td>Months 1-12</td>
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<td>38.4</td>
<td>52.1***</td>
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<td>Months 1-24</td>
<td>672.35</td>
<td>369.05</td>
<td>303.30***</td>
<td>0.000</td>
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<td>Months 1-12</td>
<td>456.32</td>
<td>201.57</td>
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<td>216.03</td>
<td>167.48</td>
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<td>223</td>
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</table>

(continued)
TABLE 4.3 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities. For experimentals, "hours of education or training" include JOBSTART hours from MIS data and non-JOBSTART hours from survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

aThe column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
C. Differences in Receipt of Education and Training by People with Selected Characteristics

The primary goal of the evaluation is to estimate the difference that access to the JOBSTART program makes for its target population (in other words, the program's impacts). While the size of the JOBSTART sample is large enough for finding policy-relevant overall impacts, it provides considerably less statistical power for estimating subgroup impacts and differences in impacts among subgroups. Keeping this limitation in mind, this section presents an analysis of the difference in service receipt among experimentals and controls in various subgroups of youths and compares the size of this difference for the selected subgroups.

To summarize the findings of this section: The service receipt differences among experimentals and controls observed for the full sample were present and large for virtually all important subgroups. Observed differences in service receipt impacts among subgroups primarily reflected variation in the level of service receipt of controls.

Table 4.3 presents impacts calculated using the same methods as were used for the full sample: Impacts for the three subgroups were calculated by comparing the experiences of the experimentals in each group to those of the controls in the group. Table 4.4 uses a different method to calculate within-subgroup impacts and between-subgroups impact differences for the most important measure of program activity: receipt of education or training during the two years of follow-up available so far. It presents impacts for each subgroup, controlling for differences other than the characteristic used to define the subgroup.9

For example, the first three rows of Table 4.4 present impacts for women and men using statistical techniques to control for gender differences, such as in employment experience, educational levels, ethnicity, and parental status. It thus shows a comparison of the impacts by gender with other characteristics held constant. The first row of Table 4.4, in the column labeled "subgroup impact difference," shows that the impact for women was 13.4 percentage points below the impact for men, and this difference in service receipt impacts was statistically

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9The impact estimates in this table were produced by conducting a two-way analysis of covariance, controlling for differences in pre-random assignment characteristics other than the characteristic used to define the subgroup. See Ostle, 1975, p. 461. The adjustments were done using a linear regression model. Characteristics that affect outcomes and impacts with a nonlinear relationship are not controlled for with this procedure. And no such procedure can control for unmeasured characteristics that affect outcomes and impacts. The adjusted outcomes for men, women living with their own children, and other women presented in Table 4.3 are very similar to the unadjusted outcomes in Table 4.2, but the adjustments do make more of a difference for many of the other subgroups listed in the table.
# Table 4.4

## Impacts on Receipt of Education or Training through Month 24, by Selected Characteristics at the Time of Random Assignment

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Ever Received Any Education or Training, Months 1-24</th>
<th>Subgroup Impact</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Subgroup Impact Difference&lt;sup&gt;b&lt;/sup&gt;</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
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<td><strong>Gender</strong></td>
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<tr>
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<td>968</td>
<td>93.5%</td>
<td>51.4%</td>
<td>42.1***</td>
<td>0.000</td>
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<tr>
<td>Men</td>
<td>871</td>
<td>91.9%</td>
<td>36.4%</td>
<td>55.5***</td>
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<td>White, non-Hispanic</td>
<td>155</td>
<td>93.8%</td>
<td>38.9%</td>
<td>54.9***</td>
<td>0.000</td>
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<tr>
<td>Black, non-Hispanic</td>
<td>840</td>
<td>91.9%</td>
<td>47.1%</td>
<td>44.7***</td>
<td>0.000</td>
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<tr>
<td>Hispanic</td>
<td>783</td>
<td>92.7%</td>
<td>42.1%</td>
<td>50.6***</td>
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<tr>
<td>Other</td>
<td>61</td>
<td>101.8%</td>
<td>47.0%</td>
<td>54.8***</td>
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<td><strong>Ethnicity, by gender</strong></td>
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<tr>
<td>Women</td>
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<td></td>
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<tr>
<td>White, non-Hispanic</td>
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<td>42.8%</td>
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<td>54.2%</td>
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<td>413</td>
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<td>50.9%</td>
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<td>Other</td>
<td>26</td>
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<tr>
<td>Men</td>
<td></td>
<td></td>
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<td>White, non-Hispanic</td>
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<td>55.8%</td>
<td>38.0***</td>
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<td>484</td>
<td>90.5%</td>
<td>44.3%</td>
<td>46.1***</td>
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<td>35.9%</td>
<td>53.5***</td>
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<th>Experimentals</th>
<th>Controls</th>
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<th>p^a</th>
<th>Subgroup Impact Difference^b</th>
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<td>20 or 21</td>
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<td>37.1</td>
<td>51.1***</td>
<td>0.000</td>
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(continued)
### Table 4.4 (continued)

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<th>Sample Size</th>
<th>Subgroup Impact</th>
<th>Subgroup Difference</th>
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<tr>
<td>Living in own household or</td>
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<td></td>
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</tr>
<tr>
<td>with boy/girlfriend</td>
<td></td>
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<td>93.3 43.6</td>
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<td>339</td>
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<td>95.5 42.9</td>
<td>52.5*** 0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>696</td>
<td>88.1 46.4</td>
<td>41.7*** 0.000</td>
</tr>
<tr>
<td>Arrested since age 16</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>No</td>
<td>1,567</td>
<td>92.5 45.1</td>
<td>47.4*** 0.000</td>
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<tr>
<td>Yes</td>
<td>272</td>
<td>93.3 39.2</td>
<td>54.1*** 0.000</td>
</tr>
<tr>
<td>Lived with both parents at</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>age 14</td>
<td></td>
<td></td>
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<tr>
<td>No</td>
<td>1,198</td>
<td>91.8 44.6</td>
<td>47.2*** 0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>641</td>
<td>94.3 43.6</td>
<td>50.7*** 0.000</td>
</tr>
<tr>
<td>(continued)</td>
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</tr>
</tbody>
</table>
### TABLE 4.4 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Experimental</th>
<th>Controls</th>
<th>Subgroup Impact</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Subgroup Impact Difference&lt;sup&gt;b&lt;/sup&gt;</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reason for leaving regular high school</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>School-related</td>
<td>886</td>
<td>92.9%</td>
<td>41.1%</td>
<td>51.8***</td>
<td>0.000</td>
<td>---**</td>
<td>0.025</td>
</tr>
<tr>
<td>Job-related</td>
<td>182</td>
<td>93.6%</td>
<td>37.6%</td>
<td>56.0***</td>
<td>0.000</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>Other</td>
<td>771</td>
<td>92.3%</td>
<td>49.4%</td>
<td>42.9***</td>
<td>0.000</td>
<td>---</td>
<td>---</td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

<sup>a</sup>A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled ." are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

<sup>b</sup>For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.
significant. The "subgroup impact" column shows that this difference in impacts was calculated as 55.5 percentage points (the impact for men, shown in the second row) minus 42.1 percentage points (the impact for women, shown in the third row).

Other important subgroup comparisons of impacts on service receipt include:

- **Age.** Very different patterns of labor market behavior are exhibited at each age in the general youth population. Labor force participation, employment, and earnings increase dramatically from age 16 to the early twenties. Thus, holding everything else constant, youth over age 20 would pay higher opportunity costs for program attendance than would the younger people. The impacts provide some support for this generalization, with teenage sample members having a slightly higher impact on service receipt (49.7 percentage points) than older youths in the sample (44.8 percentage points).

- **Prior employment.** For those with a more extensive work history, as evidenced by employment in the year before random assignment, the opportunity costs of participating in a program may be greater. Experimentals with and without prior employment had high rates of participation in education and training, though the impacts on service receipt for those with prior employment were higher because of the lower rate of participation in programs by controls.

- **Highest grade attended.** While all JOBSTART enrollees were high school dropouts, some left school before completing the tenth grade, while others dropped out during their junior or senior years. Despite the differences in past success in school, the levels of participation and impacts on service receipt were nearly identical for the two groups of youths. Apparently, JOBSTART sites found ways to engage the lower-attainment youths in an education and training program.

- **Welfare receipt.** Those who receive AFDC, General Assistance, or Home Relief may tend to get higher levels of support services such as child care, and sometimes may be mandated to participate in some program in order to maintain eligibility for their cash benefits. Impacts for those not receiving welfare at random assignment were higher than for welfare recipients because control group welfare recipients were more likely to participate in an education or training program.

D. **Receipt of Education and Training Separately**

Figure 4.3 reports separately on the monthly receipt of education, showing a large

---

10Since only about 13 percent of the men in the sample received AFDC or General Assistance at random assignment, the subgroup receiving welfare was primarily made up of women.
FIGURE 4.3
MONTHLY RECEIPT OF BASIC EDUCATION, BY RESEARCH STATUS

SOURCE: Appendix Table F.3.

NOTES: Calculations for this figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. "Basic education" includes JOBSTART and non-JOBSTART education activities. Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.
difference between experimentals and controls in their participation in education throughout much of the two-year follow-up period. During the first year, 88 percent of experimentals participated in education compared to 17 percent of controls, for a difference in education participation of 71 percentage points. In the second year, the participation of experimentals dropped sharply as they left the JOBSTART program, while that of controls remained approximately what it was during the first year, causing the service receipt difference to decline to only 4 percentage points. Analysis of subgroup impacts again indicates a somewhat stronger service differential for men than for women, because women controls tended to get more education services on their own than did men controls.

Figure 4.4 shows separately JOBSTART's impact on the receipt of training. JOBSTART achieved less of an experimental-control differential for training alone than for training and education together. The smaller impact for training than for education resulted from the failure of some JOBSTART sites to achieve transitions into training from education, as described in Chapter 3. Similar patterns of impacts on receipt of training appeared for men and women, but again the impacts tended to be larger for men.

II. Impacts on Educational Attainment

As indicated in the 1989 interim report, Implementing JOBSTART, the impacts of JOBSTART on educational attainment during the first year of follow-up were quite similar to those of the program that inspired it, the residential Job Corps. An evaluation of the Job Corps found that 24 percent of Corpsmembers, but only 5 percent of the comparison group, had high school diplomas or GEDs six months after termination from the program (the period roughly equivalent to a year of post-random assignment follow-up). This report carries the story forward to the end of the two-year follow-up period.

JOBSTART impacts on educational attainment are presented in Table 4.5 for the full sample. Table 4.6 presents separate results for men, women living with their own children, and other women. The severe and intractable problems in reading and mathematics for the young adults in JOBSTART are reflected in the low rates of completing high school or passing the

---

11The two-year impact on receipt of training was 41.6 percentage points for the full sample, 36.0 percentage points among women, and 49.0 percentage points among men. A comparison with the figures for education or training in Table 4.1 supports this point in the text.

12Mallar et al., 1982. See also Betsey et al., 1985, p. 112.
FIGURE 4.4
MONTHLY RECEIPT OF TRAINING,
BY RESEARCH STATUS

SOURCE: Appendix Table F.4.

NOTES: Calculations for this figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. "Training" includes JOBSTART and non-JOBSTART occupational skills training activities. Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.
TABLE 4.5
IMPACTS ON EDUCATIONAL ATTAINMENT THROUGH MONTH 24

<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Received GED by end of month 24</td>
<td>30.7%</td>
<td>11.9%</td>
<td>18.9***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received GED or high school diploma by end of month 24</td>
<td>33.2%</td>
<td>16.4%</td>
<td>16.7***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received trade certificate or license by end of month 24</td>
<td>19.2%</td>
<td>8.9%</td>
<td>10.3***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received associate's or 2-year college degree by end of month 24</td>
<td>0.2%</td>
<td>0.0%</td>
<td>0.2%</td>
<td>0.146</td>
</tr>
<tr>
<td>Received bachelor's or 4-year college degree by end of month 24</td>
<td>0.0%</td>
<td>0.0%</td>
<td>0.0%</td>
<td>1.000</td>
</tr>
</tbody>
</table>

Sample size: 949 890

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table 8.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

<sup>a</sup>The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
## TABLE 4.6
IMPACTS ON EDUCATIONAL ATTAINMENT THROUGH MONTH 24,
BY GENDER AND PARENTAL STATUS

<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Received GED by end of month 24 (%)</td>
<td>28.8</td>
<td>12.0</td>
<td>16.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received GED or high school diploma by end of month 24 (%)</td>
<td>32.1</td>
<td>16.9</td>
<td>15.2***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received trade certificate or license by end of month 24 (%)</td>
<td>22.1</td>
<td>9.1</td>
<td>12.9***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received associate's or 2-year college degree by end of month 24 (%)</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>1.000</td>
</tr>
<tr>
<td>Received bachelor's or 4-year college degree by end of month 24 (%)</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>1.000</td>
</tr>
<tr>
<td>Sample size</td>
<td>438</td>
<td>433</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Women living with own child(ren)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Received GED by end of month 24 (%)</td>
<td>33.6</td>
<td>10.6</td>
<td>23.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received GED or high school diploma by end of month 24 (%)</td>
<td>35.5</td>
<td>14.2</td>
<td>21.3***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received trade certificate or license by end of month 24 (%)</td>
<td>16.7</td>
<td>12.1</td>
<td>4.6</td>
<td>0.148</td>
</tr>
<tr>
<td>Received associate's or 2-year college degree by end of month 24 (%)</td>
<td>0.4</td>
<td>0.0</td>
<td>0.4</td>
<td>0.297</td>
</tr>
<tr>
<td>Received bachelor's or 4-year college degree by end of month 24 (%)</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>1.000</td>
</tr>
<tr>
<td>Sample size</td>
<td>250</td>
<td>234</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

(continued)
<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Women n= living with own child(ren), including those who did not have any</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Received GED by end of month 24 (%)</td>
<td>31.1</td>
<td>13.0</td>
<td>18.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received GED or high school diploma by end of month 24 (%)</td>
<td>32.4</td>
<td>18.2</td>
<td>14.2***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received trade certificate or license by end of month 24 (%)</td>
<td>16.2</td>
<td>5.7</td>
<td>10.6***</td>
<td>0.000</td>
</tr>
<tr>
<td>Received associate's or 2-year college degree by end of month 24 (%)</td>
<td>0.4</td>
<td>0.0</td>
<td>0.3</td>
<td>0.415</td>
</tr>
<tr>
<td>Received bachelor's or 4-year college degree by end of month 24 (%)</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>1.000</td>
</tr>
<tr>
<td>Sample size</td>
<td>261</td>
<td>223</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**SOURCE:** MDPr calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

^aThe column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
GED examination for both experimentals and controls. By month 24 of follow-up, 16.4 percent of all controls had received a high school diploma or passed the GED examination, with about equal proportions among the three subgroups in Table 4.6. Among experimentals, 33.2 percent had attained a high school diploma or GED, with about equal percentages for experimentals among men, women living with their own children, and other women.

Thus, the full-sample impact on attainment of a GED or high school diploma by month 24 was 16.7 percentage points, which compares very favorably to the results of the residential Job Corps evaluation. The impact was 15.2 percentage points among men, 21.3 percentage points among women living with their own children, and 14.2 percentage points among other women.

Compared to the impacts just described for attainment of a diploma or GED, the impact on attainment of a GED alone was slightly larger. This was the case because controls (who were not as a rule participating in a special alternative program such as JOBSTART) were slightly more likely to return to regular high school than were experimentals, although both events were rare. The impact on passing of the GED examination was 18.9 percentage points for the full sample, 16.8 percentage points among men, 23 percentage points among women living with their own children, and 18.1 percentage points among other women.

These large educational attainment impacts were present for many different subgroups in the sample, as shown in Table 4.7 for passing of the GED examination. For example, the first three rows of the table present impacts on GED receipt for women and men. For women, impacts were 20.8 percentage points, while for men they were 16.8 percentage points. The difference in impacts for these two groups, 4 percentage points, is reported in the column labeled "subgroup impact difference," and is not statistically significant. Other subgroup findings include a 23.2 percentage point impact on passing of the GED examination for women living with their own children, 17.3 percentage points for youths who quit school before the

---

13 The JOBSTART survey also asked about receipt of a trade certificate or license during the follow-up period. The precise meaning of these certificates and licenses is not clear. Some could have been awarded for completion of a program, rather than for a generally recognized occupational competency. Nevertheless, 19.2 percent of experimentals received such certificates and licenses, compared to 8.9 percent of controls, for an impact of 10.3 percentage points. The impact size was slightly higher than this for men and about half this for women living with their own children.

14 The results presented in this paragraph are based on an analysis similar to that used for Table 4.4. That is, impacts are for subgroups designated by the named characteristic, with differences in other observed characteristics statistically controlled for through linear regression.

-119-
TABLE 4.7
IMPACTS ON GED ATTAINMENT THROUGH MONTH 24,
BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Received GED by End of Month 24</th>
<th>Subgroup Impact</th>
<th>Subgroup Impact Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Experimentals</td>
<td>Controls</td>
<td>Subgroup</td>
<td>Experimentals</td>
</tr>
<tr>
<td>Gender</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td>968</td>
<td>33.3%</td>
<td>12.5%</td>
<td>20.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Men</td>
<td>871</td>
<td>27.8%</td>
<td>11.0%</td>
<td>16.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Ethnicity</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>155</td>
<td>42.5%</td>
<td>24.6%</td>
<td>17.9***</td>
<td>0.004</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>840</td>
<td>30.1%</td>
<td>11.9%</td>
<td>18.2***</td>
<td>0.000</td>
</tr>
<tr>
<td>Hispanic</td>
<td>783</td>
<td>29.9%</td>
<td>8.9%</td>
<td>21.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Other</td>
<td>61</td>
<td>18.1%</td>
<td>15.1%</td>
<td>3.0**</td>
<td>0.766</td>
</tr>
<tr>
<td>Ethnicity, by gender</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>84</td>
<td>49.5%</td>
<td>24.6%</td>
<td>24.9***</td>
<td>0.003</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>445</td>
<td>33.2%</td>
<td>12.3%</td>
<td>20.9***</td>
<td>0.000</td>
</tr>
<tr>
<td>Hispanic</td>
<td>413</td>
<td>31.6%</td>
<td>10.8%</td>
<td>20.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Other</td>
<td>26</td>
<td>13.5%</td>
<td>9.3%</td>
<td>4.2</td>
<td>0.787</td>
</tr>
<tr>
<td>Men</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>71</td>
<td>33.8%</td>
<td>24.7%</td>
<td>9.1</td>
<td>0.329</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>395</td>
<td>26.6%</td>
<td>11.3%</td>
<td>15.2***</td>
<td>0.000</td>
</tr>
<tr>
<td>Hispanic</td>
<td>370</td>
<td>27.9%</td>
<td>6.8%</td>
<td>21.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Other</td>
<td>35</td>
<td>22.2%</td>
<td>17.9%</td>
<td>4.3</td>
<td>0.746</td>
</tr>
<tr>
<td>Parental status</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women living with own child(ren)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>484</td>
<td>32.8%</td>
<td>14.5%</td>
<td>18.3***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>484</td>
<td>33.3%</td>
<td>10.1%</td>
<td>23.2***</td>
<td>0.000</td>
</tr>
<tr>
<td>Men who have own child(ren)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>765</td>
<td>28.5%</td>
<td>11.5%</td>
<td>17.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>106</td>
<td>25.0%</td>
<td>9.7%</td>
<td>15.3**</td>
<td>0.046</td>
</tr>
<tr>
<td>Characteristic and Subgroups</td>
<td>Sample Size</td>
<td>Received GED by End of Month 24</td>
<td>Subgroup Impact</td>
<td>p^a</td>
<td>Subgroup Impact Difference</td>
</tr>
<tr>
<td>----------------------------------------------------</td>
<td>-------------</td>
<td>---------------------------------</td>
<td>-----------------</td>
<td>-----</td>
<td>---------------------------</td>
</tr>
<tr>
<td><strong>Employed within past year</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>870</td>
<td>27.0%</td>
<td>14.1%</td>
<td>12.8***</td>
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</tr>
<tr>
<td>Yes</td>
<td>969</td>
<td>34.1</td>
<td>9.8</td>
<td>24.3***</td>
<td>0.000</td>
</tr>
<tr>
<td><strong>Prior employment, by gender</strong></td>
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<td></td>
<td></td>
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</tr>
<tr>
<td>Women employed within past year</td>
<td></td>
<td></td>
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</tr>
<tr>
<td>No</td>
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<td>17.7***</td>
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<td>Yes</td>
<td>421</td>
<td>36.2</td>
<td>11.4</td>
<td>24.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Men employed within past year</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>No</td>
<td>323</td>
<td>21.4</td>
<td>16.9</td>
<td>4.6</td>
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<tr>
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<td>548</td>
<td>31.8</td>
<td>8.0</td>
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</tr>
<tr>
<td><strong>Left school in grade 11 or 12</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>No</td>
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<td>29.9</td>
<td>9.9</td>
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<tr>
<td>Yes</td>
<td>761</td>
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<td>14.6</td>
<td>17.3***</td>
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<td><strong>Received occupational training within past year</strong></td>
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<td></td>
<td></td>
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</tr>
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<td>No</td>
<td>1,529</td>
<td>30.2</td>
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<tr>
<td>Yes</td>
<td>310</td>
<td>33.6</td>
<td>8.1</td>
<td>25.4***</td>
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</tr>
<tr>
<td><strong>Age</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-19</td>
<td>1,359</td>
<td>30.9</td>
<td>12.9</td>
<td>18.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>20 or 21</td>
<td>480</td>
<td>30.2</td>
<td>8.8</td>
<td>21.4***</td>
<td>0.000</td>
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<td><strong>Age, by gender</strong></td>
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<td></td>
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<td></td>
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<tr>
<td>Women</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-19</td>
<td>710</td>
<td>33.4</td>
<td>13.4</td>
<td>20.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>20 or 21</td>
<td>258</td>
<td>33.2</td>
<td>10.4</td>
<td>22.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Men</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-19</td>
<td>649</td>
<td>28.0</td>
<td>12.2</td>
<td>15.7***</td>
<td>0.000</td>
</tr>
<tr>
<td>20 or 21</td>
<td>222</td>
<td>26.9</td>
<td>7.1</td>
<td>19.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Characteristic and Subgroups</td>
<td>Sample Size</td>
<td>Received GED by End of Month 24</td>
<td>Subgroup Impact</td>
<td>p a</td>
<td>Subgroup Impact Difference b p a</td>
</tr>
<tr>
<td>----------------------------------------------------------------</td>
<td>-------------</td>
<td>---------------------------------</td>
<td>-----------------</td>
<td>-----</td>
<td>---------------------------------</td>
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<td>Marital status</td>
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<tr>
<td>Ever married</td>
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<td>40.7%</td>
<td>6.6%</td>
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<tr>
<td>Never married</td>
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<td>Living in own household or with boy/girlfriend</td>
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<td></td>
<td></td>
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<tr>
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<td>1,500</td>
<td>29.9%</td>
<td>11.5%</td>
<td>18.5***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>339</td>
<td>34.2%</td>
<td>13.4%</td>
<td>20.9***</td>
<td>0.000</td>
</tr>
<tr>
<td>Own AFDC case or receiving General Assistance</td>
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<td></td>
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<td></td>
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<tr>
<td>No</td>
<td>1,344</td>
<td>29.5%</td>
<td>10.3%</td>
<td>19.2***</td>
<td>0.000</td>
</tr>
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<td>495</td>
<td>34.1%</td>
<td>16.0%</td>
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<td>0.000</td>
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<td>Own AFDC case</td>
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<td></td>
<td></td>
<td></td>
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<tr>
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<td>31.3%</td>
<td>12.4%</td>
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<td>0.000</td>
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<tr>
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<td>393</td>
<td>28.5%</td>
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<td>18.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Receiving Food Stamps</td>
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<td></td>
<td></td>
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<td></td>
</tr>
<tr>
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<td>1,143</td>
<td>29.5%</td>
<td>11.0%</td>
<td>18.4***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>696</td>
<td>32.8%</td>
<td>13.2%</td>
<td>19.6***</td>
<td>0.000</td>
</tr>
<tr>
<td>Arrested since age 16</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>No</td>
<td>1,567</td>
<td>30.6%</td>
<td>11.9%</td>
<td>18.6***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>272</td>
<td>31.5%</td>
<td>11.3%</td>
<td>20.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Lived with both parents at age 14</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>No</td>
<td>1,198</td>
<td>31.2%</td>
<td>11.2%</td>
<td>20.0***</td>
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</tr>
<tr>
<td>Yes</td>
<td>541</td>
<td>29.3%</td>
<td>13.0%</td>
<td>16.8***</td>
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</tr>
</tbody>
</table>

(continued)
TABLE 4.7 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Experimental Impact</th>
<th>Control Impact</th>
<th>Subgroup Impact Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reason for leaving regular high school</td>
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<td></td>
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<td></td>
<td></td>
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<td>School-related</td>
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<td>31.0%</td>
<td>12.9%</td>
<td>18.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Job-related</td>
<td>182</td>
<td>29.1</td>
<td>16.0</td>
<td>13.1**</td>
<td>0.025</td>
</tr>
<tr>
<td>Other</td>
<td>771</td>
<td>30.6</td>
<td>9.6</td>
<td>21.0***</td>
<td>0.000</td>
</tr>
</tbody>
</table>

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES:
Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.
eleventh grade, and 18.8 percentage points for youths with their own AFDC case at random assignment.
CHAPTER 5

IMPACTS ON EMPLOYMENT, EARNINGS, AND OTHER OUTCOMES

Chapter 4 showed that JOBSTART had strong impacts on completion of high school or receipt of a GED for most subgroups of the full sample, and that these strong impacts remained large over time. This chapter focuses on the extent to which the impact on educational attainment has, to this point, translated into increased labor market success and less reliance on public benefit programs. The following key questions about labor market and other outcomes are addressed:

- What sacrifice of employment opportunities or earnings did experimentalists make during the first year after random assignment, which for many was primarily a period spent in JOBSTART?
- Had the experimentalists begun to catch up with the controls in employment and earnings by the end of the second year?
- How did experimentalists and controls compare in earnings and employment within important subgroups? Were the labor market effects of JOBSTART different for men, young mothers, and other women? Did key JOBSTART impacts vary according to age, grade at time of dropout, or other characteristics of the young people in the sample?
- Do more precise measures of work effort and further information on wages earned shed light on the basic employment rate and earnings impacts that were observed during the follow-up period?
- What effect did the program have on other outcomes such as time spent in productive activity (that is, employment, education, or training), receipt of public benefits, criminal activity, and childbearing?

The findings described here, based on two years of follow-up, are interim results; they represent the early post-program period for most of the sample. The analysis in this chapter does not attempt any projections of employment, earnings, or other outcomes into the future. The third wave of the follow-up survey, fielded at approximately four years after random assignment of the sample members, will extend the data for an additional two years.

A summary of this chapter may prove useful at the outset. The analysis focuses on the full sample and three key subgroups: mothers, other women, and men. In brief: The impacts
on employment and earnings were encouraging for mothers; small, but slightly positive, for other women; and generally negative throughout the period for men. The analysis presents impacts on employment and total earnings, using simple measures, and then offers possible explanations for the basic results.

In more detail: After an initial period of activity in JOBSTART, experimentals in the full sample gradually caught up with controls in employment rate. For young mothers and other women, the rate slightly exceeded that of their control group counterparts by the second year of follow-up, while men drew approximately even with controls. For the full sample over the entire 24-month period, experimentals remained below controls in cumulative earnings, but there were important differences among the key subgroups. Young mothers in the experimental group earned more than controls during the 24-month period; other women in the experimental group earned more than controls during the second 12-month period, nearly canceling out their first-year earnings loss. The earnings of male controls substantially exceeded those of male experimentals in both years.

Since earnings are a function of hours and wages, analyzing these more refined measures of labor market success helps explain the basic earnings impact findings. Despite the employment rates of experimentals and controls being nearly equal during the second year of follow-up, controls remained ahead in actual time spent working, although again there were differences among the key subgroups. Throughout most of the follow-up period, a higher proportion of experimentals than controls were attending education or training classes, which may explain, or partly explain, why experimentals spent less time working. In other words, investment in human capital continued throughout the follow-up period, supporting the view that the final story on employment impacts cannot yet be told.

I. JOBSTART as an Investment

If programs like JOBSTART are effective, they will lead to long-run gains in employment and earnings, but in the short run there may be negative impacts. During the first year after entering the sample, JOBSTART youths had less time available for work than did controls, since for much of that period, they were in an intensive program of education and skills instruction. Chapter 3 showed that more than 50 percent of experimentals were still active in the sixth month after entering the sample; 16 percent were still active in the twelfth

-126-
month; and almost 10 percent were still active in the fifteenth month. Throughout most of the
two-year follow-up period, a larger percentage of experimentals than controls were participating
in a program. Controls, therefore, got a head start in the labor market, since experimentals
could not be in JOBSTART and be working during any given hour of the day. Also, for
young people with poor skills, work experience itself can be an important source of new job
skills and higher wages.

GED attainment during or after intensive JOBSTART education ultimately may open
up many employment opportunities for JOBSTART graduates. But even after a sample
member left JOBSTART, it might still take some time to become as well-settled in the labor
market as his or her control group counterpart, who might have been learning new skills while
working. Thus, well into the second year of follow-up, controls might still be expected to have
had the edge on experimentals in employment and earnings.

The evaluation of the residential Job Corps provides an example of this point. That
study began its follow-up at the point of termination from the program, and it reported impacts
only for six-month intervals rather than for individual months. For the period from program
termination to six months thereafter, the employment and earnings of those in the program
group were slightly lower than those of the comparison group (although the difference was not
statistically significant). The employment and earnings rates of the program group began to
exceed those of the comparison group six to 12 months following termination from the
program, and they continued to be higher throughout the remainder of the 48-month follow-
up period.

These findings cannot be applied directly to the JOBSTART evaluation because follow-
up in JOBSTART began at random assignment — the point when youths were ready to enter
the program. However, since the average length of stay in JOBSTART was roughly six
months, the 12-month survey was conducted about six months after the average sample
member left the program, and the 24-month survey was conducted about 18 months after exit
from the program. In sum, the monthly employment and earnings impacts in the Job Corps
study turned positive during a period roughly corresponding to the second year of follow-up
covered by this report.

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1See Mallar et al., 1982, p. 135.
II. **JOBSTART’s Impacts on Employment Rates**

Because young adults tend to apply for employment and training programs when they are between jobs, the employment rate grew over time for both experimentals and controls (Table 5.1). For the full sample, a larger fraction of controls than experimentals was employed in each month of the first year. The difference in employment rate peaked in month five and then mainly declined over time. After month 12, the employment rate of experimentals was greater than that of controls more often than it was below it, but in no month was this difference statistically significant. About 46 percent of each group worked at some point during the last month of follow-up (month 24). During the second year as a whole, 69.5 percent of controls worked at some point, as did 72.0 percent of experimentals, for a positive employment rate impact of 2.5 percentage points. This impact was, however, not statistically significant.

A. **Impacts by Gender**

As explained in Chapter 4, many previous evaluations of youth employment and training programs have found better program effects for women than for men. One likely explanation for women’s better employment results is that it is easier to improve the employment and earnings of those who do not spend much time in the world of work (for example, young mothers) than of those who are already in the labor force but fail to find and keep steady, well-paying jobs (for example, young men with poor skills). Thus, from this perspective, women have greater potential for improved labor market outcomes than do men, and less to lose (in terms of forgone employment and earnings) by investing in education and training.

The JOBSTART sample exemplifies this pattern. During the first year after random assignment, a sharp increase in employment rates in month 13 is very likely due to survey measurement error. As explained in Appendix A, 235 of the 1,839 24-month survey completers responded to a single combination instrument covering their labor market behavior over the entire two-year period, while 1,604 of them responded to separate survey efforts 12 and 24 months after random assignment. Separate analysis of the 235 "combination" responders showed continuous growth in employment rates over the 24-month follow-up period. But many in the sample who responded to a survey after 12 and 24 months showed a discontinuity in employment rates at month 13 owing to poor and inconsistent recall. For example, respondents to the 24-month survey reported employment at the start of the period covered by the survey (month 13) that probably started somewhat later. This happened even though the survey staff prompted respondents with their previous responses to questions about employment in month 12.

---

2The appearance of a sharp increase in employment rates in month 13 is very likely due to survey measurement error. As explained in Appendix A, 235 of the 1,839 24-month survey completers responded to a single combination instrument covering their labor market behavior over the entire two-year period, while 1,604 of them responded to separate survey efforts 12 and 24 months after random assignment. Separate analysis of the 235 "combination" responders showed continuous growth in employment rates over the 24-month follow-up period. But many in the sample who responded to a survey after 12 and 24 months showed a discontinuity in employment rates at month 13 owing to poor and inconsistent recall. For example, respondents to the 24-month survey reported employment at the start of the period covered by the survey (month 13) that probably started somewhat later. This happened even though the survey staff prompted respondents with their previous responses to questions about employment in month 12.

3See Betsey et al., 1985, for a survey.
TABLE 5.1
IMPACTS ON EMPLOYMENT RATES THROUGH MONTH 24

<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experiments</th>
<th>Controls</th>
<th>Difference</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Ever employed</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>79.2%</td>
<td>77.9%</td>
<td>1.3</td>
<td>0.474</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>57.6</td>
<td>61.6</td>
<td>-4.0*</td>
<td>0.061</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>72.0</td>
<td>69.5</td>
<td>2.5</td>
<td>0.212</td>
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<tr>
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<td>20.5</td>
<td>29.8</td>
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</tr>
<tr>
<td>Quarter 2</td>
<td>30.4</td>
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<td>-9.7***</td>
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</tr>
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<td>Quarter 3</td>
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<td>44.2</td>
<td>-3.5</td>
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<td>-1.5</td>
<td>0.501</td>
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<td>52.7</td>
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<td>Quarter 7</td>
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<td>50.6</td>
<td>-0.1</td>
<td>0.954</td>
</tr>
<tr>
<td>Quarter 8</td>
<td>53.4</td>
<td>51.7</td>
<td>1.6</td>
<td>0.453</td>
</tr>
<tr>
<td><strong>Ever employed</strong></td>
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<td></td>
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<td></td>
</tr>
<tr>
<td>Month 1</td>
<td>13.1</td>
<td>20.4</td>
<td>-7.3***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 2</td>
<td>16.5</td>
<td>24.4</td>
<td>-7.9***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 3</td>
<td>18.5</td>
<td>27.0</td>
<td>-8.5***</td>
<td>0.000</td>
</tr>
<tr>
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<td>30.5</td>
<td>-9.2***</td>
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</tr>
<tr>
<td>Month 5</td>
<td>23.6</td>
<td>33.1</td>
<td>-9.5***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 6</td>
<td>26.5</td>
<td>34.0</td>
<td>-7.4***</td>
<td>0.000</td>
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<tr>
<td>Month 7</td>
<td>30.3</td>
<td>35.7</td>
<td>-5.3**</td>
<td>0.011</td>
</tr>
<tr>
<td>Month 8</td>
<td>32.2</td>
<td>37.2</td>
<td>-5.0**</td>
<td>0.019</td>
</tr>
<tr>
<td>Month 9</td>
<td>35.3</td>
<td>38.3</td>
<td>-3.0</td>
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<tr>
<td>Month 10</td>
<td>38.5</td>
<td>39.6</td>
<td>-1.2</td>
<td>0.592</td>
</tr>
<tr>
<td>Month 11</td>
<td>38.1</td>
<td>41.8</td>
<td>-3.7*</td>
<td>0.089</td>
</tr>
<tr>
<td>Month 12</td>
<td>41.0</td>
<td>42.2</td>
<td>-1.3</td>
<td>0.562</td>
</tr>
<tr>
<td>Month 13</td>
<td>47.7</td>
<td>45.4</td>
<td>2.4</td>
<td>0.281</td>
</tr>
<tr>
<td>Month 14</td>
<td>39.2</td>
<td>36.4</td>
<td>2.8</td>
<td>0.187</td>
</tr>
<tr>
<td>Month 15</td>
<td>41.2</td>
<td>39.3</td>
<td>1.9</td>
<td>0.385</td>
</tr>
<tr>
<td>Month 16</td>
<td>41.6</td>
<td>40.0</td>
<td>1.6</td>
<td>0.461</td>
</tr>
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<td>Month 17</td>
<td>41.2</td>
<td>42.1</td>
<td>-0.9</td>
<td>0.684</td>
</tr>
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<td>Month 18</td>
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<td>43.3</td>
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<td>0.645</td>
</tr>
<tr>
<td>Month 19</td>
<td>42.3</td>
<td>44.1</td>
<td>-1.8</td>
<td>0.403</td>
</tr>
<tr>
<td>Month 20</td>
<td>42.9</td>
<td>43.7</td>
<td>-0.8</td>
<td>0.717</td>
</tr>
<tr>
<td>Month 21</td>
<td>44.8</td>
<td>44.0</td>
<td>0.8</td>
<td>0.726</td>
</tr>
<tr>
<td>Month 22</td>
<td>46.2</td>
<td>44.5</td>
<td>1.7</td>
<td>0.441</td>
</tr>
<tr>
<td>Month 23</td>
<td>45.6</td>
<td>45.8</td>
<td>-0.2</td>
<td>0.941</td>
</tr>
<tr>
<td>Month 24</td>
<td>46.1</td>
<td>46.7</td>
<td>-0.7</td>
<td>0.763</td>
</tr>
</tbody>
</table>

Sample size 949 890

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. (continued)
Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
assignment, 74.8 percent of control men worked, compared to 38.3 percent of women living with their own children and 61.5 percent of other women (Table 5.2). During this period, which, for many experimentals, was largely a time of program participation, the impact of JOBSTART on employment rates was 2.7 percentage points for the young mothers, -4.5 percentage points for other women, and -8.1 percentage points for men. Thus, JOBSTART entailed an opportunity cost (forgone employment) for men and women who were not caring for children, but not for young mothers.

A second possible explanation for the poorer impacts observed for men in JOBSTART is the greater difficulty of placing men in jobs that value a GED. Many women in JOBSTART, for example, were trained in clerical occupations and sought clerical jobs, for which educational credentials were important. However, young men were more likely to train for work in occupations that did not, at least initially, value a GED — for example, in many types of blue-collar work, especially physically demanding jobs. Further, it may have been harder to find training-related jobs for men who did study for occupations in which having a GED matters. These explanations for women's stronger impacts will be explored in future work on the JOBSTART Demonstration. So far (during two years of follow-up), the investment in JOBSTART services follows the usual pattern and has not paid off for men in higher employment rates. Although they regained the ground they lost in the first year, they did not surpass the controls in the second year. Table 5.2 shows that the impact on employment rates was precisely zero for men during the second year of follow-up; exactly 84.3 percent of male experimentals and controls worked at some point during those 12 months.

In contrast, female experimentals — especially young mothers — seem to have overtaken female controls in employment during the same period. Table 5.2 shows that the impact on employment rates for young mothers was 7.2 percentage points during months 13 through 24, and for other women it was 4.2 percentage points; these impacts were not, however, statistically significant.

The month-by-month story is less clear than that told by cumulative yearly employment rates. Figure 5.1 shows monthly employment rates for experimentals and controls among men,

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4In the rest of this analysis, women living with their own children will be referred to as "mothers" even though some of the "other women" may be mothers who were not living with their children.

5Chapter 3 reported on the occupations for which men and women trained and showed a much higher percentage of women studying for clerical occupations.
TABLE 5.2

IMPACTS ON EMPLOYMENT RATES THROUGH MONTH 24,
BY GENDER AND PARENTAL STATUS

<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>89.0</td>
<td>90.1</td>
<td>-1.1</td>
<td>0.583</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>66.7</td>
<td>74.8</td>
<td>-8.1***</td>
<td>0.006</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>84.3</td>
<td>84.3</td>
<td>0.0</td>
<td>0.990</td>
</tr>
<tr>
<td>Sample size</td>
<td>438</td>
<td>433</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Women living with own child(ren)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>62.1</td>
<td>57.2</td>
<td>4.9</td>
<td>0.279</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>41.0</td>
<td>38.3</td>
<td>2.7</td>
<td>0.543</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>53.3</td>
<td>46.1</td>
<td>7.2</td>
<td>0.113</td>
</tr>
<tr>
<td>Sample size</td>
<td>250</td>
<td>234</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Women not living with own child(ren), including those who did not have any</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>79.3</td>
<td>75.8</td>
<td>3.4</td>
<td>0.363</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>57.0</td>
<td>61.5</td>
<td>-4.5</td>
<td>0.311</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>69.5</td>
<td>65.3</td>
<td>4.2</td>
<td>0.332</td>
</tr>
<tr>
<td>Sample size</td>
<td>261</td>
<td>223</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**SOURCES FOR TABLE 5.2 AND FIGURE 5.1:** MDRC calculations from JOBSTART enrollment form and survey data (Table 5.2); Appendix Table F.5 (Figure 5.1).

**NOTES FOR TABLE 5.2 AND FIGURE 5.1:** Calculations for this table and figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Osle, 1975, p. 461; Cave, 1987; and Appendix Table 8.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

^aThe column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
FIGURE 5.1
MONTHLY EMPLOYMENT RATES,
BY GENDER AND PARENTAL STATUS

*Includes women who did not have children.
young mothers, and other women. Young mothers who were in the experimental group appear
to have caught up with, and sometimes overtaken, their control group counterparts in monthly
employment rates part-way through the first year, while other women in the experimental
group caught up with their control group counterparts at about the beginning of the second
year. For the "other women" subgroup, there also appears to have been a trend toward
growing positive impacts in the last six months of the follow-up period. In contrast,
experimental men remained slightly behind control men during most of the second year. Thus,
although the jury is still out on employment rate impacts, the findings certainly are more
encouraging for women than for men.

B. Impacts for Other Subgroups

In addition to looking at impacts for subgroups separately (for example, looking at the
impacts for men, women living with their own children, and other women), it is useful to
estimate employment rate impacts for subgroups controlling for differences in characteristics
other than the one used to designate the groups (for example, gender). This helps explain
whether any observed differences in impacts for men, women living with their own children,
and other women were due to other factors correlated with gender and parenting status (such
as past employment experience) or continue to be present even after such differences are
controlled for in the analysis. This type of analysis also allows calculation of whether
differences between impacts for women and men were statistically significant.

Table 5.3 addresses these issues by presenting impacts on the percentage of the sample
ever employed during months 13 through 24. Each section of the table (such as the first for
women and men) presents impacts for the designated subgroups, along with the
between-subgroups impact difference. The entry to the right of the first line of the table

<table>
<thead>
<tr>
<th>Subgroup</th>
<th>Impacts</th>
<th>Between-subgroups Impact Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Women</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Men</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

---

6Chapter 2 highlighted ways in which JOBSTART women were different from JOBSTART men in
ccharacteristics besides gender. The impact estimates for men, young mothers, and other women, in Table
5.2 were done by splitting the entire sample into three subsamples. To make the estimates more precise,
the separate estimates for each group took into account small observed differences between experimental
and controls in each subsample through regression adjustments. But there were many differences among
the three subsamples in JOBSTART other than their gender and parenting status; for example, women
especially young mothers had less work experience than did men. This makes it difficult to understand
the sources of differences in impacts among the three groups.

7The impact estimates in this table were produced by conducting a two-way analysis of covariance
controlling for differences in pre-random assignment characteristics other than the characteristic used to
(continued...)
### TABLE 5.3

**IMPACTS ON YEAR-TWO EMPLOYMENT RATES, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT**

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Ever Employed, Months 13-24</th>
<th>Subgroup Impact</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Subgroup Impact</td>
<td></td>
<td>Subgroup Impact</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Control</td>
<td>(Difference&lt;sup&gt;b&lt;/sup&gt;)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(Impact</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gender</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td>968</td>
<td>64.7%</td>
<td>60.0%</td>
<td>4.8&lt;sup&gt;*&lt;/sup&gt;</td>
</tr>
<tr>
<td>Men</td>
<td>871</td>
<td>80.0%</td>
<td>80.1</td>
<td>-0.1</td>
</tr>
<tr>
<td>Ethnicity</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>155</td>
<td>82.9</td>
<td>78.8</td>
<td>4.1</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>840</td>
<td>69.3</td>
<td>64.1</td>
<td>5.3&lt;sup&gt;*&lt;/sup&gt;</td>
</tr>
<tr>
<td>Hispanic</td>
<td>783</td>
<td>73.9</td>
<td>73.2</td>
<td>0.7</td>
</tr>
<tr>
<td>Other</td>
<td>61</td>
<td>55.3</td>
<td>72.8</td>
<td>-17.6</td>
</tr>
<tr>
<td>Ethnicity, by gender</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>84</td>
<td>78.4</td>
<td>70.1</td>
<td>8.3</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>445</td>
<td>64.0</td>
<td>54.2</td>
<td>9.8&lt;sup&gt;**&lt;/sup&gt;</td>
</tr>
<tr>
<td>Hispanic</td>
<td>413</td>
<td>64.4</td>
<td>64.6</td>
<td>-0.3</td>
</tr>
<tr>
<td>Other</td>
<td>26</td>
<td>40.4</td>
<td>55.8</td>
<td>-15.4</td>
</tr>
<tr>
<td>Men</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>71</td>
<td>87.2</td>
<td>88.6</td>
<td>-1.4</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>395</td>
<td>74.7</td>
<td>74.7</td>
<td>0.0</td>
</tr>
<tr>
<td>Hispanic</td>
<td>370</td>
<td>84.7</td>
<td>82.9</td>
<td>1.7</td>
</tr>
<tr>
<td>Other</td>
<td>35</td>
<td>70.8</td>
<td>87.5</td>
<td>-16.7</td>
</tr>
<tr>
<td>Parental status</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women living with own</td>
<td></td>
<td></td>
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<td>child(ren)</td>
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<td></td>
<td></td>
</tr>
<tr>
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<td>484</td>
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<td>63.0</td>
<td>4.0</td>
</tr>
<tr>
<td>Yes</td>
<td>484</td>
<td>58.5</td>
<td>52.9</td>
<td>5.6</td>
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<td>Men who have own child(ren)</td>
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<td></td>
<td></td>
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<tr>
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<td>82.6</td>
<td>0.1</td>
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<tr>
<td>Yes</td>
<td>106</td>
<td>79.2</td>
<td>80.3</td>
<td>-1.1</td>
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(continued)
<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Ever Employed, Months 13-24</th>
<th>Subgroup Impact</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Experimentals</td>
<td>Controls</td>
<td></td>
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</tr>
<tr>
<td>Employed within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>870</td>
<td>70.0%</td>
<td>65.8%</td>
<td>4.2</td>
</tr>
<tr>
<td>Yes</td>
<td>969</td>
<td>73.8%</td>
<td>71.8%</td>
<td>1.0</td>
</tr>
<tr>
<td>Prior employment, by gender</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women employed within</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>547</td>
<td>61.1%</td>
<td>57.0%</td>
<td>4.1</td>
</tr>
<tr>
<td>Yes</td>
<td>421</td>
<td>68.2%</td>
<td>62.6%</td>
<td>5.6</td>
</tr>
<tr>
<td>Men employed within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>323</td>
<td>80.3%</td>
<td>76.0%</td>
<td>4.2</td>
</tr>
<tr>
<td>Yes</td>
<td>548</td>
<td>80.7%</td>
<td>83.4%</td>
<td>-2.6</td>
</tr>
<tr>
<td>Left school in grade 11 or 12</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
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<td>68.9%</td>
<td>67.7%</td>
<td>1.2</td>
</tr>
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<td>761</td>
<td>76.3%</td>
<td>72.1%</td>
<td>4.3</td>
</tr>
<tr>
<td>Received occupational training</td>
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<tr>
<td>within past year</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>1,529</td>
<td>72.3%</td>
<td>68.7%</td>
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</tr>
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<td>73.2%</td>
<td>-2.6</td>
</tr>
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</tr>
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<td>20 or 21</td>
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<td>69.9%</td>
<td>67.4%</td>
<td>2.5</td>
</tr>
<tr>
<td>Age, by gender</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-19</td>
<td>710</td>
<td>65.5%</td>
<td>60.5%</td>
<td>5.0</td>
</tr>
<tr>
<td>20 or 21</td>
<td>258</td>
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<td>58.5%</td>
<td>4.3</td>
</tr>
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<td>Men</td>
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<td></td>
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</tr>
<tr>
<td>16-19</td>
<td>649</td>
<td>80.8%</td>
<td>81.0%</td>
<td>-0.2</td>
</tr>
<tr>
<td>20 or 21</td>
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<td>77.7%</td>
<td>77.4%</td>
<td>0.3</td>
</tr>
<tr>
<td>Characteristic and Subgroups</td>
<td>Sample Size</td>
<td>Ever Employed, Months 13-24</td>
<td>Subgroup Impact</td>
<td>p&lt;sup&gt;a&lt;/sup&gt;</td>
</tr>
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<td>-----------------------------</td>
<td>-------------</td>
<td>-----------------------------</td>
<td>----------------</td>
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</tr>
<tr>
<td>Marital status</td>
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<tr>
<td>Ever married</td>
<td>174</td>
<td>77.6%</td>
<td>75.3%</td>
<td>2.2</td>
</tr>
<tr>
<td>Never married</td>
<td>1,665</td>
<td>71.4</td>
<td>68.9</td>
<td>2.5</td>
</tr>
<tr>
<td>Living in own household or</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>with boy/girlfriend</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,500</td>
<td>72.2</td>
<td>69.9</td>
<td>2.3</td>
</tr>
<tr>
<td>Yes</td>
<td>339</td>
<td>71.1</td>
<td>67.9</td>
<td>3.1</td>
</tr>
<tr>
<td>Own AFDC case or receiving</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>General Assistance</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,344</td>
<td>74.1</td>
<td>70.2</td>
<td>3.9*</td>
</tr>
<tr>
<td>Yes</td>
<td>495</td>
<td>66.3</td>
<td>67.7</td>
<td>-1.5</td>
</tr>
<tr>
<td>Own AFDC case</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,446</td>
<td>73.0</td>
<td>70.3</td>
<td>2.7</td>
</tr>
<tr>
<td>Yes</td>
<td>393</td>
<td>68.1</td>
<td>66.6</td>
<td>1.5</td>
</tr>
<tr>
<td>Receiving Food Stamps</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,143</td>
<td>73.3</td>
<td>68.9</td>
<td>4.3*</td>
</tr>
<tr>
<td>Yes</td>
<td>696</td>
<td>69.9</td>
<td>70.5</td>
<td>-0.6</td>
</tr>
<tr>
<td>Arrested since age 16</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,567</td>
<td>72.1</td>
<td>70.9</td>
<td>1.2</td>
</tr>
<tr>
<td>Yes</td>
<td>272</td>
<td>71.2</td>
<td>61.6</td>
<td>9.6*</td>
</tr>
<tr>
<td>Lived with both parents at</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>age 14</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,198</td>
<td>71.2</td>
<td>66.8</td>
<td>4.5*</td>
</tr>
<tr>
<td>Yes</td>
<td>641</td>
<td>73.4</td>
<td>74.6</td>
<td>-1.2</td>
</tr>
<tr>
<td>Reason for leaving regular</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>high school</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>School-related</td>
<td>886</td>
<td>74.0</td>
<td>69.1</td>
<td>4.9*</td>
</tr>
<tr>
<td>Job-related</td>
<td>182</td>
<td>73.8</td>
<td>77.3</td>
<td>-3.5</td>
</tr>
<tr>
<td>Other</td>
<td>771</td>
<td>69.1</td>
<td>67.9</td>
<td>1.2</td>
</tr>
</tbody>
</table>
TABLE 5.3 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic; that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.
shows that—controlling for other measured differences in the subgroups—the impact for women was 4.9 percentage points greater than the impact for men and that this "subgroup impact difference" was not statistically significant. The "subgroup impact" column shows that this difference is calculated as 4.8 percentage points (the impact for the women) less -0.1 percentage points (the impact for men). The within-subgroup impact was statistically significant for women but not for men. The more detailed categories based on parenting status by gender also show stronger impacts for women than for men, and for mothers than for other women, although here again the differences in impacts were not statistically significant. Thus, the earlier apparent differences in employment rate impacts for men, young mothers, and other women did not disappear (although they were not statistically significant) when other measured characteristics of each group were accounted for in the analysis.

The rest of Table 5.3 presents other subgroup impacts on second-year employment rates in a similar fashion. Nothing in the final column is significant in a statistical sense, although impact differences seem sizable in absolute terms (more than 5 percentage points) for arrest history, prior training, two-parent upbringing, welfare receipt, and ethnicity.

There is no clear pattern to these differences in impacts.

- In some of these cases, those with greater barriers to employment (represented by control group employment rates) had greater impacts. Women had larger impacts than men, youths with no previous training had larger impacts than those with previous training, those arrested since

---

7(...continued)
define the subgroup under review. See Ostle, 1975, p. 461. The adjustments were done using a linear regression model. Characteristics that affect outcomes and impacts through a nonlinear relationship are not controlled for with this procedure. And no such procedure could control for unmeasured characteristics that affected outcomes and impacts. The adjusted outcomes and impacts presented here for women and for men are not identical to those presented in Table 5.2 because the former attempt to take observed non-gender differences between women and men into account. The differences in impacts are very minor, but the adjustments change some outcome measures several percentage points.

8This is indicated by the absence of stars on the "subgroup impact difference."

9This subgroup impact can be statistically significant, even though it is smaller than the impact for young mothers alone, because the sample size is larger.

10As discussed in Chapter 2 and Appendix B, subgroup impact differences must be interpreted carefully because characteristics such as gender were not assigned at random and so can be correlated with other characteristics. It could very well be that it was not gender per se that explained apparently higher impacts for women than for men, but some observed or unobserved non-gender difference between the women and the men in this sample. The calculations in Table 5.4 incorporate simple adjustments for observed characteristics besides gender, but not all differences were observed, and observed characteristics may have had more complex effects on outcomes and impacts than those modeled in these adjustments.
age 16 had larger impacts than those who had not been arrested, and those who did not live with two parents at age 14 had larger impacts than those living in a two-parent family.

- For other subgroups, however, the opposite pattern emerged. Impacts were higher for those not receiving public assistance at random assignment than for recipients.

- For some subgroups — most notably those based on ethnicity — no clear pattern emerged. Blacks had the highest impacts, Hispanics had virtually no impacts, and "other nonwhites" (primarily Asians) had large negative impacts.

This mixed finding differs from the results of several studies of employment programs for welfare recipients, which did find patterns among subgroup impacts. In these programs, impacts tended to be small or nonexistent for the most job-ready and the least job-ready and positive for a group in the middle tier.11 Clearly, the subgroups used in the JOBSTART analysis are much more narrowly defined, which may contribute to the absence of a pattern in the impacts.12

Other characteristics seem to have been more weakly associated with impact differences in second-year employment rates:

- Recent employment. Employment at some point in the year before enrollment is associated with a 3.2 percentage point lower impact, weakly supporting the position that it is easier to raise employment rates for those who have less of a work history.

- Recent employment, by gender. Employment rate impacts were positive both for women who had some employment and for women who had no employment in the year before program entry.13 However, for men, the positive impacts for those who were not employed were canceled out by negative impacts for those who had some employment in the pre-program year.

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12A second reason for not highlighting differences in impacts for subgroups is that the findings are somewhat sensitive to the outcome measure chosen. For example, when a similar analysis is done for employment in the last three months of follow-up, the results are somewhat different, but the general conclusion (that there is no clear pattern to the subgroup findings) remains.
13For the previously employed women, the impact in year two was 1.5 percentage points higher than the impact for women who were not employed, although this subgroup impact difference was not statistically significant.
- **Highest grade attended.** While all JOBSTART enrollees were high school dropouts, some left school in the ninth grade, while others dropped out during their senior year. Although the difference in impacts was not statistically significant, second-year employment was improved by a wider margin for those who dropped out in later grades than for those who dropped out earlier.\(^\text{14}\)

- **Recent prior skills training.** For a substantial minority of the sample, JOBSTART was not the first try at a second-chance program. The second-year employment impact was 6.1 percentage points higher (not statistically significant) for those who had not tried another program during the 12 months preceding enrollment in JOBSTART.

- **Age.** Very different patterns of labor market behavior are shown at each age in the general youth population. Labor force participation, employment, and earnings increase dramatically from age 16 to the early twenties. Moreover, program operators often suggest that younger enrollees do not derive as much benefit from training as do somewhat older youths who are ready to "settle down" and pursue stable employment. Table 5.3 gives no support whatever to this hypothesis: The difference in impacts between teenagers and older youths is precisely zero and the younger youths have higher employment rates.\(^\text{15}\)

### III. **JOBSTART's Impacts on Earnings**

The full-sample impact of JOBSTART on earnings during the two-year follow-up period is less encouraging than on employment rates, although once again there is some evidence of movement in the right direction. As shown in Table 5.4, during the first 12 months following random assignment, controls earned $585 more than experimentals, while during the second 12 months, controls earned $205 more. Once again, the month-by-month pattern of impacts is less clear than the pattern of annual impacts: While experimentals moved even with controls during months 14 and 15, after that point the earnings impact fluctuated within a fairly narrow and slightly negative range.

\(^\text{14}\)This difference was driven by lower employment for controls who dropped out earlier rather than by higher employment for experimentals who dropped out earlier.

\(^\text{15}\)On this point, analysis of the employment rate for the last three months of follow-up suggests a different result: Younger women do better than older women, while older men do better than younger men.
### TABLE 5.4
IMPACTS ON EARNINGS THROUGH MONTH 24

<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total earnings</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>$5,859.56</td>
<td>$6,649.07</td>
<td>-$789.52***</td>
<td>0.010</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>1,965.47</td>
<td>2,550.26</td>
<td>-584.78***</td>
<td>0.000</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>3,894.08</td>
<td>4,098.81</td>
<td>-204.73</td>
<td>0.331</td>
</tr>
<tr>
<td>Earnings</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month 1</td>
<td>37.92</td>
<td>64.30</td>
<td>-26.39***</td>
<td>0.001</td>
</tr>
<tr>
<td>Month 2</td>
<td>93.65</td>
<td>141.78</td>
<td>-48.13***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 3</td>
<td>109.16</td>
<td>165.51</td>
<td>-56.35***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 4</td>
<td>125.41</td>
<td>187.20</td>
<td>-61.79***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 5</td>
<td>146.31</td>
<td>211.13</td>
<td>-64.81***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 6</td>
<td>158.06</td>
<td>222.28</td>
<td>-64.22***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 7</td>
<td>179.79</td>
<td>234.87</td>
<td>-55.08***</td>
<td>0.001</td>
</tr>
<tr>
<td>Month 8</td>
<td>196.91</td>
<td>240.63</td>
<td>-43.72**</td>
<td>0.011</td>
</tr>
<tr>
<td>Month 9</td>
<td>213.35</td>
<td>256.66</td>
<td>-43.31**</td>
<td>0.013</td>
</tr>
<tr>
<td>Month 10</td>
<td>228.26</td>
<td>265.40</td>
<td>-37.15**</td>
<td>0.036</td>
</tr>
<tr>
<td>Month 11</td>
<td>226.07</td>
<td>281.29</td>
<td>-55.22***</td>
<td>0.002</td>
</tr>
<tr>
<td>Month 12</td>
<td>250.59</td>
<td>279.20</td>
<td>-28.61</td>
<td>0.115</td>
</tr>
<tr>
<td>Month 13</td>
<td>254.54</td>
<td>273.39</td>
<td>-18.85</td>
<td>0.289</td>
</tr>
<tr>
<td>Month 14</td>
<td>294.66</td>
<td>391.32</td>
<td>3.33</td>
<td>0.839</td>
</tr>
<tr>
<td>Month 15</td>
<td>313.56</td>
<td>309.39</td>
<td>4.17</td>
<td>0.839</td>
</tr>
<tr>
<td>Month 16</td>
<td>314.47</td>
<td>324.95</td>
<td>-10.48</td>
<td>0.609</td>
</tr>
<tr>
<td>Month 17</td>
<td>317.17</td>
<td>340.33</td>
<td>-23.16</td>
<td>0.269</td>
</tr>
<tr>
<td>Month 18</td>
<td>322.40</td>
<td>346.92</td>
<td>-24.52</td>
<td>0.239</td>
</tr>
<tr>
<td>Month 19</td>
<td>320.80</td>
<td>364.48</td>
<td>-43.68**</td>
<td>0.041</td>
</tr>
<tr>
<td>Month 20</td>
<td>336.01</td>
<td>357.61</td>
<td>-21.60</td>
<td>0.320</td>
</tr>
<tr>
<td>Month 21</td>
<td>351.86</td>
<td>360.39</td>
<td>-8.53</td>
<td>0.695</td>
</tr>
<tr>
<td>Month 22</td>
<td>354.24</td>
<td>370.68</td>
<td>-16.44</td>
<td>0.445</td>
</tr>
<tr>
<td>Month 23</td>
<td>355.78</td>
<td>373.41</td>
<td>-17.63</td>
<td>0.417</td>
</tr>
<tr>
<td>Month 24</td>
<td>358.60</td>
<td>385.95</td>
<td>-27.35</td>
<td>0.214</td>
</tr>
</tbody>
</table>

Sample size 949 890

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

(continued)
Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

<table>
<thead>
<tr>
<th>Group</th>
<th>Outcome 1</th>
<th>Outcome 2</th>
<th>Outcome 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>10.5</td>
<td>12.3</td>
<td>14.4</td>
</tr>
<tr>
<td>B</td>
<td>8.9</td>
<td>9.8</td>
<td>10.7</td>
</tr>
</tbody>
</table>
A. Impacts by Gender

As with employment rates, the impacts for earnings at this point seem more encouraging for women than for men. Table 5.5 shows that for men, controls earned $1,028 more than experimentals in the first year of follow-up and $667 more in the second. For women who were living with their own children, experimentals have already pulled slightly ahead of controls; experimentals earned $89 less in the first year and then earned $172 more in the second year. The trend for other women is also in a positive direction: An earnings loss of $423 was followed by an earnings gain of $202.

B. Impacts for Other Subgroups

Table 5.6 presents second-year earnings impacts by selected subgroup and differences among these subgroup impacts. As in Table 5.3, this table presents comparisons of subgroup impacts for the designated groups, controlling for differences other than the one used to define the subgroup categories. The first line of the table shows that the difference in second-year earnings impact by gender (controlling for other measured differences of men and women) was $826 (−$639 for men versus $187 for women) and that it was statistically significant. The observed male-female earnings differences in impacts, presented in Table 5.5, therefore, do not appear to have resulted from other measured non-gender differences in their pre-random assignment characteristics (such as differences in education or work experience.)

The rest of Table 5.6 shows impacts by other subgroup splits. The results generally mirror what already has been said about employment rates, with a few exceptions.

IV. Moving Behind These Employment-Related Impacts

The analysis presented so far can be summarized as showing that employment rates for experimentals have caught up with those of controls during the follow-up period, but (especially for men) earnings of experimentals still lag behind. A starting point for understanding this result is to look behind the summary measure of employment success

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16 Male controls also out-earned experimentals in every month of the follow-up period, though during much of the second year, these differences were not statistically significant.

17 As in Table 5.4, adjusted outcomes and impacts by gender differ slightly from their counterparts in previous tables because of the different way they were calculated, using the entire sample rather than splitting by gender and making separate calculations.
# Table 5.5

**Impacts on Earnings through Month 24, by Gender and Parental Status**

<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p (^a)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Total earnings ($)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-year total</td>
<td>7,797.22</td>
<td>9,492.61</td>
<td>-1,695.39***</td>
<td>0.001</td>
</tr>
<tr>
<td>Year 1</td>
<td>2,648.50</td>
<td>3,676.88</td>
<td>-1,028.38***</td>
<td>0.000</td>
</tr>
<tr>
<td>Year 2</td>
<td>5,148.72</td>
<td>5,815.73</td>
<td>-667.01*</td>
<td>0.057</td>
</tr>
<tr>
<td>Quarter 1</td>
<td>348.17</td>
<td>551.11</td>
<td>-202.93***</td>
<td>0.001</td>
</tr>
<tr>
<td>Quarter 2</td>
<td>581.48</td>
<td>918.15</td>
<td>-336.67***</td>
<td>0.000</td>
</tr>
<tr>
<td>Quarter 3</td>
<td>796.52</td>
<td>1,022.11</td>
<td>-225.60***</td>
<td>0.008</td>
</tr>
<tr>
<td>Quarter 4</td>
<td>922.33</td>
<td>1,185.50</td>
<td>-263.18***</td>
<td>0.003</td>
</tr>
<tr>
<td>Quarter 5</td>
<td>1,119.86</td>
<td>1,286.81</td>
<td>-166.94*</td>
<td>0.075</td>
</tr>
<tr>
<td>Quarter 6</td>
<td>1,266.70</td>
<td>1,426.00</td>
<td>-159.30</td>
<td>0.112</td>
</tr>
<tr>
<td>Quarter 7</td>
<td>1,354.56</td>
<td>1,506.69</td>
<td>-152.13</td>
<td>0.134</td>
</tr>
<tr>
<td>Quarter 8</td>
<td>1,407.60</td>
<td>1,596.24</td>
<td>-188.64*</td>
<td>0.063</td>
</tr>
<tr>
<td><strong>Sample size</strong></td>
<td>438</td>
<td>433</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Women living with own child(ren)**

| Total earnings ($)                      |               |          |            |         |
| 2-year total                           | 3,035.55      | 2,952.31 | 83.24      | 0.852   |
| Year 1                                 | 1,011.83      | 1,100.96 | -89.13     | 0.647   |
| Year 2                                 | 2,023.73      | 1,851.35 | 172.38     | 0.600   |
| Quarter 1                              | 75.56         | 151.45   | -75.89**   | 0.036   |
| Quarter 2                              | 200.41        | 247.18   | -46.77     | 0.396   |
| Quarter 3                              | 336.41        | 328.49   | 7.93       | 0.905   |
| Quarter 4                              | 399.44        | 373.84   | 25.60      | 0.733   |
| Quarter 5                              | 474.35        | 355.07   | 119.27     | 0.150   |
| Quarter 6                              | 533.50        | 447.95   | 85.55      | 0.361   |
| Quarter 7                              | 543.85        | 503.85   | 40.00      | 0.682   |
| Quarter 8                              | 472.03        | 544.47   | -72.45     | 0.461   |
| **Sample size**                        | 250           | 234      |            |         |

**Women not living with own child(ren), including those who did not have any**

| Total earnings ($)                      |               |          |            |         |
| 2-year total                           | 5,070.23      | 5,290.80 | -220.58    | 0.667   |
| Year 1                                 | 1,607.50      | 2,030.02 | -422.53*   | 0.055   |
| Year 2                                 | 3,462.73      | 3,260.78 | 201.95     | 0.603   |
| Quarter 1                              | 191.97        | 285.26   | -93.29**   | 0.045   |
| Quarter 2                              | 353.67        | 482.98   | -129.31*   | 0.069   |
| Quarter 3                              | 465.73        | 617.07   | -151.34*   | 0.052   |
| Quarter 4                              | 596.12        | 644.71   | -48.59     | 0.540   |
| Quarter 5                              | 766.80        | 660.17   | 106.63     | 0.254   |
| Quarter 6                              | 796.34        | 842.71   | -46.36     | 0.662   |
| Quarter 7                              | 860.12        | 881.52   | -21.39     | 0.850   |
| Quarter 8                              | 1,039.45      | 876.38   | 163.07     | 0.177   |
| **Sample size**                        | 261           | 223      |            |         |

(continued)
TABLE 5.5 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
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<tr>
<th>Characteristic and Subgroups</th>
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<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Subgroup Impact Difference&lt;sup&gt;b&lt;/sup&gt;</th>
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(continued)
TABLE 5.6 (continued)

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<th>Subgroup Impact Difference</th>
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(continued)
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<th>Subgroup Impact</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
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<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
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TABLE 5.6 (continued)

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<th>Subgroup Impact</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Subgroup Impact Difference&lt;sup&gt;b&lt;/sup&gt;</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
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**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

<sup>a</sup>A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

<sup>b</sup>For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.
(earnings) and examine how much each group was working. Such an analysis shows that even though the proportion of experimentals working during the follow-up period drew even with that of controls, experimentals continued to lag behind controls in the actual time spent working (hours per week and weeks per month). Also, hourly wage information will be presented for those experimentals and controls who were working, as a way to provide some information on the types of jobs they found.

A. Impacts on the Hours and Weeks of Employment

As reported above, JOBSTART had a slightly positive impact on total two-year earnings for women living with their own children, a small negative impact for other women, and a clearly negative impact for men. This pattern carries through in hours and weeks worked, as shown in Table 5.7. On both measures, male experimentals worked less than did male controls in each year, although the difference was larger and statistically significant in the first year. For women living with their own children, experimentals were only slightly below controls (the differences were not statistically significant) in average hours worked per week and average weeks worked per month during the first year of follow-up. In the second year, experimentals moved slightly above controls on both measures, although the differences were again not statistically significant. Thus, for the entire follow-up period, experimentals were virtually the same as controls on both measures. For other women, negative impacts in the first year were larger than small positive impacts in the second, leaving experimentals in this group slightly behind on both measures for the entire two-year follow-up period.

Table 5.7 tells another interesting gender-related story. Control group men spent much more time working than did their control group counterparts among women—especially young mothers—so it is no wonder that male experimentals sacrificed more employment and earnings to take part in JOBSTART than did women. During the first year of follow-up, male controls worked an average of more than a week more per month than did controls among women living with their own children, and about half a week more than did other women in the control group. They also averaged about nine hours per week more than did women living with their own children, and five hours more than other women. These gender differences in

18 This effort to understand the pattern of impacts is also the topic of Chapter 6, which discusses results by site.
### TABLE 5.7

**IMPACTS ON WEEKS EMPLOYED PER MONTH AND
HOURS WORKED PER WEEK THROUGH MONTH 24,
BY GENDER AND PARENTAL STATUS**

| Subgroup, Outcome, and Follow-Up Period | Experimentals | Controls | Difference | p
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average number of weeks employed per month during</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>1.74</td>
<td>1.98</td>
<td>-0.24***</td>
<td>0.006</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>1.32</td>
<td>1.71</td>
<td>-0.40***</td>
<td>0.000</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>2.17</td>
<td>2.26</td>
<td>-0.09</td>
<td>0.432</td>
</tr>
<tr>
<td>Average number of hours worked per week during</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>15.02</td>
<td>17.36</td>
<td>-2.34***</td>
<td>0.005</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>10.74</td>
<td>14.37</td>
<td>-3.64***</td>
<td>0.000</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>19.30</td>
<td>20.34</td>
<td>-1.04</td>
<td>0.329</td>
</tr>
<tr>
<td>Sample size</td>
<td>438</td>
<td>433</td>
<td></td>
<td></td>
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<tr>
<td><strong>Women living with own child(ren)</strong></td>
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<td></td>
<td></td>
<td></td>
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<tr>
<td>Average number of weeks employed per month during</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>0.78</td>
<td>0.78</td>
<td>0.00</td>
<td>0.971</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>0.61</td>
<td>0.66</td>
<td>-0.05</td>
<td>0.577</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>0.96</td>
<td>0.90</td>
<td>0.06</td>
<td>0.621</td>
</tr>
<tr>
<td>Average number of hours worked per week during</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>6.08</td>
<td>6.32</td>
<td>-0.24</td>
<td>0.764</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>4.56</td>
<td>5.15</td>
<td>-0.59</td>
<td>0.483</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>7.60</td>
<td>7.50</td>
<td>0.10</td>
<td>0.928</td>
</tr>
<tr>
<td>Sample size</td>
<td>250</td>
<td>234</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Women not living with own child(ren), including those who did not have any</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average number of weeks employed per month during</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>1.28</td>
<td>1.35</td>
<td>-0.07</td>
<td>0.543</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>0.97</td>
<td>1.20</td>
<td>-0.23*</td>
<td>0.055</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>1.59</td>
<td>1.50</td>
<td>0.09</td>
<td>0.534</td>
</tr>
<tr>
<td>Average number of hours worked per week during</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months 1-24</td>
<td>10.20</td>
<td>11.00</td>
<td>-0.80</td>
<td>0.401</td>
</tr>
<tr>
<td>Months 1-12</td>
<td>7.12</td>
<td>9.40</td>
<td>-2.28**</td>
<td>0.017</td>
</tr>
<tr>
<td>Months 13-24</td>
<td>13.27</td>
<td>12.59</td>
<td>0.68</td>
<td>0.610</td>
</tr>
<tr>
<td>Sample size</td>
<td>261</td>
<td>223</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

(continued)
TABLE 5.7 (continued)

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
controls' hours and weeks worked were much greater than the experimental-control differences in hours within each subgroup.

B. **Differences in Wage Rates**

Earnings are the product of time spent working (discussed above) and wages earned per hour. Departing from the analysis so far, the discussion in this section excludes some experimentals and controls (those who did not work during the periods indicated) because wage rates were not observed for nonworkers. Thus, the experimental-control differences discussed here are not impacts, but they do help us understand the experience underlying the impact findings.\(^{19}\)

Overall, both experimentals and controls showed a gradual growth over time, although the trends are not always clear. Wages for working experimentals began at about $4.49 per hour and grew to about $5.14 per hour; for working controls, wages began at approximately $4.43 per hour and grew to about $5.26 per hour. Although only a 34-cent advantage for working controls over working experimentals in month 10 was statistically significant, working controls earned more per hour than working experimentals in most months.

Splitting the sample by gender reveals that positive wage differences for working women almost counterbalance negative wage differences for working men each month. For example, in month 24, among working men, experimentals earned $5.03 and controls earned $5.39 per hour, for a difference of minus 36 cents, while among women, experimentals earned $5.32 and controls earned $5.01, for a difference of plus 32 cents. Among working women, experimentals overtook controls in hourly wages at about the same time they overtook them in employment and earnings. Among working men, experimentals continue to lag behind controls on this measure just as they do in earnings.

V. **JOBSTART's Impacts on Positive Activity**

A second step in understanding the short-term employment and earnings impact findings is to examine whether access to JOBSTART led more experimentals than controls to spend

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\(^{19}\)This is because of the likelihood of selection bias in the "choice" of those experimentals and controls who worked. While random assignment created experimental and control groups with similar characteristics at random assignment — except that experimentals had access to JOBSTART — it is not likely that working experimentals and working controls were similar except for access to JOBSTART.
their time in "positive activity," defined as either work or further education and training. Experimentals might have been working less than controls because experimentals were continuing to invest time in enhancing their skills.

This does seem to have been the case for much of the follow-up period. Figures 5.2 through 5.4 present impacts on positive activity for men, women living with their own children, and other women. For men, experimentals exceeded controls by a statistically significant amount for the first 10 months of follow-up and months 15 and 16. For women living with their own children, experimentals exceeded controls for virtually all the first 18 months. For other women, experimentals exceeded controls for much of the first 10 months and then sporadically thereafter.

The basic conclusion is that one important reason for lower earnings for experimentals and controls was sustained greater investment in education and training by the JOBSTART youths. This could signal an improved earnings situation once longer follow-up is available. However, during the 24 months of follow-up, no strong turnaround occurred.20

VI. JOBSTART's Impacts on Other Outcomes

If JOBSTART is successful, other aspects of the JOBSTART youths' lives may also change. Key outcomes of interest include receipt of public benefits, criminal activity, and marital and parental status. Even at this early stage of follow-up, when the employment impact story has not yet been told, it is useful to summarize briefly early findings on these other aspects of the young people's lives.

During the first 24 months of follow-up, JOBSTART had virtually no significant pattern of impacts on the receipt of a long list of public benefits, including AFDC, Food Stamps, and unemployment insurance benefits. This general conclusion is true for the full sample and both women and men. The two exceptions are: (1) In some months, experimental men were more likely to receive General Assistance than were control men (although the payments were never higher by a statistically significant amount); and (2) in some months, experimental women were

20 For example, in the final quarter of follow-up, when employment rates and rates of productive activity were about equal, the average earnings of controls remained slightly ($61) above those of experimentals for the full impact sample.
FIGURE 5.2
MONTHLY RATES OF POSITIVE ACTIVITY
FOR MEN, BY RESEARCH STATUS

Months Since Random Assignment

Employed or in Education or Training During Month (%)

Experimentals
Controls

FIGURE 5.3
MONTHLY RATES OF POSITIVE ACTIVITY
FOR WOMEN LIVING WITH THEIR OWN CHILD(REN),
BY RESEARCH STATUS

Months Since Random Assignment

Employed or in Education or Training During Month (%)

Experimentals
Controls
MONTHLY RATES OF POSITIVE ACTIVITY FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

FIGURE 5.4

*Includes women who did not have children.

SOURCE FOR FIGURES 5.2-5.4: Appendix Table F.6.

NOTES FOR FIGURES 5.2-5.4:

*Positive activity* includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities, as well as employment.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.
slightly less likely to receive Supplemental Security Income (SSI) and received less in payments than did control women.

By this early time in post-program follow-up, JOBSTART has not had any statistically significant impact on criminal behavior, as evidenced by arrests during months one through 24 of follow-up (Table 5.8). For men and mothers, experimentals had a slightly lower rate of arrest than did controls, while for other women, experimentals were slightly above controls. However, none of the differences were large.21 Impacts were also favorable for many other subgroups, but virtually none was large enough for statistical significance.22

This picture differs from that produced by the previous study of the residential Job Corps program, but there is a clear reason to expect much less of an early impact on criminal behavior for JOBSTART. In the residential Job Corps program, young people moved from their community to a special center, which provided the education and training services. Often, these centers are in isolated areas or in communities without large gang populations or heavy involvement of youths in the drug trade. Much of the Job Corps' impact on criminal behavior came during the in-program period because of this "isolation" effect. In JOBSTART, no such change occurred in the young people’s lives: They continued to live in their own neighborhoods and spent time outside the program with their existing circle of friends.

A final dimension of program effects to consider is childbearing. According to Table 5.9, among those women who were custodial parents at random assignment, assignment to the JOBSTART experimental group was associated with a statistically significant increase in childbirth by month 24; the increase was 7.3 percentage points. Among other women, the vast majority of whom were childless at random assignment, JOBSTART lowered the rate of childbirth by 2.9 percentage points, although this difference was not statistically significant. These findings are important because they show the extent to which childbearing may be inhibiting greater labor market impacts among JOBSTART women.

21Among men, 30.2 percent of experimentals and 33.4 percent of controls were arrested during the follow-up period. Among women, 7.6 percent of experimentals and 8.0 percent of controls were arrested.
22The only exceptions were the impact on arrests for white, non-Hispanic youths (especially men) and on youths reading below the sixth-grade level at entry into the program.
TABLE 5.8  
IMPACTS ON CRIMINAL ARRESTS THROUGH MONTH 24,  
BY GENDER AND PARENTAL STATUS

<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever arrested, months 1-24 (%)</td>
<td>30.2</td>
<td>33.4</td>
<td>-3.2</td>
<td>0.290</td>
</tr>
<tr>
<td>Sample size</td>
<td>438</td>
<td>433</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Women living with own child(ren)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever arrested, months 1-24 (%)</td>
<td>6.8</td>
<td>9.4</td>
<td>-2.6</td>
<td>0.306</td>
</tr>
<tr>
<td>Sample size</td>
<td>250</td>
<td>234</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Women not living with own child(ren), including those who did not have any</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever arrested, months 1-24 (%)</td>
<td>8.0</td>
<td>6.8</td>
<td>1.1</td>
<td>0.645</td>
</tr>
<tr>
<td>Sample size</td>
<td>261</td>
<td>223</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

^aThe column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
# TABLE 5.9
IMPACTS ON PREGNANCY AND CHILDBIRTH THROUGH MONTH 24, 
BY PARENTAL STATUS AT THE TIME OF RANDOM ASSIGNMENT

<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimental</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Women living with own child(ren)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever pregnant, months 1-24 (%)</td>
<td>59.1</td>
<td>53.1</td>
<td>6.0</td>
<td>0.191</td>
</tr>
<tr>
<td>Ever gave birth, months 1-24 (%)</td>
<td>32.7</td>
<td>25.4</td>
<td>7.3*</td>
<td>0.087</td>
</tr>
<tr>
<td>Sample size</td>
<td>250</td>
<td>234</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Women not living with own child(ren), including those who did not have any</strong></td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever pregnant, months 1-24 (%)</td>
<td>45.6</td>
<td>47.5</td>
<td>-1.9</td>
<td>0.678</td>
</tr>
<tr>
<td>Ever gave birth, months 1-24 (%)</td>
<td>18.5</td>
<td>21.4</td>
<td>-2.9</td>
<td>0.443</td>
</tr>
<tr>
<td>Sample size</td>
<td>261</td>
<td>223</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

^aThe column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
CHAPTER 6

SITE VARIATION IN IMPACTS

Chapters 4 and 5 provided estimates of JOBSTART’s impacts on educational attainment, employment, and earnings for the full sample and for key demographic subgroups. This chapter addresses another important issue: How were impacts affected by program characteristics? The most direct way to explore this issue within the JOBSTART Demonstration is to examine differences in individual site impacts, since there were some programmatic differences among the 13 sites and the programs operated in a variety of settings. For reasons soon to be discussed, this analysis of impacts across sites, of necessity, is less certain than that reported in the previous two chapters.

While the limited but still relatively large number of sites in JOBSTART is clearly an advantage in this effort, it does not assure success in "teasing out" how program characteristics affected impacts. As with virtually all multi-site demonstrations, the JOBSTART Demonstration was not designed to address, with the rigor of an experiment, the question of how differences in program structure influence impacts. The major goal of the demonstration was to ascertain whether the model, as implemented in a diverse sample of sites, would lead to improved employment and earnings. For this purpose, youths in each site were randomly assigned to the experimental and control groups.

To answer, with similar rigor, the question of how program characteristics affect impacts, youths in individual sites would have had to be randomly assigned to one of the several types of programs of interest (for example, those emphasizing education versus those emphasizing training), and individual sites would have had to offer more than one type of program. This kind of study — known as a "differential impact" research design — would be needed to separate out clearly the influence of program type from other site variations such as local labor market conditions and differences in the kinds of youths drawn to the various sites. This approach was not followed in light of the main goal of the study: to use the combined cross-site sample to answer impact questions about the full sample and subgroups.

1For example, as discussed in Chapter 2, sites in the JOBSTART Demonstration did vary in the proportions of their youths who had prior work experience and welfare receipt, and in the proportion with children and from the black and Hispanic ethnic groups.

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Nevertheless, the JOBSTART Demonstration did provide an opportunity to learn more about the link between program characteristics and impacts. For example, if the respective categories of site programs had shown clear differences in impacts, certain lessons might have been drawn (including the lack of influence of other factors). As the findings in this chapter show, however, no clear pattern emerged: That is, no single category of sites did appreciably better than another in terms of two-year impacts. Instead, both positive and negative impacts appeared within each category, implying that JOBSTART can produce positive (or negative) impacts in a variety of settings and with a variety of program structures. No single approach either assures positive impacts or is necessary for them.

Before turning to the analysis per se, some additional preliminaries may be useful. Much of the previous analysis in this report divided sites into three groups — concurrent, sequential/in-house, and sequential/brokered — because these categories helped explain differences in program implementation and participation among the sites. This chapter preserves these site groupings, since the implementation research led to a hypothesis that these differences in program structure could influence impacts. But it is important to keep in mind that these categories are not a neat division of the sites into homogeneous groups. One key difference within the concurrent category was discussed in Chapter 1 and elsewhere in the report: Two sites (CET/San Jose and Chicago Commons) integrated education into the training curriculum to a much greater extent than did the other concurrent sites. Other characteristics that varied within categories included the extent to which applicants were initially screened, the quality of sites' implementation of the four JOBSTART components, and labor market conditions. Thus, disentangling the independent effect of any single program feature on impacts across sites can be very difficult.

One final point should be made: The impacts on employment and earnings at the individual site level (even when noticeably different from zero) were usually not statistically significant. There are three reasons for this: (1) Most of the reported employment and earnings impacts were not large; (2) a site's program did not affect the behavior of all youths in the same way, and controls' earnings also varied (in technical terms, this suggests a large
variance in the outcomes); and (3) the sample size in each of the sites was relatively small. Furthermore, the differences in the individual site impacts for employment and earnings were also not statistically significant, suggesting again that lessons on program structure must be tentative.4

To summarize the implications of these fairly technical issues: Only if there was a clear pattern of impacts among types of sites should lessons be drawn from this analysis about how one particular program feature affected impacts. Furthermore, in general, conclusions should not be drawn from the impacts at any single site.

Despite these difficulties of analysis and interpretation, this chapter does try to draw some conclusions about the link between program features and impacts. It moves into this type of analysis because of the strong interest in identifying more effective ways to structure education and training programs, an interest intensified by the recent publication of the first impact report from the Minority Female Single Parent (MFSP) Demonstration, sponsored by the Rockefeller Foundation.5 That study analyzed program impacts in four sites offering very different types of education and training for low-income, minority mothers who averaged 28 years of age. One site, the Center for Employment Training (CET) in the San Francisco Bay area, offered an integrated program of training and education, with little initial screening of applicants and intensive job placement efforts. This site showed strong employment and earnings impacts in the fourth quarter of follow-up, while in the remaining three sites – each with an emphasis on basic skills instruction, which was to be followed by various types of occupational training – experimentals and controls had approximately equal employment and earnings. Some have attributed this difference in impacts between CET and the other sites to specific features of its program, especially its integration of education and training. CET/San Jose was a part of both the JOBSTART and MFSP demonstrations, although in JOBSTART, the CET/San Jose sample was made up primarily of men and women without children, since most young mothers were part of the MFSP Demonstration.

This chapter proceeds with a framework for analyzing site-level impacts. It then discusses site-level impacts for the full impact sample and for the three key subgroups of men, women

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4Statistical significance in this context is a test of whether the extent of variation in impacts across sites was so systematic that it was unlikely to have arisen by chance.  
living with their own children, and other women. It closes with a summary of the findings and directions for future research.

I. A Framework for Analyzing Site Variation in Impacts

This section discusses how JOBSTART sites varied in ways that could influence program impacts. It then presents two alternative types of site impact estimates used in this chapter.

A. Factors Leading to Site Variation in Impacts

The differences in program structure among the sites, discussed earlier in this report, were among the most obvious possible influences on site impacts. However, these program factors were entangled and confounded across JOBSTART sites with several other sources of variation in impacts including the target group recruited at various sites and their relative interest in education per se as opposed to training, the extent of screening by site operators, the point at which random assignment was placed for the evaluation, the availability of local alternative services for members of the control group, and environmental factors such as the wage structure and tightness of the labor market.

1. Program structure. As detailed in Chapter 3, dimensions of program structure that are of special interest include concurrent versus sequential education and training classes, the extent of integration of education and training, months of program activities offered and delivered, brokering of services among multiple vendors versus in-house provision of all services, the relative emphasis on education as opposed to occupational skills training, the strength of job placement efforts, and the intensity of support services. Table 6.1 combines selected findings from earlier tables to highlight key aspects of the sites. Sites are grouped under the headings "concurrent," "sequential/in-house," and "sequential/brokered."

"Length of stay" is an important dimension of site variation when examining follow-up in a short period such as two years. If programs were able to provide equal hours of instruction, in sites in which experimentals participated for shorter periods of time, there will have been more post-program time during which experimental employment outcomes could have overtaken control outcomes at the two-year point. CET/San Jose and Chicago Commons are notable on this score, with average lengths of stay less than two-thirds of the full sample average, even though total hours were higher than average at Chicago and 88 percent of the average at CET/San Jose. The "average hours per month" column is a measure of the extent to which
### TABLE 6.1

SUMMARY OF JOBSTART IMPLEMENTATION, BY SITE

<table>
<thead>
<tr>
<th>Site</th>
<th>Total Hours</th>
<th>Length of Stay (Months)</th>
<th>Average Hours per Month</th>
<th>Hours of Education</th>
<th>Average Hours per Month</th>
<th>Hours of Training</th>
<th>Level of Initial Screening</th>
<th>Rating of Job Placement</th>
<th>Rating of Support Services</th>
<th>Overall Rating of Implementation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>405</td>
<td>6.4</td>
<td>60</td>
<td>104</td>
<td>291</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>366</td>
<td>5.5</td>
<td>50</td>
<td>95</td>
<td>150</td>
<td>High</td>
<td>Low</td>
<td>Medium</td>
<td>Low</td>
<td>High</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>445</td>
<td>4.5</td>
<td>80</td>
<td>72</td>
<td>337</td>
<td>Low</td>
<td>High</td>
<td>Medium</td>
<td>High</td>
<td>Medium</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>579</td>
<td>10.1</td>
<td>54</td>
<td>105</td>
<td>473</td>
<td>High</td>
<td>High</td>
<td>Medium</td>
<td>High</td>
<td>High</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>345</td>
<td>5.4</td>
<td>55</td>
<td>73</td>
<td>272</td>
<td>Medium</td>
<td>Low</td>
<td>Medium</td>
<td>Medium</td>
<td>Medium</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>274</td>
<td>7.1</td>
<td>33</td>
<td>126</td>
<td>148</td>
<td>Low</td>
<td>Low</td>
<td>Low</td>
<td>Low</td>
<td>Low</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>441</td>
<td>6.5</td>
<td>60</td>
<td>163</td>
<td>218</td>
<td>High</td>
<td>High</td>
<td>Medium</td>
<td>High</td>
<td>High</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>418</td>
<td>5.2</td>
<td>76</td>
<td>124</td>
<td>294</td>
<td>Medium</td>
<td>Low</td>
<td>High</td>
<td>Medium</td>
<td>Medium</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td>536</td>
<td>7.1</td>
<td>66</td>
<td>152</td>
<td>287</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>401</td>
<td>5.8</td>
<td>60</td>
<td>142</td>
<td>179</td>
<td>Medium</td>
<td>Medium</td>
<td>Medium</td>
<td>Medium</td>
<td>High</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>631</td>
<td>8.1</td>
<td>71</td>
<td>159</td>
<td>362</td>
<td>High</td>
<td>Medium</td>
<td>Low</td>
<td>Medium</td>
<td>High</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td>316</td>
<td>7.8</td>
<td>37</td>
<td>178</td>
<td>78</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>440</td>
<td>10.7</td>
<td>39</td>
<td>244</td>
<td>113</td>
<td>Medium</td>
<td>Low</td>
<td>High</td>
<td>Medium</td>
<td>Medium</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>280</td>
<td>5.5</td>
<td>45</td>
<td>145</td>
<td>66</td>
<td>Low</td>
<td>Low</td>
<td>Medium</td>
<td>Low</td>
<td>Low</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>166</td>
<td>6.3</td>
<td>23</td>
<td>119</td>
<td>36</td>
<td>Low</td>
<td>Low</td>
<td>Medium</td>
<td>Low</td>
<td>Low</td>
</tr>
<tr>
<td>All sites</td>
<td>415</td>
<td>6.8</td>
<td>57</td>
<td>128</td>
<td>249</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
</tbody>
</table>

**Sources:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data (participation figures); MDRC operations staff (implementation ratings).

**Notes:**

- In this site, education and training were more integrated than in other sites, and staff strongly emphasized training over passing the GED examination.
- In this site, some education hours are included in the training component hours.
education, training, and other hours were concentrated or spread out over time. In general, the shorter the length of stay, the higher were the hours per month. CET/San Jose and Chicago Commons, the two sites with the shortest length of stay, had high average hours per month because they squeezed just about as much program activity as the average site had into shorter periods of time.

CET/San Jose and Chicago Commons had two things in common that help explain their relatively short length of stay. First, the programs were concurrent, offering education and training at the same time rather than one after the other. Second, there was less emphasis on education leading to a GED than on skills training in these integrated or partly integrated programs. Thus, youths in these programs typically stayed fewer months than those in sequential programs and in other concurrent programs that emphasized education more. For these reasons, one might hypothesize that second-year earnings impacts in these two sites might be greater than those in sites that provided the same level of services over a longer period of time. Opportunity costs (forgone employment and earnings) were smaller, and experimentals in the two sites had more time to catch up with and overtake controls in employment rates and earnings by month 24. One might also hypothesize that sites providing more services over a longer period could have greater longer-term impacts. This issue will be clarified when the four-year follow-up results are known.

The "hours of education" column shows much variation, but there was a tendency for the sequential — and especially sequential/brokered — sites to emphasize education more than the concurrent sites. Four of the eight concurrent sites averaged more than 50 hours less than the average for all sites, and only one of them exceeded the all-sites average. It stands to reason that concurrent sites would have delivered fewer hours of education than sequential sites because concurrent programs had to squeeze more activities into the school day. Variation in the proportion of youths getting any education was not a major factor in site variation in hours: Table 3.9 showed that, except at CET/San Jose (which did not offer separate education classes), this proportion varied in a narrow range around the full sample average of 86 percent.

The "hours of training" column shows more site variation than the previous columns. As already indicated, the dramatic difference between the sequential/brokered sites and the others on this score probably reflected both the systemic and administrative problems the former encountered in linking education and training and differences in the interests and expectations
of recruits. Table 3.10 showed that only one quarter of experimentals at sequential/broke:red
sites got any occupational skills training at all. One surprise here is that the two
sequential/in-house programs managed to deliver hours of training comparable to the hours in
concurrent programs. This achievement is notable, since only half the experimentals in
sequential/in-house sites stayed long enough to receive any training, while more than 80 percent
of experimentals in concurrent sites received training.

The "job placement" column repeats the subjective ranking given in Table 3.4 for the
strength of job placement assistance efforts in a site. CET/San Jose, Chicago Commons, and
the Phoenix Job Corps were considered especially strong on this score; Connelley in Pittsburgh,
El Centro in Dallas, and the Los Angeles Job Corps were considered middling; and all the
other sites were considered weak. Looking at this column by itself would lead one to predict
high labor market impacts early on at CET/San Jose, Chicago Commons, and the Phoenix Job
Corps. However, the effectiveness of various types of job placement assistance could be
affected by labor market conditions.

The "support services" column is taken from Table 3.4 and assesses the strength of the
implementation of the fourth component of JOBSTART, support services. Along with the
three Job Corps nonresidential programs, Connelley in Pittsburgh, SER/Corpus Christi, and
Allentown in Buffalo stood out from the other sites in the strength of their efforts to deal
with problems that stood in the way of program attendance and completion.

To sum up this table, CET/San Jose, Chicago Commons, Connelley in Pittsburgh, El
Centro in Dallas, and two of the three Job Corps programs seem to have implemented
JOBSTART's four components more fully than did the other sites. Thus, these sites appear
to have set the stage for program impacts stronger than those in the other sites. However, the
relatively strong treatments, short lengths of stay, and resulting intensity (hours per month) at
CET/San Jose and Chicago Commons could have given these two sites an edge over the others
in a short follow-up period.

2. Recruitment. While all JOBSTART sites had a common target group for the
demonstration (disadvantaged young dropouts reading below the eighth-grade level), there
were variations in site recruiting emphases and in participant characteristics. These site
variations arose because different types of youths applied and because intake practices were
not uniform.

Some characteristics of youths vary in easily measured ways. For example, as reported
in Chapter 2, sites varied greatly in the proportion of the sample that was made up of youths who had recent work experience or who were young mothers. Factors such as these were measured at random assignment and can, to a considerable extent, be adjusted for statistically in comparing program impacts across sites. One type of impact estimate reported later in this chapter does include such statistical adjustments in an effort to control for this one source of variation among sites and to move closer to isolating variation in impacts caused by program structure.6

Other factors are much harder to observe and were, therefore, not measured at entry into the sample; these cannot be included in statistical adjustments. One especially important factor is the youths' goals in participating in the program and their resulting interest in the various JOBSTART components. Although the youths' goals and interests were not observed directly in any quantifiable way, the reputation of the site in the community probably had a strong influence on who applied for the program. Those sites with a history of providing basic skills instruction and not occupational training (BSA in New York City, Allentown in Buffalo, and CREC in Hartford) naturally would seem to have attracted youths who were more interested in GED attainment than in immediate acquisition of marketable occupational skills, while those sites emphasizing occupational skills training (CET/San Jose and Chicago Commons) seem to have attracted those who were more interested in job skills and immediate employment than in education.

Available information does indirectly support this generalization. Table 3.10 showed that JOBSTART hours of education tended to be high and that hours of occupational skills training were lowest in sequential/brokered sites, although the latter clearly resulted partly from the administrative problems those sites encountered in linking education and training. Taken together, these findings are consistent with the idea that recruits in the sequential/brokered sites were less interested in occupational training per se than in education leading to a GED. In other words, these sites may have delivered fewer hours of skills training partly because their recruits demanded fewer hours.

3. Screening. Some sites — such as CET/San Jose, EGOS in Denver, BSA in New

6Although these impacts include linear statistical adjustments for these characteristics, not all relevant characteristics were measured; there might be differential errors of measurement of characteristics; true relationships between impacts and characteristics may be nonlinear; or impacts of sites with very unusual sample characteristics might be difficult to adjust properly with any statistical model. Thus, adjusted site outcomes and impacts must be viewed with a special caution not applicable to estimated subgroup impacts.
York City, and CREC in Hartford—did not screen applicants much before they entered the sample, while others (such as Chicago Commons and the Job Corps sites) carried out extensive screening. When a great deal of screening takes place, it is reasonable to assume that those randomly assigned (including those assigned to control group status) will have high levels of perseverance, motivation to go into a program, motivation to work, and other important characteristics hard to measure directly. Thus, all else being equal, in sites that screened heavily, control group outcomes should have constituted tougher benchmarks for experimentals to surpass than in sites that carried out minimal screening. However, minimal screening may yield groups of experimentals who are harder to keep in the program and harder to place at completion.

4. **Point of random assignment.** On top of the normal steps and resulting screening in recruitment and intake, the research design introduced random assignment, which could occur at slightly different points relative to initial contact with a potential participant and actual start-up of services. As mentioned in Chapter 2, putting random assignment early in the program flow (as at CET/San Jose) tended to include in the sample individuals who might drop out of the later steps of intake, while putting random assignment relatively late (as at Allentown in Buffalo) meant that only those making it to that stage of intake were in the research sample. Late random assignment tended to raise the proportion of the experimental group participating in JOBSTART, because it meant that those randomly assigned were interested enough in the program to persevere through the steps of intake and because it reduced the wait between random assignment and the start of services. It also tended to raise the proportion of controls who were served in alternative programs, because those randomly assigned to the control group were also quite motivated to receive services.8

5. **Service availability.** More programs tend to be available in large cities, such as

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Chapter 3 pointed out the severe recruitment pressures at CET/San Jose during intake for JOBSTART. CET/San Jose may have been able to adopt this "no screening" policy because of the characteristics of disadvantaged youths in its service area. Even with open admissions, a higher percentage of the CET/San Jose youths had recent work experience than did the full sample. This probably occurred because CET/San Jose drew largely Hispanic youths, a group that, as mentioned in Chapter 1, typically has a higher labor force participation rate than do other minority school dropouts.

8The correlation coefficient of sites' percentage of experimentals and controls participating in education and training is +.37. The point of random assignment thus had implications for the proportion of experimentals and controls receiving services, but both groups were affected similarly, so there was no consistent effect on the difference in the proportion of experimentals and controls receiving some type of education and training.
New York, Chicago, and Los Angeles, than in smaller cities such as Corpus Christi or Hartford. Since the impacts reported here are the impacts of JOBSTART compared to the alternative services in which the controls participated (rather than the impacts of JOBSTART services versus no services), variation in the controls' benchmark levels of services will influence impacts. As Table 6.2 shows, the level of control services and the resulting difference between experimentals and controls in service receipt varied greatly by site. For the most part, service receipt differences were smaller in sequential/brokered sites than in other sites. The largest service differentials were found at El Centro in Dallas and SER/Corpus Christi.

6. Wage structure and labor market tightness. In some communities, jobs tend to be plentiful but low-paying; in others, they tend to be scarcer but higher-paying; and in still others, they are both scarce and low-paying. This could be the result of long-term differences in the industrial base of the communities or of how they are affected by business cycles. These labor market conditions could affect both the control group’s level of employment and earnings and the availability of jobs that reward the experimental group’s increased educational attainment and occupational training.

B. Two Alternative Types of Site Impact Estimates

In this chapter, impacts are compared across sites to help explain how differences in program operation affected program impacts. As just discussed, however, sites differed in many ways, making it very difficult to isolate the influence of program features. The most straightforward approach is to calculate separate experimental-control comparisons for each site, in effect creating 13 separate samples (or data files) and calculating impacts for each one. In doing this type of site impact analysis, the only adjustment made is to take account of differences in the observed pre-random assignment characteristics of the experimental and control groups, which may occur when sample sizes are relatively small.

It is possible to move beyond this "split-file" analysis because one type of site variation

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9. This table is taken from part of Table 4.1. Differences in service receipt across sites were statistically significant. Alternative estimates of service receipt, which result from two-way analysis of the effect of site and experimental or control status, are not materially different from those in Table 6.2 (see Appendix Table G.1).

10. As discussed in Chapters 2 and 3, at CET/San Jose some early activities were not counted in the JOBSTART data system; thus, the actual participation rate for experimentals was somewhat higher than the data suggest. Nevertheless, the reported experimental-control difference in service receipt for CET/San Jose is higher than average.
### TABLE 6.2

**EXPERIMENTAL-CONTROL DIFFERENCE IN SERVICE RECEIPT, BY SITE**

<table>
<thead>
<tr>
<th>Site</th>
<th>Sample Size</th>
<th>Ever Received Any Education or Training, Months 1-24</th>
<th>Difference</th>
<th>p&lt;sup&gt;8&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Concurrent</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>61</td>
<td>92.7&lt;sup&gt;%&lt;/sup&gt;</td>
<td>47.9&lt;sup&gt;%&lt;/sup&gt;</td>
<td>44.8***</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>73.1</td>
<td>21.0</td>
<td>52.0***</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>74</td>
<td>93.2</td>
<td>60.9</td>
<td>32.3***</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>184</td>
<td>99.8</td>
<td>45.8</td>
<td>54.0***</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>100</td>
<td>92.2</td>
<td>59.1</td>
<td>33.2***</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>183</td>
<td>94.2</td>
<td>51.6</td>
<td>42.6***</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>130</td>
<td>92.5</td>
<td>36.6</td>
<td>55.9***</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>236</td>
<td>98.8</td>
<td>34.6</td>
<td>64.2***</td>
</tr>
<tr>
<td><strong>Sequential/in-house</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>155</td>
<td>99.3</td>
<td>27.8</td>
<td>71.6***</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>218</td>
<td>87.1</td>
<td>45.5</td>
<td>41.6***</td>
</tr>
<tr>
<td><strong>Sequential/brokered</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>140</td>
<td>97.8</td>
<td>70.6</td>
<td>27.2***</td>
</tr>
<tr>
<td>PSA (NYC)</td>
<td>119</td>
<td>88.3</td>
<td>49.3</td>
<td>39.1***</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>87</td>
<td>93.5</td>
<td>47.4</td>
<td>46.1***</td>
</tr>
<tr>
<td><strong>All sites</strong></td>
<td>1,839</td>
<td>92.7</td>
<td>44.2</td>
<td>48.4***</td>
</tr>
</tbody>
</table>

**Source:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

**Notes:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

*Any education or training* includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for 19 kinds of difference in characteristics before random assignment; "all sites" outcomes are from a linear analysis of covariance procedure for the full sample controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table 8.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
the observed characteristics of the youths in the sample — can be adjusted for statistically by relying on the fact that youths with particular characteristics are rarely concentrated in just one site. This type of adjustment takes account of the program impacts for each type of youth in the full sample (for example, the impacts for blacks, those with prior work experience, or those receiving public assistance) and adjusts individual site impacts to reflect the fact that some sites served many youths who (in the full sample) tended to have lower-than-average impacts, while other sites served more than the usual percentage of youths who had higher-than-average impacts.11

The two approaches answer different questions, and each has strengths and weaknesses. The split-file approach is most valuable as a description of what actually occurred in each site: That is, it presents the best estimates for the impacts of the site’s program as it actually operated for the people recruited and randomly assigned to the experimental group. This is the approach taken, for example, in the recent report on the MFSP Demonstration; the findings for each of the four sites were calculated separately.12 Its chief drawback is that the differences in outcomes for experimental and controls within each site are not fully comparable across sites because the characteristics of the two groups differ from site to site. If, for example, youths who were employed in the year prior to random assignment tended to have lower employment and earnings impacts, sites with a sample made up of youths with higher-than-average prior-year employment will have impacts lower than they would have been had those sites served a more disadvantaged mix of recruits. Furthermore, the individual site files may have relatively small sample sizes, limiting the precision of impact estimates.

The alternative of adjusting for measured site differences in pre-random assignment characteristics is most valuable in trying to isolate the effects of location per se. Sites with populations different from the overall average are not penalized or rewarded relative to other

11 For example, Chapter 5 discussed how impacts were more favorable for women living with their own children than for other women or for men. The adjustment would take account of the fact that the sample in some sites included more than the average proportion of women living with their own children (thereby raising observed site impacts) and some served more than the average proportion of men (thereby lowering observed site impacts). The adjustment was designed to remove the difference in impacts arising from these differences in each site's sample. The statistical adjustment assumes a linear relationship between each characteristic and outcomes and is the same as the method underlying Tables 4.4, 4.7, 5.3, and 5.6, which presented impacts for designated subgroups controlling for differences between the subgroups other than the characteristic used to define them. It used linear adjustments of outcomes, with up to 19 kinds of differences in youths' characteristics entered as covariates. See Ostle, 1975.

12 See Gordon and Burghardt, 1990.
sites, as they are in the split-file approach. To stay with the same example: The estimated impacts for a site with a higher-than-average proportion of youths with prior employment would be larger using this adjustment than with the split-file approach. However, in order to make the adjustment for site differences in the mix of youths' pre-random assignment characteristics, it is necessary to assume that sites could have served — and gotten "average" impacts for — populations they actually did not serve. For example, sites set up to serve a particular type of person (for example, young men) might not be able to serve other young people. Thus, they might not attain the impact observed in the full sample for these other groups. Each way of looking at site impacts answers a particular question and has its own advantages and disadvantages, so both are presented in the following section.

For both approaches, the impacts presented are per experimental. Appendix B, Section III, discusses how impacts per experimental can be converted to impacts per participant if certain assumptions are valid. This conversion was not done here — despite the fact that participation rates for experimentals do vary across the sites — because participation rates for controls also vary, and are somewhat correlated with the experimentals' rates: For example, sites with a high participation rate for experimentals tend to have a high rate for controls. Consequently, comparisons of the difference in participation between experimentals and controls across sites show a different pattern of variation than comparisons of the participation rate for either group individually: For instance, sites with a lower-than-average participation rate for experimentals do not necessarily have a lower-than-average difference in participation between experimentals and controls. Since the impact analysis presented here compares experimental and control outcomes and shows the impact of the increment of services received by experimentals above the services received by controls, adjusting site-specific impacts to account

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13 The adjusted impacts approach uses the entire sample, lessening the problems of sample size in the analysis. However, it introduces a further complication: The adjustments implicitly assume that youths' characteristics affect outcomes in the same way in each site. More technically, the assumption is that the coefficients estimated for the regression used in making adjustment for differences in characteristics (both between experimentals and controls and across sites) are the same across all sites. The split-file approach estimates separate regressions for each site to adjust for differences in sample characteristics between experimentals and controls. Therefore, the shift from split-file impacts to those adjusted for site differences in pre-random assignment characteristics involves two types of changes: changing to the standard full sample regression coefficients and adjusting for differences across sites in sample characteristics. Consequently, it is often not possible to give a simple, intuitive explanation of why the shift from split-file to adjusted impacts caused the observed change in impacts.
for cross-site variation in experimentals' participation would confuse rather than clarify comparisons across the sites.\textsuperscript{14}

II. Full Sample Impacts, by Site

The outline of the site story is the same under both types of site impact approaches, although the adjustment does make a noticeable difference in the impacts of a few sites that served a group of young people that differed markedly from the sample as a whole and/or had smaller-than-average sample sizes. Table 6.3 presents the split-file impacts, that is, those with no adjustments for differences across sites in the youths served. Table 6.4 presents impacts with that type of adjustment. Both tables include three impacts: The left section presents experimental-control differences in educational attainment; the middle section, differences in employment rate during the second year of follow-up; and the right section, differences in earnings during the second year of follow-up.

As anticipated, few of the individual site impacts were statistically significant, so lessons would come from patterns of impacts across sites. In both tables, in each site grouping, there are one or more sites with a positive impact and one or more with a negative or very small impact for each of the three outcomes: high school completion and GED receipt, and employment and earnings in the second year. As the tables show, there was variation in impacts \textit{within} each group of sites as well as \textit{between} groups of sites. For educational attainment, the differences in site impacts were statistically significant.\textsuperscript{15} However, the differences in the individual site impacts on employment and education were not statistically significant, suggesting caution in drawing conclusions from any observed differences in impacts.

High school diploma and GED receipt impacts were mainly positive (and, in four sites, statistically significant) but fell negative at Chicago Commons and the East Los Angeles Skills

\ \textsuperscript{14}It is possible to adjust impacts to take account of participation in services by both experimentals and controls, but this involves making untestable assumptions about the impacts of the services received by controls. See Auspos, Cave, and Long, 1988, pp. 198-199.

\textsuperscript{15}Table 6.4 includes a test of the statistical significance of the variation in individual site impacts. The "p-value" of the "F-statistic" at the foot of each column in Table 6.4 is the probability that site variation in that impact could be due to chance alone. For educational attainment, the observed site variation in impacts was so great that the probability it could have risen by chance is virtually zero. However, for both employment and earnings, individual site impact estimates are less diverse, as is their precision (represented by a general lack of statistical significance on site impacts), so the probability that the observed variation could have arisen by chance is much higher: 72 percent for employment and 13 percent for earnings.
<table>
<thead>
<tr>
<th>Site</th>
<th>Sample Size</th>
<th>Received GED or High School Diploma by End of Month 24</th>
<th>Employed, Months 13-24</th>
<th>Total Earnings, Months 13-24</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Experimentals</td>
<td>Controls</td>
<td>Difference</td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>61</td>
<td>23.0%</td>
<td>8.6%</td>
<td>14.3</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>34.5</td>
<td>27.4</td>
<td>7.1</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>74</td>
<td>5.6</td>
<td>18.7</td>
<td>-13.1</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>184</td>
<td>57.1</td>
<td>15.9</td>
<td>41.2***</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>100</td>
<td>2.1</td>
<td>8.0</td>
<td>-5.9</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>183</td>
<td>22.2</td>
<td>14.8</td>
<td>7.4</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>130</td>
<td>25.2</td>
<td>11.3</td>
<td>13.9*</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>236</td>
<td>53.2</td>
<td>15.1</td>
<td>38.1***</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>155</td>
<td>57.5</td>
<td>14.1</td>
<td>43.4***</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>218</td>
<td>19.8</td>
<td>14.8</td>
<td>5.1</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>140</td>
<td>33.7</td>
<td>21.3</td>
<td>12.4</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>119</td>
<td>25.7</td>
<td>32.0</td>
<td>-6.3</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>87</td>
<td>21.8</td>
<td>12.4</td>
<td>9.4</td>
</tr>
<tr>
<td>All sites</td>
<td>1,839</td>
<td>33.2</td>
<td>16.4</td>
<td>16.7***</td>
</tr>
</tbody>
</table>

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for 19 kinds of difference in characteristics before random assignment; "all sites" outcomes are from a linear analysis of covariance procedure for the full sample controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
### TABLE 6.4

**SELECTED IMPACTS OF JOBSTART, BY SITE**

*(ADJUSTED FOR SITE DIFFERENCES IN SAMPLE CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT)*

<table>
<thead>
<tr>
<th>Site or Statistic</th>
<th>Sample Size</th>
<th>Received GED or High School Diploma by End of Month 24</th>
<th>Ever Employed, Months 13-24</th>
<th>Total Earnings, Months 13-24</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Experimentals Controls Impact</td>
<td>Experimentals Controls Impact</td>
<td>Experimentals Controls Impact</td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>61</td>
<td>21.3%</td>
<td>75.1%</td>
<td>$3,404.95</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>33.6%</td>
<td>81.4%</td>
<td>6,946.06</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>74</td>
<td>6.0%</td>
<td>82.7%</td>
<td>4,831.43</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>184</td>
<td>55.6%</td>
<td>74.3%</td>
<td>1,969.75</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>100</td>
<td>5.0%</td>
<td>70.4%</td>
<td>4,509.77</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>183</td>
<td>22.2%</td>
<td>65.6%</td>
<td>2,998.48</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>130</td>
<td>26.0%</td>
<td>65.3%</td>
<td>3,005.02</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>236</td>
<td>55.4%</td>
<td>67.5%</td>
<td>2,581.26</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>155</td>
<td>57.9%</td>
<td>77.2%</td>
<td>4,020.23</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>218</td>
<td>19.4%</td>
<td>69.2%</td>
<td>4,389.13</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>140</td>
<td>30.3%</td>
<td>72.4%</td>
<td>3,781.28</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>119</td>
<td>29.5%</td>
<td>72.1%</td>
<td>5,362.25</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>87</td>
<td>22.7%</td>
<td>73.8%</td>
<td>5,024.72</td>
</tr>
<tr>
<td>P-value of F-statistic</td>
<td></td>
<td>0.000***</td>
<td>0.724</td>
<td>0.767</td>
</tr>
<tr>
<td>All sites</td>
<td>1,839</td>
<td>33.2%</td>
<td>72.0%</td>
<td>3,894.08</td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). *All sites* outcomes are from a linear analysis of covariance procedure for the full sample controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table 8.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
Center (under both types of impact estimates) and at BSA in New York City (under the split-file approach). Sequential/brokered sites had the weakest educational attainment impacts, as one might expect from the generally higher control group receipt of a high school diploma or GED in these sites. This higher rate of control group receipt of a GED probably reflects the stronger interest in GED receipt among applicants in these sites, as discussed in an earlier section of this chapter, and the availability of alternative services in the two large cities of New York and Buffalo. El Centro in Dallas, SER/Corpus Christi, and Connelley in Pittsburgh's impacts were much larger than those for the other sites. There is a likely explanation for this that is related to program characteristics: These three sites placed great emphasis on GED attainment, in large part because of contractual provisions rewarding them for accomplishing this outcome.

None of the individual site impacts on second-year employment were statistically significant in the split-file table and only one was in Table 6.4, but there did appear to be variation within each site category (although it was not statistically significant). In both tables, each of the three categories showed both relatively positive impacts and one or more sites with negative or close to zero impacts. In both tables, the employment rate impacts were still negative in six of the sites, although in three of these sites, the difference between controls and experimentals was very small (less than 2 percentage points).

Impacts on second-year earnings showed more variation across sites than did the employment impacts; they also showed considerable variation within each of the three groups of sites. Because of the diverse earnings of youths within the individual sites and the small samples (owing to the fine level of disaggregation), only two of the 13 individual site impacts were statistically significant in Table 6.3 and only one was in Table 6.4. It is notable that there were positive and negative earnings impact estimates in each of the three groups of sites in both tables.

Earnings impacts in Tables 6.3 and 6.4 differ substantially for a few sites in the demonstration. The adjustments used in Table 6.4 make a larger difference for Chicago Commons than for all but one other site (the Phoenix Job Corps), complicating the comparison.
of impacts for Chicago Commons with other sites. According to Table 6.3 (the split-file table), which does not equalize measured baseline characteristics across sites, the largest earnings impact estimate was $563.12 for El Centro in Dallas. Adjusting for site differences in sample characteristics, the El Centro earnings impact drops slightly to $481.65. Chicago Commons, in contrast, increases from a $77.56 impact estimate in Table 6.3 to $759.63, the largest earnings impact estimate in Table 6.4. This large change is attributable in part to the site's screening practices and training offerings, which led to unusual sample characteristics (especially a larger-than-average proportion of men). Since Table 6.4 comes closer to making individual site impacts comparable, Chicago Commons should probably be viewed as producing positive, and relatively large, impacts.

It is also noteworthy that three of the four sites with positive estimates in Table 6.4 were among those described above as having implemented the components of JOBSTART most successfully. Chicago Commons and CET/San Jose had the largest point estimates for second-year earnings impacts in Table 6.4, but it is not apparent whether this was due to their relatively concentrated doses of JOBSTART, the strength of their job placement efforts, their integration of education and training, their longstanding reputations in their communities, or other factors unique to these sites, such as their close ties to employers. On a sixth important program feature, they differed: As noted above, CET/San Jose served applicants without screening, while Chicago Commons imposed strict entry requirements.

The two other sites with positive earnings impacts in both tables (Allentown in Buffalo and El Centro in Dallas) operated different types of programs from CET/San Jose and Chicago Commons. The impacts of the Phoenix Job Corps become less negative with the shift from split-file to adjusted impacts, but neither this change nor the one for Chicago Commons affects the basic conclusions of the chapter. Another important site, CET/San Jose, had the second largest earnings impact in both tables, changing from $508.42 in Table 6.3 to $739.27 in Table 6.4.

This change may also have been related to the small sample size, since in a small sample, outliers can have a strong influence on coefficient estimates. These two sites also differed somewhat in their experimental-control difference in service receipt, with CET/San Jose, at 52 percentage points, having a larger gap than Chicago, at 32 percentage points (see Table 6.2).
Commons. Allentown had a heavy education focus (with only one-third of the sample getting any training), no integration of education and training, weak job placement efforts, and the longest average length of stay in the program (nearly four months above the average). It also had the smallest experimental-control service difference of any site. El Centro’s sequential/in-house program rested somewhere in between these two extremes in both program emphasis and length of stay. Its service receipt difference may have played a crucial role in the positive impacts, being the largest of any site (72 percentage points).

Two sites assessed as implementing the JOBSTART program model well did not have positive earnings impacts in the second year of follow-up. Connelley in Pittsburgh did have strong educational attainment and employment rate impacts, but these had not yet translated into positive earnings impacts during the follow-up period. Connelley’s long length of stay in the program (at 10 months, the second longest) may have been an important contributing factor. As to the second site, the Phoenix Job Corps, there is no clear reason for the lack of employment rate and earnings impacts.

III. Subgroup Impacts, by Site

This section continues the analysis by examining impacts on employment and earnings for key subgroups by site. At this fine level of disaggregation, the sample sizes in individual sites are so small as to make only the broadest conclusions possible. In view of the tentative nature of this analysis, tables are presented in Appendix C and the text presents the outlines of the findings. This analysis is based on site impacts adjusted for observed differences in pre-random assignment characteristics across the sites; that is, it uses the same approach as Table 6.4.

A. Second-Year Impacts for Men, by Site

The sample sizes for men at each site were usually less than half of the already small site sample sizes reported in Table 6.4. Since the full sample in each site yielded site differences in impacts that were not statistically significant, it is not surprising that most site-level impacts for men were also not statistically significant.

19 A third site, SER/Corpus Christi, had a positive point estimate for the earnings impact according to the split-file method (Table 6.3) but not according to the adjusted method (Table 6.4) of calculating impacts.
With this caution in mind, the site story for men seems much the same as the full sample story. As in the full sample, employment and earnings impact differences across sites were not statistically significant, and not much should be made of them. However, the sites with substantial, positive point estimates for men were largely the same as the sites with positive estimates for the full sample. For example, CET/San Jose, Chicago Commons, and Allentown in Buffalo had point estimates of several hundred dollars for men, as was true of the full sample. However, there were two major exceptions to this pattern. First, El Centro in Dallas’s impacts, which were positive for the full sample, were negative for men. Second, the largest estimate for impacts on earnings was for the 31 men at CREC in Hartford; the earnings impact in this site was driven by a 30 percentage point impact on employment.

In view of the fairly negative results for the full sample of men reported in Chapter 5, it is encouraging that there were sites with positive impacts for men on second-year earnings.

B. Second-Year Impacts for Women Living with Their Own Child(ren), by Site

Even more than for men or other women, site impacts for this group were based on very small samples in some sites, virtually assuring that site impacts and differences in impacts were not statistically significant; for example, there were only 10 mothers at CET/San Jose. Again bearing in mind that these impact estimates are shrouded in much uncertainty, the second-year labor market story for mothers seems a bit different from the stories for the full sample and for men.

The largest of the earnings impact estimates, for El Centro in Dallas mothers, apparently is what drove the full sample impact for El Centro. This impact was large enough to be statistically significant. The next largest earnings impact, at the East Los Angeles Skills Center, was driven by an employment rate impact of 49 percentage points. Unlike the earnings impact for men at the Los Angeles Job Corps, the earnings impact at that site for women living with their own child(ren) was positive. The earnings impact of more than $500 at CREC in Hartford for women living with their own children, combined with a large positive estimate for men, implies that the full sample impact for CREC must have been driven by a large negative estimate for other women. The positive but small earnings impacts among the 10 CET/San Jose mothers and the 63 Connelley in Pittsburgh mothers were driven by employment rate impacts of 50 percentage points and 22 percentage points, respectively. Earnings impacts in
the other sites were negative, down to a statistically significant estimate of -$3,084 at BSA in New York City.

The findings for the three Job Corps sites for this subgroup hold special interest. Positive earnings impacts for women living with their own children, but not for other groups, at the Atlanta Job Corps and at the Los Angeles Job Corps contrast with earlier evaluation results for the residential Job Corps. The residential Job Corps was found to be quite effective for men and for women without child care responsibilities, but not for mothers. These preliminary findings seem to suggest that a nonresidential Job Corps program may be more appropriate for mothers.

C. Second-Year Impacts for Other Women, by Site

The largest earnings point estimate was for the 14 "other women" (women who were not living with their own children) at Chicago Commons. Other positive impacts of more than $1,000 were achieved at the East Los Angeles Skills Center, the Los Angeles Job Corps, Connelley in Pittsburgh, and Allentown in Buffalo. El Centro in Dallas and CET/San Jose also had positive earnings impacts for these women. The other five sites had negative earnings impacts for these women during the second year. The biggest earnings loss for other women was at CREC in Hartford; thus, as noted above, it was these other women rather than mothers or men who were driving the negative overall earnings impact at CREC.

IV. Summary and Future Impacts To Watch

Despite strong policy interest in the influence of program structure, no simple story emerges. With respect to the full sample in each site, JOBSTART was sometimes effective and sometimes ineffective in yielding second-year labor market gains in brokered programs and in-house programs; it also showed varying success in both concurrent programs and sequential programs. This suggests that the JOBSTART program model can be implemented successfully in a variety of settings.

Adjusting for site differences in sample characteristics, Chicago Commons and CET/San Jose had the largest positive estimates for second-year earnings impacts. Each integrated education into the training sequence more than did other sites, but each also had several other

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20See Mallar et al., 1982.
features that distinguished it from most other sites, including strong job placement and relatively intense concentration of many instruction hours into a few months' length of stay. However, the two sites differed in the extent of initial screening of applicants, with CET/San Jose doing very little and Chicago Commons being among the sites with the most screening. Two other sites operating sequential/in-house and sequential/brokered programs (each with a much stronger education emphasis, and one providing very little training) also produced positive earnings impacts in the second year of follow-up.

The picture for subgroups is similarly mixed. Sites that had positive earnings impacts for the full sample generally had positive impacts for men and women considered separately. In contrast to negative findings from an earlier study of residential Job Corps mothers, JOBSTART mothers in two of three nonresidential Job Corps sites had earnings gains during the second year. These preliminary findings seem to suggest that a nonresidential Job Corps program may be more appropriate for mothers than a residential program.

Some JOBSTART sites (such as El Centro in Dallas) achieved both GED attainment gains and second-year earnings gains; others (the East Los Angeles Skills Center and at BSA in New York City) succeeded at neither. But the findings presented in this chapter provide evidence that sometimes there is a trade-off for program designer between emphasizing GED attainment and emphasizing accelerated occupational training for a specific job. In the two sites with the largest short-run adjusted earnings impacts—Chicago Commons and CET/San Jose—GED impacts were negative or fairly small. In the short run, emphasizing a specific skill may prove more effective in producing earnings impacts. However, the long-run picture could be quite different.

Education can create new opportunities, and these may arise over a long period of time. In comparison, training for a specific job opens a few doors quickly, but in the long run, because of economic change and uncertainty, they may not be the right doors. The oldest members of the JOBSTART sample were only 23 years old at the end of the follow-up available for this report; most sample members were considerably younger. Their transitions into full-time work and independent living arrangements will not be complete for several years. If the GED is important as a credential for better jobs or job training far into the future, labor market impacts at the two-year point may be quite misleading.

In the final JOBSTART report, using four years of follow-up, it will be especially important to look at impacts at the end of follow-up.
APPENDICES
APPENDIX A

DATA SOURCES FOR THE EVALUATION

Many data sources were used in this evaluation of the JOBSTART Demonstration. Baseline demographic data were collected at the time of random assignment. Management information system (MIS) data from the sites were used to measure participation hours. Twelve-month and 24-month follow-up surveys of applicants were conducted to measure impacts on experimentals (including those who did not participate) compared to controls; the impacts concerned amounts of education and training received, employment and earnings, and other outcomes. The 12-month survey also dealt with the experiences of participants in the JOBSTART program. Much qualitative information, including interviews with program staff as well as focus groups and in-depth interviews with participants, was used in conjunction with the quantitative information. Each data source is described below.

I. JOBSTART Enrollment Forms

The JOBSTART Enrollment Form, designed by MDRC and filled out by program staff at the time of random assignment, was the major source of information about the demographic and socioeconomic characteristics of sample members. It included data on age, sex, ethnicity, family composition, educational attainment, and time since dropping out of school, as well as basic information on welfare and employment histories. The enrollment form was completed for all but one sample member.¹

II. JOBSTART Management Information System Forms

Sites used a number of MDRC-designed forms to report on the progress of participants in JOBSTART. The most important of these were:

¹This sample member was excluded from the impact analysis, since all demographic variables from the enrollment form are missing. For many of the sample members, a few specific pieces of demographic information are missing. In the impact analysis, the predicted values based on similar sample members were substituted for these missing observations.
A. Monthly Participation Report

The Monthly Participation Report provided the number of hours that participants spent in basic education, occupational skills training, or other kinds of JOBSTART activities each month. It also provided information on the type of occupational skills training in which participants in training enrolled. Sites reported actual hours attended, not the number of hours scheduled.

Participation data used in this report were collected from August 1985 — the beginning of random assignment — through January 1989. The month of random assignment was included as a month of follow-up for participation, although the participant may have been randomly assigned late in the month. Those assigned in the last month of random assignment – November 1987 – had 15 months of follow-up participation data. The vast majority of the sample had at least 24 months of follow-up.

Collecting strictly comparable data across sites was not always possible, for two reasons: First, the services provided in each site varied; second, there was some inconsistency in the way sites reported hours for activities other than basic education or occupational training classes. For example, a number of sites supplemented education and/or training classes with formal classroom instruction in a variety of topics generally termed "life skills." Some sites reported these as education hours; others counted them as training hours. In order to have similar definitions of the basic components — education and training — MDRC modified the reported hours at sites, so that time spent in such activities as life skills classes was counted under "other activities." The education hours reported by CET/San Jose also were adjusted to reflect only hours spent in the site's GED class.

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2The sites were El Centro in Dallas, the Los Angeles Job Corps, the Atlanta Job Corps, and Allentown in Buffalo. At El Centro, one-half of all education hours prior to December 1986 were spent in life skills. After 1986, one-fourth of the reported education hours were spent in life skills. The hours were counted as hours in "other activities" by MDRC. At the Los Angeles Job Corps, participants spent one-half of their reported education hours in activities such as art, gym, and "world of work" for the first three months after enrollment. MDRC moved one-half of the education hours to hours in "other activities" for those months. At the Atlanta Job Corps, 10 hours each week were spent in activities such as life skills, driver education, and health. MDRC moved 28.6 percent of the reported education hours to hours in "other activities." Allentown in Buffalo included such hours in its reported occupational training hours. MDRC moved all reported occupational training hours that did not have an associated type of training to hours in "other activities."

3CET/San Jose reported 30 percent of each participant's occupational training hours as education, which included time spent on training-related basic skills in occupational training courses as well as hours in the site's GED class. For consistency with other sites, the education and training hours at CET were (continued...)
Other differences remained, however. A number of sites offered limited amounts of work experience as part of the JOBSTART program. Some sites reported these hours as training hours; others reported them under "other activities." No adjustments were made in these hours. Finally, the Phoenix Job Corps did not report hours spent by participants in life skills or avocational activities, although the other two Job Corps sites did.

Appendix Table A.1 shows the common elements and variations in component activities across sites. In general, participation hours reported as being in the education component consisted of time spent in classes devoted to basic education or GED preparation; they did not include work on training-related basic skills done in occupational training courses. In all sites, participation hours that were counted in the training component included all activities offered in occupational training curricula, including units on training-related educational skills (such as Business English or Business Math) and employability development (instruction in work behaviors and job search). In the following sites, the hours counted as training also included time spent in work experience or on-the-job training: Connelley in Pittsburgh, El Centro in Dallas, the Phoenix Job Corps, and the Los Angeles Job Corps. Hours spent in "other activities" varied considerably across sites and included instruction in life skills, work experience, and orientation and avocational activities.

In order to assess the quality and completeness of the participation data, MDRC staff reviewed the teachers' class attendance records and other sources of data for a randomly selected sample of participants. For the most part, there was agreement between hours found in teachers' records and the Monthly Participation Reports. If more than 20 percent of the cases in a quality control sample had discrepancies greater than 10 percent between site-reported hours and hours obtained in the check, MDRC scheduled either a re-collection of the data or retraining of site staff, depending on the seriousness of the discrepancies.

3(...continued)
recalculated by MDRC, and only hours spent in the separate GED class were included as education hours in this report.

4At EGOS in Denver, hours spent by participants in "work study" were not reported.

5Because it was necessary to obtain records from a number of service providers, many of which did not maintain complete records for long periods, occupational training hours in brokered sites were the most difficult to confirm and probably have the greatest variation between actual and reported hours. The difficulty MDRC staff had in obtaining and verifying data from training providers reflects the difficulty sites had in monitoring hours for participants once they were no longer in the site. Problems were found even in the two sites with the best data from service providers: One site apparently over-reported hours, while one site apparently under-reported hours. Because the number of participants who entered training in (continued...)
<table>
<thead>
<tr>
<th>Site</th>
<th>Education</th>
<th>Training</th>
<th>Other Activities</th>
</tr>
</thead>
<tbody>
<tr>
<td>All sites</td>
<td>Classes in basic education or GED preparation</td>
<td>Classroom occupational skills training, including classes in training-related basic skills and employability development</td>
<td>Varies</td>
</tr>
<tr>
<td>Exceptions, by site</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>---&lt;sup&gt;a&lt;/sup&gt;</td>
<td>Life skills&lt;sup&gt;a&lt;/sup&gt;</td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>---&lt;sup&gt;a&lt;/sup&gt;</td>
<td>10-day orientation, work experience and on-the-job training, life skills and avocational activities&lt;sup&gt;a&lt;/sup&gt;</td>
<td></td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>May include a few hours per week in computer-assisted life skills curriculum</td>
<td></td>
<td>Life skills</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>---&lt;sup&gt;a&lt;/sup&gt;</td>
<td>---&lt;sup&gt;a&lt;/sup&gt;</td>
<td>None</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td></td>
<td>Work experience mentorships</td>
<td>None&lt;sup&gt;b&lt;/sup&gt;</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>Includes some hours in employability development activities</td>
<td></td>
<td>Work experience internships</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td></td>
<td></td>
<td>None</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>---&lt;sup&gt;c&lt;/sup&gt;</td>
<td></td>
<td>None</td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>---&lt;sup&gt;a&lt;/sup&gt;</td>
<td>Work experience internships</td>
<td>Life skills&lt;sup&gt;a&lt;/sup&gt;</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>---&lt;sup&gt;a&lt;/sup&gt;</td>
<td>Work experience and on-the-job training</td>
<td>5-day orientation, life skills and avocational activities&lt;sup&gt;a&lt;/sup&gt;</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td></td>
<td>Work experience and on-the-job training</td>
<td>8-day orientation&lt;sup&gt;d&lt;/sup&gt;</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td></td>
<td></td>
<td>None</td>
</tr>
</tbody>
</table>

**SOURCE:** Adapted from Auspos et al., 1989.

**NOTES:**
- <sup>a</sup>Reported hours were adjusted by MDRC.
- <sup>b</sup>Site did not report participation in a one-hour after-school component consisting of counseling and other support services in school year 1986-87.
- <sup>c</sup>Site did not report participation hours in work-study positions.
- <sup>d</sup>Site did not report participation hours in life skills and avocational activities.
B. Other Management Information System Data

As part of the monthly monitoring system, sites also reported on the end-of-month status of each participant, the participants who had been terminated and the reason for termination, and job placement and GED receipt among participants. The follow-up surveys proved to be a more complete source of data for employment and GED receipt, since they included activity by experimentals that might not have been reported to site operators as well as the experiences of the control group. Consequently, the surveys are the only source of these data used in this report.

III. Test of Adult Basic Education

The Test of Adult Basic Education (TABE), a modification of the California Achievement Test, was used to measure reading levels of experimentals. Prior research has shown the test to be a reliable and valid measure of reading ability. The test was used at two points in time: shortly after random assignment (as a baseline measure) and after participants had spent some time in the program (usually after about 100 hours of education), as a measure of reading level gains.

About 20 percent of the total experimental sample did not take a baseline TABE. The percentage tested varied by site from a high of 100 percent to a low of 42 percent. The Job Corps sites and CET/San Jose had the lowest percentage of experimentals with baseline TABEs.

---

5(...continued)
sequential/brokered sites was small, the misreporting of training hours did not greatly affect the average hours of training presented in the report.

6In five sites, the TABE was also used as a test of reading-level eligibility and consequently was administered to controls as well as to experimentals. A number of other reading tests were administered in the other sites. Data from these sites were not included in the analysis because the data were not comparable across sites. Scores on the eligibility test were used as the baseline measure for experimentals in sites where the TABE was used.

7The actual number of hours of education between random assignment and the first follow-up test varied considerably because of differences in measuring hours of education and delays in administering the tests. Also, in the first few months of the demonstration, sites were asked to test every three months, which resulted in considerable variation in the number of hours after which participants were tested.
IV. Follow-Up Surveys

Eighty percent (1,839) of the 2,311 sample members randomly assigned to the experimental or control group between August 1985 and November 1987 were interviewed. (Table A.2 presents survey response rates by site.) These 1,839 youths constituted the sample for this report, and each of them provided follow-up information for 24 months after the date of his or her random assignment. Most responded to both the 12- and 24-month follow-up surveys (1,604 or 87 percent of responders), while the remainder responded to a special combination survey covering the entire 24 months, which was fielded for youths who did not respond to the 12-month survey but were located at 24 months. The surveys were conducted either in person or, for the approximately one-fifth of the sample who had moved out of the area, by telephone, one and two years after random assignment. The interviews lasted about 45 minutes and provided information about the applicant’s experience during the period of follow-up covered in that survey wave. Respondents were asked about their employment history, family status, welfare receipt, and receipt of education or training outside of JOBSTART. During the 12-month survey, experimentals who did not participate in JOBSTART were asked why; participants were asked what they like and disliked about the program and their reasons for leaving. (Appendix B discusses issues of sample bias and data quality for the survey.)

Sample members who could be located were generally willing to be interviewed. Some could not be located while others simply could not be contacted.

Some completed surveys lacked some information that was important in calculating impacts. Because the presence of missing data might have been correlated with an observed or unobserved prior attribute, dropping cases with missing data from the analysis might have biased the impact estimates or produced month-to-month inconsistencies. Imputing values is possible using a procedure that does not bias results. A separate regression was run for each variable with missing values, yielding predicted values for the missing data. These predicted values were used as estimates of the missing values. Continuous outcomes may contain outliers — extreme values that overly influence estimates. In the analysis, these were treated as missing, and the usual procedures for missing values were applied.

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8MDRC contracted with Abt Associates, a Boston-based survey firm, to implement, manage, and monitor the survey. Completed surveys were data-entered and checked for completeness by Abt. Members of the Abt staff also assisted in the design of the survey instrument.
TABLE A.2
RESPONSE RATES FOR 24-MONTH SURVEY, BY SITE

<table>
<thead>
<tr>
<th>Site</th>
<th>Sample Size</th>
<th>Responded</th>
<th>Did Not Respond</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Concurrent</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>80</td>
<td>76.3%</td>
<td>23.8%</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>200</td>
<td>76.0</td>
<td>24.0</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>93</td>
<td>79.6</td>
<td>20.4</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>219</td>
<td>84.0</td>
<td>16.0</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>126</td>
<td>79.4</td>
<td>20.6</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>237</td>
<td>77.2</td>
<td>22.8</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>153</td>
<td>85.0</td>
<td>15.0</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>300</td>
<td>78.7</td>
<td>21.3</td>
</tr>
<tr>
<td><strong>Sequential/in-house</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>200</td>
<td>77.5</td>
<td>22.5</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>296</td>
<td>73.6</td>
<td>26.4</td>
</tr>
<tr>
<td><strong>Sequential/brokered</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>147</td>
<td>95.2</td>
<td>4.8</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>151</td>
<td>78.8</td>
<td>21.2</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>109</td>
<td>79.8</td>
<td>20.2</td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all 2,311 sample members. Rows may not total 100.0 percent because of rounding.
V. Qualitative Data

Qualitative descriptions of the program and participants’ experiences in it were obtained from a variety of sources and were used to complement the analysis of the quantitative data.

MDRC research staff visited sites and conducted structured interviews with program administrators, counselor/coordinators, and teaching staff to determine recruitment practices, the content of services in the education and training components, job placement and other activities, the range of support services and retention strategies, and staffing patterns and staff experience with JOBSTART. Staff also observed education and training classes in each site and visited some of the organizations that provided occupational training to JOBSTART participants in the sequential/brokered sites. Sites were typically visited by research staff once during the early phase of the demonstration and twice in the second year of program operations. This information was supplemented by ongoing reports on program operations and classroom observations provided by MDRC operations staff, who visited each site at regular intervals: once every month in year 1 and once every two months in year 2 of the operational period. (Interviews and observations concerning the education component were developed in conjunction with an education expert, who worked with MDRC as a consultant.)

Information about participants’ reactions to JOBSTART was obtained from focus group discussions with 46 JOBSTART participants in four sites between May 1987 and February 1988. Female participants were interviewed at Connelley in Pittsburgh and at BSA in New York City; men were interviewed at El Centro in Dallas and at the Los Angeles Job Corps. Each session was attended by between 9 and 14 participants and lasted between two and two and a half hours. At Connelley and El Centro, the groups were made up of participants in attendance on the session day; at the Los Angeles Job Corps, staff selected students who were doing well in the program; the BSA group included both current participants in education and women who had already moved on to occupational skills training. Because they included many participants who stayed longer than the average and/or were doing well in the program, the groups were not representative of all JOBSTART participants. Nevertheless, used in conjunction with the survey responses, the focus group discussions provided valuable insights into participants’ expectations about the program, what helped and hindered their participation, their opinions of the education and training components, and their recommendations for
improving the program. MDRC hired consultants to develop the discussion topics, moderate the groups, and analyze the responses.

A series of in-depth interviews was conducted by another consultant with 15 JOBSTART participants in four other sites (CREC in Hartford, EGOS in Denver, Allentown in Buffalo, and the Atlanta Job Corps) between November 1986 and September 1987. These profiles provided additional, although impressionistic, information about the lives of some JOBSTART participants prior to and during the demonstration. The report also drew on the observations of JOBSTART staff and selected participants who attended a conference on Youth Employment Initiatives, sponsored by MDRC, in October 1987.9

9See Manpower Demonstration Research Corporation, 1988, for a summary of the conference discussions.
As outlined in Chapter 2, several methodological issues had to be addressed to answer the key evaluation questions.

I. Selection Bias

Did random assignment succeed in creating a group of JOBSTART controls with the same pre-program characteristics as JOBSTART experimentals? If sample members become experimentals or controls completely at random, there are no systematic measured or unmeasured differences between the two groups before program treatment. Under those circumstances, average outcomes among controls measure what average outcomes would have been among experimentals had the treatment not been available to them, and the difference in average outcomes between experimentals and controls measures the program's effect. If there are systematic pre-existing differences between experimentals and controls, then measured differences in post-treatment outcomes confound true program effects with biases due to the selection of more people from some groups to be experimentals and more people from other groups to be controls.

Table B.1 presents, one at a time, average characteristics for experimentals, controls, and both groups together. There were only slight differences between groups in a few individual characteristics, and no overall pattern of systematic differences between groups.

An alternative, more rigorous way to deal with this issue is to use linear regression analysis. To implement statistical tests for systematic experimental-control differences in those characteristics used in impact regressions, Table B.2 presents linear regression results measuring the extent of selection bias for the 2,311 members of the JOBSTART sample who filled out enrollment forms. The first column of Table B.2 shows the same slight differences in individual characteristics and the same absence of systematic differences as did Table B.1. The final entry in the column, the p-value of the F-statistic, is very close to one, providing strong

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1One sample member who did not complete an enrollment form was excluded from the impact analysis.
<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Experimental</th>
<th>Controls</th>
<th>Both Groups</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Gender</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td>968</td>
<td>53.8%</td>
<td>51.3%</td>
<td>52.6%</td>
<td>0.284</td>
</tr>
<tr>
<td>Men</td>
<td>871</td>
<td>46.2%</td>
<td>48.7%</td>
<td>47.4%</td>
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<tr>
<td><strong>Ethnicity</strong></td>
<td></td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>155</td>
<td>7.6%</td>
<td>9.3%</td>
<td>8.4%</td>
<td>0.340</td>
</tr>
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<td>Black, non-Hispanic</td>
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<td>46.3%</td>
<td>45.1%</td>
<td>45.7%</td>
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<tr>
<td>Hispanic</td>
<td>783</td>
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<tr>
<td>Other</td>
<td>61</td>
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<td>3.8%</td>
<td>3.3%</td>
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<tr>
<td><strong>Ethnicity, by gender</strong></td>
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</tr>
<tr>
<td>Women</td>
<td></td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>84</td>
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<td>25.2%</td>
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<td>24.2%</td>
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<td>Hispanic</td>
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<tr>
<td>White, non-Hispanic</td>
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<td>Black, non-Hispanic</td>
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<td>Other</td>
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<td><strong>Parental status</strong></td>
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<td>Women living with own child(ren)</td>
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<td><strong>Employed within past year</strong></td>
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<td>47.4%</td>
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<td><strong>Prior employment, by gender</strong></td>
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<td>Men employed within past year</td>
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<td>548</td>
<td>28.7%</td>
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<tr>
<td><strong>Sample size</strong></td>
<td>1,839</td>
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<td>890</td>
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(continued)
## TABLE B.1 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Both Groups</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Left school in grade 11 or 12</strong></td>
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<td></td>
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</tr>
<tr>
<td>No</td>
<td>1,078</td>
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<td>59.6%</td>
<td>58.6%</td>
<td>0.432</td>
</tr>
<tr>
<td>Yes</td>
<td>761</td>
<td>42.3%</td>
<td>40.4%</td>
<td>41.4%</td>
<td></td>
</tr>
<tr>
<td><strong>Received occupational training within past year</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,529</td>
<td>84.0%</td>
<td>82.2%</td>
<td>83.1%</td>
<td>0.321</td>
</tr>
<tr>
<td>Yes</td>
<td>310</td>
<td>16.0%</td>
<td>17.8%</td>
<td>16.9%</td>
<td></td>
</tr>
<tr>
<td><strong>Age</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-19</td>
<td>1,359</td>
<td>74.1%</td>
<td>73.7%</td>
<td>73.9%</td>
<td>0.857</td>
</tr>
<tr>
<td>20 or 21</td>
<td>480</td>
<td>25.9%</td>
<td>26.3%</td>
<td>26.1%</td>
<td></td>
</tr>
<tr>
<td><strong>Marital status</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever married</td>
<td>174</td>
<td>9.3%</td>
<td>9.7%</td>
<td>9.5%</td>
<td>0.775</td>
</tr>
<tr>
<td>Never married</td>
<td>1,665</td>
<td>90.7%</td>
<td>90.3%</td>
<td>90.5%</td>
<td></td>
</tr>
<tr>
<td><strong>Living in own household or with boy/girlfriend</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,500</td>
<td>82.4%</td>
<td>80.7%</td>
<td>81.6%</td>
<td>0.340</td>
</tr>
<tr>
<td>Yes</td>
<td>339</td>
<td>17.6%</td>
<td>19.3%</td>
<td>18.4%</td>
<td></td>
</tr>
<tr>
<td><strong>Own AFDC case or receiving General Assistance</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,344</td>
<td>74.0%</td>
<td>72.1%</td>
<td>73.1%</td>
<td>0.375</td>
</tr>
<tr>
<td>Yes</td>
<td>495</td>
<td>26.0%</td>
<td>27.9%</td>
<td>26.9%</td>
<td></td>
</tr>
<tr>
<td><strong>Own AFDC case</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,446</td>
<td>79.2%</td>
<td>78.0%</td>
<td>78.6%</td>
<td>0.509</td>
</tr>
<tr>
<td>Yes</td>
<td>393</td>
<td>20.8%</td>
<td>22.0%</td>
<td>21.4%</td>
<td></td>
</tr>
<tr>
<td><strong>Receiving Food Stamps</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,143</td>
<td>62.0%</td>
<td>62.4%</td>
<td>62.2%</td>
<td>0.860</td>
</tr>
<tr>
<td>Yes</td>
<td>696</td>
<td>38.0%</td>
<td>37.5%</td>
<td>37.8%</td>
<td></td>
</tr>
<tr>
<td><strong>Arrested since age 16</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,567</td>
<td>84.8%</td>
<td>85.6%</td>
<td>85.2%</td>
<td>0.633</td>
</tr>
<tr>
<td>Yes</td>
<td>272</td>
<td>15.2%</td>
<td>14.4%</td>
<td>14.8%</td>
<td></td>
</tr>
<tr>
<td><strong>Lived with both parents at age 14</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,198</td>
<td>66.0%</td>
<td>64.3%</td>
<td>65.1%</td>
<td>0.446</td>
</tr>
<tr>
<td>Yes</td>
<td>641</td>
<td>34.0%</td>
<td>35.7%</td>
<td>34.9%</td>
<td></td>
</tr>
<tr>
<td><strong>Sample size</strong></td>
<td>1,839</td>
<td>949</td>
<td>890</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
### TABLE B.1 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Both Groups</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>61</td>
<td>3.5%</td>
<td>3.1%</td>
<td>3%</td>
<td>1.000</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>7.9</td>
<td>8.7</td>
<td>8.1%</td>
<td></td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>74</td>
<td>3.9</td>
<td>4.2</td>
<td>4.0%</td>
<td></td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>184</td>
<td>9.9</td>
<td>10.1</td>
<td>10.0%</td>
<td></td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>100</td>
<td>5.4</td>
<td>5.5</td>
<td>5.4%</td>
<td></td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>183</td>
<td>9.8</td>
<td>10.1</td>
<td>10.0%</td>
<td></td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>130</td>
<td>7.1</td>
<td>7.1</td>
<td>7.1%</td>
<td></td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>236</td>
<td>12.5</td>
<td>13.1</td>
<td>12.8%</td>
<td></td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>155</td>
<td>8.5</td>
<td>8.3</td>
<td>8.4%</td>
<td></td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>218</td>
<td>12.1</td>
<td>11.6</td>
<td>11.9%</td>
<td></td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>140</td>
<td>7.8</td>
<td>7.4</td>
<td>7.6%</td>
<td></td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>119</td>
<td>6.8</td>
<td>6.1</td>
<td>6.5%</td>
<td></td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>87</td>
<td>4.7</td>
<td>4.7</td>
<td>4.7%</td>
<td></td>
</tr>
</tbody>
</table>

Sample size: 1,839

**Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.**

**Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.**

**Notes:** Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires. Distributions may not total 100.0 percent because of rounding. The column labeled "p" is the statistical significance level of the difference in distributions of characteristics between groups; that is, p is the probability that observed proportions in each subgroup differ by research status only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions.
### TABLE B.2
ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF ASSIGNMENT TO THE EXPERIMENTAL GROUP

<table>
<thead>
<tr>
<th>Regressor or Statistic</th>
<th>Full Sample</th>
<th>24-Month Impact Sample</th>
<th>Combination-Survey Responders</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Constant</strong></td>
<td>0.503*** (0.010)</td>
<td>0.516*** (0.012)</td>
<td>0.464*** (0.032)</td>
</tr>
<tr>
<td><strong>Site</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>-0.001 (0.054)</td>
<td>0.001 (0.060)</td>
<td>-0.077 (0.175)</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>-0.004 (0.049)</td>
<td>-0.026 (0.056)</td>
<td>0.081 (0.177)</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>-0.000 (0.045)</td>
<td>0.002 (0.052)</td>
<td>0.083 (0.176)</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>0.000 (0.065)</td>
<td>-0.003 (0.074)</td>
<td>-0.152 (0.188)</td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>-0.003 (0.051)</td>
<td>0.026 (0.058)</td>
<td>0.258 (0.215)</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>0.002 (0.054)</td>
<td>0.054 (0.062)</td>
<td>0.207 (0.152)</td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>0.031 (0.050)</td>
<td>0.042 (0.063)</td>
<td>0.175 (0.175)</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>0.003 (0.059)</td>
<td>0.012 (0.067)</td>
<td>-0.051 (0.156)</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>0.000 (0.053)</td>
<td>0.015 (0.058)</td>
<td>0.211 (0.368)</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>0.007 (0.055)</td>
<td>0.003 (0.062)</td>
<td>0.501** (0.205)</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>-0.006 (0.047)</td>
<td>0.024 (0.054)</td>
<td>0.192 (0.134)</td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>-0.001 (0.069)</td>
<td>0.044 (0.079)</td>
<td>-0.176 (0.290)</td>
</tr>
<tr>
<td>Male</td>
<td>-0.048* (0.027)</td>
<td>-0.050 (0.031)</td>
<td>-0.042 (0.091)</td>
</tr>
</tbody>
</table>

(continued)
<table>
<thead>
<tr>
<th>Regressor or Statistic</th>
<th>Full Sample</th>
<th>24-Month Impact Sample</th>
<th>Combination-Survey Responders</th>
</tr>
</thead>
<tbody>
<tr>
<td>White, non-Hispanic</td>
<td>-0.023</td>
<td>-0.040</td>
<td>-0.352**</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.048)</td>
<td>(0.166)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.018</td>
<td>0.028</td>
<td>0.072</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.035)</td>
<td>(0.097)</td>
</tr>
<tr>
<td>Other ethnicity</td>
<td>-0.014</td>
<td>-0.073</td>
<td>-0.034</td>
</tr>
<tr>
<td></td>
<td>(0.068)</td>
<td>(0.076)</td>
<td>(0.162)</td>
</tr>
<tr>
<td>Age 20 or 21</td>
<td>0.010</td>
<td>-0.002</td>
<td>-0.063</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.029)</td>
<td>(0.088)</td>
</tr>
<tr>
<td>No phone number on enrollment form</td>
<td>-0.063</td>
<td>-0.059</td>
<td>-0.265*</td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.057)</td>
<td>(0.148)</td>
</tr>
<tr>
<td>Male parent</td>
<td>0.078</td>
<td>0.076</td>
<td>0.135</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.054)</td>
<td>(0.134)</td>
</tr>
<tr>
<td>Female parent living with own child(ren)</td>
<td>0.008</td>
<td>-0.002</td>
<td>0.123</td>
</tr>
<tr>
<td></td>
<td>(0.035)</td>
<td>(0.039)</td>
<td>(0.113)</td>
</tr>
<tr>
<td>Limited English</td>
<td>-0.013</td>
<td>0.048</td>
<td>-0.076</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.066)</td>
<td>(0.155)</td>
</tr>
<tr>
<td>Arrested since age 16</td>
<td>0.049</td>
<td>0.036</td>
<td>0.109</td>
</tr>
<tr>
<td></td>
<td>(0.037)</td>
<td>(0.042)</td>
<td>(0.102)</td>
</tr>
<tr>
<td>Convicted since age 16</td>
<td>-0.059</td>
<td>-0.023</td>
<td>-0.186</td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
<td>(0.063)</td>
<td>(0.181)</td>
</tr>
<tr>
<td>Own AFDC case</td>
<td>-0.012</td>
<td>-0.008</td>
<td>0.016</td>
</tr>
<tr>
<td></td>
<td>(0.037)</td>
<td>(0.041)</td>
<td>(0.122)</td>
</tr>
<tr>
<td>Receiving Food Stamps</td>
<td>-0.020</td>
<td>-0.007</td>
<td>-0.027</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.034)</td>
<td>(0.090)</td>
</tr>
<tr>
<td>Never married</td>
<td>-0.010</td>
<td>0.019</td>
<td>0.090</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td>(0.044)</td>
<td>(0.132)</td>
</tr>
<tr>
<td>Household AFDC case</td>
<td>0.069**</td>
<td>0.070*</td>
<td>0.010</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.037)</td>
<td>(0.112)</td>
</tr>
<tr>
<td>Receiving Medicaid</td>
<td>-0.030</td>
<td>-0.027</td>
<td>0.043</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.035)</td>
<td>(0.099)</td>
</tr>
<tr>
<td>Left school in grade 11 or 12</td>
<td>0.013</td>
<td>0.024</td>
<td>-0.016</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.025)</td>
<td>(0.075)</td>
</tr>
</tbody>
</table>
### TABLE B.2 (continued)

<table>
<thead>
<tr>
<th>Regressor or Statistic</th>
<th>Full Sample</th>
<th>24-Month Impact Sample</th>
<th>Combination-Survey Responders</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lived with both parents at age 14</td>
<td>-0.020</td>
<td>-0.012</td>
<td>-0.141*</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.026)</td>
<td>(0.082)</td>
</tr>
<tr>
<td>Employed within past year</td>
<td>-0.013</td>
<td>0.008</td>
<td>0.048</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.025)</td>
<td>(0.074)</td>
</tr>
<tr>
<td>Number of observations</td>
<td>2,311</td>
<td>1,839</td>
<td>235</td>
</tr>
<tr>
<td>Number of experimentals</td>
<td>1,162</td>
<td>949</td>
<td>109</td>
</tr>
<tr>
<td>Number of controls</td>
<td>1,149</td>
<td>890</td>
<td>126</td>
</tr>
<tr>
<td>Degrees of freedom for error</td>
<td>2,279</td>
<td>1,807</td>
<td>203</td>
</tr>
<tr>
<td>Error mean square</td>
<td>0.251</td>
<td>0.252</td>
<td>0.245</td>
</tr>
<tr>
<td>R square</td>
<td>0.008</td>
<td>0.009</td>
<td>0.148</td>
</tr>
<tr>
<td>Mean of dependent variable</td>
<td>0.503</td>
<td>0.516</td>
<td>0.464</td>
</tr>
<tr>
<td>F-statistic</td>
<td>0.596</td>
<td>0.538</td>
<td>1.140</td>
</tr>
<tr>
<td>P-value of F-statistic</td>
<td>0.963</td>
<td>0.983</td>
<td>0.291</td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** The dependent variable in each regression equation was unity for each experimental and zero for each control. Each characteristic on the right-hand side of each equation was measured as a deviation from its mean. The standard error of each coefficient estimate is enclosed in parentheses.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The p-value of the F-statistic is the probability of obtaining these coefficient estimates if the true chance of becoming an experimental did not vary with any characteristic. Thus, the closer the p-value is to unity, the more successful was random assignment in equating average characteristics of experimentals and controls.
evidence that there was no overall pattern of differences between experimentals and controls. It shows that random assignment created two groups without systematic overall differences in characteristics before enrollment. There were statistically significant differences in only two individual characteristics. For the full sample, experimentals were slightly less likely to be male and more likely to live in a household with someone else who received AFDC.

Among the 1,839 survey responders (see column two of Table B.2) and among the 235 combination survey responders (column three), the results of random assignment were similar. Although, judging from the statistically insignificant p-values for the survey responders, there were no systematic overall differences, the experimental survey responders were slightly more likely to live in a household with someone else who received AFDC. Combination survey responders were less likely to be white non-Hispanic, slightly less likely to have no phone number on their enrollment form, and slightly less likely to have lived with both parents at age 14. They were, however, more likely to have been at the East Los Angeles Skills Center.

The procedure used to calculate all the impacts presented in this report took these slight differences in characteristics into account, and estimated the impacts that would have occurred had these slight differences not existed.

II. Nonresponse Bias

Were those sample members for whom there are continuous data for 24 months representative of the full JOBSTART sample, including nonresponders? A high degree of mobility among disadvantaged young dropouts makes it difficult for survey interviewers to locate all of them a year or two after they have been enrolled into a research sample. As noted in Appendix A, 1,839 of the 2,311 full sample members furnished data covering 24 months, either at both the 12-month and 24-month junctures or at the 24-month juncture, for an overall response rate of 79.6 percent (81.7 percent for experimentals and 77.5 percent for controls).2 See Table A.2 for site-specific information on response rates.

There were systematic differences in characteristics between those who responded to the surveys and those who did not respond. Table B.3 presents linear regression results measuring the extent to which average characteristics for the 1,839 survey responders differed from

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2 There are two types of nonresponse. Unit nonresponse is the failure to ascertain answers to any of the questionnaire items. Item nonresponse is the failure to obtain only some answers. All the response rates mentioned here are unit response rates.
### TABLE B.3
ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF UNIT SURVEY RESPONSE

<table>
<thead>
<tr>
<th>Regressor or Statistic</th>
<th>Sample Unit Survey Response</th>
<th>Combination Survey Response</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>24-Month Impact</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.796***</td>
<td>0.128***</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Experimental status</td>
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<td>Atlanta Job Corps</td>
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<td>Regressor or Statistic</td>
<td>Sample and Dependent Variable</td>
<td>Combination Survey</td>
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<tr>
<td></td>
<td>Sample Unit Survey Response</td>
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<td></td>
<td>1,839/2,311</td>
<td>235/1,839</td>
</tr>
<tr>
<td>Male</td>
<td>-0.027 (0.022)</td>
<td>0.035* (0.020)</td>
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<td>White, non-Hispanic</td>
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<tr>
<td>Hispanic</td>
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<td>Other ethnicity</td>
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<td>Age 20 or 21</td>
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<td>-0.046** (0.019)</td>
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<tr>
<td>No phone number on enrollment form</td>
<td>-0.047 (0.040)</td>
<td>0.041 (0.037)</td>
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<tr>
<td>Male parent</td>
<td>-0.029 (0.038)</td>
<td>0.051 (0.035)</td>
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<tr>
<td>Female parent living with own child(ren)</td>
<td>0.032 (0.028)</td>
<td>0.022 (0.025)</td>
</tr>
<tr>
<td>Limited English</td>
<td>-0.038 (0.045)</td>
<td>0.013 (0.042)</td>
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<tr>
<td>Arrested since age 16</td>
<td>-0.002 (0.029)</td>
<td>0.072*** (0.027)</td>
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<tr>
<td>Convicted since age 16</td>
<td>-0.095** (0.043)</td>
<td>-0.071* (0.041)</td>
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<td>-0.016 (0.026)</td>
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<td>Receiving Food Stamps</td>
<td>0.007 (0.024)</td>
<td>0.001 (0.022)</td>
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<td>Never married</td>
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<td>Receiving Medicaid</td>
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(continued)
TABLE B.3 (continued)

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<th>Regressor or Statistic</th>
<th>Sample Unit Survey</th>
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<tr>
<td></td>
<td>24-Month Impact</td>
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<tr>
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<td>Sample</td>
<td>Combination</td>
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<td>Response</td>
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</tr>
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<td>1,839/2,311</td>
<td>235/1,839</td>
</tr>
<tr>
<td>Left school in grade 11 or 12</td>
<td>0.028</td>
<td>-0.035**</td>
</tr>
<tr>
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<td>(0.018)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Lived with both parents at age 14</td>
<td>0.050***</td>
<td>-0.007</td>
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<td>(0.017)</td>
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<tr>
<td>Employed within past year</td>
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<td>(0.018)</td>
<td>(0.016)</td>
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<td>Number of observations</td>
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<tr>
<td>Number of experimentals</td>
<td>1,162</td>
<td>949</td>
</tr>
<tr>
<td>Number of controls</td>
<td>1,149</td>
<td>890</td>
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<tr>
<td>Degrees of freedom for error</td>
<td>2,278</td>
<td>1,806</td>
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<tr>
<td>Error mean square</td>
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<td>0.106</td>
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<td>R square</td>
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<td>Mean of dependent variable</td>
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<td>F-statistic</td>
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<tr>
<td>P-value of F-statistic</td>
<td>0.000</td>
<td>0.000</td>
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</table>

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: The dependent variable in each regression equation was unity for survey response or combination-survey response and zero otherwise. Each characteristic on the right-hand side of each equation was measured as a deviation from its mean. The standard error of each coefficient estimate is enclosed in parentheses.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The p-value of the F-statistic in column one or column two is the probability of obtaining these coefficient estimates if the true chance of responding to the survey or to the combination survey did not vary with any characteristic. Thus, the closer the p-value is to zero, the more important are differences in characteristics between survey responders and nonresponders or between combination-survey responders and those who responded to only one of the two surveys.
average characteristics at random assignment for the 472 nonresponders. Since the final entry, the p-value of the F-statistic, is zero to three decimal places, there is strong evidence of systematic differences between responders and nonresponders. Responders were significantly less likely to have been convicted between the age of 16 and the time of random assignment, and significantly more likely to have lived with two parents at age 14. Also, better response was found at Allentown in Buffalo, even after taking differences in individual characteristics into account. In addition, responders were somewhat more likely to be experimentals than controls.

When nonresponse is randomly distributed among members of both treatment and control groups, it is troublesome only because it reduces the sample size and thus the statistical power to find impacts of a given size. Randomly distributed nonresponse does not alter the expected values of adjusted mean outcomes, and thus does not bias impacts. However, when nonresponse is greater among one research group (such as controls) or among members of either research group with certain characteristics (such as men), impacts may be biased slightly unless corrected for nonresponse. The most flexible correction for nonresponse is incorporation of an additional equation for survey response into a two-equation system with the impact equation. The success of attempts to implement such corrections is data-dependent, and the differential response rates found do not seem quite large enough to warrant such uncertain measures.

III. Impact of Participation Versus Impact of Assignment

Because the target population for the JOBSTART Demonstration consisted of young people who had histories of dropping out of education programs, it was difficult to get those selected for the program to attend and to retain attendees for substantial periods of time. However, everyone assigned to experimental status was included when calculating average impacts of JOBSTART. Therefore, impacts do not measure the impacts of participation in JOBSTART, but rather of assignment to the group eligible to receive JOBSTART services.3

3Some might suggest that nonparticipants be excluded from impact analyses. However, such exclusions would expose impacts to possible selection biases, undermining the control group’s validity in measuring what would have happened without the program. When nonparticipants are excluded from the experimental group, average measured and unmeasured characteristics of experimentals may no longer be the same as average control group characteristics. See Cave, 1988.
Thus, impact estimates average net outcomes for all experimentals, including nonparticipants. Nonparticipation "waters down" the program effect the experiment seeks to detect. Fortunately, only 106 of the 949 experimentals in the impact sample never participated in the program. Such low nonparticipation may have been due in part to successful negotiation with sites to place the point of random assignment after initial assessment but immediately before program services started.

When substantial nonparticipation occurs during an experimental evaluation of a program, techniques are available for calculating impacts of participation as well as impacts of assignment. When the proportion of assignees to the program who are not counted as participants is an unbiased measure of the proportion of controls who would not have participated, when the program has no effect on nonparticipants, and when the sample is large enough, it is approximately valid to use the formula:

$$\text{Impact of participation} = \frac{\text{Impact of assignment}}{\text{Fraction participating}}$$

Using this formula necessitates validating all of the assumptions underlying it, and thus makes impact analysis more complicated than a simple comparison of average outcomes for those assigned to the experimental group and those assigned to the control group. The assumption of zero effects on nonparticipants is troublesome, because the process of recruiting experimentals, screening them, and contacting them when they do not appear may alter their behavior. Thus, in this report, impacts of assignment were reported instead of impacts of participation.

As outlined above, impacts of assignment to JOBSTART were calculated by comparing average outcomes for all those assigned to the experimental group with average outcomes for all those assigned to the control group. In order to increase the statistical precision of the impact estimate, a variant of simple group averaging known as one-way linear analysis of covariance was used for the impact analysis in this report. As shown for the full sample of 1,839 responders in Table B.4, in a multiple regression of outcome on covariates measured at

---

4See Cave, 1988; Auspos, Cave, and Long, 1988, Appendix E; Bloom, 1984; and Farkas et al., 1984, p. 85. If such an adjustment factor were appropriate here, its value would be approximately the reciprocal of the rate of participation in JOBSTART or $1 / (1 - 106/949) = 1.126$.

<table>
<thead>
<tr>
<th>Regressor or Statistic</th>
<th>Ever Received Any Education or Training, Months 1-24 (%)</th>
<th>Received GED or High School Diploma by End of Month 24 (%)</th>
<th>Ever Employed, Months 13-24 (%)</th>
<th>Total Earnings, Months 13-24 ($)</th>
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<td>Constant</td>
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<td>69.523***</td>
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<td>(1.389)</td>
<td>(1.420)</td>
<td>(151.034)</td>
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<td>(1.794)</td>
<td>(1.938)</td>
<td>(1.981)</td>
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<td>Site</td>
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</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>17.030***</td>
<td>-9.820*</td>
<td>3.485</td>
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<td>(5.171)</td>
<td>(5.266)</td>
<td>(562.332)</td>
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<td>(558.026)</td>
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<td>(5.056)</td>
<td>(6.190)</td>
<td>(658.529)</td>
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<td>(5.549)</td>
<td>(5.671)</td>
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<td>Age 20 or 21</td>
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(continued)
TABLE B.4 (continued)

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<th>Regressor or Statistic</th>
<th>Ever Received Any Education or Training, Months 1-24 (%)</th>
<th>Ever Received GED or High School Diploma by End of Month 24 (%)</th>
<th>Ever Employed, Months 13-24 (%)</th>
<th>Total Earnings, Months 13-24 ($)</th>
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<tr>
<td>Male</td>
<td>-8.165*** (2.341)</td>
<td>-0.803 (2.528)</td>
<td>17.626*** (2.584)</td>
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<td>White, non-Hispanic</td>
<td>-3.244 (3.633)</td>
<td>13.447*** (3.924)</td>
<td>14.017*** (4.010)</td>
<td>1,752.633*** (426.657)</td>
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<td>Black, non-Hispanic</td>
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<td>Hispanic</td>
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<td>-1.529 (2.885)</td>
<td>6.708** (2.948)</td>
<td>581.451* (313.667)</td>
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<td>Other</td>
<td>4.504 (5.798)</td>
<td>-11.583* (6.262)</td>
<td>-1.077</td>
<td>964.634 (680.916)</td>
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<td>Left school in grade 11 or 12</td>
<td>-1.804 (1.938)</td>
<td>6.357*** (2.093)</td>
<td>5.989*** (2.139)</td>
<td>819.836*** (227.585)</td>
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<td>Limited English</td>
<td>-5.127 (5.000)</td>
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<td>Never married</td>
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<td>-0.809 (3.652)</td>
<td>-6.274*</td>
<td>-982.086** (397.063)</td>
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<td>Male parent</td>
<td>-3.261 (4.152)</td>
<td>-5.026 (4.484)</td>
<td>-3.077</td>
<td>629.133 (487.604)</td>
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<tr>
<td>Female parent living with own child(ren)</td>
<td>-7.048** (2.949)</td>
<td>-1.715 (3.185)</td>
<td>-9.240***</td>
<td>-320.469 (346.287)</td>
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<td>Lived with both parents at age 14</td>
<td>0.768 (2.007)</td>
<td>0.732 (2.167)</td>
<td>4.965**</td>
<td>680.035*** (235.652)</td>
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<tr>
<td>Own AFDC case</td>
<td>5.091* (3.096)</td>
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<td>Receiving Food Stamps</td>
<td>-1.989 (2.585)</td>
<td>5.231* (2.792)</td>
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<td>38.620 (303.572)</td>
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(continued)

-209-
<table>
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<tr>
<th>Dependent Variable</th>
<th>Ever Received Any Education or Training, Months 1-24 (%)</th>
<th>Received GED or High School Diploma by End of Month 24 (%)</th>
<th>Ever Employed, Months 13-24 (%)</th>
<th>Total Earnings, Months 13-24 ($)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Employed within past year</td>
<td>-2.521</td>
<td>0.081</td>
<td>5.370**</td>
<td>1,001.186***</td>
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<td>(1.943)</td>
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<td>(2.145)</td>
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<tr>
<td>Arrested since age 16</td>
<td>-2.371</td>
<td>-2.763</td>
<td>-4.801</td>
<td>-384.891</td>
</tr>
<tr>
<td>(3.187)</td>
<td>(3.442)</td>
<td>(3.518)</td>
<td>(374.237)</td>
<td></td>
</tr>
<tr>
<td>Convicted since age 16</td>
<td>2.605</td>
<td>-0.232</td>
<td>-0.793</td>
<td>-377.090</td>
</tr>
<tr>
<td>(4.835)</td>
<td>(5.222)</td>
<td>(5.337)</td>
<td>(567.844)</td>
<td></td>
</tr>
</tbody>
</table>

Number of observations 1,839 1,839 1,839 1,839
Number of experimentals 949 949 949 949
Number of controls 890 890 890 890
Degrees of freedom for error 1,806 1,806 1,806 1,806
Error mean square 1,465.154 1,708.907 1,785.134 20,205,656.707
R square 0.325 0.107 0.152 0.203
Mean of dependent variable 69.222 25.068 70.799 3,993.162
F-statistic 27.13 6.73 10.12 14.36
P-value of F-statistic 0.000 0.000 0.000 0.000

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Ordinary least squares regression coefficients in this table correspond to impact estimates presented in Tables 4.1, 4.5, 5.1, and 5.4. A one-way linear analysis of covariance procedure was used to control for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; and Cave, 1987). The standard error of each coefficient estimate is enclosed in parentheses.

Each characteristic on the right-hand side of each equation was measured as a deviation from its mean.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
the time of enrollment and on a dummy variable for research status, the coefficient of the dummy variable is the impact. This coefficient may be interpreted as the difference between the adjusted mean outcome for those assigned to the experimental group and the adjusted mean outcome for those assigned to the control group. Adjustment removes the effect of slight differences at the time of enrollment in characteristics related to the outcome, and yields a purer measure of the effect of research status alone.

Some of the subgroup results presented in the report were based on slightly more complex regression equations, which include terms for interactions between experimental status and subgroup characteristics. Such "two-way ANCOVA" impacts may differ to some extent from "split-file" impacts estimated by eliminating other subgroups from "one-way ANCOVA" analyses for Table B.4. However, calculating two-way ANCOVA impacts permits determining the statistical significance of impact differences, and is less burdensome computationally.

IV. The Internal Validity of Comparisons Among Subgroups and Types of Sites

Youths in the impact sample can be grouped based on their individual, pre-random assignment characteristics or on the characteristics of the sites at which they applied for JOBSTART. Because such subgroup comparisons are a central part of the analysis presented in this report, it is important to discuss briefly the complications in drawing conclusions from any observed differences. Crucial comparisons of this type are between men and women, and between types of sites. To summarize, impacts can be compared across subgroups defined by individual or site characteristics, but more caution is advised in interpreting such results than in interpreting the full sample or within-site impacts just described. This is especially true for comparisons of site types.

The basic reason is that, since sample members were not assigned randomly to these subgroups or types of sites, it may be impossible to isolate the difference in impact attributable to the single characteristic used to designate the groups. For example, using an example of a group "defined" by an individual characteristic, if women have bigger impacts than men, it may not be because they are women; the impact difference might really be because they had less prior work experience so controls were less likely to be working in the follow-up period. Further, using site or site groupings for a subgroup impact comparison is fundamentally different from using an individual characteristic such as gender. Many things about sites differ (such as labor market, participant characteristics and interests, and program characteristics), and
there is a real danger that impact differences for site groupings may be misinterpreted as measuring the relative efficiency of a single feature of a site’s program, such as its curricula, facilities, or program structure (that is, brokered versus in-house services or concurrent versus sequential education and training) rather than other factors that could be driving inter-site variation.

The internal validity of impact comparisons by individual characteristics is difficult to test. However, there is a simple test for internal validity of impact comparisons by program features. If groups of individuals randomly assigned at two locations really differed only in the features of the programs experimentals could attend, then the post-random assignment experience of controls at the two locations should be identical. This rarely happens; more typically, the experience of controls varies between sites, just as that of experimentals does.6

This problem can also affect impact comparisons for subgroups defined by characteristics of individuals as well as subgroup impacts by site or site grouping, although ordinarily the concerns about misinterpretation are less severe. For example, if virtually all the Hispanics recruited into a demonstration were concentrated in one or two sites, then "Hispanic" impacts really were impacts for "those sites." However, normally (and in the JOBSTART Demonstration, as discussed below) most measured characteristics of individuals are distributed fairly evenly across locations.7 There will have been younger and older sample members, people reading at higher and lower levels, and parents and childless youths in samples recruited in all JOBSTART sites. Moreover, relevant unmeasured characteristics of individuals such as strength of motivation to attend a program and desire for a GED are likely to have been distributed fairly evenly among younger versus older sample members, those with higher versus lower reading levels, those who were parents versus those who were childless, and other subgroups defined by the observed characteristics of individuals.8 If the impact on educational attainment, for example, was higher among low-reading-level than among high-reading-level sample members, it is reasonable to interpret this as evidence that the programs were more

---

6The usual situation is what is known in the evaluation literature as "ecological correlation bias."
7As discussed in the following section, several ethnic groups were highly concentrated in a few JOBSTART sites, however.
8To lessen this problem, impact estimates presented in this report always used site dummies as covariates when calculating impacts by individual characteristics; these dummies can correct for small differences between subgroups defined by individual characteristics in unmeasured characteristics associated with site.
effective with the former rather than concluding that higher educational attainment occurred because the average sample member who had a low reading level was better motivated or wanted a GED more than did the average sample member who had a higher reading level.

In contrast, when a sample is split by site or site group, unmeasured characteristics will be distributed unevenly across groups. For example, sites that were known for providing education services and that offered a sequence of basic skills instruction followed by occupational training at another agency (sequential/brokered sites) may have been more likely to recruit clients motivated to get a GED than did concurrent sites with a reputation for training, whose typical client may have wanted to learn occupational skills. Thus, a finding that JOBSTART's impact on GED attainment was less at concurrent sites than at sequential/brokered sites does not necessarily mean that someone with average motivation and desire for a GED has a better chance of getting a GED in a sequential program. Such a finding could mean that those recruited in sequential locations had on average very different levels of desire for skills training relative to GED preparation from those recruited at concurrent locations. Even if the usual statistical adjustment methods are employed in calculating impacts, little can be done about this problem, since motivation to participate in particular components was not measured.9

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9Subgroup impact equations for groupings by site type cannot use individual site dummies to correct for small unobserved differences in groups in the same way that equations for groups defined by individual characteristics can: Individual site dummies would be highly correlated with the site groupings. To measure which delivery system is better for those with average levels of motivation, desire for GEDs, and other unobserved characteristics, the best approach is to randomly assign people to each delivery system in each location after carrying out a common recruitment effort at that location. In that way, unmeasured characteristics would be the same for each delivery system, because each delivery system would be fairly represented in each location and in each recruitment effort. Other, nonexperimental approaches to this problem will be attempted in future stages of the JOBSTART research when longer follow-up is available.
APPENDIX C

SUPPLEMENTAL TABLES TO CHAPTER 2
### TABLE C.1
CHARACTERISTICS IN THE YEAR BEFORE RANDOM ASSIGNMENT, BY EMPLOYMENT STATUS

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Employed Within the Year Before Random Assignment</th>
<th>Both Groups</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>No</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td><strong>Gender</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Woman</td>
<td>968</td>
<td>62.9%</td>
<td>43.4%</td>
<td>52.64***</td>
</tr>
<tr>
<td>Men</td>
<td>871</td>
<td>37.1%</td>
<td>56.6%</td>
<td>47.4</td>
</tr>
<tr>
<td><strong>Ethnicity</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>155</td>
<td>7.1%</td>
<td>9.6%</td>
<td>8.4**</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>840</td>
<td>46.4%</td>
<td>45.0%</td>
<td>45.7</td>
</tr>
<tr>
<td>Hispanic</td>
<td>783</td>
<td>42.0%</td>
<td>43.1%</td>
<td>42.6</td>
</tr>
<tr>
<td>Other</td>
<td>61</td>
<td>4.5%</td>
<td>2.3%</td>
<td>3.3</td>
</tr>
<tr>
<td><strong>Ethnicity, by gender</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>84</td>
<td>4.5%</td>
<td>4.6%</td>
<td>4.6***</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>445</td>
<td>28.3%</td>
<td>20.5%</td>
<td>24.2</td>
</tr>
<tr>
<td>Hispanic</td>
<td>413</td>
<td>28.3%</td>
<td>17.2%</td>
<td>22.5</td>
</tr>
<tr>
<td>Other</td>
<td>26</td>
<td>1.8%</td>
<td>1.0%</td>
<td>1.4</td>
</tr>
<tr>
<td>Men</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>71</td>
<td>2.6%</td>
<td>5.0%</td>
<td>3.9</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>395</td>
<td>18.2%</td>
<td>24.5%</td>
<td>21.5</td>
</tr>
<tr>
<td>Hispanic</td>
<td>370</td>
<td>13.7%</td>
<td>25.9%</td>
<td>20.1</td>
</tr>
<tr>
<td>Other</td>
<td>35</td>
<td>2.6%</td>
<td>1.2%</td>
<td>1.9</td>
</tr>
<tr>
<td><strong>Parental status</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women living with own child(ren)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>484</td>
<td>28.2%</td>
<td>24.7%</td>
<td>26.3***</td>
</tr>
<tr>
<td>Yes</td>
<td>484</td>
<td>34.7%</td>
<td>18.8%</td>
<td>26.3</td>
</tr>
<tr>
<td>Men who have own child(ren)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>765</td>
<td>33.6%</td>
<td>48.8%</td>
<td>41.6</td>
</tr>
<tr>
<td>Yes</td>
<td>106</td>
<td>3.6%</td>
<td>7.7%</td>
<td>5.8</td>
</tr>
<tr>
<td><strong>Prior employment, by gender</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women employed within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>547</td>
<td>62.9%</td>
<td>0.0%</td>
<td>29.7***</td>
</tr>
<tr>
<td>Yes</td>
<td>421</td>
<td>0.0%</td>
<td>13.4%</td>
<td>22.9</td>
</tr>
<tr>
<td>Men employed within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>323</td>
<td>37.1%</td>
<td>0.0%</td>
<td>17.6</td>
</tr>
<tr>
<td>Yes</td>
<td>548</td>
<td>0.0%</td>
<td>56.6%</td>
<td>29.8</td>
</tr>
</tbody>
</table>

Sample size: 1,839

(continued)
<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Employed Within the Year Before Random Assignment</th>
<th>Both Groups</th>
<th>p*</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>No</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Left school in grade 11 or 12</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,078</td>
<td>59.1%</td>
<td>58.2%</td>
<td>58.6%</td>
</tr>
<tr>
<td>Yes</td>
<td>761</td>
<td>40.9</td>
<td>41.8</td>
<td>41.4</td>
</tr>
<tr>
<td>Received occupational training within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,529</td>
<td>87.0</td>
<td>79.7</td>
<td>83.1***</td>
</tr>
<tr>
<td>Yes</td>
<td>310</td>
<td>13.0</td>
<td>20.3</td>
<td>16.9</td>
</tr>
<tr>
<td>Age</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16-19</td>
<td>1,359</td>
<td>74.5</td>
<td>73.4</td>
<td>73.9</td>
</tr>
<tr>
<td>20 or 21</td>
<td>480</td>
<td>25.5</td>
<td>26.6</td>
<td>26.1</td>
</tr>
<tr>
<td>Marital status</td>
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<td></td>
<td></td>
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<tr>
<td>Ever married</td>
<td>174</td>
<td>9.2</td>
<td>9.7</td>
<td>9.5</td>
</tr>
<tr>
<td>Never married</td>
<td>1,665</td>
<td>90.8</td>
<td>90.3</td>
<td>90.5</td>
</tr>
<tr>
<td>Living in own household or with boy/girlfriend</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,500</td>
<td>79.2</td>
<td>83.7</td>
<td>81.6**</td>
</tr>
<tr>
<td>Yes</td>
<td>339</td>
<td>20.8</td>
<td>16.3</td>
<td>18.4</td>
</tr>
<tr>
<td>Own AFDC case or receiving General Assistance</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>1,344</td>
<td>66.0</td>
<td>79.5</td>
<td>73.1***</td>
</tr>
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<td>Yes</td>
<td>495</td>
<td>34.0</td>
<td>20.5</td>
<td>26.9</td>
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<tr>
<td>Own AFDC case</td>
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<td></td>
<td></td>
</tr>
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<td>No</td>
<td>1,446</td>
<td>70.6</td>
<td>85.9</td>
<td>78.6***</td>
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<td>Yes</td>
<td>393</td>
<td>29.4</td>
<td>14.1</td>
<td>21.4</td>
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<td>Receiving Food Stamps</td>
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<tr>
<td>No</td>
<td>1,143</td>
<td>58.0</td>
<td>65.8</td>
<td>62.2***</td>
</tr>
<tr>
<td>Yes</td>
<td>696</td>
<td>42.0</td>
<td>34.2</td>
<td>37.8</td>
</tr>
<tr>
<td>Arrested since age 16</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,567</td>
<td>88.5</td>
<td>82.2</td>
<td>85.2***</td>
</tr>
<tr>
<td>Yes</td>
<td>272</td>
<td>11.5</td>
<td>17.8</td>
<td>14.8</td>
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<tr>
<td>Lived with both parents at age 14</td>
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<td></td>
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<tr>
<td>No</td>
<td>1,198</td>
<td>68.5</td>
<td>62.1</td>
<td>65.1***</td>
</tr>
<tr>
<td>Yes</td>
<td>641</td>
<td>31.5</td>
<td>37.9</td>
<td>34.9</td>
</tr>
<tr>
<td>Sample size</td>
<td>1,839</td>
<td>870</td>
<td>969</td>
<td></td>
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</tbody>
</table>
### TABLE C.1 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Employed Within the Year Before Random Assignment</th>
<th>Both Groups</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Site</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>61</td>
<td>2.6%</td>
<td>3.9%</td>
<td>3.3%*** 0.000</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>6.4</td>
<td>9.9</td>
<td>8.3</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>74</td>
<td>4.6</td>
<td>3.5</td>
<td>4.0</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>184</td>
<td>6.1</td>
<td>13.5</td>
<td>10.0</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>100</td>
<td>6.3</td>
<td>4.6</td>
<td>5.4</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>183</td>
<td>8.4</td>
<td>11.4</td>
<td>10.0</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>130</td>
<td>8.3</td>
<td>6.0</td>
<td>7.1</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>236</td>
<td>9.1</td>
<td>16.2</td>
<td>12.8</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>155</td>
<td>8.0</td>
<td>8.8</td>
<td>8.4</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>218</td>
<td>18.9</td>
<td>5.6</td>
<td>11.9</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>140</td>
<td>9.3</td>
<td>6.1</td>
<td>7.6</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>119</td>
<td>8.9</td>
<td>4.3</td>
<td>6.5</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>87</td>
<td>3.1</td>
<td>6.2</td>
<td>4.7</td>
</tr>
</tbody>
</table>

Sample size 1,839 870 969

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires. Distributions may not total 100.0 percent because of rounding. The column labeled "p" is the statistical significance level of the difference in distributions of characteristics between groups: that is, p is the probability that observed proportions in each subgroup differ by employment status only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
TABLE C.2
CHARACTERISTICS IN THE YEAR BEFORE RANDOM ASSIGNMENT, 
BY GENDER AND EMPLOYMENT STATUS

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Women Not Employed</th>
<th>Women Employed</th>
<th>Men Not Employed</th>
<th>Men Employed</th>
<th>All Categories</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Ethnicity</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>155</td>
<td>7.1%</td>
<td>10.7%</td>
<td>7.1%</td>
<td>8.8%</td>
<td>8.4***</td>
<td>0.001</td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>840</td>
<td>45.0</td>
<td>47.3</td>
<td>48.9</td>
<td>43.2</td>
<td>45.7</td>
<td>0.0</td>
</tr>
<tr>
<td>Hispanic</td>
<td>703</td>
<td>45.0</td>
<td>39.7</td>
<td>36.8</td>
<td>45.8</td>
<td>42.6</td>
<td>0.0</td>
</tr>
<tr>
<td>Other</td>
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(continued)
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<td>323</td>
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### TABLE C.2 (continued)

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<th>Men</th>
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<th>All Categories</th>
<th>p*</th>
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<td>1.5%</td>
<td>3.6%</td>
<td>3.3*** 0.000</td>
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<tr>
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</tr>
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</tr>
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<td>5.3%</td>
<td>5.4</td>
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<tr>
<td>EGOS (Denver)</td>
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<td>Phoenix Job Corps</td>
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<td>7.1</td>
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<td>8.4%</td>
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<td>8.4</td>
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<td>11.9</td>
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<tr>
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<td>1.9%</td>
<td>4.6%</td>
<td>4.7</td>
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Sample size = 1,839

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<th></th>
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<th>Men</th>
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<tbody>
<tr>
<td></td>
<td>547</td>
<td>421</td>
<td>323</td>
<td>548</td>
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</tbody>
</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

*p* is the statistical significance level of differences among groups in distributions of characteristics: that is, *p* is the probability that observed proportions in each subgroup differ from one column to another only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
### TABLE C.3

**CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY AGE**

<table>
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<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Age 16-19</th>
<th>Age 20 or 21</th>
<th>Both Groups</th>
<th>p^2</th>
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<td>968</td>
<td>52.2%</td>
<td>53.7%</td>
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<td>9.8%</td>
<td>4.6%</td>
<td>8.4***</td>
<td>0.000</td>
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<tr>
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</tr>
<tr>
<td><strong>Ethnicity, by gender</strong></td>
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</tr>
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<tr>
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<td>21.7%</td>
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<td>24.2%</td>
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<tr>
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<td>24.0%</td>
<td>18.1%</td>
<td>22.5%</td>
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<td>Other</td>
<td>26</td>
<td>1.5%</td>
<td>1.2%</td>
<td>1.4%</td>
<td></td>
</tr>
<tr>
<td>Men</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, non-Hispanic</td>
<td>71</td>
<td>4.7%</td>
<td>1.5%</td>
<td>3.9%</td>
<td></td>
</tr>
<tr>
<td>Black, non-Hispanic</td>
<td>395</td>
<td>20.9%</td>
<td>23.1%</td>
<td>21.5%</td>
<td></td>
</tr>
<tr>
<td>Hispanic</td>
<td>370</td>
<td>20.3%</td>
<td>19.6%</td>
<td>20.1%</td>
<td></td>
</tr>
<tr>
<td>Other</td>
<td>35</td>
<td>1.8%</td>
<td>2.1%</td>
<td>1.9%</td>
<td></td>
</tr>
<tr>
<td><strong>Parental status</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women living with own child(ren)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>484</td>
<td>30.3%</td>
<td>15.0%</td>
<td>26.3***</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>484</td>
<td>21.9%</td>
<td>38.7%</td>
<td>26.3%</td>
<td></td>
</tr>
<tr>
<td>Men who have own child(ren)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>765</td>
<td>43.9%</td>
<td>35.2%</td>
<td>41.6%</td>
<td></td>
</tr>
<tr>
<td>Yes</td>
<td>106</td>
<td>3.9%</td>
<td>11.0%</td>
<td>5.8%</td>
<td></td>
</tr>
<tr>
<td><strong>Employed within past year</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>870</td>
<td>47.7%</td>
<td>46.2%</td>
<td>47.3%</td>
<td>0.589</td>
</tr>
<tr>
<td>Yes</td>
<td>969</td>
<td>52.3%</td>
<td>53.7%</td>
<td>52.7%</td>
<td></td>
</tr>
<tr>
<td><strong>Prior employment, by gender</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Women employed within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>547</td>
<td>29.4%</td>
<td>30.8%</td>
<td>29.7%</td>
<td>0.532</td>
</tr>
<tr>
<td>Yes</td>
<td>421</td>
<td>22.9%</td>
<td>22.9%</td>
<td>22.9%</td>
<td></td>
</tr>
<tr>
<td>Men employed within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>323</td>
<td>18.3%</td>
<td>15.4%</td>
<td>17.6%</td>
<td></td>
</tr>
<tr>
<td>Yes</td>
<td>548</td>
<td>29.4%</td>
<td>30.8%</td>
<td>29.8%</td>
<td></td>
</tr>
<tr>
<td><strong>Sample size</strong></td>
<td>1,839</td>
<td>1,359</td>
<td>480</td>
<td></td>
<td></td>
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</tbody>
</table>

(continued)
<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Age 16-19</th>
<th>Age 20 or 21</th>
<th>Both Groups</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td>Left school in grade 11 or 12</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,078</td>
<td>61.4%</td>
<td>50.6%</td>
<td>58.6%</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>761</td>
<td>38.6%</td>
<td>49.4%</td>
<td>41.4%</td>
<td></td>
</tr>
<tr>
<td>Received occupational training within past year</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,529</td>
<td>83.4%</td>
<td>82.5%</td>
<td>83.1%</td>
<td>0.662</td>
</tr>
<tr>
<td>Yes</td>
<td>310</td>
<td>16.6%</td>
<td>17.5%</td>
<td>16.9%</td>
<td></td>
</tr>
<tr>
<td>Marital status</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever married</td>
<td>174</td>
<td>7.4%</td>
<td>15.2%</td>
<td>9.5%</td>
<td>0.000</td>
</tr>
<tr>
<td>Never married</td>
<td>1,665</td>
<td>92.6%</td>
<td>84.8%</td>
<td>90.5%</td>
<td></td>
</tr>
<tr>
<td>Living in own household or with boy/girlfriend</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,500</td>
<td>86.3%</td>
<td>68.1%</td>
<td>81.6%</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>339</td>
<td>13.7%</td>
<td>31.9%</td>
<td>18.4%</td>
<td></td>
</tr>
<tr>
<td>Own AFDC case or receiving General Assistance</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,344</td>
<td>78.7%</td>
<td>57.3%</td>
<td>73.1%</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>495</td>
<td>21.3%</td>
<td>42.7%</td>
<td>26.9%</td>
<td></td>
</tr>
<tr>
<td>Own AFDC case</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,446</td>
<td>83.0%</td>
<td>66.2%</td>
<td>78.6%</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>393</td>
<td>17.0%</td>
<td>33.7%</td>
<td>21.4%</td>
<td></td>
</tr>
<tr>
<td>Receiving Food Stamps</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,143</td>
<td>65.0%</td>
<td>54.0%</td>
<td>62.2%</td>
<td>0.000</td>
</tr>
<tr>
<td>Yes</td>
<td>696</td>
<td>35.0%</td>
<td>46.0%</td>
<td>37.8%</td>
<td></td>
</tr>
<tr>
<td>Arrested since age 16</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,567</td>
<td>85.2%</td>
<td>85.2%</td>
<td>85.2%</td>
<td>0.999</td>
</tr>
<tr>
<td>Yes</td>
<td>272</td>
<td>14.8%</td>
<td>14.8%</td>
<td>14.8%</td>
<td></td>
</tr>
<tr>
<td>Lived with both parents at age 14</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>1,198</td>
<td>65.7%</td>
<td>63.5%</td>
<td>65.1%</td>
<td>0.392</td>
</tr>
<tr>
<td>Yes</td>
<td>641</td>
<td>34.3%</td>
<td>36.5%</td>
<td>34.9%</td>
<td></td>
</tr>
<tr>
<td>Sample size</td>
<td>1,839</td>
<td>1,359</td>
<td>480</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
### TABLE C.3 (continued)

<table>
<thead>
<tr>
<th>Characteristic and Subgroups</th>
<th>Sample Size</th>
<th>Age 16-19</th>
<th>Age 20 or 21</th>
<th>Both Groups</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Site</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>61</td>
<td>3.5%</td>
<td>2.9%</td>
<td>3.3%***</td>
<td>0.000</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>8.8%</td>
<td>6.7%</td>
<td>8.3%</td>
<td></td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>74</td>
<td>2.6%</td>
<td>8.1%</td>
<td>3.3%</td>
<td></td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>184</td>
<td>7.4%</td>
<td>17.3%</td>
<td>10.0%</td>
<td></td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>100</td>
<td>5.7%</td>
<td>4.6%</td>
<td>5.4%</td>
<td></td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>183</td>
<td>10.3%</td>
<td>9.0%</td>
<td>10.0%</td>
<td></td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>130</td>
<td>8.2%</td>
<td>3.7%</td>
<td>7.1%</td>
<td></td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>236</td>
<td>12.2%</td>
<td>14.6%</td>
<td>12.8%</td>
<td></td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>155</td>
<td>9.6%</td>
<td>5.2%</td>
<td>8.4%</td>
<td></td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>218</td>
<td>12.4%</td>
<td>10.2%</td>
<td>11.9%</td>
<td></td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>140</td>
<td>7.6%</td>
<td>7.7%</td>
<td>7.6%</td>
<td></td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>119</td>
<td>6.5%</td>
<td>6.5%</td>
<td>6.5%</td>
<td></td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>87</td>
<td>5.2%</td>
<td>3.5%</td>
<td>4.7%</td>
<td></td>
</tr>
</tbody>
</table>

Sample size 1,839 1,359 480

**SOURCE:** MDRC calculations from JOBSTART enrollment form and survey data.

**NOTES:** Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires. Distributions may not total 100.0 percent because of rounding.

The column labeled "p" is the statistical significance level of the difference in distributions of characteristics between groups: that is, p is the probability that observed proportions in each subgroup are different only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
APPENDIX D

SUPPLEMENTAL TABLE TO CHAPTER 3
### TABLE D.1

**Ratings and Special Features of the Implementation of JobStart Components, by Site**

<table>
<thead>
<tr>
<th>Site</th>
<th>Education</th>
<th>Training</th>
<th>Support Services</th>
<th>Job Placement</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Concurrent</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>Rating: Medium</td>
<td>Rating: Medium</td>
<td>Rating: High</td>
<td>Rating: Low</td>
</tr>
<tr>
<td></td>
<td>- self-paced, individualized instruction supplemented by group instruction</td>
<td>- good mix of on- and off-site training</td>
<td>- incentives for participation</td>
<td>- few linkages to employers</td>
</tr>
<tr>
<td></td>
<td>- some employer and union presence</td>
<td>- lacked state-of-the-art equipment</td>
<td>- on-site child care</td>
<td>- little special attention devoted to JOBSTART youths</td>
</tr>
<tr>
<td></td>
<td>- unclear objectives</td>
<td>- some employer and union presence</td>
<td>- strong counseling</td>
<td></td>
</tr>
<tr>
<td></td>
<td>- frequent staff turnover</td>
<td></td>
<td>- health services</td>
<td></td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>Rating: None</td>
<td>Rating: High</td>
<td>Rating: Medium</td>
<td>Rating: High</td>
</tr>
<tr>
<td></td>
<td>- a rating of this component is inappropriate because education and training were more integrated than in other sites, although there was a separate GED preparation</td>
<td>- open entry and exit</td>
<td>- on-site child care; not subsidized, but sliding scale</td>
<td>- good relations with employers</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>- little individualized counseling; most provided by instructors</td>
<td>- job placement a high priority</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>- no incentive payments</td>
<td></td>
</tr>
</tbody>
</table>

(continued)
## TABLE D.1 (continued)

### Ratings and Special Features of Components

<table>
<thead>
<tr>
<th>Site</th>
<th>Education</th>
<th>Training</th>
<th>Support Services</th>
<th>Job Placement</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>• rigorous curricula coordinated with</td>
<td>• training provided in</td>
<td>• relatively small caseloads, but</td>
<td>• staffperson fully dedicated to job development</td>
</tr>
<tr>
<td></td>
<td>vocational requirements, allowing for</td>
<td>word processing and in 4</td>
<td>counselors carried full</td>
<td>• extensive ties to employer community</td>
</tr>
<tr>
<td></td>
<td>immediate application and reinforcement of</td>
<td>semiskilled industrial trades</td>
<td>responsibility for recruitment</td>
<td>• all placements training-related</td>
</tr>
<tr>
<td></td>
<td>learning</td>
<td>• rigorous curricula that</td>
<td>during periods</td>
<td>• given demanding nature of potential places,</td>
</tr>
<tr>
<td></td>
<td>• special tutorial workshops for JOBSTART</td>
<td>incorporated input from employers</td>
<td>• referrals for child care</td>
<td>youths needed more exposure to employment</td>
</tr>
<tr>
<td></td>
<td>participants</td>
<td>• hands-on instruction</td>
<td>• JTPA needs-based payments for</td>
<td>environment prior to job interviews (began visits</td>
</tr>
<tr>
<td></td>
<td>• no computer-assisted instruction</td>
<td>almost daily</td>
<td>transportation and lunch, but checks</td>
<td>to workplaces during later months of the</td>
</tr>
<tr>
<td></td>
<td>• because of vocational focus, GED preparation</td>
<td>• certified instructors recruited from</td>
<td>were frequently late</td>
<td>demonstration's operating period)</td>
</tr>
<tr>
<td></td>
<td>almost precluded</td>
<td>work settings students were preparing to</td>
<td>• adult-oriented</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>enter state-of-the-art equipment</td>
<td>environment</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>• instructors adult-oriented, not always</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>sensitive to youths' developmental needs</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• clear objectives</td>
<td>• multiple training areas</td>
<td>• referrals to other agencies</td>
<td>• informal, individualized job search</td>
</tr>
<tr>
<td></td>
<td>• excellent infrastructure</td>
<td>• combined basic skills and</td>
<td>• on-site child care</td>
<td>• too few job developers to provide focused</td>
</tr>
<tr>
<td></td>
<td>• computer-assisted instruction</td>
<td>vocational training</td>
<td>• needs-based payments and incentives</td>
<td>assistance to JOBSTART youths</td>
</tr>
<tr>
<td></td>
<td>• combined occupational training with basic</td>
<td>• interactive learning</td>
<td>• special JOBSTART counselors</td>
<td>• poor attention to world of work/job-readiness</td>
</tr>
<tr>
<td></td>
<td>education and GED preparation in some cases</td>
<td>• strong employer presence</td>
<td>• some clothing assistance</td>
<td></td>
</tr>
<tr>
<td></td>
<td>• tutorial assistance</td>
<td></td>
<td>• special retention activities</td>
<td></td>
</tr>
<tr>
<td></td>
<td>• multimedia approach</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

(continued)
TABLE D.1 (continued)

Ratings and Special Features of Components

<table>
<thead>
<tr>
<th>Site</th>
<th>Education</th>
<th>Training</th>
<th>Support Services</th>
<th>Job Placement</th>
</tr>
</thead>
<tbody>
<tr>
<td>East LA Skills Center</td>
<td>Rating: Medium</td>
<td>Rating: Medium</td>
<td>Rating: Medium</td>
<td>Rating: Low</td>
</tr>
<tr>
<td></td>
<td>• clear objectives</td>
<td>• good mix of training areas on-site</td>
<td>• counseling provided</td>
<td>• some supervised, individualized job search</td>
</tr>
<tr>
<td></td>
<td>• licensed instructors</td>
<td>• some staff turnover and some uninspired instructors</td>
<td>• no incentives for participation</td>
<td>• one placement staff-person</td>
</tr>
<tr>
<td></td>
<td>• individualized instruction supplemented by group instruction</td>
<td>• combined occupational training with basic education and GED preparation</td>
<td>• limited transportation, meal, or financial assistance</td>
<td>• little attention to JOBSTART youths</td>
</tr>
<tr>
<td></td>
<td>• some effort to combine occupational training and GED preparation</td>
<td>• good relations with employers</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• some use of the computer lab</td>
<td>• crowded classrooms made it difficult for some youths to get the attention of instructors</td>
<td></td>
<td></td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>Rating: Medium</td>
<td>Rating: Medium</td>
<td>Rating: Medium</td>
<td>Rating: Low</td>
</tr>
<tr>
<td></td>
<td>• open entry and exit</td>
<td>• not always open entry and exit</td>
<td>• child care payments</td>
<td>• no specific job development staff</td>
</tr>
<tr>
<td></td>
<td>• self-paced, individualized instruction</td>
<td>• several training programs required more time than participants had available</td>
<td>• free bus passes</td>
<td>• few successful completions of training and transitions to employment</td>
</tr>
<tr>
<td></td>
<td>• JOBSTART-only classes</td>
<td>• training staff used to teaching adults, not always pleased to have JOBSTART participants in class</td>
<td>• trained, caseload-carrying counselors for participants</td>
<td></td>
</tr>
<tr>
<td></td>
<td>• on-site classes</td>
<td>• good attendance</td>
<td>• referrals for other services</td>
<td></td>
</tr>
<tr>
<td></td>
<td>• on-site GED testing with almost immediate reporting of results</td>
<td>• enforcement</td>
<td>• good attendance</td>
<td></td>
</tr>
<tr>
<td></td>
<td>• computer-assisted instruction available, but not widely used</td>
<td>• no incentives to participate except occasional parties</td>
<td>• enforcement</td>
<td></td>
</tr>
</tbody>
</table>

(continued)
<table>
<thead>
<tr>
<th>Site</th>
<th>Education</th>
<th>Training</th>
<th>Support Services</th>
<th>Job Placement</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>· individualized, competency-based instruction</td>
<td>· individualized, competency-based instruction</td>
<td>· financial incentives for attendance and performance</td>
<td>· several staffpersons fully dedicated to job development</td>
</tr>
<tr>
<td></td>
<td>· computer-assisted instruction</td>
<td>· open entry and exit</td>
<td>· structured procedures for reviewing participants' performance</td>
<td>· job development services offered to non-completers in reasonably good standing at program departure</td>
</tr>
<tr>
<td></td>
<td>· open entry and exit</td>
<td>· wide range of courses available</td>
<td>· youth-oriented environment</td>
<td>· many placements not training-related</td>
</tr>
<tr>
<td></td>
<td>· little opportunity for group instruction/interaction</td>
<td>· certified instructors</td>
<td>· relatively large caseloads</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>· little opportunity for group instruction/interaction</td>
<td>· referrals for child care, but limited community resources</td>
<td></td>
</tr>
<tr>
<td></td>
<td>· well-articulated curriculum and weekly plans</td>
<td>· on-site training, but limited to clerical and automotive skills</td>
<td>· needs-based payments for transportation and meals</td>
<td>· responsibility for placement rested with Texas Employment Commission, which did not consider placement of JOBSTART youths a priority</td>
</tr>
<tr>
<td></td>
<td>· used computer-assisted instruction to supplement GED preparation</td>
<td>· lacked state-of-the-art equipment</td>
<td>· referrals for child care to other SER locations or other providers</td>
<td></td>
</tr>
<tr>
<td></td>
<td>· well-articulated attendance and punctuality standards</td>
<td>· difficulty attracting and retaining qualified staff</td>
<td>· incentive payments</td>
<td></td>
</tr>
<tr>
<td></td>
<td>· some effort to integrate vocational instruction with GED preparation</td>
<td>· limited linkages with the private sector</td>
<td>· strong counseling</td>
<td></td>
</tr>
<tr>
<td></td>
<td>· clear performance outcomes were attained</td>
<td>· maintained fairly high student morale and attendance despite shortcomings</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

(continued)
TABLE D.1 (continued)

<table>
<thead>
<tr>
<th>Site</th>
<th>Education</th>
<th>Training</th>
<th>Support Services</th>
<th>Job Placement</th>
</tr>
</thead>
<tbody>
<tr>
<td>El Centro (Dallas)</td>
<td>Rating: High</td>
<td>Rating: Medium</td>
<td>Rating: Medium</td>
<td>Rating: Medium</td>
</tr>
<tr>
<td></td>
<td>• clear objectives</td>
<td>• clear objectives</td>
<td>• needs-based payments</td>
<td>• job placement staff</td>
</tr>
<tr>
<td></td>
<td>• well-designed curriculum</td>
<td>• -7 on-site training areas</td>
<td>• bus passes</td>
<td>• responsible for all El Centro students</td>
</tr>
<tr>
<td></td>
<td>• good mix of individualized and group instruction</td>
<td>• certified instructors with employer contacts</td>
<td>• incentive payments</td>
<td>• limited attention to JOBSTART youths</td>
</tr>
<tr>
<td></td>
<td>• efforts to integrate vocational curriculum</td>
<td>• state-of-the-art equipment in cable TV and clerical training;</td>
<td>• some emergency rent assistance</td>
<td></td>
</tr>
<tr>
<td></td>
<td>• tutorial assistance</td>
<td>• fairly current equipment in other areas</td>
<td>• good counseling</td>
<td></td>
</tr>
<tr>
<td></td>
<td>• reading and learning lab for those needing extra help</td>
<td>• interactive learning</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• flexible program</td>
<td>• good curriculum</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• integration of field trips and current events with GED preparation</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• multimedia approach</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• licensed instructors</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• community college infrastructure</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• clear objectives</td>
<td>• good mix of training areas</td>
<td>• incentives</td>
<td>• good relations with employers</td>
</tr>
<tr>
<td></td>
<td>• stable staff</td>
<td>• -stable staff</td>
<td>• strong counseling</td>
<td>• some supervised job search</td>
</tr>
<tr>
<td></td>
<td>• multimedia approach</td>
<td>• good employer and union presence</td>
<td>• health and other services</td>
<td>• limited attention to JOBSTART youths</td>
</tr>
<tr>
<td></td>
<td>• interactive learning</td>
<td>• state-of-the-art equipment in most areas</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>• on- and off-site linkages</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

(continued)
<table>
<thead>
<tr>
<th>Site</th>
<th>Education</th>
<th>Training</th>
<th>Support Services</th>
<th>Job Placement</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Sequential/brokered</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>• computer-assisted instruction</td>
<td>• difficulty in transition of youths to JTPA and proprietary schools</td>
<td>• counseling</td>
<td>• limited direct placement assistance provided to JOBSTART youths</td>
</tr>
<tr>
<td></td>
<td>• individualized instruction supplemented by group instruction</td>
<td>• no on-site training</td>
<td>• needs-based payments</td>
<td>• reliance on placement staff at training agencies</td>
</tr>
<tr>
<td></td>
<td>• interactive learning</td>
<td></td>
<td>• meals and transportation</td>
<td></td>
</tr>
<tr>
<td></td>
<td>• GED-preparation focus</td>
<td></td>
<td>• referrals for medical, dental, and other services</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>• no incentive payments for participation</td>
<td></td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>Rating: Medium</td>
<td>Rating: Low</td>
<td>Rating: Medium</td>
<td>Rating: Low</td>
</tr>
<tr>
<td></td>
<td>• good mix of academic, job-readiness, and life skills activities</td>
<td>• limited referrals</td>
<td>• strong counseling</td>
<td>• few successful completions of training and transitions to employment</td>
</tr>
<tr>
<td></td>
<td>• computer-assisted instruction</td>
<td>• lack of success linking up with JTPA</td>
<td>• strong relationship between staff and participants</td>
<td>• limited involvement with employers</td>
</tr>
<tr>
<td></td>
<td>• fairly limited GED preparation</td>
<td></td>
<td>• some needs-based payments for a portion of the participants</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>• referrals for other services</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>• incentive payments for part of demonstration</td>
<td></td>
</tr>
<tr>
<td>Site</td>
<td>Education</td>
<td>Training</td>
<td>Support Services</td>
<td>Job Placement</td>
</tr>
<tr>
<td>--------------</td>
<td>-----------</td>
<td>----------</td>
<td>-----------------</td>
<td>---------------</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>Rating: Low</td>
<td>Rating: Low</td>
<td>Rating: Medium</td>
<td>Rating: Low</td>
</tr>
<tr>
<td></td>
<td>- computer-assisted instruction</td>
<td>- limited access to training</td>
<td>- counseling</td>
<td>- limited involvement with training or employer community</td>
</tr>
<tr>
<td></td>
<td>- limited GED preparation</td>
<td>- poor linkages to employers</td>
<td>- bus passes</td>
<td></td>
</tr>
<tr>
<td></td>
<td>- underutilized computer learning center</td>
<td>- few transitions to training</td>
<td>- referrals for other services</td>
<td>- limited attention to JOBSTART youths</td>
</tr>
<tr>
<td></td>
<td>- limited use of group instruction</td>
<td></td>
<td>- no needs-based payments</td>
<td>- informal job search assistance</td>
</tr>
<tr>
<td></td>
<td>- attrition and low attendance</td>
<td></td>
<td>- no incentive payments for participation</td>
<td></td>
</tr>
</tbody>
</table>

SOURCE: Observations of MDRC operations and research staff during the period of program operation in each site.
APPENDIX E

COST OF THE JOBSTART PROGRAM

I. General Approach

This appendix describes the data sources and methodology used to estimate the cost of the JOBSTART program in each of the 13 demonstration sites. It also discusses the factors contributing to the wide variation in costs across sites and examines the relative influence of different JOBSTART components on overall program costs.

The central objective of the analysis was to identify the market value of all resources used in providing JOBSTART services. It therefore counted as program costs not only the expenditures made by the agencies sponsoring the program, but also those made by outside organizations responsible for providing certain components (such as occupational skills training in the three sequential/brokered sites). Furthermore, in sites where goods and services that affected the nature of the program treatment were donated to the sponsoring agency, the analysis estimated the market value of those contributions and counted it, too, as a program cost. For these reasons, the costs presented here may differ from those reflected in a sponsoring agency's own fiscal records.

This appendix does not present estimates of the cost of education and training services received by members of the control group. Thus, it provides no insights into the incremental investment that the JOBSTART sites made for the experimental group. An estimate of these incremental or "net" costs would be an important part of a full benefit-cost analysis, where the value of experimental-control differences in earnings and other outcomes (the "benefits") is compared with the experimental-control differences in the cost of services producing those benefits. As Chapter 4 showed, a substantial proportion of the control group did receive

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1For example, at Chicago Commons, many of the basic supplies essential to operating some of the training courses were donated to the program. The estimated value of these supplies, as reported in the agency's annual audit report, was thus counted as a program cost. As another illustration, the life skills workshops at both Chicago Commons and Connelley in Pittsburgh were conducted free of charge at the program site by outside organizations. These donated services were thus valued and included in the total cost. Their estimated value was based on the number of sessions conducted and by a proxy value of the average cost per session to the agency providing the service.
education and training services during the research follow-up period, which means that the net
cost of JOBSTART in some sites may be considerably smaller than the gross cost reported here.

A. Data Sources and Accounting Periods

Data for the cost analysis were gathered from a variety of sources. These include:

- individual staff salary information;
- site expenditure reports, which showed overall expenditures on salaries and fringe benefits, rent, utilities, supplies, equipment, administration, and so on;
- program enrollment and participation data covering JOBSTART and non-JOBSTART participants, both for the program as a whole and for individual components (such as education classes and training classes);
- JTPA expenditure data in sites where JTPA funds were used to provide program services;
- agency data on support service expenditures covering needs-based payments, transportation, food, child care, and other participant payments;
- interviews with program staff concerning the allocation of staff time across program components and between JOBSTART and non-JOBSTART functions, and other aspects of site operations that affected the use of resources; and
- MDRC's MIS data on the experimental group's degree of participation in
  JOBSTART activities.

In most cases, data from these sources covered a one-year "steady-state" period sometime
between 1985 and 1988 (depending on the site), the years during which JOBSTART was funded.2
However, the actual calendar months of this accounting period varied according to each site's date
of entry into the demonstration and the particular months covered by its annual fiscal reporting
period.3

Ideally a steady-state period should reflect a time during which program operations are
relatively stable. Although it was difficult to define such a period for JOBSTART because of

2The JOBSTART program at Connelley in Pittsburgh and SER/Corpus Christi changed substantially
from the first year of operations to the next. Consequently, cost and participation data for both years were
used.
3In some sites, participation data and expenditure reports did not cover exactly the same time period,
so a number of additional adjustments had to be made in estimating average steady-state expenditures.
the demonstration's relatively short duration, the period selected in most sites began at least several months after the initiation of the project (in order to avoid the start-up costs associated with beginning a new program), and ended at least several months prior to the termination of the demonstration (in order to exclude the phasedown period). To remove the influence of inflation resulting from the use of costs from different calendar periods in different sites, all estimates were inflated or deflated to 1986 dollars.

B. Excluded Costs

In estimating the average cost per JOBSTART experimental, adjustments were made to exclude two categories of expenditures embedded in the sites' fiscal data: (1) research-related costs, and (2) the costs of services or activities that were offered to or used by non-JOBSTART participants. A fraction of program expenditures during the steady-state period resulted exclusively from research requirements. These included the extra costs involved in recruiting and processing individuals who became part of the control group, as well as the costs of staff time spent on conducting random assignment, completing the research enrollment forms, and participating in research-related interviews with MDRC personnel. These activities are not part of the normal effort of operating a JOBSTART program. Thus, the resources spent on them are not counted in the average cost estimates reported here.

Several of the sites also offered a number of services that were not a part of the JOBSTART program but were nonetheless captured in the agencies' aggregate expenditure reports. These, too, had to be excluded from the estimates of JOBSTART costs. This issue was most significant in the three Job Corps sites, where some Corpsmembers lived in dormitories at the centers, while others lived at home while attending Job Corps activities. All of the JOBSTART experimentals were nonresidents in these sites, and were thus unaffected by the services intended exclusively for the residents. These residential-only services included: dormitory provisions, most night-time and weekend recreational activities, and supervision by residential advisors and dormitory attendants. The share of total Job Corps costs associated with exclusively

---

4Because the core education and training services in most sites were already in place prior to JOBSTART and continued after the demonstration, start-up and phasedown costs for the JOBSTART Demonstration were not an issue for these components.

5Although nonresidents were invited to participate in all recreation activities, they did so much less frequently than residential Corpsmembers.
residential aspects of the program was thus estimated and eliminated from the resources counted in determining the cost of JOBSTART.6

C. Calculating the Average Cost per Experimental

In each site, the total average cost of JOBSTART per experimental was determined by summing the average cost of several relatively distinct program components and services. Determining these component costs involved several steps. First, an average unit cost during the steady-state period — that is, the cost of serving one person in the component for a specified unit of time — was calculated. The unit of measure varied for some components, mostly depending on whether the activity operated on an open-entry/open-exit or a fixed-cycle basis.7 Thus, for open-entry components, the average cost of serving one person for one month in the activity was estimated; for fixed-cycle activities, the average cost per person who ever entered a given cycle of the activity was estimated.

The numerator in these unit costs incorporated total expenditures for personnel and overhead functions, including expenses incurred for non-JOBSTART participants in sites where the experimentals were enrolled along with other persons in regular agency activities.8 The value of donated goods and services was also counted as a program expenditure. The denominator includes all participants (both JOBSTART and non-JOBSTART) in the component. Thus, for example, the unit cost of basic education at a site is the full cost of classes in which JOBSTART participants were enrolled, divided by the total number of students in the classes.

The average unit cost was then multiplied by a corresponding participation measure.9 For open-entry components, the unit cost was multiplied by the average number of months in which experimentals spent any hours in the activity (including zero months for experimentals with no

---

6A technical assistance project at BSA in New York City is another example of separate activities whose costs had to be excluded in estimating JOBSTART costs.

7When participants leave open-entry activities, they are typically replaced by other individuals. Thus, the average value of resources expended per person for these components varies with the length of time an average participant receives that service. However, if entry into a training class is based on a fixed cycle and a student who drops out is not replaced by another student, the costs for that student's "slot" are still incurred by the agency on that student's behalf, regardless of his or her length of stay.

8For some activities, sites mainstreamed JOBSTART participants with non-JOBSTART participants. Other activities (for example, counseling, life skills instruction, basic education, or training services that were not normally provided as part of the agency's program) included JOBSTART participants only. See Auspos et al., 1989, for more details on the adaptations the sites made for JOBSTART.

9In order to spread average unit costs among all experimentals and to cover the full period of their involvement in the program, the participation measures captured participation that occurred at any time during the demonstration, not just within the steady-state period.
hours in the activity). For components that operated on a fixed-cycle basis, the unit cost (the cost per person who entered the activity) was multiplied by the proportion of JOBSTART experimentals who ever entered the activity. The values for both types of participation measures were based on the experiences of the experimentals in the "impact sample" (the sample of survey responders used in this report), not of all experimentals who were randomly assigned.

II. Accounting for Site Variations in Average Costs

The average total cost of JOBSTART's core components per experimental varied widely across the sites. (See Table E.1.) Although it fell within $4,500 to $6,500 in most sites, it ranged from less than $2,100 in CET/San Jose, EGOS in Denver, and SER/Corpus Christi to a high of about $7,500 in BSA in New York City. Several factors account for this diversity. Most notably, the sites differed in terms of both the amount of experimentals' participation in JOBSTART and the unit cost of that participation. This can be seen in Table E.1, which presents information for each site on the average number of months that the experimental group participated in program activities and the average monthly cost of participation. Sites where the value of both of these variables was lower than in other sites were among the least expensive JOBSTART programs. For example, this combination of factors helps to explain why CET/San Jose (where experimentals participated for only 4.4 months and the average monthly cost was only $462) had the least expensive JOBSTART program.

In several sites, higher unit costs were somewhat offset by shorter participation, yielding lower total average costs than were observed in some other sites. For example, the average

10This approach (that is, multiplying the average cost of serving one person for one month by the average number of months that JOBSTART experimentals spent in the component) allocates costs between JOBSTART and non-JOBSTART participants on the basis of their respective lengths of stay in the activity.

11This approach assumes that there was no difference in the average cost of serving JOBSTART and non-JOBSTART participants who actually began the component. The costs were considered fixed, whether or not the students stayed until completion. No data were available for comparing the lengths of stay of JOBSTART and non-JOBSTART participants.

12See Chapter 2 for a discussion of the sample used in this report. In general, the experimentals in this sample, on average, had slightly more months with any hours of participation than did all experimentals, and thus may have been slightly more expensive to serve than those not in this sample.

13The average monthly cost for each site — created to facilitate comparisons across sites — was calculated by dividing the average total cost (for experimentals) by the average number of months active in JOBSTART (for experimentals). Although this calculation assumes that the average total cost in all sites was variable, as noted earlier, the costs of fixed-cycle activities were actually calculated on a fixed-cycle basis.
### TABLE E.1

**AVERAGE MONTHLY AND TOTAL COSTS PER EXPERIMENTAL AND PER PARTICIPANT, BY SITE**

<table>
<thead>
<tr>
<th>Site</th>
<th>Percent of Experimentals with Any Hours of Participation</th>
<th>Average Number of Months Active in JOBSTART&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Average Cost per Month Active in JOBSTART</th>
<th>Average Total Cost for Core Components&lt;sup&gt;b&lt;/sup&gt;</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Experiments</td>
<td>Participants</td>
<td>Experiments</td>
<td>Participants</td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>84.80%</td>
<td>4.94</td>
<td>5.82</td>
<td>$845</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>64.00</td>
<td>4.40&lt;sup&gt;c&lt;/sup&gt;</td>
<td>6.88&lt;sup&gt;c&lt;/sup&gt;</td>
<td>462</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>91.90</td>
<td>4.32</td>
<td>4.71</td>
<td>1,499</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>98.90</td>
<td>9.16</td>
<td>9.26</td>
<td>566</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>82.40</td>
<td>5.04</td>
<td>6.12</td>
<td>970</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>93.55</td>
<td>6.86&lt;sup&gt;d&lt;/sup&gt;</td>
<td>7.33&lt;sup&gt;d&lt;/sup&gt;</td>
<td>303</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>86.57</td>
<td>6.25</td>
<td>7.22</td>
<td>793</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>98.30</td>
<td>5.03</td>
<td>5.12</td>
<td>417</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>100.00</td>
<td>5.25</td>
<td>5.25</td>
<td>1,011</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>79.10</td>
<td>7.17</td>
<td>9.05</td>
<td>774</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>100.00</td>
<td>8.88</td>
<td>8.88</td>
<td>660</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>75.38</td>
<td>4.77</td>
<td>6.33</td>
<td>1,569</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>88.89</td>
<td>5.60</td>
<td>6.30</td>
<td>923</td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC calculations from site and MDRC participation, fiscal, and administrative data.

**NOTES:**
- Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate. "Participants" are the subset of experimentals who were active for at least one hour in any JOBSTART component within 24 months of random assignment.
- All costs are in 1986 dollars.
- Unless otherwise stated, the number of months active in JOBSTART is defined as the number of months with hours in any JOBSTART component.
- These estimates were obtained by dividing the average total cost per experimental by the percentage of experimentals with any hours of participation.
- For consistency with the definition of unit costs in this site, the number of months active in JOBSTART is measured from the month of random assignment to the last month with hours in any component.
- For consistency with the definition of unit costs in this site, the number of months active in JOBSTART is measured from the first month with hours in any JOBSTART component to the last month with hours in any component.
monthly cost of JOBSTART at the Atlanta Job Corps was higher than at Connelley in Pittsburgh ($845 compared to $566). However, Atlanta's overall average cost was lower ($4,173 compared to $5,185) because its experimentals were active in the program for less time (4.94 months compared to 9.16 months).

The wide variation in average monthly costs across the sites (ranging from $303 in EGOS in Denver to $1,569 in BSA in New York City) has a number of sources. One is enrollment levels. For example, if the number of participants "on-board" a program in a typical month is high relative to the number of program instructors, the total monthly instructional costs (and the corresponding overhead expenditures) will be spread over many people, lowering the average unit cost per participant. This factor helps to account for the relatively low monthly cost of JOBSTART at EGOS in Denver, a large public vocational school with more than 15,000 students. In contrast, at BSA in New York City, high monthly costs were partly the result of its having enrolled only about half the number of students the school had the capacity to serve at any one time. Staffing decisions can also affect costs. For example, Chicago Commons assigned two instructors to all training classes, an unusual practice among the JOBSTART sites, and this raised its average monthly cost per participant. Differences in wage scales further explain some of the variation in monthly costs. As an illustration, the average hourly wage paid to instructors at SER/Corpus Christi was about half the hourly rate received by teachers at the East Los Angeles Skills Center.

Differences in the scope of activities and services across the sites also account for differences in average monthly costs. For instance, as will be seen below, the three least expensive sites had no life skills or work-readiness instruction, and one of them (CET/San Jose) spent little on support service payments and basic education as a separate activity. Differences in overhead costs, such as those for rent and administration, also varied across the JOBSTART sites.

Table E.1 shows that in some sites a substantial proportion of experimentals left the program after random assignment and so never entered any program component. At the East Los Angeles Skills Center, for example, only 82 percent of the experimental group ever received JOBSTART services. One consequence of such attrition is that a site's average cost per person
actually served by the program is higher than its average cost per experimental. At the Los Angeles Job Corps, it was 26 percent higher ($7,016 compared to $5,550). Although the average cost per experimental would be the appropriate number to include in a benefit-cost analysis for the JOBSTART evaluation, the average cost per participant may be a better guide for administrators interested in the implications for an agency's budget of operating a JOBSTART program.

III. Component Costs

This section discusses how the costs of the individual JOBSTART components contributed to the total average cost at each site and further illustrates the sources of variation in those total costs across the sites.

A. Definitions of Components

For purposes of the cost analysis, JOBSTART functions were divided into eight main components. The category of recruitment, intake, and orientation was defined to encompass sites' efforts to attract and enroll individuals into the JOBSTART program and to prepare them, through special presentations or workshops, for attending the agency's regular education and training classes. This process involved screening applicants to determine whether they met all JOBSTART as well as JTPA or Job Corps eligibility criteria. Random assignment, special data collection, and the additional efforts devoted to recruiting and processing extra individuals to allow the creation of a control group also occurred during the recruitment and intake stages. As previously mentioned, these latter activities were defined as research-only costs, and hence they were not counted in the average cost of this component.

Following orientation, experimentals in all sites were scheduled to attend basic education classes. Occupational skills training classes were offered concurrently or following the completion

\[\text{The average cost per participant was calculated by dividing the average cost per experimental by the percentage of experimentals with any hours of participation.}\]

\[\text{The costs included here for JTPA eligibility determination only cover a site's efforts to help applicants identify and collect the necessary documents and complete the required paperwork as part of the application process. It generally does not include the time JTPA staff spent reviewing those documents and approving the applications.}\]
of basic education. The costs of these two components were estimated separately, although in some sites the line between them was not sharp.\textsuperscript{16}

Several sites also enriched their programs by offering \textit{work-readiness classes or life skills workshops} that covered topics such as work habits, health, and financial management. In addition, the Job Corps sites offered avocational classes in drivers education, sewing, and physical education. These were counted as part of the work-readiness/life skills component for the cost analysis.

\textit{Job placement} was defined to include instruction in job-seeking techniques as well as direct placement efforts. \textit{Coordination and counseling} include staff efforts to monitor participants' attendance and progress in JOBSTART activities and to counsel them on an as-needed basis. In a number of sites where JOBSTART was operated alongside other programs, a special counselor was designated to perform this function exclusively for the experimental group.

\textit{Support services} are defined as the special expenditures intended to help motivate participants to attend program activities regularly, or to help offset some of the potential barriers to attendance. The particular types of support services that were available varied across sites, but included payments for child care, transportation, needs-based payments, food, and attendance and achievement awards.

\textit{Medical and dental services} were an additional component offered in the three Job Corps sites through an on-site clinic. To a much lesser extent, such services were also offered at EGOS in Denver through a formal agreement with an outside agency to which staff routinely referred participants.\textsuperscript{17} These services are not considered to be part of the core JOBSTART model, however. Consequently, their costs have not been included in the total average costs reported above (although they have been estimated).

It should be noted that the information used in allocating total site costs across components (such as the proportion of staff time spent on recruitment and intake versus counseling and

\textsuperscript{16}Data limitations have precluded perfect consistency across all sites in the definition of each component. Especially problematic is the distinction between basic education and skills training at CET/San Jose and Chicago Commons. To a large extent, basic education instruction in those two sites was integrated with occupational skills training. However, those sites also operated separate remedial education classes. The cost analysis counts only participation in those remediation classes as basic education. This definition of basic education is consistent with that used in the calculation of education hours, as reported in Chapter 3.

\textsuperscript{17}Some of the other JOBSTART sites also referred participants to such services at outside agencies, but on a much less formal basis. In these sites, the costs incurred by those agencies were not estimated.
coordination) was often imprecise. As a result, some component cost estimates are much less certain than others. However, this does not affect the estimate of any site's average total cost.

B. Variations in the Cost of JOBSTART Components

Table E.2 shows the estimated average cost per experimental of each JOBSTART component for each site. In addition, for the four sites where medical and dental services were provided, the table shows how the total average cost changes when expenditures for these services are counted. In the three Job Corps sites, these amounted to fairly sizable expenditures — $564 per experimental at Los Angeles, $690 at Atlanta, and $357 at Phoenix. In addition, EGOS in Denver, which, through routine referrals, provided eyeglasses and dental examinations, spent $24 per experimental on medical expenses.

Recruitment, intake, and orientation activities accounted for between 9 and 13 percent of the total average cost of the core JOBSTART components in most of the sites, but reached as high as 19 percent at the East Los Angeles Skills Center. In absolute value, BSA in New York City spent the most on these upfront efforts ($1,313), while SER/Corpus Christi spent the least ($227 per experimental).

Basic education, skills training, and coordination and counseling were usually among the most expensive components to operate across the sites. When taken together, they accounted for no less than 44 percent (the estimate for the Atlanta Job Corps) of the total average cost of the core JOBSTART components, and they reached as high as 86 percent (the estimate for SER/Corpus Christi). In most of the sites, this combination of functions accounted for at least two-thirds of the total average cost.

Particularly notable are the high costs of basic education at the three sequential/brokered sites. At BSA in New York City and CREC in Hartford, basic education alone accounted for half the total cost ($3,836 and $2,634 per experimental, respectively). The cost of basic education was also high at Chicago Commons ($1,400), which added a separate education class specifically for JOBSTART participants, and at El Centro in Dallas ($1,301) and the East Los Angeles Skills Center ($1,114). Overall, the percentage of total average costs devoted strictly to basic education ranged from 4 percent at CET/San Jose to 51 percent at BSA and CREC. Education costs were especially low at CET/San Jose ($88 per experimental) in part because most of the hours that experimentals spent in that site were spent in training classes, which, it should be recalled, also
<table>
<thead>
<tr>
<th>Component</th>
<th>Concurrent</th>
<th>Sequential/In-House</th>
<th>Sequential/Broke d</th>
</tr>
</thead>
<tbody>
<tr>
<td>Recruitment, intake, and orientation</td>
<td>$602$ $245$ $327$ $445$ $923$ $245$ $645$ $227$</td>
<td>$568$ $568$</td>
<td>$328$ $1,313$ $689$</td>
</tr>
<tr>
<td>Basic education</td>
<td>529 $88$ $1,400$ $644$ $1,114$ $384$ $939$ $632$</td>
<td>1,301 $648$</td>
<td>1,147 $3,836$ $2,634$</td>
</tr>
<tr>
<td>Occupational skills training</td>
<td>283 $1,031$ $2,931$ $793$ $1,531$ $297$ $1,446$ $533$</td>
<td>1,175 $1,478$</td>
<td>529 $453$ $332$</td>
</tr>
<tr>
<td>Work-readiness or life skills</td>
<td>839 $n/a$ $35$ $283$ $n/a$ $343$ $n/a$</td>
<td>392 $442$</td>
<td>1,438 $920$ $n/a$</td>
</tr>
<tr>
<td>Job development and placement assistance</td>
<td>92 $308$ $262$ $334$ $36$ $19$ $188$ $73$</td>
<td>639 $302$</td>
<td>628 $n/a$ $n/a$</td>
</tr>
<tr>
<td>Counseling and program coordination</td>
<td>1,031 $301$ $1,159$ $2,239$ $1,196$ $664$ $498$ $633$</td>
<td>719 $705$</td>
<td>1,058 $757$ $1,279$</td>
</tr>
<tr>
<td>Support services°</td>
<td>797 $61$ $363$ $447$ $87$ $467$ $897$ $593^b$</td>
<td>512 $1,389$</td>
<td>734 $205$ $232$</td>
</tr>
<tr>
<td>Subtotal for core JOBSTART components</td>
<td>$4,173$ $2,034$ $6,477$ $5,185$ $4,887$ $2,076$ $4,956$ $2,098$</td>
<td>$5,306$ $5,550$</td>
<td>$5,862$ $7,484$ $5,166$</td>
</tr>
<tr>
<td>Medical/dental</td>
<td>$690$ $n/a$ $n/a$ $n/a$ $n/a$ $24$ $357$ $n/a$</td>
<td>$n/a$ $564$</td>
<td>$n/a$ $n/a$ $n/a$</td>
</tr>
<tr>
<td>Total</td>
<td>$4,863$ $2,034$ $6,477$ $5,185$ $4,887$ $2,100$ $5,313$ $2,098$</td>
<td>$5,306$ $6,114$</td>
<td>$5,862$ $7,484$ $5,166$</td>
</tr>
</tbody>
</table>

**SOURCE:** MORC calculations from site and MDRC participation, fiscal, and administrative data.

**NOTES:** Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate.

All costs are in 1986 dollars.° Includes such costs as needs-based and incentive payments; transportation, child care, and clothing allowances; and food.

Because of data limitations, the support services cost for JOBSTART and non-JOBSTART participants in this site could not be separated from other expenditures in the general overhead rate used in estimating the cost of the other program components. Thus, the per-experimental cost of each component includes the cost for support services. To avoid double-counting this expenditure in the average total cost per experimental, the $593 estimated value of support services, which was calculated from individual-level data available only for JOBSTART youths, is not included in the sum of component costs.

These services were routinely available in and measured for the Job Corps sites and EGOS only.
included some work on basic education skills. (Basic education that occurred in the context of occupational skills training was counted as skills training.)

The resources spent on skills training also varied widely across the sites. Not surprisingly, as a proportion of total average costs, expenditures for this component were lowest at the sequential/brokered sites (accounting for less than 10 percent of those costs), where only about one-quarter of the experimentals made the transition to a training activity. In the other sites, this component accounted for between 7 percent and 51 percent of total costs. In absolute value, it was most expensive at Chicago Commons ($2,931), where the training classes were small and operated on a fixed-cycle basis (dropouts within a cycle were usually not replaced with other students). The classes also involved fairly technical instruction and used significant amounts of purchased and donated supplies.

Across all sites, the per-experimental cost of coordination and counseling ranged from $301 at CET/San Jose to $2,239 at Connelley in Pittsburgh, where it accounted for an unusually high 43 percent of the total average cost per experimental. In contrast, job search assistance and placement in most sites accounted for no more than 6 percent of total average costs, although it ranged from $19 per experimental at EGOS in Denver to $639 at El Centro in Dallas. In most sites, this tended to be a relatively inexpensive component because relatively few staff members were usually assigned to instruct or assist many different participants. (In two of the three sequential/brokered sites, job search assistance was not offered by the sponsoring agencies but, instead, was a function expected to be performed by the outside training vendors.)

The sites also varied widely in their expenditures on work-readiness and life skills instruction. Indeed, some sites did not offer these activities at all, while others gave them considerable emphasis. Allentown in Buffalo, for example, spent more per experimental on life skills than it did on education ($1,438, or 25 percent of total average costs).

Expenditures on support service costs ranged from $61 per experimental at CET/San Jose to $1,389 (or 25 percent of the total average cost) at the Los Angeles Job Corps. The other Job Corps sites, as well as SER/Corpus Christi, also devoted a relatively high proportion of resources to these services — about one-fifth to more than one-fourth of total average costs.

Table E.3 shows the breakdown of support services costs for child care, transportation, food, needs-based payments, and other purposes (which included incentives for attendance and
TABLE E.3

AVERAGE JOBSTART SUPPORT SERVICES AND PARTICIPANT PAYMENT COSTS PER EXPERIMENTAL, BY SITE

<table>
<thead>
<tr>
<th>Support Service</th>
<th>Concurrent</th>
<th>Sequential/In-House</th>
<th>Sequential/Brokered</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Atlanta Job Corps</td>
<td>CET/ San Jose</td>
<td>Chicago Commons (Pittsburgh)</td>
</tr>
<tr>
<td>Child care</td>
<td>n/a</td>
<td>n/a</td>
<td>n/a</td>
</tr>
<tr>
<td>Transportation</td>
<td>n/a</td>
<td>n/a</td>
<td>n/a</td>
</tr>
<tr>
<td>Food</td>
<td>$153</td>
<td>n/a</td>
<td>n/a</td>
</tr>
<tr>
<td>Needs-based and incentive payments,</td>
<td>644</td>
<td>$61</td>
<td>$363</td>
</tr>
<tr>
<td>clothing allowances, and miscellaneous</td>
<td></td>
<td>$61</td>
<td>$363</td>
</tr>
<tr>
<td>Total</td>
<td>797</td>
<td>61</td>
<td>363</td>
</tr>
</tbody>
</table>

SOURCE: MDRC calculations from site and MDRC participation, fiscal, and administrative data.

NOTES: Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate.

All costs are in 1986 dollars.
achievement). Almost all the sites offered needs-based payments, while about half of them paid for child care and transportation. Food costs were substantial in the three Job Corps sites (ranging from $153 to $259 per experimental), where the on-site cafeterias offered regular meals to both residential and nonresidential participants.

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18 All Job Corps participants (including those in JOBSTART) received a $40 monthly living allowance, which increased to $60 after they were active for 61 ("good") days, and increased again to $80 after they were active for 181 days. After that, participants were eligible to receive a merit pay level of $90 to $100 per month. In addition, upon termination from the Job Corps, those who remained in the program for a specified length of time received a separate "readjustment allowance" for each month of participation. This allowance ranged from $75 to $100 per month depending on the number of days they were active. Participants could have a portion of this allowance sent to a dependent family member. If they made that choice, the Job Corps contributed an equal amount to the family member.
APPENDIX F

SUPPLEMENTAL TABLES TO CHAPTERS 4 AND 5
<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p^6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Received education or training</td>
<td>68.6%</td>
<td>5.4%</td>
<td>63.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 1</td>
<td>68.6%</td>
<td>5.4%</td>
<td>63.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 2</td>
<td>82.6%</td>
<td>8.0%</td>
<td>74.6***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 3</td>
<td>75.4%</td>
<td>10.0%</td>
<td>65.4***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 4</td>
<td>67.5%</td>
<td>11.1%</td>
<td>56.3***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 5</td>
<td>60.7%</td>
<td>11.7%</td>
<td>49.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 6</td>
<td>52.4%</td>
<td>12.1%</td>
<td>40.4***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 7</td>
<td>43.5%</td>
<td>13.5%</td>
<td>30.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 8</td>
<td>35.3%</td>
<td>12.7%</td>
<td>22.6***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 9</td>
<td>32.0%</td>
<td>13.8%</td>
<td>18.2***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 10</td>
<td>28.6%</td>
<td>14.6%</td>
<td>14.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 11</td>
<td>24.5%</td>
<td>14.6%</td>
<td>9.9***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 12</td>
<td>19.3%</td>
<td>12.5%</td>
<td>6.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 13</td>
<td>21.3%</td>
<td>15.6%</td>
<td>5.7***</td>
<td>0.001</td>
</tr>
<tr>
<td>Month 14</td>
<td>17.2%</td>
<td>12.0%</td>
<td>5.2***</td>
<td>0.001</td>
</tr>
<tr>
<td>Month 15</td>
<td>16.6%</td>
<td>11.1%</td>
<td>5.6***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 16</td>
<td>15.5%</td>
<td>10.9%</td>
<td>4.6***</td>
<td>0.003</td>
</tr>
<tr>
<td>Month 17</td>
<td>14.8%</td>
<td>11.3%</td>
<td>3.6**</td>
<td>0.022</td>
</tr>
<tr>
<td>Month 18</td>
<td>13.8%</td>
<td>11.1%</td>
<td>2.7*</td>
<td>0.075</td>
</tr>
<tr>
<td>Month 19</td>
<td>13.2%</td>
<td>11.1%</td>
<td>2.0</td>
<td>0.183</td>
</tr>
<tr>
<td>Month 20</td>
<td>12.1%</td>
<td>12.2%</td>
<td>-0.1</td>
<td>0.968</td>
</tr>
<tr>
<td>Month 21</td>
<td>12.3%</td>
<td>12.6%</td>
<td>-0.2</td>
<td>0.881</td>
</tr>
<tr>
<td>Month 22</td>
<td>11.7%</td>
<td>12.8%</td>
<td>-1.1</td>
<td>0.473</td>
</tr>
<tr>
<td>Month 23</td>
<td>11.3%</td>
<td>11.3%</td>
<td>0.0</td>
<td>0.982</td>
</tr>
<tr>
<td>Month 24</td>
<td>11.2%</td>
<td>10.3%</td>
<td>0.8</td>
<td>0.559</td>
</tr>
</tbody>
</table>

Sample size

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table 8.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
# TABLE F.2

**IMPACTS ON MONTHLY AVERAGE HOURS OF EDUCATION OR TRAINING RECEIVED**

<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hours of education or training received</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month 1</td>
<td>29.44</td>
<td>2.51</td>
<td>26.93***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 2</td>
<td>69.84</td>
<td>6.41</td>
<td>63.43***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 3</td>
<td>63.16</td>
<td>8.57</td>
<td>54.59***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 4</td>
<td>52.57</td>
<td>10.41</td>
<td>42.16***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 5</td>
<td>46.26</td>
<td>11.06</td>
<td>35.20***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 6</td>
<td>39.43</td>
<td>11.99</td>
<td>27.44***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 7</td>
<td>31.59</td>
<td>12.91</td>
<td>18.68***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 8</td>
<td>27.02</td>
<td>12.92</td>
<td>14.10***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 9</td>
<td>24.79</td>
<td>14.41</td>
<td>10.38***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 10</td>
<td>21.97</td>
<td>14.15</td>
<td>7.82***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 11</td>
<td>17.35</td>
<td>12.91</td>
<td>4.43**</td>
<td>0.012</td>
</tr>
<tr>
<td>Month 12</td>
<td>15.60</td>
<td>11.79</td>
<td>3.81**</td>
<td>0.031</td>
</tr>
<tr>
<td>Month 13</td>
<td>16.60</td>
<td>10.78</td>
<td>5.82***</td>
<td>0.001</td>
</tr>
<tr>
<td>Month 14</td>
<td>18.66</td>
<td>10.50</td>
<td>8.16***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 15</td>
<td>18.42</td>
<td>10.00</td>
<td>8.41***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 16</td>
<td>17.86</td>
<td>9.71</td>
<td>8.15**</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 17</td>
<td>15.72</td>
<td>9.77</td>
<td>5.95***</td>
<td>0.001</td>
</tr>
<tr>
<td>Month 18</td>
<td>15.40</td>
<td>9.38</td>
<td>6.01***</td>
<td>0.001</td>
</tr>
<tr>
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<td>3.73**</td>
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</tr>
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</tr>
<tr>
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<td>10.54</td>
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<tr>
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<td>10.02</td>
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<tr>
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</table>

**Sample size**

| (949) | (890) |

**SOURCE:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate. For experimentals, "hours of education or training" include JOBSTART hours from MIS data and non-JOBSTART hours from survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
## TABLE F.3
### IMPACTS ON MONTHLY RECEIPT OF BASIC EDUCATION

<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experimental</th>
<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td>Received basic education</td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Month 1</td>
<td>47.8%</td>
<td>3.1%</td>
<td>44.7***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 2</td>
<td>78.9</td>
<td>3.7</td>
<td>75.2***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 3</td>
<td>70.3</td>
<td>4.9</td>
<td>65.5***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 4</td>
<td>59.1</td>
<td>5.5</td>
<td>53.6***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 5</td>
<td>50.3</td>
<td>6.5</td>
<td>43.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 6</td>
<td>36.1</td>
<td>7.3</td>
<td>28.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 7</td>
<td>29.0</td>
<td>7.9</td>
<td>21.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 8</td>
<td>23.0</td>
<td>8.0</td>
<td>15.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 9</td>
<td>18.7</td>
<td>8.6</td>
<td>10.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 10</td>
<td>15.5</td>
<td>8.6</td>
<td>6.9***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 11</td>
<td>14.4</td>
<td>8.3</td>
<td>6.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 12</td>
<td>11.3</td>
<td>7.2</td>
<td>4.1***</td>
<td>0.002</td>
</tr>
<tr>
<td>Month 13</td>
<td>13.9</td>
<td>10.3</td>
<td>3.6**</td>
<td>0.017</td>
</tr>
<tr>
<td>Month 14</td>
<td>12.3</td>
<td>8.4</td>
<td>3.9***</td>
<td>0.006</td>
</tr>
<tr>
<td>Month 15</td>
<td>11.5</td>
<td>7.1</td>
<td>4.4***</td>
<td>0.001</td>
</tr>
<tr>
<td>Month 16</td>
<td>10.8</td>
<td>6.8</td>
<td>4.0***</td>
<td>0.003</td>
</tr>
<tr>
<td>Month 17</td>
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<td>7.1</td>
<td>3.1**</td>
<td>0.017</td>
</tr>
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<td>7.8</td>
<td>-0.1</td>
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</tr>
<tr>
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<td>8.1</td>
<td>-0.7</td>
<td>0.570</td>
</tr>
<tr>
<td>Month 23</td>
<td>7.6</td>
<td>7.0</td>
<td>0.6</td>
<td>0.652</td>
</tr>
<tr>
<td>Month 24</td>
<td>7.4</td>
<td>6.3</td>
<td>1.1</td>
<td>0.352</td>
</tr>
</tbody>
</table>

Sample size: 949 890

**SOURCE:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Basic education" includes JOBSTART and non-JOBSTART education activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

^aThe column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
### TABLE F.4

**IMPACTS ON MONTHLY RECEIPT OF TRAINING**

<table>
<thead>
<tr>
<th>Outcome and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Received training</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month 1</td>
<td>30.7%</td>
<td>3.8%</td>
<td>26.9***</td>
<td>0.000</td>
</tr>
<tr>
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<td>47.4</td>
<td>6.0</td>
<td>41.4***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 3</td>
<td>47.8</td>
<td>7.6</td>
<td>40.2***</td>
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</tr>
<tr>
<td>Month 4</td>
<td>45.7</td>
<td>8.8</td>
<td>37.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 5</td>
<td>44.2</td>
<td>9.2</td>
<td>35.1***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 6</td>
<td>41.7</td>
<td>9.3</td>
<td>32.4***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 7</td>
<td>34.6</td>
<td>10.3</td>
<td>24.3***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 8</td>
<td>28.8</td>
<td>9.8</td>
<td>19.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 9</td>
<td>26.4</td>
<td>10.7</td>
<td>15.7***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 10</td>
<td>24.1</td>
<td>11.1</td>
<td>13.0***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 11</td>
<td>18.3</td>
<td>11.5</td>
<td>6.8***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 12</td>
<td>15.5</td>
<td>10.0</td>
<td>5.5***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 13</td>
<td>16.5</td>
<td>12.2</td>
<td>4.6***</td>
<td>0.005</td>
</tr>
<tr>
<td>Month 14</td>
<td>12.6</td>
<td>8.1</td>
<td>4.5***</td>
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<tr>
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<td>7.8</td>
<td>4.4***</td>
<td>0.002</td>
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<td>3.1**</td>
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<td>2.3*</td>
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<td>8.2</td>
<td>1.6</td>
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</tr>
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**Sample size**

<table>
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<th>Experimentals</th>
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</tr>
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<tbody>
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<td>890</td>
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</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Training" includes JOBSTART and non-JOBSTART occupational skills training activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table 8.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month 1</td>
<td>16.5</td>
<td>27.7</td>
<td>-11.2***</td>
<td>0.000</td>
</tr>
<tr>
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<td>32.7</td>
<td>-11.4***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 3</td>
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<td>36.9</td>
<td>-12.9***</td>
<td>0.000</td>
</tr>
<tr>
<td>Month 4</td>
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<td>-13.3***</td>
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</tr>
<tr>
<td>Month 5</td>
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<td>44.2</td>
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<tr>
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<td>45.7</td>
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<td>-3.2</td>
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<td>2.0</td>
<td>0.560</td>
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<tr>
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<td>1.1</td>
<td>0.742</td>
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<td>-2.1</td>
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<td>-3.0</td>
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<td>-0.3</td>
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</tr>
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<td>57.5</td>
<td>-0.4</td>
<td>0.913</td>
</tr>
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<td>-2.7</td>
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</tr>
<tr>
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<td>62.9</td>
<td>-3.2</td>
<td>0.339</td>
</tr>
<tr>
<td>Sample size</td>
<td>438</td>
<td>433</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| **Women living with own child(ren)**     |               |          |            |         |
| Ever employed (%)                        |               |          |            |         |
| Month 1                                  | 6.1           | 8.4      | -2.3       | 0.340   |
| Month 2                                  | 5.9           | 11.7     | -5.8**     | 0.026   |
| Month 3                                  | 7.9           | 13.4     | -5.5*      | 0.054   |
| Month 4                                  | 9.6           | 15.4     | -5.8*      | 0.058   |
| Month 5                                  | 12.2          | 16.0     | -3.8       | 0.232   |
| Month 6                                  | 15.6          | 17.0     | -1.4       | 0.678   |
| Month 7                                  | 18.2          | 19.0     | -0.8       | 0.827   |
| Month 8                                  | 22.2          | 19.9     | 2.3        | 0.533   |
| Month 9                                  | 24.6          | 22.4     | 2.2        | 0.574   |
| Month 10                                 | 26.1          | 23.0     | 3.2        | 0.423   |
| Month 11                                 | 24.9          | 22.9     | 2.0        | 0.609   |
| Month 12                                 | 25.6          | 23.5     | 2.0        | 0.611   |
| Month 13                                 | 30.8          | 23.5     | 7.4*       | 0.073   |
| Month 14                                 | 24.8          | 18.4     | 6.3*       | 0.096   |
| Month 15                                 | 24.7          | 22.3     | 2.4        | 0.526   |
| Month 16                                 | 25.9          | 21.1     | 4.8        | 0.210   |
| Month 17                                 | 26.1          | 22.9     | 3.2        | 0.414   |
| Month 18                                 | 26.2          | 23.3     | 2.9        | 0.460   |
| Month 19                                 | 26.9          | 24.3     | 2.6        | 0.525   |
| Month 20                                 | 25.6          | 24.8     | 0.8        | 0.846   |
| Month 21                                 | 23.5          | 25.7     | -2.2       | 0.583   |
| Month 22                                 | 23.1          | 25.8     | -2.7       | 0.498   |
| Month 23                                 | 23.3          | 25.1     | -1.8       | 0.657   |
| Month 24                                 | 23.0          | 24.1     | -1.1       | 0.783   |
| Sample size                              | 250           | 234      |            |         |

(continued)
TABLE F.5 (continued)

<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Women not living with own child(ren), including those who did not have any</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever employed (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month 1</td>
<td>12.8</td>
<td>20.0</td>
<td>-7.3**</td>
<td>0.031</td>
</tr>
<tr>
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<td>23.6</td>
<td>-6.7*</td>
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<td>-6.9**</td>
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</tr>
<tr>
<td>Month 4</td>
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<td>27.0</td>
<td>-9.4**</td>
<td>0.011</td>
</tr>
<tr>
<td>Month 5</td>
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<td>32.1</td>
<td>-9.8**</td>
<td>0.014</td>
</tr>
<tr>
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<td>24.3</td>
<td>34.4</td>
<td>-10.1**</td>
<td>0.014</td>
</tr>
<tr>
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<td>28.8</td>
<td>36.3</td>
<td>-7.5*</td>
<td>0.080</td>
</tr>
<tr>
<td>Month 8</td>
<td>30.4</td>
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Sample size 261 223

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

<sup>a</sup>The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as ** = 1 percent; *** = 5 percent; * = 10 percent.
# Table F.6

## Impacts on Monthly Rates of Positive Activity, by Gender and Parental Status

<table>
<thead>
<tr>
<th>Subgroup, Outcome, and Follow-Up Period</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Difference</th>
<th>p&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Employed or in education or training (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>29.4</td>
<td>46.3***</td>
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<tr>
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<td>53.7***</td>
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<tr>
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<td>45.5***</td>
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<tr>
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<td>45.0</td>
<td>35.2***</td>
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</tr>
<tr>
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<td>49.4</td>
<td>27.2***</td>
<td>0.000</td>
</tr>
<tr>
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<td>21.7***</td>
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</tr>
<tr>
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<td>51.2</td>
<td>16.4***</td>
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</tr>
<tr>
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<td>52.0</td>
<td>9.9***</td>
<td>0.003</td>
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<td>6.8**</td>
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**Sample size:** 438  433

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**Sample size:** 250  234

(continued)
TABLE F.6 (continued)

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<th>Controls</th>
<th>Difference</th>
<th>p^a</th>
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<td>6.9</td>
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<td>9.0*</td>
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**Sample size**

| 261 | 223 |

**SOURCE:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Positive activity" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities, as well as employment.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

^aThe column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
APPENDIX G

SUPPLEMENTAL TABLES TO CHAPTER 6
## TABLE G.1

**EXPERIMENTAL-CONTROL DIFFERENCE IN SERVICE RECEIPT, BY SITE**

<table>
<thead>
<tr>
<th>Site</th>
<th>Sample Size</th>
<th>Ever Received Any Education or Training, Months 1-24</th>
<th>Difference</th>
<th>p²</th>
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<td></td>
<td></td>
<td></td>
</tr>
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<td>61</td>
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<td>47.6%</td>
<td>42.8***</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>152</td>
<td>71.7%</td>
<td>24.0%</td>
<td>47.7***</td>
</tr>
<tr>
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<td>74</td>
<td>96.3%</td>
<td>60.7%</td>
<td>35.6***</td>
</tr>
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</tr>
<tr>
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<td>57.8%</td>
<td>35.1***</td>
</tr>
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<td>50.0%</td>
<td>45.4***</td>
</tr>
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<td>130</td>
<td>90.3%</td>
<td>38.6%</td>
<td>51.7***</td>
</tr>
<tr>
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<td>236</td>
<td>101.2%</td>
<td>34.8%</td>
<td>66.4***</td>
</tr>
<tr>
<td>Sequential/in-house</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
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<td>71.9***</td>
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<td>Sequential/brokered</td>
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<td></td>
<td></td>
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<td>1,839</td>
<td>92.7%</td>
<td>44.2%</td>
<td>48.4***</td>
</tr>
</tbody>
</table>

**SOURCE:** MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

**NOTES:** Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). "All sites" outcomes are from a linear analysis of covariance procedure for the full sample controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
TABLE G.2
IMPACTION ON YEAR-TWO EMPLOYMENT RATES AND EARNINGS FOR MEN,
BY SITE

<table>
<thead>
<tr>
<th>Site or Statistic</th>
<th>Sample Size</th>
<th>Sample</th>
<th>Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Experiments</td>
<td>Controls</td>
</tr>
<tr>
<td><strong>Ever Employed, Months 13-24</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>25</td>
<td>93.3%</td>
<td>104.0%</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>77</td>
<td>88.5%</td>
<td>87.5%</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>42</td>
<td>96.4%</td>
<td>88.6%</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>86</td>
<td>89.3%</td>
<td>91.1%</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>56</td>
<td>81.1%</td>
<td>90.5%</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>65</td>
<td>75.6%</td>
<td>82.9%</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>63</td>
<td>80.7%</td>
<td>88.3%</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>143</td>
<td>76.3%</td>
<td>75.7%</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>71</td>
<td>92.9%</td>
<td>79.0%</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>88</td>
<td>71.3%</td>
<td>76.9%</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
<td>58</td>
<td>83.5%</td>
<td>86.0%</td>
</tr>
<tr>
<td>BSA (NYC)</td>
<td>66</td>
<td>89.6%</td>
<td>88.8%</td>
</tr>
<tr>
<td>CREC (Hartford)</td>
<td>31</td>
<td>107.1%</td>
<td>77.4%</td>
</tr>
<tr>
<td><strong>P-value of F-statistic</strong></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
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<td>$5,138.56</td>
<td>$9,153.12</td>
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<td>CET/San Jose</td>
<td>77</td>
<td>8,028.96</td>
<td>7,475.22</td>
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<td>Chicago Commons</td>
<td>42</td>
<td>6,894.20</td>
<td>6,273.93</td>
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<td>2,667.97</td>
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<td>8,340.70</td>
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<td>4,915.83</td>
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<td>4,847.08</td>
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<td>SER/Corpus Christi</td>
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<td>3,421.01</td>
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<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>71</td>
<td>4,714.69</td>
<td>5,289.66</td>
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<tr>
<td>LA Job Corps</td>
<td>88</td>
<td>4,570.40</td>
<td>7,537.41</td>
</tr>
<tr>
<td>Sequential/brokered</td>
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<td></td>
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<td>Allentown (Buffalo)</td>
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<td>5,198.10</td>
<td>4,480.79</td>
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<td>BSA (NYC)</td>
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<td>8,154.38</td>
<td>8,880.16</td>
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<td>8,844.29</td>
<td>6,040.01</td>
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<tr>
<td><strong>P-value of F-statistic</strong></td>
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<td></td>
</tr>
</tbody>
</table>

(continued)
SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take effect on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

At a two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
<table>
<thead>
<tr>
<th>Site or Statistic</th>
<th>Sample Size</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Impact</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Ever Employed, Months 13-24</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>18</td>
<td>39.5%</td>
<td>49.1%</td>
<td>-9.6</td>
<td>0.689</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>10</td>
<td>86.1</td>
<td>36.4</td>
<td>49.7</td>
<td>0.125</td>
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<tr>
<td>Chicago Commons</td>
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<td>60.9</td>
<td>46.8</td>
<td>14.1</td>
<td>0.553</td>
</tr>
<tr>
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<td>53.3</td>
<td>31.6</td>
<td>21.7*</td>
<td>0.084</td>
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<td>53.0</td>
<td>4.4</td>
<td>48.7</td>
<td>0.138</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>67</td>
<td>46.0</td>
<td>37.8</td>
<td>8.2</td>
<td>0.500</td>
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<td>43.6</td>
<td>2.7</td>
<td>0.868</td>
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<tr>
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<td>58.1</td>
<td>69.7</td>
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<td>0.394</td>
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</tr>
<tr>
<td>El Centro (Dallas)</td>
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<td>61.7</td>
<td>49.4</td>
<td>12.3</td>
<td>0.393</td>
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<tr>
<td>LA Job Corps</td>
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<td>54.8</td>
<td>44.9</td>
<td>9.9</td>
<td>0.422</td>
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</tr>
<tr>
<td>Allentown (Buffalo)</td>
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<td>56.2</td>
<td>62.6</td>
<td>-6.4</td>
<td>0.661</td>
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<tr>
<td>BSA (NYC)</td>
<td>19</td>
<td>44.3</td>
<td>58.5</td>
<td>-14.2</td>
<td>0.536</td>
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<tr>
<td>CREC (Hartford)</td>
<td>24</td>
<td>40.9</td>
<td>25.8</td>
<td>15.1</td>
<td>0.457</td>
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<td><strong>P-value of F-statistic</strong></td>
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<tr>
<td>Concurrent</td>
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</tr>
<tr>
<td>Atlanta Job Corps</td>
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<td>$1,863.38</td>
<td>$1,452.36</td>
<td>411.02</td>
<td>0.813</td>
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<td>CET/San Jose</td>
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<td>336.04</td>
<td>0.886</td>
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<td>Chicago Commons</td>
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<td>2,074.73</td>
<td>-72.26</td>
<td>0.966</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
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<td>755.92</td>
<td>459.09</td>
<td>296.83</td>
<td>0.743</td>
</tr>
<tr>
<td>East LA Skills Center</td>
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<td>2,521.82</td>
<td>305.39</td>
<td>2,216.44</td>
<td>0.349</td>
</tr>
<tr>
<td>EGOS (Denver)</td>
<td>67</td>
<td>1,903.33</td>
<td>1,973.14</td>
<td>-69.82</td>
<td>0.936</td>
</tr>
<tr>
<td>Phoenix Job Corps</td>
<td>37</td>
<td>1,208.87</td>
<td>1,969.39</td>
<td>-760.51</td>
<td>0.520</td>
</tr>
<tr>
<td>SER/Corpus Christi</td>
<td>53</td>
<td>1,575.21</td>
<td>2,324.46</td>
<td>-749.25</td>
<td>0.446</td>
</tr>
<tr>
<td>Sequential/in-house</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>El Centro (Dallas)</td>
<td>51</td>
<td>4,349.84</td>
<td>1,959.85</td>
<td>2,389.99**</td>
<td>0.022</td>
</tr>
<tr>
<td>LA Job Corps</td>
<td>66</td>
<td>2,450.62</td>
<td>1,713.48</td>
<td>737.13</td>
<td>0.404</td>
</tr>
<tr>
<td>Sequential/brokered</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Allentown (Buffalo)</td>
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<td>2,162.68</td>
<td>2,379.27</td>
<td>-216.59</td>
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</tr>
<tr>
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<td>-3,083.73*</td>
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<td>CREC (Hartford)</td>
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<td>508.47</td>
<td>0.729</td>
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<td><strong>P-value of F-statistic</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.492</td>
</tr>
</tbody>
</table>

(continued)
SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
TABLE G.4
IMPACTS ON YEAR-TWO EMPLOYMENT RATES AND EARNINGS
FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN)
AT THE TIME OF RANDOM ASSIGNMENT, BY SITE

<table>
<thead>
<tr>
<th>Site or Statistic</th>
<th>Sample Size</th>
<th>Experimentals</th>
<th>Controls</th>
<th>Impact</th>
<th>p^a</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Concurrent</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>18</td>
<td>77.6%</td>
<td>70.9%</td>
<td>6.7</td>
<td>0.768</td>
</tr>
<tr>
<td>CET/San Jose</td>
<td>65</td>
<td>79.8</td>
<td>71.2</td>
<td>8.6</td>
<td>0.457</td>
</tr>
<tr>
<td>Chicago Commons</td>
<td>14</td>
<td>88.4</td>
<td>74.3</td>
<td>14.2</td>
<td>0.588</td>
</tr>
<tr>
<td>Connelley (Pittsburgh)</td>
<td>35</td>
<td>69.0</td>
<td>39.9</td>
<td>29.1*</td>
<td>0.068</td>
</tr>
<tr>
<td>East LA Skills Center</td>
<td>32</td>
<td>67.8</td>
<td>50.1</td>
<td>17.7</td>
<td>0.294</td>
</tr>
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<td>EGOS (Denver)</td>
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<td>62.7</td>
<td>66.8</td>
<td>-4.2</td>
<td>0.748</td>
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<tr>
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<td>58.7</td>
<td>86.0</td>
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<tr>
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<td>58.9</td>
<td>61.3</td>
<td>-2.3</td>
<td>0.875</td>
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</tr>
<tr>
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<td>69.8</td>
<td>49.5</td>
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<td>64.4</td>
<td>9.2</td>
<td>0.434</td>
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<td></td>
<td></td>
<td></td>
</tr>
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<td>Allentown (Buffalo)</td>
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<td>59.8</td>
<td>13.2</td>
<td>0.432</td>
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<td>-0.8</td>
<td>0.961</td>
</tr>
<tr>
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<td>32</td>
<td>66.4</td>
<td>95.3</td>
<td>-28.9*</td>
<td>0.086</td>
</tr>
<tr>
<td><strong>P-value of F-statistic</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Earnings, Months 13-24</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atlanta Job Corps</td>
<td>18</td>
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<td>-2,587.98</td>
<td>0.203</td>
</tr>
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<td>6,440.66</td>
<td>5,824.94</td>
<td>615.72</td>
<td>0.550</td>
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<td>Chicago Commons</td>
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<td>1,498.89</td>
<td>3,331.03</td>
<td>0.153</td>
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<td>2,091.86</td>
<td>2,412.69</td>
<td>0.109</td>
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<td>EGOS (Denver)</td>
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<td>1,667.32</td>
<td>2,741.52</td>
<td>-1,074.19</td>
<td>0.355</td>
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<td>-1,826.47</td>
<td>0.247</td>
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<tr>
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<td>1,566.22</td>
<td>-445.09</td>
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</tr>
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<td>Sequential/in-house</td>
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<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>El Centro (Dallas)</td>
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<td>2,792.78</td>
<td>1,838.33</td>
<td>954.46</td>
<td>0.531</td>
</tr>
<tr>
<td>LA Job Corps</td>
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<td>3,832.21</td>
<td>1,647.96</td>
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<tr>
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<td>-386.42</td>
<td>0.790</td>
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<tr>
<td>CREC (Hartford)</td>
<td>32</td>
<td>3,795.73</td>
<td>7,219.70</td>
<td>-3,423.97**</td>
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<tr>
<td><strong>P-value of F-statistic</strong></td>
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<td>Total Earnings, Months 13-24</td>
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TABLE G.4 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.
REFERENCES


-271-


-273-


SELECTED MDRC PUBLICATIONS ON YOUTH PROJECTS

Book


Monographs


Reports

Ohio's Learning, Earning, and Parenting (LEAP) Program


The New Chance Demonstration


Project Redirection


**The Career Beginnings Demonstration**


**The Youth Incentive Entitlement Pilot Projects (YIEPP)**


**Community Service Projects**


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(415) 441-7607

MDRC