

The Enhanced Transitional Jobs Demonstration

Implementation and Early Impacts of the Next Generation of Subsidized Employment Programs

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Chapter 1

Introduction

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For decades, policymakers and practitioners have searched for program models that can increase employment rates and earnings for adults who are considered “hard to employ”: those with limited work experience, low levels of formal education, and other obstacles to employment. One approach that has been implemented and tested fairly extensively is called “transitional jobs.” Transitional jobs programs offer temporary subsidized jobs that aim to teach participants basic work skills or get a foot in the door with an employer. The programs also help participants address personal issues that impede their ability to work (for example, a lack of transportation, identification, clothing, or supplies) and assist them in finding unsubsidized jobs when the transitional jobs end. A number of transitional jobs programs have been evaluated in the past, with mixed results.¹

In late 2010, the Employment and Training Administration of the U.S. Department of Labor (DOL) launched the Enhanced Transitional Jobs Demonstration (ETJD), which provided about \$40 million to seven transitional jobs programs that were chosen through a national grant competition.² The programs targeted either low-income parents who did not have custody of their children (“noncustodial” parents, usually fathers) and who owed child support, or individuals who had recently been released from prison. They were designed to build on the lessons of past research. MDRC, a nonprofit, nonpartisan research organization, was selected to conduct a multifaceted evaluation of the programs using a random assignment research design. MDRC is partnering with Abt Associates and MEF Associates.

At about the same time, the U.S. Department of Health and Human Services (HHS) launched the Subsidized and Transitional Employment Demonstration (STED), which is evaluating subsidized employment programs for a range of populations. MDRC is leading the STED project as well.³ The two projects are closely coordinated. For example, DOL and HHS agreed to coordinate the timing of the projects’ follow-up surveys and to use many of the same data-collection instruments. Notably, two of the ETJD programs targeting noncustodial parents are also being evaluated as part of the STED evaluation.

This report provides the first evidence about the implementation and effects of the ETJD programs. Each chapter focuses on one of the programs, describing its design, operation, and impacts on participants’ outcomes during a one-year follow-up period. The final chapter summarizes the results and looks forward, identifying unanswered questions that will be addressed in a future report. Because this report includes all seven ETJD programs —

¹See, for example, Bloom (2010); Valentine (2012); Redcross, Millenky, Rudd, and Levshin (2012); Butler et al. (2012).

²For additional information, see U.S. Department of Labor, Employment and Training Administration (2011).

³MDRC’s partners for the STED project are MEF Associates, Branch Associates, and Decision Information Resources.

including the two that are also being evaluated under STED — it is funded and released by both DOL and HHS.

The report presents comprehensive information about each program’s implementation. However, the data on the programs’ effects on participants’ outcomes should be considered interim results because one year of follow-up is not sufficient to draw conclusions about models of this type.

Background and Policy Context

This section discusses some of the factors that shaped the design of the ETJD and STED projects: a resurgence of interest in subsidized employment, the findings of past studies of transitional jobs programs, and developments in the child support and corrections systems, including policy efforts to address the effects of mass incarceration and the employment needs of people returning to their communities.

A Renewed Focus on Subsidized Employment

Subsidized employment programs use public funds to create jobs for people who cannot find employment in the regular labor market. The first large-scale subsidized employment programs in the United States — the Works Progress Administration and other New Deal programs — employed millions of people during the Great Depression, built thousands of roads and bridges, and improved many other public facilities.⁴ A smaller subsidized employment program operated in the 1970s under the auspices of the Comprehensive Employment and Training Act. These relatively large, “countercyclical” subsidized employment programs were designed primarily to put money into the pockets of jobless workers during periods of high unemployment, and to stimulate the economy. They were usually targeted broadly, rather than focusing on specific disadvantaged populations.

In 2009, when the national unemployment rate reached 10 percent, states used funds from the American Recovery and Reinvestment Act’s Temporary Assistance for Needy Families Emergency Fund (TANF-EF) to create jobs for about 280,000 people.⁵ Forty states put at least some people to work in this way before the funding expired in late 2010, and 14 states and the District of Columbia each placed at least 5,000 people in subsidized jobs. In contrast to earlier countercyclical programs that placed workers with public agencies, many of the largest TANF-EF programs placed most subsidized workers with private-sector firms.

⁴Taylor (2009).

⁵Farrell, Elkin, Broadus, and Bloom (2011).

Importantly, most of the TANF-EF programs (particularly the larger ones) broadly targeted unemployed workers. Eligibility was not limited to recipients of Temporary Assistance for Needy Families (TANF), people with criminal records, or other disadvantaged groups (notably, about half the placements nationwide were summer jobs for young people). Also, many of the programs did not put a strong emphasis on helping participants make a transition to unsubsidized jobs. Like other countercyclical programs before them, the TANF-EF programs served many people who had steady work histories, and the models assumed that these people would return to regular jobs once the labor market improved. The TANF-EF programs were popular in many states, with governors from both parties expressing strong support. The experience, while relatively short-lived, rekindled interest in subsidized employment more broadly.⁶

Evaluations of Transitional Jobs Programs

While the relatively positive experiences of the TANF-EF-funded programs helped create momentum for research projects like ETJD and STED, the specific models being tested in these projects are quite different from the countercyclical programs described in the previous section. Whereas the countercyclical programs were designed primarily to provide work-based income support to the unemployed, the programs in ETJD and STED use subsidized employment as a training tool to help prepare the hard-to-employ for regular employment, and they typically offer comprehensive services and other forms of support in addition to the jobs themselves. Programs of this type have operated sporadically since the 1970s, usually on a relatively small scale. These programs are usually assessed by measuring whether they improve the longer-term employment patterns of participants, and whether they improve outcomes in related areas like recidivism (for people with a history of incarceration) or reliance on public benefits (for welfare recipients).⁷

The first rigorous evaluation of this approach, the National Supported Work Demonstration, operated by MDRC from 1974 to 1980, tested a highly structured subsidized employment model for four disadvantaged groups: long-term welfare recipients, formerly incarcerated people, young people who had dropped out of high school, and recovering substance abusers. That evaluation found mixed results.⁸ Another study, in the 1980s, tested a model that provided both classroom training and subsidized jobs to recipients of public assistance who were preparing to become home health aides. The program, which was tested in several locations, led to

⁶Farrell, Elkin, Broadus, and Bloom (2011); Pavetti, Schott, and Lower-Basch (2011).

⁷In this report “recidivism” refers to the rate at which people with criminal records are rearrested, convicted of new crimes, or reincarcerated. For a review of evaluations of subsidized employment programs, see Dutta-Gupta, Grant, Eckel, and Edelman (2016).

⁸Hollister, Kemper, and Maynard (1984).

sustained increases in earnings in most of these locations.⁹ Interest in this type of model reemerged in the 1990s in the context of state and federal welfare-reform efforts, and the term “transitional jobs” emerged to describe the general approach.

Transitional jobs programs take many forms. In some models, participants work directly for the program sponsor, which may be a social enterprise (a business with a social purpose). In others, they work for nonprofit organizations or businesses in the community but remain on the payroll of the program sponsor, which serves as the “employer of record.” In models with the latter structure, the subsidized workers may be supervised by staff members from the host employer, or by staff members from the program sponsor who are stationed at the work site. Finally, some models place participants with businesses and subsidize only a portion of those individuals’ wages for a set time period; there may be an expectation that the participant will “roll over” and become a permanent employee of the business when the subsidy ends.

Between 2004 and 2010, MDRC (with support from HHS, DOL, and private foundations) evaluated six transitional jobs programs, five targeting formerly incarcerated people and one targeting long-term TANF recipients.¹⁰ All of the transitional jobs programs provided participants with temporary subsidized jobs, usually lasting two to four months. In some models, the participants worked directly for the program, while in others they worked for other nonprofit organizations in the community. In either case, there were very few opportunities for participants to move into permanent, unsubsidized jobs with the host employers. The programs therefore helped participants look for permanent, unsubsidized jobs, and provided a range of support services. The evaluations randomly assigned eligible applicants to a program group that had access to the transitional jobs program or to a control group that did not. In most of the studies, the control group was offered basic job-search assistance, but not subsidized jobs.

The studies found that all of the programs dramatically increased employment initially: Rates of employment were typically 30 to 50 percentage points higher for the program group than for the control group in the early months of the study period. This difference means that the programs gave jobs to many people who would not have worked otherwise. However, the employment gains were the result of the subsidized jobs themselves, and those gains faded quickly as people left those jobs — a result consistent with previous research on transitional jobs programs. None of the programs consistently increased *unsubsidized* employment over follow-up periods ranging from two to four years. One of the programs for formerly incarcerated people (the New York City-based Center for Employment Opportunities) produced

⁹Bell and Orr (1994).

¹⁰Redcross, Millenky, Rudd, and Levshin (2012); Valentine and Bloom (2011); Valentine (2012).

statistically significant reductions in recidivism, but the others did not.¹¹ The results of these evaluations led to a search for transitional jobs models that could produce sustained increases in unsubsidized employment and improvements in other areas.

Developments in the Criminal Justice and Child Support Systems

The ETJD project in particular reflects broader trends in corrections and child support, two systems that interact with many disadvantaged men. Both of these systems are heavily focused on enforcement. Put simply, the corrections system aims to segregate and punish criminals, and the child support system aims to identify noncustodial parents and establish and enforce child support orders. In recent years, however, both systems have begun to rethink their priorities and practices.

After a four-decade surge in incarceration, many states are now looking for ways to reduce their prison populations by changing sentencing laws and emphasizing alternatives to incarceration, such as drug treatment and mental health services. Similarly, states are increasingly providing services and support to individuals who are released from prison, in order to reduce high rates of recidivism. The most recent national data show that two-thirds of individuals released from state prisons are rearrested and half are reincarcerated within three years.¹² While the connection between crime and employment is far from straightforward, employment services are central to most reentry initiatives, probably because work provides a source of legitimate income and an opportunity for individuals to spend time in socially productive activities. These trends can also be seen at the local level, as many municipalities are developing jail reentry programs.¹³

Parallel changes are occurring in the child support system, which is run by states and counties but heavily funded by the federal government. Over the past three decades, that system has become increasingly adept at collecting child support, primarily by withholding payments from noncustodial parents' paychecks. The system now works relatively efficiently for parents who are steadily employed in the mainstream economy, but improved enforcement may have different effects for noncustodial parents who do not have the means to pay. These parents may accrue large child support debts (also known as arrears).¹⁴ In fact, some argue that tighter

¹¹Researchers hypothesized that the program reduced recidivism because some aspect of its unusual structure — participants worked in small crews supervised by Center for Employment Opportunities staff members — led to changes in participants' attitudes and behavior.

¹²Durose, Cooper, and Snyder (2014).

¹³Counties and cities operate jails, which are used to detain defendants awaiting trial and to incarcerate people who are given relatively short sentences (that is, those of less than one year).

¹⁴Child support debt may be owed to the custodial parent or to the state, since payments made on behalf of custodial parents who receive public assistance may be retained by the state as reimbursement for those costs. Noncustodial parents are generally required to pay child support until their children reach age 18, but

(continued)

enforcement may drive some disadvantaged noncustodial parents underground, where their earnings will be invisible to the system. In recent years, many child support agencies have begun to pay more attention to noncustodial parents' ability to pay, to rethink the way child support orders are set and modified for parents with little or no earned income, and to provide services to help unemployed or underemployed parents find jobs. This evolution reflects the fact that researchers and policymakers are increasingly emphasizing the role fathers — including noncustodial fathers — play in providing financial and emotional support to their children.

Transitional jobs programs are seen as a potentially effective approach for unemployed noncustodial parents and individuals who are reentering the community from prison, in part because these models provide immediate income while participants are learning work skills. Temporary subsidized jobs also recognize the reality that many private-sector employers are extremely reluctant to hire people with criminal records.

The ETJD Project

In 2011, DOL held a national competition to select programs for the ETJD project. The grant competition required each applicant to provide core components of a strong, basic transitional jobs program as well as specific enhancements intended to address the employment barriers of the applicant's specified population. The applicants had to justify why the particular enhancement(s) they proposed were likely to yield stronger long-term outcomes than those achieved by programs previously tested, and they had to conduct screening to ensure that only individuals who were not "job ready" could enroll. DOL also required applicants to demonstrate that they had established partnerships with One-Stop Career Centers and employers.¹⁵ DOL identified the top 14 applicants, and then the MDRC team visited each contender to assess the applicant's ability to operate a strong program and meet the requirements of the evaluation.

Ultimately, seven applicants were selected, and each received a four-year grant totaling approximately \$6 million to recruit, screen, and conduct random assignment of 1,000 interested participants, ultimately serving 500 of them. The 4-year grant period included approximately 6 months for planning, 2 years to enroll participants, and an additional 18 months to complete service delivery for all participants. DOL also engaged Coffey Consulting, a small business based in Maryland, to provide technical assistance to the grantees as they set up their programs. (Box 1.1 provides further information about the technical assistance effort.)

the child support system will continue to pursue unpaid debt after a parent is no longer required to pay "current support." These cases are known as "arrear-only" cases.

¹⁵Established under the Workforce Investment Act of 1998, One-Stop Career Centers offer a range of services to job seekers under one roof. They are now known as American Job Centers.

Box 1.1

Technical Assistance in the ETJD Project

The ETJD grantees had access to two types of technical assistance over the course of the project period. The evaluation team, led by MDRC, was responsible for training grantee staff members in the research procedures and for monitoring the implementation of the procedures over time. A separate DOL contractor, Coffey Consulting, was responsible for providing programmatic technical assistance. This separation of responsibilities reflected DOL's view that an evaluation's objectivity could be compromised if the evaluator was responsible for helping to strengthen the program being tested. Each grantee was also assigned to a DOL Federal Project Officer located in its region. (DOL has six regional offices, each overseeing programs in a number of states.)

Although the division of labor was generally clear, there were instances when the lines between the two types of technical assistance became blurred. For example, participant recruitment is obviously a programmatic issue, but it also affects the evaluation directly because the study's ability to detect statistically significant impacts depends on the sample size; in fact, the grantees had specific recruitment targets. Similarly, the procedures for screening and enrolling applicants involve both programmatic and research considerations. Thus, both teams worked with grantees on recruitment issues.

There were also instances where the two types of assistance could be in conflict. One such area involved performance standards. DOL required the grantees to achieve certain levels of performance on important participant outcomes, for example, by placing a specified percentage of program group members into unsubsidized jobs. Outcome goals such as these, while important for accountability, can push programs to screen applicants more intensively in an effort to enroll more job-ready participants. However, many prior random assignment evaluations have shown that programs with better participant *outcomes* do not necessarily produce larger participant *impacts*. In fact, pursuing better outcomes can actually lead to smaller impacts: in this case, enrolling more job-ready participants would make the control group more likely to find employment as well. DOL announced that ultimately the results from the random assignment evaluation would take precedence over the grantees' performance outcomes, but the outcomes continued to be monitored and reported throughout the project period. These conflicting messages sometimes made it difficult for the evaluation team and the technical assistance team to speak with a unified voice in their interactions with grantees.

In a confidential survey, all grantees gave high ratings to the evaluation-related technical assistance they received from the MDRC team. When surveyed about the Coffey team, grantees were most likely to say that they received programmatic technical assistance regarding the DOL project-wide management information system and DOL grant reporting requirements. Most of the grantees expressed strong dissatisfaction with the management information system, with one describing it as the worst aspect of the project. Most but not all of the grantees also reported that they received technical assistance from the Coffey team regarding recruitment or programmatic functions such as case management. Their assessments of the quality of this assistance were mixed. Some grantees found it helpful, while others did not. Several grantees also reported that they received valuable assistance from their Federal Project Officers on issues related to compliance with DOL grant rules.

What Is Being Tested in ETJD?

Figure 1.1 shows the locations of the seven ETJD programs, and Table 1.1 provides some basic information about each grantee and its intended model.

As Table 1.1 shows, most of the grantees were private, nonprofit organizations. As discussed in later chapters, all of them worked closely with state or local government partners. Each ETJD grantee was required to choose a primary target population, either noncustodial parents or people recently released from prison. These two groups tend to overlap, since many people involved in the criminal justice system are parents, but each grantee needed to ensure that everyone it served met the eligibility criteria for its chosen target group.¹⁶ Table 1.1 shows that four of the grantees targeted noncustodial parents and three targeted formerly incarcerated people. Each of the seven ETJD programs was designed somewhat differently but, as required by DOL, all of them were enhanced in some ways relative to the transitional jobs models that were studied earlier. Those earlier programs all provided temporary subsidized jobs either within the program itself or with other nonprofit organizations. They also assigned participants to job coaches or case managers (who helped them address barriers to employment) and to job developers (who helped them search for unsubsidized jobs). As shown in Table 1.2, the ETJD enhancements fell into three general categories:

- **Structural changes.** The transitional jobs programs that were tested earlier placed participants into relatively sheltered positions and then helped them find regular jobs. Two of the ETJD programs (those in Atlanta and New York City) used “staged” models in which participants started in program jobs, but then progressed to subsidized jobs in the community that more closely resembled “real” jobs. A third program (the one in Fort Worth) focused entirely on placing participants directly into subsidized jobs in the private sector that were intended to evolve into permanent positions. A fourth (the one in San Francisco) used a “tiered” model that placed participants into different types of transitional jobs based on their educational and work histories. For the most part, these new structural approaches were designed to promote smoother transitions from subsidized to unsubsidized jobs.

¹⁶Eligibility criteria in programs choosing to target noncustodial parents required that participants be low-income (as defined by Title I of the Workforce Investment Act) divorced, separated, or never-married parents ages 18 or older who were not the primary physical custodians of their children, and who either had child support orders in place or who had agreed to start the process of establishing orders within 30 days of program enrollment. In programs targeting people recently released from prison, eligibility criteria required that participants be offenders ages 18 or older who had been convicted as adults under federal or state law, who had never been convicted of sex-related offenses, and who had been released from state or federal prison within the 120 days before they enrolled.

Table 1.1**ETJD Individual Program Characteristics**

| Program Name, Operator, and Location | Target Group | Program Overview |
|---|------------------------------|--|
| GoodTransitions Goodwill of North Georgia <i>Atlanta, GA</i> | Noncustodial parents | Participants work at a Goodwill store for approximately one month, then move into a less supported subsidized position with a private employer in the community for about three months. The program offers case management and short-term training. |
| Supporting Families Through Work YWCA of Southeast Wisconsin <i>Milwaukee, WI</i> | Noncustodial parents | Participants start in a three- to five-day job-readiness workshop. They are then placed in transitional jobs, mostly with private-sector employers. The program supplements wages in unsubsidized employment to bring them up to \$10 an hour for six months. The program also provides child support-related assistance. |
| TransitionsSF Goodwill Industries, with San Francisco Dept. of Child Support Services <i>San Francisco, CA</i> | Noncustodial parents | Participants begin with an assessment, followed by two weeks of job-readiness training. Then they are placed into one of three tiers of subsidized jobs depending on their job readiness: (1) nonprofit, private-sector jobs (mainly at Goodwill); (2) public-sector jobs; or (3) for-profit, private-sector jobs. They may receive modest financial incentives for participation milestones and child support assistance. |
| Parent Success Initiative Center for Community Alternatives <i>Syracuse, NY</i> | Noncustodial parents | Groups of 15 to 20 participants begin the program together with a two-week job-readiness course. They are then placed in work crews with the local public housing authority, a business improvement district, or a nonprofit organization. The program offers family life-skills workshops, job-retention services, case management, civic restoration services, child support legal aid, and job-search and job-placement assistance. |
| Next STEP Workforce Solutions of Tarrant County <i>Fort Worth, TX</i> | Formerly incarcerated people | Participants begin with a two-week “boot camp” that includes assessments and job-readiness training. They are then placed in jobs with private employers. The program pays 100 percent of the wages for the first eight weeks and 50 percent for the following eight weeks. Employers are expected to retain participants who perform well. Other services include case management, group meetings, high school equivalency classes, and mental health services. |
| RecycleForce RecycleForce, Inc. <i>Indianapolis, IN</i> | Formerly incarcerated people | Participants are placed at one of three social enterprises, including an electronics recycling plant staffed by formerly incarcerated workers, who provide training and supervision to participants and serve as their peer mentors. Participants begin working in subsidized jobs on the day of random assignment. The program also offers occupational training, case management, job development, work-related financial support, and child support-related assistance. |
| Ready, Willing and Able Pathways2Work The Doe Fund <i>New York, NY</i> | Formerly incarcerated people | After a one-week orientation, participants work on the program’s street-cleaning crews for six weeks, then move into subsidized internships for eight weeks. If an internship does not turn into unsubsidized employment, the program will pay the participant to search for jobs for up to nine weeks. Additional services include case management, job-readiness programs, opportunities for short-term training and certification, and parenting and computer classes. |

Table 1.2
Description of ETJD Enhancements

| Enhancement Type | Example of Enhancement Approaches | ETJD Programs Implementing Enhancement |
|---|---|--|
| Structure of subsidized job | <ol style="list-style-type: none"> 1. Staged: begin in program transitional job and progress to the private sector in the second stage 2. Tiered: three types based on client need 3. Private-sector subsidy | Atlanta San Francisco Fort Worth New York City |
| Enhanced support | <ol style="list-style-type: none"> 1. Cognitive behavioral therapy-based workshops 2. Peer mentoring 3. Wage supplement 4. Occupational training 5. Criminal justice system-related assistance | Fort Worth Indianapolis Milwaukee Syracuse New York City |
| Child support system-generated incentives/sanctions | <ol style="list-style-type: none"> 1. Child support orders modified downward contingent on program participation. Reinstated to prior levels for nonparticipation. 2. Interest on debt forgiven in progressively greater proportions, contingent on length of participation in program, up to 100 percent of state-owed debt. | San Francisco Milwaukee |

SOURCE: MDRC implementation research.

- **Enhanced support.** Several of the ETJD programs aimed to provide special support or assistance that was not available in the earlier programs — for example, cognitive behavioral workshops, opportunities for short-term training in occupational skills, or help correcting errors in their official criminal records. However, the ETJD programs did not offer direct services to address issues like substance abuse or housing instability.
- **Child support incentives.** In two of the programs targeting noncustodial parents (those in Milwaukee and San Francisco), the child support agency offered special “carrots,” “sticks,” or both to encourage participants to remain active in the ETJD program. For example, in San Francisco, participants’ child support orders were reduced as long as they participated steadily in the program.

The programs used these enhancements in various combinations. Some of them were structured much like traditional transitional jobs programs, but included enhanced support or child support incentives. (In subsequent chapters, these programs are referred to as “modified transitional jobs models.”) Others used one of the innovative structural approaches described above and also included one or both of the other types of enhancements.

How Were the Programs Intended to Work?

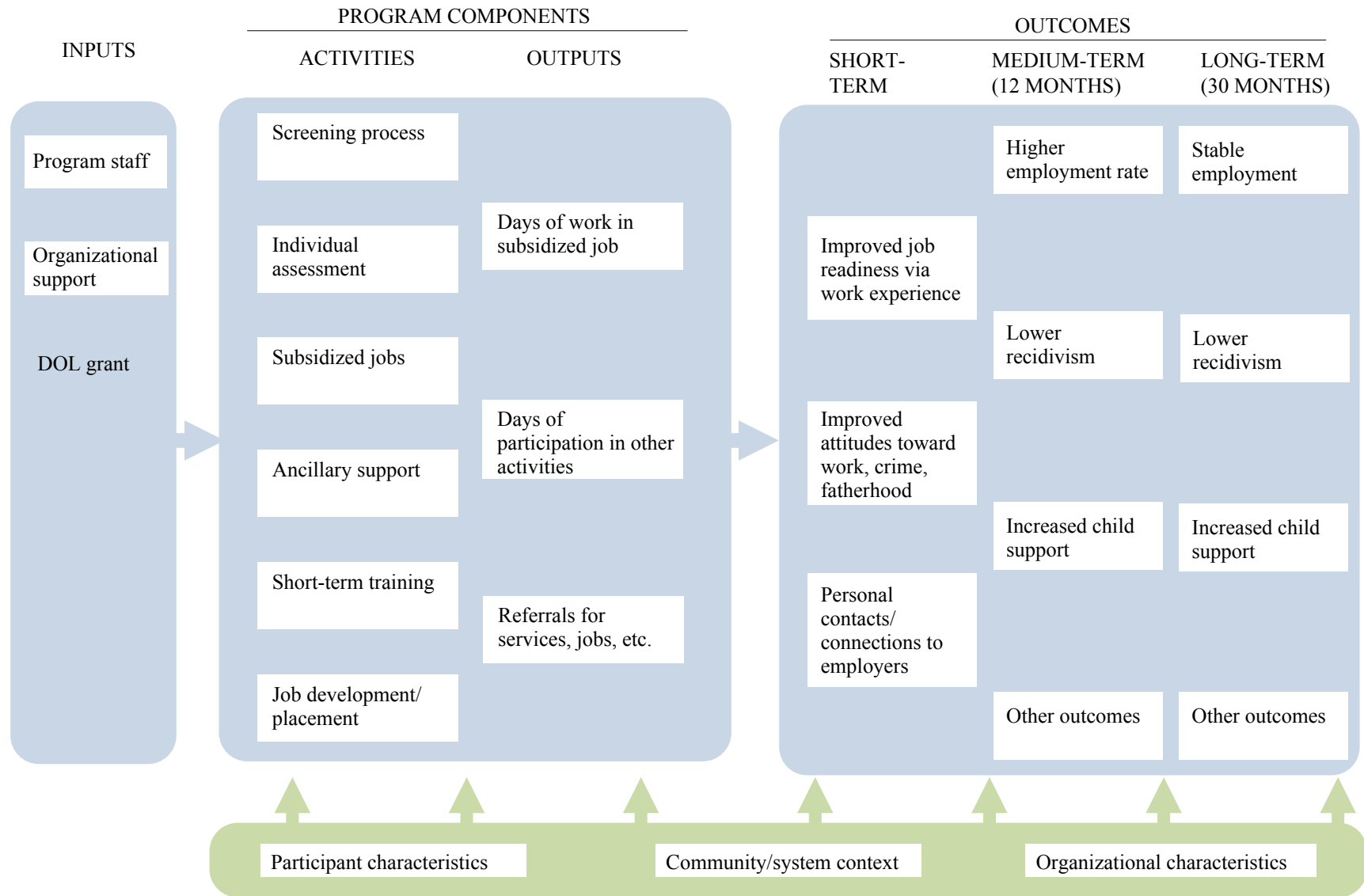
When designing an evaluation, it is critical to understand a program’s “theory of change”: the underlying assumptions about how the program components will ultimately lead to the desired outcomes for participants. Figure 1.2 depicts a generic logic model (a graphical illustration of the theory of change) for the ETJD programs (though it is important to note that the evaluation is not able to measure everything shown in the figure). The left-hand side of the figure shows the major “inputs”: the grantee organizations’ internal resources, their community partners, the DOL grants, and the DOL-funded technical assistance. The next section shows the major program activities or components, including participant recruitment, screening, and assessment; job development (outreach to employers to identify potential job openings) and job placement (help applying for open jobs); and, of course, the subsidized jobs. The grant required the programs to screen for job readiness, as only individuals who were not considered “job ready” were eligible. Each program defined job readiness slightly differently, but each had a screening process in place. As noted earlier, most of the programs also included enhanced support or arrangements with child support agencies to reduce obstacles to participants’ success. The inputs and activities lead to the outputs: hours or days of participation in subsidized jobs (that is, work experience) and other program activities, referrals for assistance to other agencies in the community, and so forth.

Finally, the right side of the figure shows the desired outcomes. While program group members are working in subsidized jobs, the impacts on employment and earnings are likely to be quite large. After the program, in the short term, ETJD is expected to assist participants in obtaining formal, unsubsidized employment. Transitional jobs are designed to make participants more employable not only by providing work experience that can be recorded on a résumé, but also by improving their “soft skills” (the general habits and competencies of a good employee, for example, the ability to show up to work on time, take direction, and work well with others).

Transitional employment can also effect long-run unsubsidized employment by sending signals to employers. Successful completion of ETJD activities can signal to employers that a graduate is ready for the world of work, for example. On the other hand, ETJD programs might send negative signals if employers assume graduates “needed” the program to address issues that may affect their performance on the job. Finally, some of the programs rely on wage subsidies to convince employers to try out employees whom they might not ordinarily hire. The theory is that, with a foot in the door, a subsidized worker who performs well might have a good chance to “roll over” into an unsubsidized position.

It is hypothesized that higher levels of employment among noncustodial parents and people recently released from prison will lead to more regular child support payments and lower recidivism, respectively. In addition, some of the ETJD programs offer specialized assistance to

Figure 1.2
Generic Logic Model



the two target groups — for example, training in parenting skills for noncustodial parents or cognitive behavioral workshops to address criminal attributes common among the formerly incarcerated. These services may affect child support payments and recidivism directly, not necessarily as a result of employment.¹⁷

The ETJD Evaluation

The ETJD evaluation set out to answer three broad questions:

- How were the ETJD programs designed and operated, and whom did they serve?
- How did the ETJD programs affect participants' receipt of services, and their outcomes in three primary domains: employment, child support, and recidivism (that is, arrests, convictions, and incarceration)?
- How do the programs' costs compare with any benefits they produce?

To address these questions, the evaluation includes three main components: an implementation study, an impact study, and a benefit-cost study. This report focuses mainly on the implementation study and also presents early findings from the impact study. Impact findings 30 months after random assignment and results from the benefit-cost study will be presented in a later report.

The central goal of the evaluation is to assess whether the ETJD programs were successful in improving participants' outcomes in the first three areas shown in Figure 1.2: employment, recidivism, and child support payments. The basic approach is a randomized controlled trial, in which individuals who were eligible for and expressed interest in an ETJD program were assigned, through a lottery-like process, to a program group that had access to program services or a control group that did not. This process created two groups that were comparable at the start of the study in both measurable and unmeasurable ways. Thus, one can be fairly confident that any statistically significant differences in the groups' outcomes that emerge over time — for example, differences in employment rates — can be attributed to the ETJD program rather than to preexisting differences between the groups.

Three points about the evaluation approach are worth noting. First, there is a critical distinction to be made between a program model as it is written on paper (or in a grant application)

¹⁷The link between employment and child support is relatively straightforward, since child support payments are often deducted directly from noncustodial parents' paychecks. The link between employment and recidivism is the subject of much scholarly research, and is considerably less direct.

and the program services that are actually offered or that participants receive. Models are not always implemented with fidelity and potential participants do not necessarily accept services that programs offer. The implementation study examines all the links in this chain.

Second, the evaluation assumes that program impacts — that is, the differences in outcomes between the program and control groups — are the product of the service contrast — the differences in the services, support, and incentives received by members of the two groups. While the ETJD programs are the central focus of the evaluation, the design assumes that the control group will make use of other services in the community. This assumption is particularly likely to be true because all of the sample members are involved with child support or criminal justice — two systems that may urge or even require their “clients” to get help finding work or addressing barriers to employment. As discussed further below, the evaluation is using surveys to measure the services received by both groups.

Third, the characteristics of the service providers — in this case, the ETJD grantees and their partners — shape the implementation process. Similarly, the characteristics of the clients and the local context (including factors such as the labor market, the service environment, and the operation of the child support and criminal justice systems) shape a program’s implementation, the services the program and control groups receive, and the groups’ outcomes. The implementation study also examines these contextual factors.

Implementation Study

The implementation study set out to describe how ETJD operated on the ground in each city. As discussed earlier, doing so means describing each grantee’s model as it was designed, the steps that local managers took to put it in place, the nature of the services that were ultimately offered to clients, the “dosage” of services that they actually received (that is, the frequency and intensity of those services), and the context in which the program operated. The study especially aimed to assess whether the ETJD programs were truly “enhanced” relative to earlier transitional jobs programs. The implementation study used several data sources:

- **Staff interviews and observations.** The research team made two formal visits to each program to interview staff members and observe program activities (plus an additional visit to conduct an early assessment of program operations). In addition, the team gathered important information through regular contact with grantees that was part of providing technical assistance on the research procedures. Lastly, the team conducted a time study in the fall of 2013, in which the staff at each ETJD program recorded the time spent on various ETJD program activities and non-ETJD responsibilities over a two-week period.

- **Management information system data.** Grantees were required to track clients' participation in program activities using a management information system developed by DOL (that is, a computerized database organized and programmed to produce regular reports on operations). The research team extracted data on client characteristics, service receipt and attendance, work support and incentive payments, and subsidized employment earnings. In some cases, the research team supplemented these data with additional payroll and participation records available from the grantee's own data systems. It is important to note there were persistent problems with the management information system throughout the life of the project. Several delays in the development of the system meant that it was not fully functional until April 2014, four months after the final ETJD participant was enrolled, in December 2013.
- **Focus group discussions, case-file reviews, and in-depth interviews with participants.** The research team conducted focus groups with participants in each program during the first formal site visit. During the second site visit, the team reviewed four to nine case files in each location. In addition, researchers conducted a series of longer interviews with four to eight participants in each program to learn more about program offerings and participants' experiences.
- **Questionnaires.** The team administered four questionnaires to gain "real-time" information from participants, program staff members, employers, and work-site supervisors. The research team attempted to administer the participant questionnaires on paper to all participants working in transitional jobs at the time of the two site visits to each program. A total of 531 participant questionnaires were completed. The program staff, employer, and work-site supervisor questionnaires were administered via a website link sent by e-mail in the weeks shortly following each site visit. In total, 93 program staff members, 105 work-site supervisors, and 85 employers completed questionnaires.¹⁸

Impact Study

This report provides early, interim evidence on the ETJD programs' impacts on employment, child support, and recidivism. More than 12 months of follow-up is needed to provide reliable evidence on the longer-term impacts of the ETJD programs. These results lay

¹⁸Note that these categories (program staff member, work-site supervisor, and employer) were not mutually exclusive, as some individuals fit into multiple categories.

the groundwork for the final report, which will address the impact questions more definitively, based on 30 months of follow-up data.

Because ETJD is testing a variety of new models and enhancements that have not yet been “proven” in rigorous studies, this evaluation is primarily exploratory in nature. That is, the evaluation as a whole will be providing suggestive evidence on which program innovations are effective, so that these enhancements can be more widely replicated and studied. Hence, the evaluation will engage in an analysis that encompasses all measured outcomes for all programs. Hypothesis testing is conducted independently for each outcome for each program, and findings are interpreted as suggestive evidence of program effectiveness.¹⁹

Samples and data sources. The evaluation approach is very similar for each experimental “site” (a term that encompasses the program, the program group, the control group, and their environment): Grantees recruited eligible participants by establishing connections with child support and corrections agencies and other partners, and interested individuals were referred to the programs. Although the specific process differed from place to place, each grantee took some steps to verify that each potential participant was both eligible and appropriate for the program according to the DOL grant requirements for eligibility (which included being in the target population as the grant defined it and having limited employment history and low levels of education) as well as other, program-specific eligibility criteria. After determining eligibility, staff members explained the study and obtained the individual’s consent to participate in it.²⁰ They then logged into a web-based system maintained by MDRC that randomly assigned the individual to the program group or to the control group. Those in the program group were offered access to the ETJD program, with initial activities usually starting within a few days. Control group members were not offered services from the program being tested, but received a list of other services in their communities or, at a few sites, were referred to specific, preexisting programs that provided relatively modest services.²¹

Individuals were enrolled into the study and randomly assigned to the groups from November 2011 through December 2013. Each grantee was expected to enroll a total of 1,000

¹⁹These analyses will be as meaningful and reliable as findings of previous studies that did not stipulate any confirmatory hypotheses ahead of time (that is, the great majority of rigorous job-training evaluations).

²⁰The steps that were required before random assignment differed from site to site, and these differences may affect the characteristics of the sample at each site. This issue is discussed further in the program-specific chapters.

²¹As discussed in the site-specific chapters, at two sites (Milwaukee and New York City) there were other large, transitional jobs programs operating that were not connected with ETJD. Data obtained in New York City suggest that nearly 40 percent of control group members received services from the Center for Employment Opportunities, a very large transitional jobs program that has been shown to reduce recidivism in a previous evaluation. See Jacobs Valentine (2012); Redcross, Millenky, Rudd, and Levshin (2012); Butler et al. (2012).

people into the study — 500 in the program group and 500 in the control group — and all of them met that goal.

The evaluation team is collecting the following data for sample members in both research groups. In general, a year and a quarter of follow-up data are available for the sample.

Baseline data. The research team extracted baseline data on sample members' demographic characteristics, work histories, and other information from the management information system described earlier and from MDRC's web-based random assignment system. Grantee staff members collected this information from sample members during the enrollment process.²²

Employment and earnings records. Data from the National Directory of New Hires (NDNH) are used to measure quarterly earnings. Maintained by the federal Office of Child Support Enforcement at HHS, the NDNH contains quarterly earnings data collected by state workforce agencies and quarterly earnings data from federal agencies. These data include jobs covered by unemployment insurance, which comprise most jobs in the formal labor market. Jobs in the informal economy, those in which workers are treated as independent contractors, and some other types of employment are not included.

At some sites, the ETJD grantee was the employer of record for most or all transitional and subsidized jobs. Unfortunately, employer identification numbers were not available to determine which employer a sample member was working for in each quarter. In addition, at some sites (Atlanta, New York City, and San Francisco), at least some of the transitional or subsidized jobs were not reported to state unemployment insurance programs; as a result, earnings from those jobs do not appear in the NDNH data. To address this issue, subsidized job payroll records were obtained from the DOL management information system and combined with the NDNH records, so that the analysis includes both NDNH-reported jobs and transitional

²²Due to problems with the DOL management information system, baseline data for the first four months of study participants were missing at higher-than-normal rates in several important categories. Data in these categories had been entered by program staff members but not retained in the system's data file. DOL later allowed grantees to reenter the lost data. As a result, data were missing at lower rates for program group members in these areas than for control group members, presumably because program staff members were more likely to be in contact with or know details about program group members (who were their clients) than control group members (who did not receive the grantees' program services). Because of this imbalance between program and control group members for early enrollees, and general concerns regarding the reliability of baseline data that were entered retroactively, baseline data from the DOL management information system have been used only for descriptive purposes and not as covariates in any impact models. Covariates were drawn only from MDRC's random assignment system baseline data — which did not suffer from these issues — and from comprehensive administrative records regarding employment, criminal justice involvement, and child support. (Administrative records are data used for the management of programs and public services.)

jobs that were not covered by the NDNH.²³ Including payroll records improved the estimate of employment and earnings and made it possible for the research team to analyze whether employment was subsidized or not. However, without the employer identification numbers that would have made it possible to identify the ETJD programs in the NDNH quarterly wage records, the research team could not definitively parse unsubsidized earnings and employment in quarters when an individual worked a subsidized job.²⁴

Criminal justice records. The evaluation uses statewide criminal justice records to measure contacts with the criminal justice system including arrests, convictions, and incarceration in state prison. Although there may be gaps and inaccuracies in official records — and they only cover activity in a particular state — these data should be more accurate than study members’ own reports, because they are not subject to errors in memory or intentional misreporting. In addition, the administrative records are available for all sample members, not just those who responded to the survey. These state administrative data containing several essential measures of recidivism were collected from state agencies for all ETJD sites.²⁵ In order to measure admissions to jail facilities, the research team made an effort to collect local jail data from the county immediately surrounding the ETJD program, for programs that targeted people who were recently incarcerated.²⁶ In addition, at sites where such data were unavailable, the follow-up surveys include questions that capture all incarceration, rather than only incarceration in state prisons.

²³At one site (San Francisco), additional payroll records were obtained directly from the ETJD program to supplement the records available in the DOL management information system. For all sites, payroll records from the DOL management information system, program payroll systems, or both were processed into quarterly earnings totals to make them align with the NDNH quarterly wage records. Although this analysis provides a measure of ETJD subsidized employment and earnings, it does not account for other potential transitional jobs providers in the community that do not report to the unemployment insurance system and that are not included in the NDNH.

²⁴For example, for a site where all subsidized program wages appeared in the NDNH, one could ideally subtract subsidized earnings from total NDNH quarterly earnings to obtain a measure of unsubsidized earnings. However, in reality slight differences between NDNH quarterly wages and payroll records could occur (such as timing issues that may arise from pay periods near the beginning or end of a quarter, or missing payroll records in the DOL management information system due to data entry errors for earnings that were properly reported to state unemployment insurance systems). These differences could make there appear to be more total earnings than subsidized earnings in a quarter, potentially leading to an incorrect calculation of unsubsidized earnings. Without an employer identification number for the NDNH quarterly wage records, the research team could not resolve these types of discrepancies. This report can still speak confidently about subsidized employment and earnings in particular quarters and about total earnings and employment, but the analysis cannot accurately identify unsubsidized earnings and unsubsidized employment for individuals who also had subsidized earnings in the same quarter.

²⁵At the time of this report, prison records for the state of California were not available.

²⁶Typically these data are not reflected in state administrative records, as jails are operated by counties or localities. Jail data generally cover only jail incarcerations in the county or locality from which they were obtained, and in most cases do not cover jail incarcerations in neighboring areas, nor in the rest of the state.

Child support payment records. The research team obtained data on child support payments from the state child support agency for each of the ETJD sites. These data were collected for each child support case associated with a noncustodial parent in the sample and were analyzed for all cases combined.

Surveys. The evaluation team attempted to contact each sample member for an interview approximately 12 months after his or her random assignment date, and will attempt to do so again at approximately 30 months. The surveys include questions on topics that are not covered in the administrative records described above. This report includes findings from the 12-month survey, which was administered to a total of 5,195 sample members across all seven sites. The response rate was 74.2 percent. The mean time of survey response was 13 months after random assignment. Appendix H contains additional information regarding survey response bias analyses.

An additional survey was administered at the two sites that are part of both ETJD and STED, Atlanta and San Francisco. Known as the “in-program survey,” it was administered to program and control group members at all STED sites about four to six months after random assignment, when many program group members were still working in transitional jobs. The survey was designed to measure some of the potential financial and nonfinancial benefits of employment during a period when there was expected to be a very large difference in the employment rates of the two groups.

The program-specific chapters in this report integrate qualitative and quantitative data from these various sources to create a coherent picture of the implementation of the ETJD program in each city.

Outcomes. Substantively, the measures of effectiveness used in this evaluation fall into three domains: labor market outcomes, child support, and recidivism. As described in the logic model above, the ETJD programs are designed to affect outcomes in each of these domains. All primary outcomes are measured using administrative records.

The analysis of labor-market impacts for the ETJD programs focuses on both employment and earnings. The primary measures in this domain are quarterly employment rates and quarterly earnings; the quarterly time frame for these measures is dictated by the data source (unemployment insurance quarterly wage records obtained from the NDNH). Since each of the programs offers participants a period of paid employment, program group members are expected to experience higher employment and earnings in the program period as long as program participation rates are sufficiently high and the programs target people who would not otherwise be working.

The goal of ETJD programs is to permanently alter an individual's trajectory of employment, earnings, and income through work experience and nonemployment support. However, this report primarily covers a period in which sample members were working in transitional or subsidized jobs. Although participants typically remained in subsidized jobs for only a few months, some of them left the jobs and returned, or entered the subsidized jobs later than initially expected. As a result, at all sites, some program group members were still working in subsidized jobs in the last quarter of the follow-up period. It is therefore too early to answer questions about the programs' longer-term impacts after participants leave.

The ETJD programs are expected to affect earnings mainly through effects on employment (rather than due to increases in wages). There are three reasons to emphasize employment independently of earnings. First, some people posit that increasing employment provides benefits to society in and of itself, by keeping people at risk of recidivism occupied in the workplace so they have less time to commit crimes and are more likely to develop positive peer relationships, or by helping noncustodial parents be more positive role models to their children. Second, employment is linked more directly to program activities than earnings, which capture many other dimensions of labor-market experiences. Third, few of the programs offered intensive training that could increase participants' skills. Research suggests that in the segment of the labor market where most ETJD participants find themselves, wages do not rise simply as a result of job experience. Workers in this segment of the labor market are not expected to see increases in wages without skills training.²⁷ As a result, the impact analysis is more likely to detect an impact on employment than wages.²⁸

The primary child support measures included in the report are (1) number of months or quarters in which child support payments are made, and (2) dollar amounts of child support paid. Some of the ETJD programs are modifying child support orders to bring them in line with earnings during the program period. As a result, it is possible that the ETJD programs could lead to a reduction in payment *amounts* in the first year, even if program group members pay child support at higher *rates* than control group members. Regardless, effects on child support payments are expected to emerge early in the follow-up period.

The primary recidivism outcomes included in the report are (1) arrests, (2) convictions, and (3) incarcerations during the year following random assignment.²⁹ Recidivism is most likely in the time shortly after release from prison. Therefore, any effects on recidivism would be expected to emerge early in the follow-up period.

²⁷See Hendra and Hamilton (2015) for a review.

²⁸A possible fourth reason is that earnings estimates are highly variable, making it more difficult to detect impacts on earnings than employment.

²⁹Incarceration includes admissions to prison at all sites, and in programs targeting recently released individuals includes admissions to jail.

Secondary outcomes covered in this report include outcomes in these domains measured from the survey as well as a range of other measures of overall well-being.³⁰ In testing for these effects, the analysis uses two-tailed hypothesis test procedures (as is standard), since transitional jobs programs might produce negative effects (for example if they provide a negative signal to employers or if they delay entry into regular employment that would have otherwise occurred).

Analytic methods. To estimate program impacts, the analysis compares the average outcomes of program and control group members. The study's random assignment design ensures that there are no systematic differences between the program and control groups at the time of randomization. This design ensures that any statistically significant differences in the two groups' outcomes can be attributed to the intervention. While the simple program-control mean outcome comparison would provide an unbiased estimate of the true impact, the impacts are estimated using multivariate regression models that predict outcomes as a function of assignment to the program group and participant baseline characteristics. This method, which is conventional, is used to improve the statistical precision of the estimates. See Box 1.2 for information on how to read and interpret the tables in the subsequent chapters of the report. Unless otherwise stated, all analytic methods used in this study were prespecified before data analysis in the study's design report and a subsequent memo to DOL. In brief, linear regression models were used to estimate impacts on all outcomes in the body of the report.³¹

As noted earlier, because this report includes only one year of follow-up data and is exploratory in nature, it does not draw any firm conclusions about the impacts of the ETJD programs. Nevertheless, the analysis approach recognizes the fact that examining a larger number of outcomes increases the odds that some differences between the program and control

³⁰Survey outcomes are considered secondary because they are only available for the sample of individuals that participated in the survey. Further, the surveys were fielded around a year after random assignment and in some cases capture only point-in-time measures at the time of the survey, or measures that happened up until the time of the survey. Administrative records have more comprehensive data available for primary outcomes over time. Administrative data are also not subject to errors in memory.

³¹In particular, the analysis estimates models of the following general form:

$$Y_i = \alpha + \beta P_i + \delta X_i + \varepsilon_i$$

where

Y_i is the outcome measure for sample member i ;

P_i equals one for program group members and zero for control group members;

X_i is a set of baseline characteristics for sample member i drawn from MDRC's random-assignment system and from administrative records; and

ε_i is a random error term for sample member i .

In this model, the coefficient β is interpreted as the impact of the program on the outcome.

Box 1.2

How to Read the Impact Tables in This Report

Most tables in this report use a format like the one illustrated below. The table shows two employment outcomes for the program group and the control group. For example, the table shows that about 98 (98.4) percent of the program group and about 71 (70.9) percent of the control group ever was employed in Year 1 in Atlanta.

Because individuals were assigned randomly either to the program group or to the control group, the effects of the program can be estimated by the difference in outcomes between the two groups. The “Difference (Impact)” column in the table shows the estimated differences between the two research groups’ employment rates — that is, the program’s estimated *impacts* on employment. The estimated impact on employment can be calculated by subtracting 70.9 percent from 98.4 percent, yielding 27.5 percentage points.

The number of asterisks in the table indicates whether an estimated impact is *statistically significant* (or that the impact is large enough that it is unlikely to have occurred by chance). One asterisk corresponds with an estimated impact that is statistically significant at the 10 percent level; two asterisks reflect the 5 percent level; and three asterisks reflect the 1 percent level, meaning there is less than a 1 percent likelihood that a program with no effect could have generated such a large difference by chance. In 90 percent of experiments of this type, the true value of the impact would fall within the range shown in the “confidence interval” column. To illustrate, the program group experienced an impact on employment of 27.5 percentage points that is statistically significant at the 1 percent level. There is a 90 percent chance that the true value of this impact is between 24.1 and 30.9 percentage points.

One-Year Impacts on Employment and Earnings, Atlanta

| Outcome | Program Group | Control Group | Difference (Impact) | | Ninety Percent Confidence Interval |
|-----------------------------|---------------|---------------|---------------------|--|------------------------------------|
| Employment (%) | 98.4 | 70.9 | 27.5 *** | | [24.1, 30.9] |
| Number of quarters employed | 3.4 | 1.9 | 1.5 *** | | [1.4, 1.7] |

groups will be found to be statistically significant by chance, when there was no real difference. The analysis addresses this issue by limiting the number of outcomes that are examined in each of the primary domains (employment, child support, and criminal justice).

Because of the random assignment design, the crucial difference between the program and control groups is *access* to ETJD services: Individuals in the program group had access to

program services and possibly other, potentially similar services available in the community (for example, One-Stop service providers), while control group members had access only to the other services. In the evaluation literature, the estimate of the average impact of access is referred to as the “intent-to-treat” impact parameter. It measures the impact of having the *opportunity* to participate in the intervention, not the average impact on program group members who actually participate in the intervention.

This report examines impacts for the full study sample and for subgroups of the sample. Subgroups were defined using pre-random assignment characteristics hypothesized to affect impacts. These included employment in the year before random assignment for the programs targeting noncustodial parents, and risk of recidivism (based on a risk index determined by age, number of past convictions, and number of months incarcerated) for programs targeting people recently released from prison. At all sites, subgroups were defined based on whether sample members entered the evaluation during the first or second year of recruitment and enrollment. Because of their small sample sizes, subgroup impact estimates are considered less precise than full-sample impact estimates and should therefore be interpreted cautiously. Subgroup impacts also require an additional test of statistical significance to assess the magnitude of differences in impacts across subgroups. Whenever such differences are statistically significant, one can have greater confidence that the underlying impacts for the subgroups involved are actually different from one another.³²

In an evaluation such as ETJD, in which distinct programs are implemented in multiple sites, it is important to decide whether to pool results across programs or to study their impacts individually. For this first report, the impacts are estimated separately. There are several reasons why. First, given the nature of this project, readers are likely to be interested to know which enhancements to the basic transitional jobs model have been successful. Second, it is too early to know the long-run effects of the ETJD programs, and it is therefore premature to compare programs or to look for patterns among them.³³

³²A statistical test was used to test for statistically significant differences in impact estimates across subgroups. Statistical significance levels for differences in subgroup impacts are indicated in the impact tables using daggers, as follows: ††† = 1 percent; †† = 5 percent; † = 10 percent.

³³Pooling data from multiple programs can be an important part of an analysis strategy. It provides larger sample sizes and makes it possible to detect smaller effects. Pooling can also be a strategy for reducing the bias that can result from making too many comparisons. Another approach is to pool common intervention models or similar target populations as appropriate, based on what is learned about the interventions during implementation. This pooling would also be done to increase statistical power. It would provide additional suggestive evidence regarding the impacts of particular transitional job approaches for particular target populations, without sacrificing the information collected on the effectiveness of the individual programs taken one at a time. The research team and DOL will decide whether to pool results in future reports (overall or for clusters of programs) based on the pattern of implementation findings in this report.