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ABSTRACT

A study researched effects of the Parents' Fair Share (PFS) program on employment and earnings. In exchange for cooperation with the child support system, PFS offered services designed to help low-income noncustodial fathers (NCFs) find better and more stable jobs, become more involved and better parents, and pay support on a consistent basis. PFS effects were estimated for the full sample of fathers using data from the unemployment insurance (UI) system and for a subsample of fathers using data from a survey administered one year after entering PFS. Survey data described characteristics and circumstances of low-income fathers eligible for PFS. Results indicated that many fathers had unstable living arrangements; half did not complete high school; the majority had been arrested; a sizable proportion did not work in the year during which they were observed; and those who worked had fairly low earnings. UI and survey data presented PFS effects on fathers' employment and earnings. Findings were that PFS did not significantly increase employment or earnings during the two years after NCFs entered the project; it increased earnings among men who were "less

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employable"; and for more-employable men, PFS had little effect on average earnings and somewhat reduced employment among those in part-time, lower-wage jobs. (Appendixes contain survey response analysis, participation rate by sites, and 14 references.) (YLB)

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Parenting and Providing

The Impact of Parents' Fair Share on Paternal Involvement

Virginia Knox
Cindy Redcross

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Working and Earning

**The Impact of
Parents' Fair
Share on
Low-Income
Fathers'
Employment**

**John M. Martinez
Cynthia Miller**

MDRC

October 2000

Manpower Demonstration Research Corporation

This report is based on research conducted for the Parents' Fair Share Demonstration, a national demonstration project that combined job training and placement, peer support groups, and other services with the goal of increasing the earnings and child support payments of unemployed noncustodial parents (usually fathers) of children on welfare, improving their parenting and communication skills, and providing an opportunity for them to participate more fully and effectively in the lives of their children.

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- The Responsible Fatherhood Curriculum.* 2000. Eileen Hayes, with Kay Sherwood.
- Fathers' Fair Share: Helping Poor Men Manage Child Support and Fatherhood* (Russell Sage Foundation). 1999. Earl Johnson, Ann Levine, Fred Doolittle.
- Building Opportunities, Enforcing Obligations: Implementation and Interim Impacts of Parents' Fair Share.* 1998. Fred Doolittle, Virginia Knox, Cynthia Miller, Sharon Rowser.
- Working with Low-Income Cases: Lessons for the Child Support Enforcement System from Parents' Fair Share.* 1998. Fred Doolittle, Suzanne Lynn.
- Low-Income Parents and the Parents' Fair Share Demonstration.* 1998. Earl Johnson, Fred Doolittle. In *Fathers Under Fire: The Revolution in Child Support Enforcement*, edited by Irwin Garfinkel et al. (Russell Sage Foundation).
- Matching Opportunities to Obligations: Lessons for Child Support Reform from the Parents' Fair Share Pilot Phase.* 1994. Dan Bloom, Kay Sherwood.
- Child Support Enforcement: A Case Study.* 1993. Dan Bloom.
- Caring and Paying: What Fathers and Mothers Say About Child Support.* 1992. Frank Furstenberg, Jr., Kay Sherwood, Mercer Sullivan.

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Preface

For the past two decades, the nation's efforts to reform the welfare system and the child support system have often proceeded on separate tracks. Welfare reform has been focused on reworking the social contract between government and single mothers who received assistance from what was the Aid to Families with Dependent Children (AFDC) system and is now Temporary Assistance for Needy Families (TANF). Child support enforcement has been moving toward an increasingly standardized structure that enables states to collect support more effectively, particularly from men who are stably employed. As both systems have moved ahead, however, there has been a growing realization that neither has very explicitly considered how to work with the group of men who bridge them both: low-income noncustodial fathers whose children receive welfare. With this realization has come an array of new activities at the community, state, and federal levels aimed at building new supports for the efforts of low-income men to support, and father, their children.

These new efforts face the difficulty that, relative to research on single mothers and the programs that serve them, there is surprisingly little information available about how best to support the efforts of low-income fathers at providing for their children. What proportion of men whose children are on TANF can realistically be expected to provide substantial support for their children? How can TANF, child support, or the Workforce Investment system increase their capacity to do so? In what proportion of "single-parent" families receiving TANF are the fathers actually a significant presence in their children's lives, and how should this affect our thinking about how to work with these families?

The Parents' Fair Share (PFS) Demonstration, run from 1994 to 1996, was aimed at increasing the ability of these fathers to attain well-paying jobs, to increase their child support payments, and to increase their involvement in parenting in other ways. This report — one of two being issued concurrently from MDRC's evaluation of the Parents' Fair Share Demonstration — provides some important insights into these current questions by examining the effectiveness of the PFS approach at increasing fathers' employment and earnings.

First, a profile of the PFS population — men whose children were receiving AFDC, who were behind in their child support payments, and who were unemployed or underemployed — reveals that the men are a diverse group. Over half of the men did not complete high school, the majority had been arrested prior to entering the program, and many had unstable work histories. Low education levels and limited work experience, in particular, appear to be important barriers to finding and keeping jobs. Other men, in contrast, faced fewer employment barriers and had worked more consistently prior to entering the program. This profile points to the challenge of tailoring employment services to a population facing differing degrees of disadvantage.

The report also shows that although PFS did not increase employment and earnings for the full sample of fathers, it did achieve some success at increasing the earnings of men with more barriers to employment, in particular, those with low education levels and limited work experience. The results presented here differ from those presented in the interim report, in which there were few effects on employment and earnings, because they include men who entered the

program later in the intake period — there is some evidence to suggest that the program became more effective over time, as the coordination and content of the services were strengthened. The results also differ because this report uses data both from administrative records, covering employment reported to the Unemployment Insurance (UI) system, and from a survey given to the men one year after they entered the program. The survey data are an important complement to the records data, since many low-income men work in jobs, such as those in the informal economy, that are not fully reported to the UI system.

The findings presented here highlight the challenges ahead for designing programs to work with a diverse group of men. These include providing some men with the necessary help to overcome serious barriers to finding and keeping jobs and providing others, who may have fewer barriers to work, with the skills needed to find better jobs.

The PFS Demonstration has been supported by a group of forward-looking private foundations, federal agencies, and the participating states, which shared a vision that comprehensive welfare reform and antipoverty efforts should encompass both obligations and opportunities for low-income noncustodial fathers. The foundation and federal partners are listed at the front of this report. To them, the participating states and localities, and the staff and participants in each site who worked daily to reach the goals of the program and to support our research efforts, we are deeply grateful.

Judith M. Gueron
President

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This report is part of a final report series on the Parents' Fair Share (PFS) Demonstration that represents the culmination of a multi-year partnership at many levels. Those who operated the program in the seven sites supported this study in the most fundamental ways, through their willingness to keep their doors open to field researchers and through all their help in collecting necessary data. To the funders who have supported and been actively involved in this demonstration, all listed at the front of this report, we are deeply grateful as well.

Within MDRC, the report represents the work of a large group of collaborators. The authors would like to thank Gordon Berlin and Fred Doolittle for their thoughtful leadership throughout the PFS Demonstration and during the analysis and writing of this report, which also benefited greatly from the guidance of Virginia Knox. Greg Hoerz directed the survey effort at MDRC, with assistance from Gigi Taylor. George Cave played an important role in designing the survey instrument, and Abt Associates conducted the excellent survey tracking and fielding that resulted in unusually high response rates for a difficult-to-survey population. An advisory group including Irwin Garfinkel, Linda Mellgren, Daniel Meyer, and Judith Seltzer reviewed the design of the survey instrument and sampling strategies.

Throughout random assignment, Joel Gordon oversaw this complex enrollment process and managed the processing of data. Frank Yang assisted with random assignment and demographic data. Gaston Murray developed the PFS management information system, continuously refining it throughout the demonstration. Programming of the MIS was done by Maryno Demesier and Juanita Vega-Chetcuti. Margarita Agudelo collected and coordinated the processing of the administrative records data. Martin Gaynor developed the system for processing child support enforcement data. Charles Daniel, Joyce Dees, Donna George, Marguerite Payne, and Carmen Troche handled random assignment calls and entered data, with supervision from Shirley James.

MDRC colleagues Ute Appenzeller, Judith Greissman, Judith Gueron, Earl Johnson, Cindy Redcross, Sharon Rowser, and Evan Weissman provided helpful comments on an earlier draft. Irwin Garfinkel provided thoughtful comments and insights to help shape the analysis and report. We also extend our gratitude to the following individuals, who provided useful comments on a near-final draft of the report: Dennis Lieberman of the Office of Welfare to Work, U.S. Department of Labor; Kristen Hyatt of the U.S. Department of Agriculture; and Karl Koerper, Howard Rolston, and Larry Wolf, of the Administration for Children and Families of the U.S. Department of Health and Human Services. Finally, Kimberly Torres, Jevon Nicholson, and James Schumm provided research assistance at important stages of the work, and Jevon Nicholson ably coordinated the entire report process. Bob Weber edited the report, and Stephanie Cowell did the word processing.

The Authors

I. Introduction and Summary

Child support enforcement has increased dramatically over the past 10 years, with new policies continuing to be implemented at both the state and the federal levels to find “deadbeat dads” and make them pay. This increased enforcement is viewed as especially relevant in the context of welfare reform. Because benefits are time-limited in most states, nonwelfare sources of support, such as child support, will become increasingly important for low-income custodial mothers. However, the increase in enforcement comes at a time when many of the noncustodial fathers associated with these mothers may not be able to pay. Average earnings for men with low levels of education have fallen over the past two decades, with employment prospects relatively poorer for African-American and Hispanic men in urban areas (Gottschalk, 1997; Bound and Freeman, 1992). The economic status of these men raises doubts about whether stricter enforcement will provide more resources for low-income mothers.

Only very recently did the research and policy community begin to look more closely at fathers and how they interact with and are affected by the enforcement system (for example, see Garfinkel et al., 1998). This attention has been especially focused on low-income noncustodial fathers and programs that might increase their employment and earnings and help them become more involved in their children’s lives. The money made available through the Department of Labor’s welfare-to-work grant, for example, enables states to provide employment and training services for the hard-to-serve, which can include low-income noncustodial fathers.

Parents’ Fair Share (PFS) is an early example of such a program. PFS, a demonstration conducted in seven urban areas across the country, targeted unemployed or underemployed noncustodial fathers who had support orders in place but had not been paying child support. A key goal of the program was to increase the fathers’ ability to pay child support by increasing their employment and earnings. Fathers assigned to PFS were subject to enhanced child support enforcement (CSE), but they also received an array of employment and training services. A complementary goal of the program was to help the men become better and more involved parents. Interim results for an early cohort of fathers were published recently and indicated that although the program increased the child support payment rate, it did not produce statistically significant increases in the fathers’ employment or earnings (Doolittle et al., 1998). Employment and earnings data for the interim report were obtained from employers’ quarterly reports to the state unemployment insurance (UI) system.

This report presents a final and more comprehensive look at the program’s effects on employment and earnings. We estimate the effects of PFS for the *full sample* of fathers using data from the UI system and for a subsample of fathers (the *survey sample*) using data from a survey administered one year after they entered the program. The survey data are an important complement to the UI data: in addition to providing information on hours worked and employment stability, they capture earnings from jobs not covered or reported by the UI system. These types of jobs may be an important source of employment for low-income men. In addition, the survey provides us with a rare opportunity to present a picture of these men, given that few existing surveys are targeted specifically to noncustodial fathers. The survey also focuses on low-income minority men, a group typically underrepresented in national surveys.

The first half of the report uses the survey data to describe the characteristics and circumstances of low-income fathers eligible for PFS and to examine how these characteristics are associated with their ability to find and keep jobs. The results indicate that these men are disadvantaged in many ways. Many of the respondents had unstable living arrangements, half did not complete high school, and the majority had been arrested. Not surprisingly, a sizable proportion of the men did not work in the year during which we observed them, and those who did work had fairly low earnings. Low levels of education and limited work experience, in particular, seem to be important barriers to finding and keeping jobs. These factors may be important to consider when designing employment programs that are tailored to the needs of low-income fathers.

The second half of the report uses both UI and survey data to present the effects of PFS on fathers' employment and earnings. For the sample as a whole, the program did not significantly increase employment or earnings during the two years after they entered the program. However, it did increase earnings among men who might be characterized as "less employable" — those without a high school diploma and those with little recent work experience. For these men, particularly those who entered the program later in the evaluation, PFS increased the amount they worked during the year and helped them get better jobs, jobs that paid relatively high wages and offered some benefits. For more-employable men, the program had little effect on average earnings and somewhat reduced employment among those who would have worked in part-time, lower-wage jobs, perhaps by encouraging them to hold out for better jobs.

The results differ from those shown in the interim report for two reasons. First, the results presented here are for the full sample and for a subsample of fathers (the survey sample) who entered the program toward the end of the intake period, and the program had larger impacts on this later cohort, either because, for example, the program became more effective over time or the economy improved. The results from the interim report, in contrast, are for an early cohort. Second, the results differ because the analysis for this report is based not only on UI data but also on a survey that asked the men about earnings from all jobs. Many low-income men may work in jobs that are not fully reported to the UI system (because they work for cash, for example), so that the UI data are likely to miss some percentage of their earnings. The results highlight the importance of using both UI and survey data to assess the effects of programs for low-income populations.

The results are encouraging in that the program increased earnings among some fathers. However, they also point to the challenges of increasing employment among low-income men who on average have low levels of education and are only loosely connected to the labor market. One-quarter of the men assigned to the program, for example, did not work during the follow-up year (according to UI records and the survey). Helping such men find and keep jobs may require offering them a different and more intensive set of services from those offered through PFS.

This is one in a series of final reports on PFS. A companion report (Knox and Redcross, 2000) presents findings on the program's effects on noncustodial parents' visitation and involvement with their nonresident children, as well as on their provision of informal or in-kind support (all PFS publications are listed at the front of this report). A final monograph will bring together findings from the evaluation's ethnographic, implementation, and impact results to summarize the lessons from PFS for policymakers and program operators.

A. Background on Parents' Fair Share

The PFS Demonstration is designed to test the effects of a new approach to working with low-income noncustodial fathers. In exchange for their cooperation with the child support system, PFS offers services designed to help them find better and more stable jobs, become more involved and better parents, and pay support on a consistent basis. Noncustodial parents were eligible to be referred to the program if they (1) were under- or unemployed, (2) were not currently paying child support, and (3) owed support for children who were receiving or had received AFDC. Participation in PFS services was mandatory, and child support orders were lowered during the period in which fathers participated. Child support obligations were restored to an appropriate level once fathers found a job. CSE staff restored child support orders to their pre-PFS level for those who stopped cooperating with PFS program requirements.

Program services were built around four core components: peer support, structured around a Responsible Fatherhood Curriculum; employment and training services; enhanced child support enforcement; and mediation. Sites were encouraged to offer a wide array of employment and training services, such as job search assistance, skills training, education services, and on-the-job training slots.

The demonstration began in 1992 with a two-year pilot phase designed to test the feasibility of implementing the PFS model. A full-scale evaluation of the program began in early 1994 in seven sites across the country: Los Angeles, California; Jacksonville, Florida; Springfield, Massachusetts; Grand Rapids, Michigan; Trenton, New Jersey; Dayton, Ohio; and Memphis, Tennessee. Between March 1994 and June 1996, 5,611 noncustodial parents who were deemed eligible for PFS were randomly assigned to either a PFS group or a control group. Those assigned to the PFS group were eligible for PFS services, while those assigned to the control group were subject to standard enforcement practices. The effects of referral to PFS versus traditional enforcement can be estimated by comparing outcomes over time for the two groups.

An interim report was published in 1998 examining the program's implementation across the seven sites and its effects after 18 months on child support payments, employment, and earnings for an early cohort of fathers, those who entered the evaluation before July 1995. Data on employment and earnings were obtained from each state's UI system, and data on child support were obtained from each state's CSE agency. Although PFS increased the percentage of fathers who paid child support, it did not produce consistent and statistically significant increases in employment rates or earnings. The report offered several reasons for the lack of employment and earnings effects. For example, many of the men faced what would appear to be significant barriers to employment, such as having an arrest record or lacking a high school diploma. In addition, although a fairly high percentage of men participated in at least one PFS activity, most sites were not able to offer on-the-job training slots or skills training activities.

B. Data and Methods

The random assignment design of the PFS evaluation provides a simple, yet reliable, way to estimate its effects. The full sample consisted of 5,611 noncustodial fathers who appeared at a case review hearing or another review of their child support status and met the eligibility criteria mentioned earlier. Half of these parents were randomly assigned to be referred to PFS services and were subject to the program's mandates, and the other half were assigned to a control group and subject

to standard enforcement procedures. Because fathers were assigned to one of the two groups at random, there should be no systematic differences between the groups in terms of background characteristics, including prior earnings and child support payments. Any differences between the two groups that emerged after the period of random assignment can be attributed to PFS. Program impacts are estimated as the difference in outcomes between the two groups.

Several data sources are used for the evaluation. A Background Information Form providing demographic data was filled out for all fathers at the time of random assignment, or program entry. Earnings and employment data were provided by each state's UI system. These records contain information on quarterly employment and earnings for all jobs covered by the UI system. The effects of PFS on fathers during the two years after random assignment, presented in Section II of the report, are estimated using the UI data for the full sample.

The effects of PFS are also estimated for the survey sample. A survey, covering such topics as child support payments, visitation, employment and earnings, and living arrangements, was administered to a subset of the full sample 12 months after program entry. The survey was administered to a random subsample within each site of fathers who entered the evaluation between October 1995 and February 1996 (making it a subset of a relatively late entry cohort). The survey achieved a response rate of 78 percent, for a total of 553 fathers.¹ The effects of PFS on additional employment outcomes, available from the survey, are shown in Section III for the survey sample.

II. Low-Income Fathers

This section presents data on the characteristics and circumstances of the low-income fathers found eligible for referral to PFS. To be found eligible, fathers had to be under- or unemployed, have a child support order in place but not be consistently making payments, and owe support for a family who were receiving or had received welfare. Because of these eligibility criteria, this sample is not strictly representative of all low-income noncustodial fathers. We focus on the fathers assigned to the control group, or those not referred to services, in order to capture what the men's circumstances would have been in the absence of PFS.

A. Demographic Characteristics

Table 1 presents selected characteristics of the fathers. Most characteristics were measured at the time of the 12-month survey; those measured at program entry (or the point of random assignment) are marked with an asterisk. The sample comprises relatively young men (73.5 percent were under age 35), and 60.3 percent had never been formally married. In addition, 59.6 percent of the men are African-American, and 23.2 percent are Hispanic.

At the time of the survey, 36.9 percent of the sample reported living alone; 23.0 percent reported living with parents; and 24.8 percent reported living with a spouse or partner,² although only

¹Since some sites are represented differently in the survey sample than in the full sample, all analyses using survey data are weighted to make the survey sample representative of the full sample.

²Those who lived with a spouse or partner may also have lived with their own children and/or their partner's children.

Table 1
Parents' Fair Share

Demographic Characteristics of Control Group Members
(Survey Sample)

Characteristic	Percentage
Age*	
Under 25	26.8
26 - 34	46.7
35 or over	26.4
Race/ethnicity*	
White, non-Hispanic	14.8
African-American	59.6
Hispanic	23.2
Other	2.3
Marital status	
Never married	60.3
Currently married	12.4
Separated or divorced	27.4
Living arrangements	
Lives alone	36.9
Lives with spouse or partner	24.8
Lives with parents	23.0
Other	15.3
Housing	
Current status	
Owns home	5.8
Rents home	29.3
Lives with family and friends and contributes to rent	31.5
Lives with family and friends and does not contribute to rent	7.9
Other	25.5
Housing stability	
Among those who own, rent, or contribute to rent	
Stayed in 3 places or more since random assignment	21.5
Slept in shelter, car, or public place in last 3 months	5.7
Among those with other arrangements	
Stayed in 3 places or more since random assignment	26.1
Slept in shelter, car, or public place in last 3 months	13.6
Education*	
No high school diploma or GED	49.5
High school diploma or GED	49.9
Associate's degree or higher	0.6

(continued)

Table 1 (continued)

Characteristic	Percentage
Health	
Rates health as good or excellent	73.6
Reported disability ^a	12.4
Reported drug use in past month	13.7
Reported alcohol use in past month	31.7
Arrest and conviction history	
Arrested and charged with a crime since random assignment	31.6
Drug-related	24.8
Driving without a license	19.6
Other	55.6
Convicted of criminal offense since age 16*	68.5

Sample size = 261

SOURCES: MDRC calculations from the noncustodial parent survey and the PFS Background Information Form.

NOTES: The data source for characteristics marked with an asterisk (*) is the PFS Background Information Form. Otherwise, the data source is the noncustodial parent survey.

^aA noncustodial parent is considered disabled if he lists ill health or disability as a reason for not looking for work, receives SSI, or reports being bedridden for 30 days or more.

12.4 percent were married. This suggests that while very few of the fathers were in a formal marriage at the same time of the survey, many shared a household with a significant other and may have been supporting other children.

Data on residential status show that fathers' living situations at a point in time (the survey) mask a considerable amount of mobility. While 66.6 percent of the sample reported contributing to housing expenses either by owning or renting their own home or by living with family or friends and contributing to rent, 21.5 percent of these men had stayed in three places or more since random assignment, and 5.7 percent had stayed in a shelter, car, or public place in the three months before the survey. Housing was less stable among those who did not contribute to rent (7.9 percent) or reported some other type of living arrangement (25.5 percent); 26.1 percent of these men had stayed in three places or more since random assignment, and 13.6 percent had slept in a shelter, car, or public place in the preceding three months.

This lack of stability in housing may hinder men's ability to enter the labor market and stay employed. Research based on interviews with a small group of PFS fathers (Johnson, 1999) suggests the ways in which this can occur. Men with unstable housing, for example, were often unable to give a permanent address and phone number to prospective employers and frequently relied on beeper numbers. The lack of a residence made it more difficult for employers to contact them and also made the men themselves feel less comfortable seeking employment, knowing that they would be unable to provide permanent contact information. Johnson also found that the lack of stable housing reduced the fathers' opportunity for ongoing social contact, an important link to potential employers.

Table 1 also suggests that these men faced what could be important barriers to employment: 49.5 percent of the fathers did not have a high school diploma or General Educational Development (GED) certificate at program entry, and 68.5 percent reported having been convicted of a criminal offense since age 16. In addition, 31.6 percent had also been arrested in the year after program entry. The most common reasons given for the more recent arrests were drug-related charges and driving without a license.³ Regardless of the reason for arrest, employers may be reluctant to hire candidates with criminal records.

Data on health status indicate that although 73.6 percent of the fathers rated their health as good or excellent, 12.4 percent reported some type of disability, a potential barrier to employment.⁴ And 13.7 percent of the sample reported using some type of drug in the month before the survey (80 percent of this was marijuana use); 31.7 percent reported alcohol use in the past month. The rate of drug use is roughly comparable to the extent of drug use reported in other research on young men. An analysis of the 1988 National Longitudinal Survey of Youth (NLSY) data found that 17.6 percent of young single men reported using cocaine in the past year and 33.4 percent reported using marijuana (Kaestner, 1994). Drug or alcohol use may pose a barrier to getting or keeping a job. Some of the PFS fathers, for example, did not get jobs because they failed employer drug tests (Johnson, Levine, and Doolittle, 1999).

³Other reasons listed included drunkenness (9 percent), aggravated assault (6 percent), and unpaid fines (6 percent).

⁴Fathers were classified as disabled if they listed ill health or disability as a reason for not looking for work, received SSI, or reported being bedridden for 30 days or more in the past year.

B. Employment and Earnings

Table 2 presents data on the control group fathers' employment and earnings during the 12 months after program entry,⁵ based on both UI administrative records data and the survey. An individual was counted as employed if he reported on the survey that he had held at least one job since random assignment. The UI records, in contrast, consist of employer reports to the state UI agency. All employers subject to the state UI tax are required to report employee earnings on a quarterly basis. Although these data will cover most civilian employees, earnings reports are not required, for example, for self-employed individuals, most independent contractors, military employees, and federal government employees. In addition, the UI records will miss earnings for individuals who work "off the books" or for cash, for those who work out of state (since records are collected at the state level), and for those who work for employers who fail to fully report employee earnings. Employers have incentives to underreport both employment and earnings, since UI taxes are based on employee earnings. A recent study in Illinois found that 13 percent of UI-eligible workers were not reported to the state UI agency by their employers in 1987 (Blakemore et al., 1996). Thus, the UI data are expected to miss some percentage of employment, and there are several reasons to suspect that the percentage missed may be larger for the PFS sample. Blakemore et al. found that the types of firms that are more likely to underreport (for example, smaller firms and firms with high turnover rates) are those that may be most likely to employ lower-skilled workers.

The survey data, however, also have limitations: respondents might have failed to remember relatively short spells of employment, and they might have had an incentive to underreport employment or earnings. They might have underreported earnings, for example, if they thought that the information would be used for child support collection purposes (although the survey interviewers took care to explain that the information given to them would not be reported).

On the survey, 70.0 percent of the sample reported having worked in the year after random assignment. In contrast, according to UI records 80.5 percent of this sample were employed at some point during the year after random assignment. Average annual earnings during the year were \$5,449 according to UI records and \$5,894 according to the survey.⁶ (These averages include zeros for those fathers who did not work during the year.) The fact that survey-reported earnings are higher than UI-reported earnings is not surprising and is consistent with other research (Kornfeld and Bloom, 1999). However, it is somewhat surprising that UI-reported employment is higher than survey-reported employment, since we would expect UI records to miss some jobs. Kornfeld and Bloom found that survey-reported employment rates tend to be higher than UI-reported rates, although the difference is small compared with the difference in earnings. This pattern of results might arise because of the incentive that PFS survey respondents had to

⁵Although most of the sample was surveyed close to 12 months after random assignment, some fathers were interviewed more than 15 months after random assignment. To make the time period comparable for all fathers, this section refers to only the first 12 months after program entry and does not include any employment that may have occurred after 12 months.

⁶Annual earnings are calculated from the survey using the average of the starting and ending wage at each job, hours worked per week, and the number of months employed. Earnings are included only for jobs held during the first 12 months after random assignment.

Table 2
Parents' Fair Share
Employment, Earnings, and Job Search Activities
for Control Group Members
(Survey Sample)

Measure	Percentage or Dollars
Employed within 12 months of random assignment (%)	
Reported on survey	70.0
UI administrative records	80.5
Earnings during the first 12 months after random assignment (UI records)^a	
Average earnings (\$)	5,449
Earnings during the first 12 months after random assignment (survey)	
Average earnings (\$)	5,894
Average earnings among workers (%)	
\$1 - \$1,000	8.0
\$1,001 - \$5,000	29.5
\$5,001 - \$10,000	32.7
\$10,001 - \$15,000	16.0
Over \$15,000	13.9
Average earnings among those employed (\$)	8,204
Characteristics of most recent job, for those who were employed (n=180)	
Average hourly wage (\$)	7.10
Average weekly hours (%)	
1 - 19	3.9
20 - 34	23.6
35 - 49	61.2
Over 50	10.7
Employer-provided benefits (%)	
Paid sick days	18.4
Paid vacation	28.2
Health insurance	27.3
Job search activities if not employed at survey (n=126) (%)	
Looking for work	63.9
Method of job search, for those looking	
Read and/or answer ads	86.3
Apply directly	90.0
Ask friends	80.5
Check public/state employment agency	39.0
Check private employment agency	23.8
Check school/training program	11.4
Hours in last month spent looking for work	
Less than 20	36.6
21-40	32.7
Over 40	30.6

(continued)

Table 2 (continued)

Measure	Percentage or Dollars
If not employed at survey, and not looking for work, reason: (n=46) (%)	
Ill health, disability, or other personal handicap	24.7
In jail	23.9
Other	51.4
Sample size = 261	

SOURCES: MDRC calculations from the noncustodial parent survey and UI earnings records.

NOTE: ^aTotal sample size for this measure is 251; Springfield is not included because only 3 quarters of follow-up are available for the full sample.

underreport employment. However, it might also arise if men failed to recall and report short spells of employment that were recorded by the UI data. (Note that, with the UI data, a father is counted as employed if he worked for only a few days within the quarter.) A separate analysis (not reported) found evidence to support the latter hypothesis — UI earnings were much lower for fathers who were employed according to UI records but did not report employment on the survey than for fathers who were employed according to both sources, and fathers were less likely to have worked in all four follow-up quarters according to the UI records. Kornfeld and Bloom found similar results; the employment reported by the UI data but not reported by survey respondents tended to be low-earnings employment, suggesting that it consisted of short-term or minor jobs.

Among those who worked during the year, average earnings were \$8,204. Nearly 40 percent of those who worked earned less than \$5,000. Not surprisingly, average earnings for this sample are much lower than estimates of the average incomes of all nonresident fathers.⁷ For example, Garfinkel et al. (1998) used National Survey of Families and Households (NSFH) data to obtain earnings estimates ranging from \$26,864 to \$28,832 (in 1995 dollars). They also found that 20 percent of nonresident fathers had income under \$6,000. Sorenson (1997) used NSFH and Survey of Income and Program Participation data to obtain estimates of \$23,505 and \$23,070 (in 1996 dollars). These estimates and the numbers in Table 2 illustrate that not all fathers are “deadbeat dads.” Many men cannot afford to pay child support.

Table 2 also presents data on the current or most recently held job (as of the 12-month survey) for men who worked. On average, they earned \$7.10 per hour, and 71.9 percent worked full time; 18.4 percent of those who worked were provided with paid sick days, 28.2 percent with paid vacation, and 27.3 percent with health coverage. Not surprisingly, these benefits were more often provided to full-time workers. However, even those who worked full time were less likely than other workers to receive benefits: nationally, over 60 percent of employees received employer-provided health insurance in 1996, and over 50 percent received paid sick days (U.S. Bureau of Labor Statistics, 1998).

The panel of Table 2 that reports on job search activities for those who were not employed at the time of the survey shows that 63.9 percent said that they were currently looking for work. The most frequently cited methods of job search included reading and/or answering want ads, applying directly to employers, and asking friends. Among those who were not employed but reported that they were not looking for work (about 17 percent of the full sample), nearly half may have been unable to work: 24.7 percent reported ill health or disability as the reason for not looking, and 23.9 percent reported being in jail.

Table 3 presents data for several subgroups. Most of the differences across subgroups are not statistically significant, owing in part to small sample sizes. Significant differences are noted

⁷Calculations of fathers' incomes are considered estimates because the national surveys used for this purpose are thought to miss a substantial number of fathers. Reasons for this undercount include the fact that some men do not identify themselves as nonresident fathers and the fact that some men, primarily low-income and minority men, are underrepresented in national surveys. Both Garfinkel et al. and Sorenson used statistical adjustments to correct for this undercount.

Table 3
Parents' Fair Share
Characteristics of Noncustodial Parents, by Subgroup
(Survey Sample)

Characteristic	African-American	Hispanic	White	High School Diploma	No High School Diploma	Arrest Prior to		
						Random Assignment	No Arrest Prior to Random Assignment	
Race/ethnicity (%)								
African-American	100.0	0.0	0.0	71.0	58.5	69.3	54.6	
Hispanic	0.0	100.0	0.0	14.5	22.9	11.6	34.6	
White ^a	0.0	0.0	100.0	14.5	18.6	19.1	10.8	
Education, arrest, and drug use (%)								
With high school diploma	53.0	43.7	46.5	100.0	0.0	49.8	52.0	
With prior arrest	74.9	47.4	80.4	68.9	72.4	100.0	0.0	
Arrested since random assignment	34.0	18.6	36.5	30.8	31.9	36.9	17.9	
Reported any drug use	14.1	8.0	15.9	13.8	12.9	15.2	8.8	
Employment and earnings (survey)								
Employed within 12 months of random assignment (%)	66.3	73.7	78.4	78.4	61.0	69.2	71.8	
Hourly wage ^b (\$)	6.88	7.21	7.18	6.89	7.02	6.91	7.00	
Earnings (\$)	4,963	6,346	8,762	6,474	5,053	5,612	6,231	
Last employment 6 to 11 months prior to random assignment (%)	12.0	13.4	14.7	15.2	10.5	14.0	10.3	
Last employment 12 months or more prior to random assignment (%)	28.7	20.9	8.7	20.4	27.4	24.8	21.4	
Sample size	167	49	45	128	133	185	76	

SOURCES: MDRC calculations from the noncustodial parent survey and the PFS Background Information Form.

NOTES: ^aThis category includes five respondents who fell into the "other" category.

^bThis category includes only those who were employed within 12 months after random assignment.

in the text. Columns 1–3 present characteristics by race/ethnicity.⁸ A greater proportion of African-American fathers than Hispanic or white fathers had a high school diploma or GED. A greater proportion of white fathers reported being arrested prior to random assignment and being arrested since random assignment. Hispanic men were the least likely of the three groups to have been arrested before or after random assignment. Hispanic fathers were also least likely to report drug use in the month prior to the survey. African-American fathers had lower wage rates and earnings than the other two groups, and they were the least likely to have had recent work experience prior to random assignment.

Columns 4 and 5 present data by education level. Not surprisingly, the key differences among these groups relate to employment and earnings. Those with a high school diploma were more likely than those without one to have worked since random assignment, and they had higher average earnings. Men with a diploma were also less likely to have been unemployed for more than 12 months prior to random assignment (20.4 percent versus 27.4 percent).

Columns 6 and 7 present arrest history data. The data on drug use and arrests after random assignment show that these behaviors are correlated to some degree: 36.9 percent of fathers with a prior arrest had been arrested since random assignment, compared with 17.9 percent of those without a prior arrest; and 15.2 percent of those with a prior arrest reported drug use in the past month, compared with 8.8 percent without a prior arrest.

Although 70 percent of the fathers reported working during the 12 months after random assignment, this overall employment rate does not indicate how long the average father stayed employed, for example, or whether his employment during the year consisted of several spells of unemployment. When tailoring program services to the needs of low-income fathers, it is important to know whether their employment problems are characterized by employment instability (moving from short-term job to short-term job) or long spells of unemployment (difficulty finding a job).

Number of months employed during the year is a simple way to illustrate employment over the period. Figure 1 shows the percentages of men in the survey sample who were employed for between zero and 12 months of the 12-month period. The figure presents only a rough measure of employment dynamics, since we cannot observe fathers beyond the 12-month period. For example, fathers who got jobs in the tenth month after random assignment and stayed employed until the survey (at 12 months) are defined as employed for two months, even though they may have stayed employed for longer. Nonetheless, the figure illustrates that a significant proportion of the men in the sample (30.3 percent) never got a job during the period or were employed for zero months and that almost as many (22.1 percent) were employed for the entire year.

C. Factors Associated with Employment Outcomes

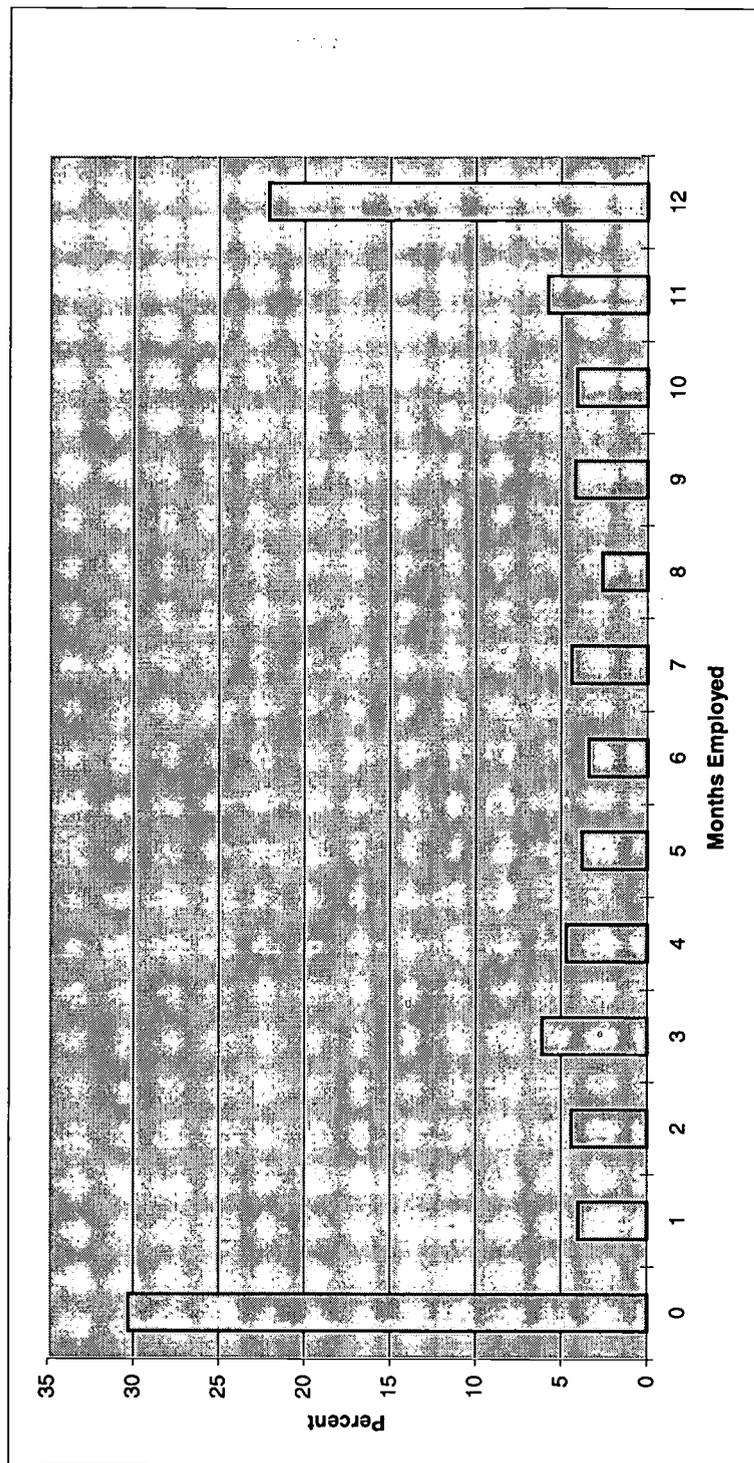
The previous tables presented data on the characteristics and circumstances of the fathers and suggested that many of them faced important barriers to employment. Table 4 addresses this issue in a multivariate regression context by estimating the effect of fathers' characteristics on the likelihood that they worked during the year after program entry. These estimates are presented for

⁸The "white" category includes five respondents who classified themselves as American Indian, Asian/Pacific Islander, or other.

Figure 1

Parents' Fair Share

**Number of Months Employed During the 12 Months
After Random Assignment
(Survey Sample)**



SOURCE: MDRC calculations from the noncustodial parent survey (sample size = 524).

Table 4
Parents' Fair Share
Effects of Noncustodial Parents' Characteristics
on the Likelihood of
Working After Random Assignment
(Survey Sample)

Characteristic	Likelihood of Working
Employment	
Any employment in the 9 months prior to random assignment (UI records)	-.689 (.730)
Last employment less than 6 months prior to random assignment (omitted)	---
Last employment 6 to 11 months prior to random assignment	-.067 (.083)
Last employment 12 months or more prior to random assignment	-.356 *** (.083)
Race/ethnicity*	
African-American	-.035 (.087)
Hispanic	.026 (.110)
White (omitted)	---
Age*	
Under 26 (omitted)	---
26 - 34	-.011 (.071)
35 or over	-.168 * (.088)
Education*	
No diploma at random assignment	-.136 ** (.057)
Number of children	
Has 1 child (omitted)	---
2 or 3	-.015 (.064)
4 or more	-.122 (.095)
Marital status	
Never married	-.105 (.074)

(continued)

Table 4 (continued)

Characteristic	Likelihood of Working
Prior arrest	
Non-CSE arrest since age 16*	.035 (.063)
Living arrangements	
Lives with parents	.061 (.067)
Lives with spouse (and children if any)	.088 (.074)
Other living arrangement (omitted)	--- ---
Drug use	
Used marijuana within 1 month of survey	-.206 ** (.094)
Used other drugs within 1 month of survey ^a	-.068 (.145)
Site	
Dayton	-.071 (.124)
Grand Rapids	-.059 (.108)
Jacksonville	.116 (.115)
Los Angeles (omitted)	--- ---
Memphis	-.091 (.123)
Springfield	-.060 (.124)
Trenton	.186 (.128)

Sample size = 261

SOURCES: MDRC calculations from the noncustodial parent survey, the PFS Background Information Form, and UI earnings records.

NOTES: Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent. Standard errors are in parentheses.

The data source for characteristics marked with an asterisk (*) is the PFS Background Information Form.

Otherwise, the data source is the noncustodial parent survey.

The r^2 for this model is .2642

^a"Other drugs" include cocaine, crack, and heroin.

the control group only. The regression framework allows us to estimate the association between a given characteristic and the likelihood of working that is independent of the effects of other factors. Table 3, for example, shows that African-American fathers were less likely than other fathers to have worked during the year. However, this association may be due to the fact that these fathers also tended to have less recent work experience than other men. The regression in Table 4 allows us to estimate the association between race/ethnicity and employment, accounting for differences in work experience.

Each coefficient shows the increase or decrease in the likelihood of employment if the father possesses that particular characteristic, relative to the likelihood of employment for a father with the “omitted” characteristic. For example, the results indicate that employment experience prior to random assignment has a statistically significant effect on employment after random assignment — fathers who had not worked in the 12 months prior to random assignment were less likely to have worked after random assignment than fathers who had worked within six months prior to random assignment. Coefficients that are statistically significant are noted with asterisks.

Fathers without a high school diploma were less likely to have worked than those with a diploma, older fathers were less likely to have worked than younger fathers, and those who reported drug use were less likely to have worked. Drug use was measured as of the survey, and thus at the same time as employment, so the direction of causality cannot be determined. Prolonged unemployment could cause drug use, for example. On the other hand, drug use in the month preceding the survey may reflect a longer pattern of use, suggesting that drug use per se may have caused unemployment. Controlling for other factors, African-American and Hispanic fathers were not less likely to work than white fathers. Finally, there are no significant differences across the sites in the likelihood of employment.

Table 5 examines the effects of fathers’ characteristics on employment transitions. The amount of time fathers spent unemployed during the year depended on the rate at which they moved from unemployment to employment and from employment to unemployment. The rate of transition to employment influences the duration of unemployment spells. The rate of transition from employment to unemployment, on the other hand, affects the length of employment spells and addresses the issue of job retention.

The models present estimates of the effects of fathers’ characteristics on the rate of transition between these two states. The dependent variable is the monthly probability of moving from one state to the other. Column 1 presents the results of transitions from unemployment to employment; they are similar to results shown in Table 4.⁹ The coefficients indicate that men without recent work experience and men without a high school diploma were less likely than their counterparts to move to employment in a given month. The coefficients on the site variables indicate that fathers in Memphis were less likely to work in a given month than those in Los Angeles (the omitted category). The coefficient on Dayton just misses significance at the 10 percent level. These differences across sites may reflect aspects of the local economies or differences in characteristics

⁹The models are not expected to provide identical results, since they are measuring somewhat different outcomes. The monthly transition model estimates the probability of gaining employment next month among the sample of fathers who were unemployed this month, regardless of their employment status in earlier months. Thus, employment transitions in this model also include gaining employment after a recent job loss.

Table 5
Parents' Fair Share
Effects of Noncustodial Parents' Characteristics
on Monthly Employment Transitions
(Survey Sample)

Characteristic	From Unemployed to Employed ^a	From Employed to Unemployed ^b
Employment		
Any employment in the 9 months prior to random assignment (UI records)	4.39 (1.87)	-1.30 (1.75)
Last employment less than 6 months prior to random assignment (omitted)	---	---
Last employment 6 to 11 months prior to random assignment	-.007 (.022)	.013 (.021)
Last employment 12 months or more prior to random assignment	-.055 *** (.021)	-.011 (.020)
Race/ethnicity*		
African-American	.016 (.024)	.018 (.019)
Hispanic	.013 (.032)	-.003 (.021)
White (omitted)	---	---
Age*		
Under 26 (omitted)	---	---
26 - 34	-0.27 (-0.17)	-0.42 *** (.016)
35 or over	-.043 ** (.021)	-.033 (.020)
Education*		
No diploma at random assignment	-.040 *** (.015)	.023 * (.013)
Number of children		
Has 1 child (omitted)	---	---
2 or 3	-.021 (.017)	-.006 (.015)
4 or more	-.032 (.024)	-.015 (.022)
Marital status		
Never married	-.006 (.020)	.008 (.017)

(continued)

Table 5 (continued)

Characteristic	From Unemployed to Employed	From Employed to Unemployed
Prior arrest		
Non-CSE arrest since age 16* (at random assignment)	.019 (.017)	-.001 (.014)
Living arrangements		
Lives with parents (at random assignment)	-.031 * (.017)	-.013 (.015)
Lives with spouse (and children if any) (at random assignment)	.011 (.020)	-.005 (.016)
Other living arrangement (omitted)	---	---
Drug use		
Used marijuana within 1 month of survey	-.017 (.023)	.004 (.025)
Used other drugs within 1 month of survey ^c	-.034 (.034)	.047 (.041)
Site		
Dayton	-.060 * (.033)	.052 * (.029)
Grand Rapids	-.038 (.029)	.003 (.023)
Jacksonville	.013 (.032)	-.006 (.025)
Los Angeles (omitted)	---	---
Memphis	-.059 * (.031)	-.004 (.031)
Springfield	.029 (.031)	.025 (.024)
Trenton	.030 (.036)	.036 (.027)

Sample size = 261

SOURCES: MDRC calculations from the noncustodial parent survey, the PFS Background Information Form, and UI earnings records.

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

Standard errors are in parentheses.

Also included in the model is a variable measuring the number of spells of employment (unemployment) the father experienced prior to the current spell.

The data source for characteristics marked with an asterisk (*) is the PFS Background Information Form. Otherwise, the data source is the noncustodial parent survey.

^aThe r^2 for this model is .0401

^bThe r^2 for this model is .0277

^c"Other drugs" include cocaine, crack, and heroin.

of the fathers across the cities that are not accounted for in the model. As noted earlier, these are associations and do not indicate causality.

Column 2 shows transitions from employment to unemployment.¹⁰ Education level has a statistically significant effect on the rate of leaving employment — men without a high school diploma were more likely to leave employment in a given month than their more educated counterparts. Prior work experience, on the other hand, affects the likelihood of getting a job but not the likelihood of losing a job; men with less experience were no more likely than other men to leave employment in a given month.

This section has described the sample of low-income noncustodial fathers who met the eligibility criteria for PFS. Most of the fathers are African-American or Hispanic and were under age 30, and most had never been formally married. A look at a range of other characteristics suggests that their employment prospects were dim. For example, many had unstable housing arrangements, nearly 25 percent had little recent work experience, 50 percent did not have a high school diploma, and almost 70 percent had prior involvement with the criminal justice system. Perhaps not surprisingly, nearly 30 percent of the fathers did not work in the year after random assignment, and average earnings for those who did work were fairly low.

We also examined the associations between fathers' characteristics and employment outcomes and found that low levels of education, limited work experience, and drug use appear to be important barriers to getting jobs. In addition, less-educated men seem to have more difficulty staying employed once they find jobs. Although it is difficult to determine whether these relationships are causal (less-educated men, for example, may be less likely to find work for reasons we were not able to capture), they are informative, and they are consistent with findings from other research. For example, Ballen and Freeman (1986) found that more-educated men are more likely than less-educated men to move to employment. In addition, Holzer and Lalonde (1998) found that high school nongraduates were more likely than graduates to leave jobs, and this difference was partly driven by differences in cognitive ability (proxied by test scores). Thus, less-educated men may get screened out of jobs because of low skill levels, but they also may be less able to adapt to changes in the work environment or to deal with workplace conflicts. Programs that work with low-income fathers might consider focusing on lack of education and limited prior work experience as barriers to employment and devising specific services to address these problems.

III. The Effects of PFS on Employment and Earnings

As noted earlier, random assignment allows us to estimate program effects by comparing outcomes over time for the PFS and control groups. To estimate impacts on a variety of employment outcomes, we use data from both the UI administrative records, available for the full sample of 5,611 fathers, and the survey, available for the survey sample of 553 fathers.

¹⁰Our data do not capture the reason for job loss.

A. Impacts for the Full Sample Using UI Administrative Records

Table 6 presents impacts for the full sample for two years, or eight quarters, after random assignment. The full sample results do not include the Springfield sample, because the data available from this site are limited to only three quarters of follow-up. The results for Springfield are shown in the site-specific analysis.

The numbers indicate no significant differences. The upper panel of Table 6 shows the percentage of fathers employed in each follow-up quarter, and the lower panel shows average quarterly earnings. The outcomes for the control group show the typical pattern of employment and earnings for these fathers that would have occurred in the absence of PFS. In the second quarter (months 4–6) after random assignment, for example, 50.9 percent of control group members worked, and their average earnings (including zeros for those who did not work) were \$1,216. Although employment increased somewhat from quarters 1 to 8 (from 49.9 percent to 51.9 percent), average earnings increased by a substantial amount (from \$1,011 to \$1,606). Further analyses (not shown) indicated that the increase in earnings was driven by an increase in earnings over time for men who worked consistently over the entire period.

A comparison of program and control group employment and earnings outcomes shows that PFS did not increase the employment or earnings of the program group by a statistically significant amount. In quarter 7, for example, 52.4 percent of the PFS group worked, compared with 52.8 percent of the control group.

Table 7 presents impacts for each site. Although sample sizes are fairly small at the site level, the programs varied enough across the sites to warrant a look at their impacts separately (see Doolittle et al., 1998, for information about implementation across sites). In addition, a site-specific analysis provides information on the employment and earnings levels of the control group fathers in each area, showing to some extent the hurdles that each program faced. Data for the control groups show that employment and earnings did vary across the sites, owing to differences in local labor market conditions or to differences in the types of fathers in the sites. Control group members in Los Angeles, Dayton, and Trenton, for example, tended to have lower employment rates than those in Grand Rapids and Jacksonville. Fathers in Memphis earned substantially less than fathers in the other sites: in quarters 1–4, control group fathers in Memphis earned an average of \$3,591, compared with \$4,265 to \$5,945 in the other sites.

Aside from a few quarters with statistically significant impacts, PFS did not consistently or significantly affect employment or earnings in most sites, with the exceptions of Grand Rapids and Dayton.¹¹ The program in Grand Rapids increased earnings by a statistically significant amount in the later quarters. Comparing earnings in quarters 5–8 of follow-up shows that program group members earned an average of \$6,691 and control group members earned \$5,913, for a statistically significant difference of \$778. There was no corresponding increase in employment rates. In Dayton, on the other hand, the program increased both employment rates and earnings in the early quar-

¹¹These were also two of the three sites (with Los Angeles) that were found in the interim report to have positive effects on child support payments, suggesting that generally strong program implementation may affect more than one outcome or that child support payments and earnings may affect each other.

Table 6
Parents' Fair Share
Impacts of PFS on Employment and Earnings
for All Sites Combined^a
(Full Sample)

Outcome	Program Group	Control Group	Impact
Employed (%)			
Quarter 0 ^b	46.3	47.8	-1.5
Quarter 1	50.9	49.9	0.9
Quarter 2	51.6	50.9	0.7
Quarter 3	51.4	51.9	-0.6
Quarter 4	52.1	51.6	0.5
Quarter 5	51.8	52.7	-0.9
Quarter 6	52.9	52.3	0.5
Quarter 7	52.5	52.8	-0.3
Quarter 8	52.0	51.9	0.0
Quarters 1-4	72.0	70.4	1.6
Quarters 5-8	70.3	69.6	0.7
Average earnings (\$)			
Quarter 0 ^b	721	742	-21
Quarter 1	1,036	1,011	24
Quarter 2	1,202	1,216	-13
Quarter 3	1,304	1,277	27
Quarter 4	1,385	1,371	14
Quarter 5	1,453	1,454	-1
Quarter 6	1,565	1,485	80
Quarter 7	1,587	1,533	54
Quarter 8	1,634	1,606	28
Quarters 1-4	4,928	4,876	52
Quarters 5-8	6,239	6,078	161
Sample size	2,525	2,495	

SOURCES: MDRC calculations from UI earnings records and the PFS Background Information Form.

NOTES: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

^aSpringfield is not included because only 3 quarters of follow-up are available for the full sample.

^bQuarter of random assignment.

Table 7
Parents' Fair Share
Impacts of PFS on Employment and Earnings,
by Site
(Full Sample)

Outcome	Dayton			Grand Rapids			Jacksonville			Los Angeles		
	Program Mean	Control Mean	Impact	Program Mean	Control Mean	Impact	Program Mean	Control Mean	Impact	Program Mean	Control Mean	Impact
Employed (%)												
Quarter 0 ^a	48.1	47.9	0.2	51.5	53.3	-1.8	62.0	63.7	-1.7	34.8	37.6	-2.8
Quarter 1	57.3	49.8	7.4 **	55.0	53.4	1.6	67.9	64.9	2.9	39.4	39.3	0.1
Quarter 2	55.1	50.5	4.7	54.0	56.4	-2.4	62.6	61.5	1.2	42.7	42.5	0.2
Quarter 3	51.9	52.5	-0.5	55.3	56.3	-1.0	62.2	63.3	-1.1	43.6	45.5	-1.9
Quarter 4	54.3	52.8	1.6	57.4	53.7	3.7	60.6	61.7	-1.1	46.8	47.9	-1.2
Quarter 5	52.0	52.7	-0.7	55.2	57.0	-1.8	60.2	61.4	-1.2	51.9	49.1	2.8
Quarter 6	53.6	49.9	3.7	56.6	55.5	1.1	60.8	61.0	-0.1	51.9	50.0	1.9
Quarter 7	51.6	51.6	0.0	56.9	56.6	0.3	61.1	62.8	-1.7	52.3	51.4	0.9
Quarter 8	54.7	48.5	6.2	57.7	56.5	1.2	59.3	61.5	-2.2	47.5	47.2	0.3
Quarters 1-4	74.9	67.5	7.4 **	77.4	76.4	1.0	83.4	80.1	3.2	61.8	59.5	2.3
Quarters 5-8	71.2	67.9	3.3	74.8	74.8	0.1	78.2	77.9	0.4	66.9	63.4	3.5
Average earnings (\$)												
Quarter 0 ^a	686	705	-19	746	792	-45	944	931	14	739	770	-31
Quarter 1	1,148	838	310 ***	1,052	992	60	1,249	1,259	-10	1,068	1,117	-49
Quarter 2	1,244	1,020	224 **	1,208	1,228	-20	1,351	1,416	-64	1,323	1,471	-148
Quarter 3	1,183	1,160	24	1,386	1,301	85	1,433	1,454	-21	1,470	1,557	-87
Quarter 4	1,335	1,248	87	1,441	1,387	54	1,520	1,427	93	1,660	1,800	-139
Quarter 5	1,226	1,235	-9	1,537	1,444	93	1,542	1,519	23	1,883	1,893	-9
Quarter 6	1,281	1,270	12	1,669	1,425	245 *	1,582	1,557	25	2,128	1,972	156
Quarter 7	1,487	1,280	207	1,709	1,484	225 *	1,671	1,647	23	2,009	2,021	-12
Quarter 8	1,535	1,493	42	1,775	1,560	215	1,773	1,688	85	2,003	1,940	63
Quarters 1-4	4,910	4,265	645 *	5,086	4,907	178	5,553	5,556	-2	5,521	5,945	-424
Quarters 5-8	5,530	5,278	252	6,691	5,913	778 *	6,568	6,411	157	8,023	7,826	197
Sample size	330	333		543	532		400	375		543	545	

(continued)

Table 7 (continued)

Outcome	Memphis			Springfield ^b			Trenton		
	Program Mean	Control Mean	Impact	Program Mean	Control Mean	Impact	Program Mean	Control Mean	Impact
Employed (%)									
Quarter 0 ^a	39.3	43.1	-3.8	43.8	48.6	-4.8	43.2	43.5	-0.4
Quarter 1	44.4	47.3	-2.9	46.6	53.2	-6.6	43.5	47.7	-4.2
Quarter 2	51.1	50.5	0.6	52.9	54.0	-1.1	45.6	45.0	0.5
Quarter 3	48.1	48.3	-0.1	43.2	55.5	-12.3	46.0	47.8	-1.9
Quarter 4	49.3	46.3	3.0				41.4	48.2	-6.8 *
Quarter 5	46.4	47.2	-0.8				42.0	47.7	-5.7
Quarter 6	47.9	48.8	-0.9				42.9	48.0	-5.1
Quarter 7	47.8	46.3	1.5				40.5	46.2	-5.7
Quarter 8	47.8	48.4	-0.6				42.2	49.7	-7.4 *
Quarters 1-4	70.6	70.6	0.1				64.0	70.1	-6.0 *
Quarters 5-8	67.2	68.3	-1.2				60.6	66.1	-5.5
Average earnings (\$)									
Quarter 0 ^a	392	460	-68	558	565	-7	815	790	25
Quarter 1	677	804	-126	909	1,081	-172	1,021	1,014	7
Quarter 2	850	935	-86	1,267	1,483	-216	1,167	1,115	52
Quarter 3	913	913	0	1,297	1,441	-144	1,285	1,184	100
Quarter 4	963	939	24				1,198	1,252	-54
Quarter 5	1,033	1,082	-49				1,210	1,347	-137
Quarter 6	1,041	1,072	-30				1,340	1,421	-81
Quarter 7	1,035	1,106	-72				1,361	1,435	-73
Quarter 8	1,132	1,221	-89				1,310	1,627	-317 *
Quarters 1-4	3,403	3,591	-188				4,670	4,566	104
Quarters 5-8	4,240	4,481	-240				5,222	5,830	-608
Sample size	400	403		294	297		309	307	

SOURCE: MDRC calculations from UI earnings records.

NOTES: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

^aQuarter of random assignment.

^bOnly 3 quarters of follow-up are available for the full sample.

ters. During the first year of follow-up, or quarters 1–4, 74.9 percent of the PFS group worked, compared with 67.5 percent of the control group. The program group’s earnings were also \$645 higher during this period.

The results for Dayton are similar to those found in the interim report for an early cohort, in which the program produced an increase in employment and earnings over the first six quarters of follow-up. The results for Grand Rapids are more positive than those found in the interim report. As noted in the earlier report, Grand Rapids was one of only two sites that provided a significant number of on-the-job training slots. The increase in earnings without an increase in employment rates is consistent with the fact that these slots provided men with higher-paying jobs than they would have otherwise obtained. Springfield was the other site, but longer-term data are not available.

Although not generally significant, Memphis, Trenton, and Los Angeles tended to have negative earnings and employment impacts, and these results are somewhat similar to those found in the interim report. In quarter 8, for example, 47.8 percent of the PFS group in Memphis worked, compared with 48.4 percent of the control group. The PFS group’s earnings were also \$89 lower than the control group’s earnings. As noted in the interim report, both the Los Angeles and the Memphis programs placed an emphasis on skills training (primarily basic education in Memphis), so that positive impacts on earnings would not be expected in the short term. These two-year results indicate, however, that this strategy does not appear to have had longer-term impacts. The interim report provides one potential explanation for the somewhat negative impacts: PFS may have heightened some fathers’ expectations about the types of jobs they could get, and, as such, they might have been less likely than men in the control group to accept lower-wage job offers.

B. Impacts for the Survey Sample

Table 8 presents impacts on additional employment outcomes for the survey sample.¹² The survey data, with information on hours worked, wage rates, and benefits, help to provide a more detailed picture of fathers’ employment than is available from the UI records. For comparison purposes, the impacts calculated using UI data for the full sample of fathers (5,020 excluding Springfield) are shown at the top of the table: the program did not significantly increase earnings or employment rates in the first two years of follow-up.

Presenting survey sample impacts in year 1 on UI-reported employment and earnings serves two purposes. First, we can compare employment impacts using UI data for the two samples, in order to assess whether the survey sample is representative of the full sample. The survey sample tends to have higher UI-reported employment rates and earnings than the full sample. In addition, survey sample data suggest a different pattern of impacts: the program decreased employment rates (by 5.8 percentage points) and increased earnings, although the earnings impact is not statistically significant. The differences in outcomes and impacts for the two samples might be due to several factors. First, although the survey achieved a response rate of 78 percent, the responder sample could represent a select group of fathers who, for example, had more stable living situations and thus were easier to locate. The higher average earnings for the survey sample is consistent with this type of selectivity. Second, the survey sample is a subset of the sample of fathers who were

¹²As mentioned, all analyses using survey data are weighted so that the survey sample reflects the full sample. Unweighted results are generally similar to those reported here.

Table 8
Parents' Fair Share
Impacts of PFS on Employment, Earnings, and Benefits
(Full Sample and Survey Sample)

Outcome	Program Group	Control Group	Impact
Full PFS sample (n=5,020)^a			
Year 1 of follow-up (UI)			
Percent employed (%)	71.9	70.4	1.5
Earnings (\$)	4,928	4,876	52
Year 2 of follow-up (UI)			
Percent employed (%)	70.3	69.7	0.6
Earnings (\$)	6,238	6,079	159
PFS survey sample (n=553)			
Year 1 of follow-up			
Percent employed (UI)(%) ^b	75.4	81.2	-5.8 *
Earnings (UI)(\$) ^b	6,090	5,412	678
Percent employed (survey)(%) ^c	70.2	70.2	0.0
Earnings (survey) (%)	7,150	5,779	1,371 **
Average number of months employed in the year after random assignment			
	5.6	5.3	0.3
Number of quarters employed (UI) (%)			
0	24.6	18.8	5.8 *
1	10.1	18.9	-8.8 ***
2 - 3	29.4	28.4	1.0
4	35.9	34.0	1.9
Characteristics of most recent job (%)^d			
Weekly hours			
Information missing	0.0	0.4	-0.4
Less than 35 hours	15.7	19.9	-4.2
35 hours or more	54.5	49.9	4.6
Hourly wage			
Information missing	1.8	4.4	-2.6 *
Less than \$5.00	4.6	7.4	-2.8
\$5.00 - \$6.99	24.2	30.1	-5.8
\$7.00 - \$8.99	22.4	18.4	4.0
\$9.00 or more	17.1	9.9	7.2 **
Benefits			
Information missing	1.5	1.1	0.4
Job offered paid sick days	20.0	13.5	6.4 **
Job did not offer paid sick days	48.7	55.5	-6.8
Information missing	2.1	1.3	0.8
Job offered health insurance	25.6	19.0	6.6 *
Job did not offer health insurance	42.5	49.9	-7.4 *

(continued)

SOURCES: MDRC calculations from the noncustodial parent survey, the PFS Background Information Form, and UI earnings records.

NOTES: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

^aSpringfield is not included because only 3 quarters of follow-up are available for the full sample.

^bCalculated for sample members who also had valid employment history data on the survey (n = 526).

^cCalculated over 526 observations since it excludes some respondents who did not provide complete employment histories and a few sample members in Springfield for whom comparable UI data are not available.

^dThese categories include all sample members and are therefore experimental impacts. The sum of the categories is equal to the percentage of fathers who worked during the period, and the sum of the impacts across categories equals the impact on the percentage who worked since random assignment (according to the survey).

randomly assigned toward the end of the intake period, making it a late cohort. Outcome levels and program impacts might differ by cohort if the program became more or less effective over time or if the local economies changed over time. (Appendix A examines these issues.) The results suggest that differences in impacts between the two samples are due in part to the fact that the survey is a somewhat select sample and in part to cohort differences: about half of the impact difference is accounted for by a difference between cohorts, and the remainder appears to be due to a difference between survey responders and nonresponders.

For the survey sample, we can also examine how UI-reported employment differs from employment reported by the men themselves. Average UI-reported earnings were lower than survey-reported earnings, despite the fact that employment rates were higher according to the UI data. For the control group, for example, average earnings and employment were \$5,779 and 70.2 percent from the survey, compared with \$5,412 and 81.2 percent from the UI data. As noted in Section I, this difference may be due to the fact that the survey missed relatively short spells of UI-recorded employment.

The two data sources also tell different stories in terms of the program's effects. First, the survey data showed no change in employment, and the UI data showed a decrease in employment. Second, the survey data showed an increase in earnings of \$1,371, and the UI data showed an increase in earnings of only \$678.¹³ While the difference in earnings impacts is somewhat expected, given that UI data are likely to miss earnings for some men, the difference in employment impacts is unexpected.¹⁴

This difference may be related to the fact, mentioned earlier, that surveys tend to miss relatively short spells of UI employment and to the fact that the decrease in UI employment was primarily for men whose employment would have been short term in the absence of the program. In fact, the employment decrease (5.8 percentage points) is accounted for entirely by a decrease in the proportion of men who worked only one quarter during the year (8.7 percentage points). In other words, this relatively short-term employment was less likely to have been reported reliably on the survey, so a decrease in this type of employment was also unlikely to be captured by the survey.

Another possible explanation for why the survey showed no decrease in employment rates but an increase in earnings is that men who did work during the year earned more on average than they would have in the absence of PFS by working longer or in better jobs. That men who worked earned more can be seen by the fact that PFS decreased UI employment rates but increased UI earnings. Thus, the pattern of impacts in both data sources (although not statistically significant) indicates that earnings increased among some men who would have worked anyway. Earnings may have increased because the men worked longer within a quarter or because they got better jobs.

¹³As noted in the table, because of missing data the survey earnings measure is calculated over a slightly smaller sample than the UI earnings measure. Impacts on UI earnings were similar when the analysis was restricted to the sample with nonmissing data on both sources; the impact on UI-reported earnings was \$932 and is not statistically significant. The UI impact of \$932 and the survey impact of \$1,371 are not significantly different from each other.

¹⁴Another possible explanation for the difference in earnings impacts is that PFS, by stressing the payment of child support, had the unintended consequence of increasing the attractiveness of underground or "off-the-books" jobs, which might have allowed the fathers to hide their earnings more easily.

The “characteristics of most recent job” panel in Table 8 suggests that the men got better jobs. These outcomes and impacts are experimental because they are calculated over the full sample. Thus, the percentages who worked part time and full time, for example, will sum to the percentage who worked during the year according to the survey (also, the sum of the impacts across categories will equal the impact on the percentage who worked). Data on hours worked in the most recent job indicate that the program did not significantly affect hours worked, although there does appear to be some, albeit statistically insignificant, movement toward full-time work. PFS increased the number of men who earned relatively high wages, or \$9 or more per hour, and also increased the number working in jobs that offered paid sick days and the number working in jobs that offered health insurance. Thus, although the program did not increase employment rates, it does appear to have changed the types of jobs the fathers obtained.

A final point is that the increase in job quality, measured using survey data, would seem to be inconsistent with the finding that the survey earnings impact is larger than the UI earnings impact, since lower-quality jobs are more likely to be missed by the UI data and uncovered by the survey. Nonetheless, it is possible that PFS did increase job quality and also induced some men to take off-the-books jobs, either between periods of more formal employment or instead of formal employment. In addition, even for jobs reported by both the UI and the survey data, employers may underreport employee earnings. Kornfeld and Bloom (1999) found that for individuals who were reported employed by both the UI and the survey data, UI earnings were on average lower than survey earnings.

Thus, PFS appears to have affected employment by increasing the quality and possibly the duration of work among men who would have worked anyway. The result was an increase in survey-reported earnings for men who entered the program toward the end of the intake period. The impact on survey earnings was larger possibly because some men worked in unreported jobs or because some of their earnings in formal jobs were not reported by employers. PFS also caused some men to drop out of work, primarily those who would have worked for only a short period (one quarter) in the absence of the program. The next section shows that these overall effects mask the fact that PFS had very different impacts on more-employable and less-employable men.

C. Subgroup Impacts

Tables 9–11 present impacts for several subgroups. As shown in Section I, several of the fathers’ characteristics were associated with their employment outcomes. For example, men with low levels of education and limited work experience were less likely than other men to work during the year. It is easy to imagine that a program like PFS might have different effects on men who have more or less difficulty finding jobs on their own. This section presents impacts for subgroups defined by education status, employment status, and race/ethnicity.¹⁵ In addition to noting whether the impact for each subgroup is statistically significant, we indicate whether the difference in impacts across subgroups is statistically significant. A difference between impacts that is statistically significant is likely to represent a true subgroup difference rather than a difference due to sampling variability.

¹⁵We also estimated impacts for one additional subgroup — the two sites that offered a significant number of on-the-job training slots (Grand Rapids and Springfield) compared with all other sites. The impacts across these two groups were generally not significantly or substantively different.

Although the number of subgroups analyzed was kept to a minimum and the groups were chosen based on their associations with employment and earnings, it is important to note that when multiple significance tests are conducted, some differences will be significant simply by chance. For example, when testing 50 impacts at the 10 percent significance level, we would expect to find 5 ($50 \times .10$) impacts statistically significant owing to chance and not to a true difference between the program and control groups. Thus, as the number of subgroups examined increases, the likelihood of finding significant impacts increases. More conservative methods of judging statistical significance could be used to decrease the likelihood of finding spurious impacts. Instead, we simply note this caveat and assess whether those impacts that are statistically significant are part of a more general pattern of effects.

Education Status. As shown in Table 9, based on the UI records for the full sample, the program generally produced larger employment and earnings impacts on men without a high school diploma, although only two of the subgroup differences are statistically significant (see last column). The program produced a statistically significant increase in employment for fathers without a diploma (69.6 percent versus 64.6 percent) and no impact for their more-educated counterparts (74.2 percent versus 75.8 percent). This difference in impacts is statistically significant. The impacts in year 2 are also larger for the less-educated group.

Impacts on UI employment and earnings are somewhat different for the survey sample, showing a small negative impact on employment but a fairly similar impact on earnings. As noted earlier, this difference between samples appears to be due in part to impact differences between the early and late cohorts and in part to differences between survey responders and nonresponders.

A comparison of the UI and survey data shows that the discrepancy between the two sources in terms of program impacts is especially pronounced for men without a high school diploma. The survey data show a substantial increase in earnings, while the UI data show no significant effect on employment and earnings. (The impacts of \$2,507 and \$743 are significantly different from each other.) For men with a diploma, in contrast, the UI data and survey data are more consistent, both showing a decrease in employment and a small increase in earnings.

For the full sample, PFS increased the quality or duration of employment among less-educated men who would have worked anyway. This can be seen by the decrease (by 13.0 percentage points) in the proportion of men working only one quarter of the year and the increase, although insignificant, in the proportion working two quarters or more. Because men are more likely to remember and report longer or higher earning spells of work, this pattern of results may also explain the increase in survey-reported employment of 6.4 percentage points.¹⁶ The increase in survey-reported earnings for less-educated men is quite large (\$2,507), and the difference between survey and UI earnings is larger than that found for the full sample. It is easy to imagine that less-educated

¹⁶One possible interpretation of the increase in survey-reported employment is that the program did not have a true effect on employment but simply reduced the extent of underreporting of employment among PFS men. However, if PFS affected the likelihood of reporting, there is no reason for it to have affected only less-educated men. In addition, it is equally likely that PFS would have increased the incidence of employment underreporting, since men in the program were more connected to the child support system and thus had more reason to hide earnings.

Table 9
Parents' Fair Share
Impacts of PFS on Employment, Earnings, and Benefits,
by Education Status
(Full Sample and Survey Sample)

Outcome	High School Diploma			No High School Diploma			Difference in Subgroup Impact p-value ^a
	Program Group	Control Group	Impact	Program Group	Control Group	Impact	
Full PFS sample (n=5,020)							
Year 1 of follow-up (UI)							
Percent employed (%)	74.2	75.8	-1.6	69.6	64.6	5.0 ***	0.00 ***
Earnings (\$)	5,603	5,776	-173	4,206	3,899	308	0.13
Year 2 of follow-up (UI)							
Percent employed (%)	72.1	73.3	-1.3	68.4	65.8	2.6	0.11
Earnings (\$)	7,157	7,099	57	5,250	4,970	280	0.59
PFS survey sample (n=553)							
Year 1 of follow-up ^b							
Percent employed (UI) (%)	78.6	86.6	-8.0 *	72.0	75.3	-3.3	0.50
Earnings (UI) (\$)	6,926	6,304	622	5,171	4,428	743	0.91
Percent employed (survey) (%)	71.8	79.2	-7.4	69.0	60.4	8.5	0.04 **
Earnings (survey) (\$)	6,897	6,641	256	7,431	4,924	2,507 **	0.10
Average number of months employed in the year after random assignment	5.7	6.0	-0.3	5.6	4.6	1.0 *	0.10 *
Number of quarters employed (%)							
0	21.3	13.4	8.0 *	28.0	24.7	3.3	0.50
1	7.4	12.5	-5.1	12.8	25.8	-13.0 ***	0.19
2-3	30.0	32.0	-3.0	28.9	24.5	4.4	0.43
4	41.2	42.1	-0.9	30.2	25.0	5.2	0.45
Characteristics of most recent job (%)							
Weekly hours							
Information missing	0.0	0.8	-0.8	0.1	0.0	0.1	0.24
Less than 35 hours	15.5	24.5	-9.1 *	16.4	14.9	1.5	0.12
35 hours or more	56.4	53.9	2.5	52.5	45.5	7.0	0.61
Hourly wage							
Information missing	1.6	8.1	-6.5 ***	2.4	0.4	1.9	0.01 ***
Less than \$5.00	5.8	8.8	-3.1	3.2	5.8	-2.6	0.91
\$5.00 - \$6.99	21.8	34.8	-13.0 **	27.5	25.1	2.4	0.05 **
\$7.00 - \$8.99	26.2	16.1	10.1 **	17.8	20.8	-3.0	0.06 *
\$9.00 or more	16.4	11.4	5.1	18.0	8.3	9.7 **	0.46

(continued)

Table 9 (continued)

Outcome	High School Diploma			No High School Diploma			Difference in Subgroup Impact p-value ^a
	Program Group	Control Group	Impact	Program Group	Control Group	Impact	
Benefits							
Information missing	0.6	1.6	-1.0	2.6	0.7	2.0	0.18
Job offered paid sick days	23.4	14.8	8.6 *	16.0	12.0	4.0	0.49
Job did not offer paid sick days	47.8	62.7	-15.0 **	50.3	47.8	2.6	0.05 **
Information missing	0.8	1.3	-0.5	3.7	1.4	2.3	0.23
Job offered health insurance	29.2	26.5	2.7	21.8	10.8	11.1 **	0.25
Job did not offer health insurance	41.8	51.4	-9.6	43.4	48.3	-4.9	0.59

SOURCES: MDRC calculations from the noncustodial parent survey and the PFS Background Information Form.

NOTES: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

^aAn F-test was applied to each difference in impacts across subgroups. The "p-value" is the statistical significance level of these differences.

^bPercent employed and earnings from the UI data are presented for those who reported employment on the survey.

men are more likely than their more-educated counterparts to work in jobs where earnings are not fully reported to the UI system. This is not inconsistent with the fact that PFS increased the proportion of less-educated men working in relatively high-wage jobs (\$9 or more per hour) and in jobs that offered health benefits, since employers may have failed to fully report earnings or many of the men may have also worked in unreported jobs.

PFS had a different impact on more-educated men. The program decreased the proportion of men who worked during the year (by about 8 percentage points according to the UI and survey data). It also decreased the number of men working part time and decreased the number working in jobs that did not offer paid sick days and health benefits (the impact on health insurance is not statistically significant). Thus, PFS caused some men to drop out of work, and the pattern of impacts suggests that the men who dropped out were those who would have worked in fairly low-quality jobs in the absence of the program.

Employment Status. Table 10 presents impacts for subgroups defined by employment prior to random assignment. Men who worked within the six months prior to random assignment are defined as “recently employed,” and all others are defined as “not recently employed.” The pattern of impacts by prior work experience is somewhat similar to that by education level: the program produced bigger impacts for the more-disadvantaged group, and the impacts on survey-reported earnings tended to be larger than those on UI-reported earnings. In addition, this similarity does not appear to be due to the fact that the two disadvantaged subgroups consisted largely of the same men; among those in the “not recently employed” group, half had a high school diploma. Also, as mentioned in Section I, both education and prior work had independent effects on the likelihood of working during the year.

The primary difference between the impacts for the less-educated group and the group with little prior work experience is that the UI-reported increase in earnings is large and statistically significant (\$1,669) for those not recently employed. Thus, the UI and survey data tell a similar story for this group in terms of the program’s impact on earnings. These men may have been more likely than those with no high school diploma to work in jobs in which earnings were more fully reported to the UI system.

Another notable difference for the group with less work experience is the difference in impacts on UI earnings between the full sample (\$33) and the survey sample (\$1,669). Further analyses (not reported) indicated that this difference is due largely to cohort effects: the program had a much bigger effect on earnings for men in this subgroup who entered the program during the second half of the intake period. The implementation research presented in the interim report suggests that the program became more effective over time; the job search services improved as technical assistance was provided, and the coordination between PFS staff and the CSE agencies also improved.

For men with recent work experience, the pattern of impacts is similar to those for the more-educated subgroup. PFS decreased employment (according to both sources), largely among men who would have worked only one quarter of the year and in jobs without benefits.

Table 10
Parents' Fair Share
Impacts of PFS on Employment, Earnings, and Benefits
by Employment Status
(Full Sample and Survey Sample)

Outcome	Recently Employed ^a			Not Recently Employed			Difference in Subgroup Impact p-value ^b
	Program Group	Control Group	Impact	Program Group	Control Group	Impact	
Full PFS sample (n=5,020)							
Year 1 of follow-up (UI)							
Percent employed (%)	78.6	77.9	0.7	61.8	58.9	2.9	0.36
Earnings (\$)	5,540	5,468	73	4,003	3,970	33	0.90
Year 2 of follow-up (UI)							
Percent employed (%)	74.1	75.4	-1.4	64.5	60.9	3.6 *	0.04 **
Earnings (\$)	6,743	6,657	85	5,479	5,194	285	0.64
PFS survey sample (n=553)							
Year 1 of follow-up ^c							
Percent employed (UI) (%)	80.0	88.6	-8.6 *	69.3	68.3	1.0	0.19
Earnings (UI) (\$)	6,630	6,606	24	5,221	3,551	1,669 *	0.15
Percent employed (survey) (%)	74.5	81.7	-7.2	64.0	51.3	12.7 **	0.01 ***
Earnings (survey) (\$)	7,912	7,100	811	6,051	3,766	2,285 **	0.29
Average number of months employed in the year after random assignment	6.1	6.2	-0.1	4.8	4.0	0.8	0.32
Number of quarters employed (%)							
0	20.0	11.4	8.6 *	30.7	31.7	-1.0	0.18
1	6.6	18.1	-11.5 ***	15.7	20.1	-4.3	0.25
2-3	31.3	30.0	1.2	27.2	24.8	2.4	0.89
4	42.2	40.5	1.7	26.3	23.4	3.0	0.88
Characteristics of most recent job (%)							
Weekly hours							
Information missing	0.1	0.0	0.1	-0.1	1.0	-1.1 *	0.13
Less than 35 hours	15.8	21.5	-5.7	15.7	17.3	-1.6	0.55
35 hours or more	58.7	60.2	-1.6	48.4	33.0	15.4 **	0.05 *
Hourly wage							
Information missing	1.5	2.9	-1.5	2.2	7.0	-4.8 *	0.31
Less than \$5.00	5.0	9.3	-4.3	4.0	4.3	-0.3	0.36
\$5.00 - \$6.99	28.2	36.8	-8.7 *	18.6	18.8	-0.2	0.28
\$7.00 - \$8.99	23.5	22.2	1.3 *	21.1	12.2	8.9	0.29
\$9.00 or more	16.4	10.5	5.9	18.1	9.0	9.1 *	0.62

(continued)

Table 10 (continued)

Outcome	Recently Employed			Not Recently Employed			Difference in Subgroup Impact p-value
	Program Group	Control Group	Impact	Program Group	Control Group	Impact	
Benefits							
Information missing	1.9	1.4	0.5	1.0	0.6	0.4	0.99
Job offered paid sick days	20.8	13.7	7.1 *	18.5	13.4	5.1	0.76
Job did not offer paid sick days	51.8	66.6	-14.8 ***	44.5	37.3	7.2	0.01 ***
Information missing	2.0	1.0	1.0	2.3	1.7	0.6	0.89
Job offered health insurance	26.9	22.5	4.4	23.5	13.3	10.2 *	0.43
Job did not offer health insurance	45.6	58.2	-12.6 **	38.2	36.3	1.9	0.11

SOURCES: MDRC calculations from the noncustodial parent survey and the PFS Background Information Form.

NOTES: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

^aThe "recently employed" worked within the six months prior to random assignment. All others were "not recently employed."

^bAn F-test was applied to each difference in impacts across subgroups. The "p-value" is the statistical significance level of these differences.

^cPercent employed and earnings from the UI data are presented for those who reported employment on the survey.

Race/Ethnicity. Table 11 presents impacts by race/ethnicity. The differences in impacts are not as pronounced or consistent as those for the two other subgroups, although they tend to be larger for non-African-American men. The program increased survey-reported earnings for non-African-American men but not for African-American men. In addition, the patterns of impacts on job quality are different. Although there is no reported increase in employment rates for non-African-American men, PFS appears to have moved them into better jobs than they would have obtained otherwise — jobs that paid \$9 or more per hour and jobs that offered health insurance. In contrast, PFS had little effect on the types of jobs that African-American fathers obtained.

D. Participation in PFS Services

The previous section showed that PFS did not consistently increase employment and earnings for all fathers, but it appears to have affected the duration and quality of employment among less-employable men. In an effort to explain this pattern of effects, this section presents program impacts on participation in PFS services.

Table 12 presents impacts on participation in six (plus “other”) activities. Participation in each activity is self-reported from the survey.¹⁷ Although fathers assigned to the control group were not offered PFS services, they could seek out these activities (through non-PFS providers) on their own. The rates shown in Table 12, however, indicate that participation in these activities would have been fairly infrequent in the absence of PFS; only 24.9 percent of control group members participated in any of the activities. In contrast, over 61.3 percent of PFS group members participated in at least one activity. The increase in overall participation is due to participation in job club and peer support. For example, 32.6 percent of program group members participated in job club, versus 4.5 percent of control group members, for a statistically significant impact of 28.1 percentage points.

On-the-job training and skills training (“vocational training” on Tables 12 and 13) were also components of PFS. However, the difference between the PFS and control groups is not large, given that Los Angeles was the only site to enroll a significant number of fathers in skills training. (See Appendix Table B1 for participation rates by site.) Participation in on-the-job training, on the other hand, was not asked about on the survey, so we are not able to estimate program impacts for participation in this component. Nevertheless, the overall treatment difference is not likely to be large, since the MIS data show that less than 10 percent of the PFS fathers participated in on-the-job training; Springfield and Grand Rapids were the only two sites to provide a significant number of on-the-job training slots. As mentioned, Grand Rapids had positive UI-reported impacts on earnings in the later quarters; longer-term impacts for Springfield are not available. No differences in survey-reported impacts were found for Grand Rapids and Springfield compared with the other sites.

¹⁷Survey data are likely to underestimate true participation rates if the fathers failed to recall short spells of participation or spells that occurred in the distance past. Management information system (MIS) data for men in the PFS group generally show higher participation rates than those reported here.

Table 11
Parents' Fair Share
Impacts of PFS on Employment, Earnings, and Benefits,
by Race/Ethnicity
(Full Sample and Survey Sample)

Outcome	African-American			Non-African-American			Difference in Subgroup Impact p-value ^a
	Program Group	Control Group	Impact	Program Group	Control Group	Impact	
Full PFS sample (n=5,020)							
Year 1 of follow-up (UI)							
Percent employed (%)	73.1	72.2	0.9	69.5	66.8	2.7	0.48
Earnings (\$)	4,632	4,593	39	5,532	5,452	81	0.90
Year 2 of follow-up (UI)							
Percent employed (%)	71.1	71.0	0.1	68.6	67.0	1.6	0.57
Earnings (\$)	5,717	5,687	30	7,313	6,868	445	0.35
PFS survey sample (n=553)							
Year 1 of follow-up ^b							
Percent employed (UI) (%)	75.0	83.7	-8.7 **	75.7	76.9	-1.1	0.28
Earnings (UI) (\$)	5,168	4,983	185	7,856	6,380	1,476	0.26
Percent employed (survey) (%)	69.3	67.0	2.3	72.2	76.0	-3.8	0.45
Earnings (survey) (\$)	5,870	5,010	861	9,629	7,442	2,187 **	0.34
Average number of months employed in the year after random assignment	5.2	4.7	0.5	6.4	6.5	-0.1	0.43
Number of quarters employed (%)							
0	25.0	16.3	8.7 **	24.3	23.1	1.1	0.28
1	9.8	20.2	-10.4 ***	10.4	16.5	-6.1	0.48
2-3	30.0	29.7	0.3	27.9	25.9	2.0	0.83
4	35.1	33.7	1.4	37.4	34.5	2.9	0.86
Characteristics of most recent job (%)							
Weekly hours							
Information missing	0.0	0.6	0.6	0.0	0.0	0.0	0.40
Less than 35 hours	16.7	19.5	-2.8	14.0	20.3	-6.4	0.60
35 hours or more	52.6	46.9	5.7	58.2	55.7	2.5	0.73
Hourly wage							
Information missing	2.4	4.0	-1.6	0.8	5.0	-4.2	0.43
Less than \$5.00	6.3	6.1	0.2	1.5	9.4	-7.9 **	0.06 *
\$5.00 - \$6.99	27.6	31.4	-3.9	17.7	27.0	-9.3	0.50
\$7.00 - \$8.99	20.1	17.1	3.0	27.0	21.3	5.7	0.71
\$9.00 or more	12.9	8.4	4.5	25.1	13.4	11.8 **	0.25

(continued)

Table 11 (continued)

Outcome	Race/Ethnicity						Difference in Subgroup Impact p-value ^a
	African-American			Non-African-American			
	Program Group	Control Group	Impact	Program Group	Control Group	Impact	
Benefits							
Information missing	2.5	1.0	1.5	-0.3	1.2	-1.5	0.19
Job offered paid sick days	18.3	15.3	3.0	22.8	10.6	12.2 **	0.17
Job did not offer paid sick days	48.5	50.7	-2.2	49.7	64.3	-14.6 **	0.17
Information missing	3.3	1.4	1.9	-0.1	0.8	-1.0	0.22
Job offered health insurance	23.1	20.2	2.9	29.8	17.2	12.6 **	0.19
Job did not offer health insurance	42.9	45.4	-2.5	42.5	58.0	-15.4 **	0.16

SOURCES: MDRC calculations from the noncustodial parent survey and the PFS Background Information Form.

NOTES: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

^aAn F-test was applied to each difference in impacts across subgroups. The "p-value" is the statistical significance level of these differences.

^bPercent employed and earnings from the UI data are presented for those who reported employment on the survey.

Table 12
Parents' Fair Share
Participation in PFS Services in the First Year After Random Assignment
(Survey Sample)

Outcome	Program Group	Control Group	Impact
Any participation (%)	61.3	24.9	36.5 ***
Job club (%)	32.6	4.5	28.1 ***
Peer support (%)	46.8	4.0	42.8 ***
Vocational training (%)	7.6	4.9	2.7
High school diploma/GED/ESL (%)	8.0	6.4	1.7
College classes (%)	5.5	6.5	-1.0
Other (%)	1.6	3.1	-1.5
Sample size	292	261	

SOURCE: MDRC calculations from the noncustodial parent survey.

NOTE: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Table 13
Parents' Fair Share
Participation in PFS Services in the First Year After Random Assignment, by Subgroup
(Survey Sample)

Outcome	No High School Diploma		High School Diploma		Not Recently Employed		Recently Employed ^a				
	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group			
Any participation (%)	55.3	18.3	37.1 ***	31.0	35.9 ***	58.3	25.2	33.1 ***	63.5	24.3	39.1 ***
Job club (%)	27.8	4.1	23.7 ***	4.8	31.9 ***	28.6	3.3	25.3 ***	35.2	5.0	30.2 ***
Peer support (%)	37.2	1.8	35.4 ***	5.8	49.3 ***	42.0	5.7	36.3 ***	50.0	2.6	47.4 ***
Vocational training (%)	5.9	2.7	3.2	7.0	2.3	12.3	3.5	8.8 **	4.8	5.7	-0.9
High school diploma/GED/ESL (%)	13.8	12.1	1.7	1.1	1.6	9.4	7.7	1.7	7.0	5.7	1.2
College classes (%)	1.3	0.4	1.0	9.5	12.2	4.8	6.5	-1.6	6.1	6.3	-0.2
Other (%)	2.6	1.0	1.6	1.1	5.2	0.2	2.8	-2.6	2.5	3.3	-0.8
Sample size	160	128		132	133	110	100		182	161	

SOURCES: MDRC calculations from the noncustodial parent survey and the PFS Background Information Form.

NOTES: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

^aThe "recently employed" worked within the six months prior to random assignment. All others were "not recently employed."

Table 13 presents participation impacts for subgroups defined by education and employment status. The results show that although the overall participation rates are lower for the less-employable subgroups, the impacts across the groups are generally of a similar magnitude. For example, 27.8 percent of program group members without a high school diploma participated in job club, compared with 4.1 percent of control group members, for an impact of 23.7 percentage points. The comparable impact for the group with a diploma is 32.0 percentage points.

The impacts on participation suggest that the effects of PFS on fathers' employment and earnings were generated by participation in job club and/or participation in peer support, because the treatment differences are not large for the other activities. In addition, participation data for subgroups do not explain why the program produced bigger effects on employment and earnings for less-employable men, since the increase in participation is similar in magnitude for all groups. It is important to note, however, that the impacts could have been caused by the overall existence of the PFS mandate and its coordination with child support enforcement.

IV. Conclusions

Noncustodial parents have figured in the welfare reform debate primarily as targets of increased child support enforcement efforts. But if child support is to become a viable source of support for low-income children, many of the fathers associated with these families will need to be given opportunities to meet their obligations. PFS was a program designed to do just that. In exchange for fathers' cooperation with the child support system, PFS offered them services to help them find more stable and better-paying jobs and become better parents.

In general, the fathers who were referred to PFS were a disadvantaged group. Many had very unstable living situations, half did not have a high school diploma, and most had been arrested prior to entering the program. Low education levels and limited work experience, in particular, seem to be associated with problems finding and keeping jobs. The program was moderately successful at increasing earnings among those without a high school diploma and those with little recent work experience. For these men, PFS increased the extent of their employment during the year and helped them get better jobs than they would have otherwise. They were more likely to work in relatively high-wage jobs and in jobs that offered benefits. For the more-employable fathers, in contrast, PFS did not affect their earnings on average and caused a slight reduction in employment. The results suggest that fathers who dropped out of the workforce in response to PFS were those who would have worked part time and earned relatively low wages. For this group, PFS may have increased their expectations about the types of jobs they could obtain, leading them to hold out for better jobs.

The results are different from those shown in the interim report in part because the program had larger employment effects for men who entered the program toward the end of the intake period. Implementation findings from the last report indicate that the services provided improved over time, as technical assistance was provided and both the coordination and the content of services were strengthened. Also, the previous report used only UI data to measure earnings, but these data miss earnings from jobs not reported to the UI system. The results point to the importance of combining survey and UI data when evaluating programs for low-income individuals, since many may work for cash or in unreported jobs.

The participation data suggest that the program achieved its impacts on employment and earnings through the imposition of the PFS mandate and/or participation in peer support and job club, since these were the primary services used by PFS fathers. However, the fact that one-quarter of the men assigned to PFS did not work during the year suggests that some men may need more intensive services to find and keep jobs. Lack of a high school diploma, for example, has been found to be strongly associated with unemployment. Although the program did increase employment among those without a diploma, it might have achieved bigger effects if services were designed that explicitly dealt with lack of education as a barrier to employment, perhaps by providing additional skills training in combination with part-time work. For men who are not able to find jobs through such a program, community service employment may also be a way to provide them with much needed work experience.

Appendix A
Survey Response Analysis

Survey Response Analysis

This appendix examines program impacts for several samples of the PFS evaluation in order to assess whether the survey sample is representative of the full PFS sample. Table A1 presents employment and earnings impacts for the survey sample (that is, all those who responded to the survey) and three other samples: (1) the survey-fielded group, sample members who were randomly selected to have the survey administered to them; (2) the survey-eligible group, sample members who were randomly assigned between October 1995 and February 1996, and were eligible to be part of the survey-fielded group; (3) the full sample, the full PFS sample randomly assigned between March 1994 and June 1996.

The columns labeled “survey sample” and “full sample” reproduce the results for year 1 discussed in the report. As already shown, the earnings impact of \$636 during year 1 for the survey sample is much different from the impact of \$8 obtained for the full sample. Outcomes and impacts are presented for the fielded and eligible samples in an effort to explain this difference.

The pattern of impacts across the groups indicates that, beginning with the eligible sample and moving to the fielded and survey samples, the impacts tend to be larger. The earnings impact during year 1 for the eligible sample, for example, is \$334, compared with \$8 for the full sample. (Impacts for the eligible and fielded samples are fairly similar, which is not surprising, since the fielded sample is a random subset of the eligible sample.) As we move from the eligible sample to the survey sample, the earnings impact increases to \$636. The results suggest that the difference in impacts between the full and survey samples is due partly to cohort difference (shown by the difference between the eligible and the full sample, since the eligible sample is a late cohort) and partly to the fact that the survey sample — those who responded to the survey — is a somewhat select group (shown by the difference between the survey sample and the eligible sample, since both are from the same cohort). That responders (survey sample members) are somewhat different from nonresponders can be seen from the outcomes of the control group in each sample. Average earnings for control group responders were \$5,423 in year 1, compared with average earnings for the control group in the fielded sample of \$5,185. This pattern is somewhat typical, in that individuals who are located and respond to surveys tend to be less disadvantaged than those who do not respond.

Table A1
Parents' Fair Share

Impacts of PFS on Employment and Earnings
for Survey Sample, Survey-Fielded Group, Survey-Eligible Group,
and Full Sample

Outcome	Survey Sample ^a			Survey-Fielded Group ^b		
	Program Group	Control Group	Non-Weighted Impact	Program Group	Control Group	Impact
Employed (%)						
Quarter 0	50.7	51.1	-0.4	48.8	50.0	-0.2
Quarter 1	53.5	50.3	3.2	50.5	49.9	0.7
Quarter 2	54.2	57.6	-3.4	52.7	54.5	-1.8
Quarter 3	55.8	63.4	-7.6 *	52.7	60.2	-7.5 **
Quarter 4	60.1	57.1	3.0	55.9	54.1	1.8
Quarters 1-4	75.0	81.5	-6.5 *	71.7	78.2	-6.5 **
Average earnings (\$)						
Quarter 0	843	782	61	813	766	47
Quarter 1	1,246	919	327 **	1,157	940	217 *
Quarter 2	1,415	1,307	107	1,305	1,268	37
Quarter 3	1,593	1,569	23	1,468	1,489	-21
Quarter 4	1,737	1,599	137	1,582	1,521	61
Quarters 1-4	6,059	5,423	636	5,558	5,185	373
Sample size	292	260		346	313	

(continued)

Table A1 (continued)

Outcome	Survey-Eligible Group ^c			Full Sample		
	Program Group	Control Group	Impact	Program Group	Control Group	Impact
Employed (%)						
Quarter 0	42.9	46.0	-3.0	46.0	47.9	-1.9 *
Quarter 1	48.5	46.0	2.5	50.4	50.3	0.2
Quarter 2	51.3	51.1	0.1	51.8	51.2	0.6
Quarter 3	50.0	54.6	-4.5	50.6	52.3	-1.7
Quarter 4	52.7	52.0	0.7	51.6	51.6	0.0
Quarters 1-4	70.1	73.1	-3.0	71.8	70.9	0.9
Average earnings (\$)						
Quarter 0	719	703	16	705	722	-17
Quarter 1	1,068	904	165 *	1,026	1,016	10
Quarter 2	1,260	1,178	82	1,212	1,241	-28
Quarter 3	1,425	1,372	54	1,306	1,292	14
Quarter 4	1,484	1,460	24	1,393	1,388	5
Quarters 1-4	5,226	4,892	334	4,944	4,936	8
Sample size	584	584		2,819	2,792	

SOURCES: MDRC calculations from the noncustodial parent survey, the PFS Background Information Form, and UI earnings records.

NOTES: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent, ** = 5 percent; * = 10 percent.

^aThose who responded to the noncustodial parent survey.

^bThose who were randomly selected to have the noncustodial parent survey administered to them; this group includes responders (the survey sample) and nonresponders.

^cThose who were randomly assigned between October 1995 and February 1996.

Appendix B
Participation Rates, by Site

Table B1
Parents' Fair Share
Participation in PFS Services in the First Year After Random Assignment,
by Site
(Survey Sample)

Outcome	Dayton			Grand Rapids			Jacksonville			Los Angeles		
	Program Mean	Control Mean	Impact									
Any participation	62.1	36.0	26.1 *	51.0	12.2	38.8 ***	73.9	27.5	46.4 ***	67.1	28.0	39.1 ***
Job club	43.1	7.3	35.8 ***	17.0	1.2	15.8 ***	44.9	1.9	43.0 ***	13.0	1.3	11.6
Peer support	45.8	3.8	41.9 ***	36.4	1.7	34.7 ***	68.0	3.7	64.3 ***	47.0	1.6	45.4 ***
Vocational training	11.3	8.7	2.6	3.4	3.7	-0.3	-0.9	9.6	-10.5 *	23.1	5.6	17.5 *
High school diploma/GED/ESL	4.0	11.5	-7.5	6.4	0.9	5.5	-0.4	6.3	-6.7	9.5	-1.0	10.5 *
College classes	15.1	13.7	1.5	1.7	5.3	-3.6	0.0	0.0	0.0 ***	9.8	10.5	-0.8
Other	0.1	3.1	3.0	1.6	0.2	1.4	-0.3	6.2	-6.5	-1.6	7.6	-9.2 *
Sample size	39	30		55	57		32	34		35	34	34

(continued)

Table B1 (continued)

Outcome	Memphis			Springfield			Trenton		
	Program Mean	Control Mean	Impact	Program Mean	Control Mean	Impact	Program Mean	Control Mean	Impact
Any participation	51.2	20.4	30.9 ***	62.1	26.3	35.8 ***	82.2	27.7	54.6 ***
Job club	22.5	3.6	18.8 ***	42.7	11.6	31.1 ***	64.1	2.3	61.8 ***
Peer support	37.1	2.3	34.9 ***	42.8	8.1	34.7 ***	72.9	-2.5	75.4 ***
Vocational training	3.9	3.5	0.4	0.9	2.3	-1.3	8.1	10.4	-2.2
High school diploma/ESL/GED	7.8	11.2	-3.4	19.2	4.9	14.3 *	12.9	11.0	1.9
College classes	5.7	1.3	4.4	8.0	1.0	7.0	6.9	8.5	-1.5
Other	0.8	1.1	-0.3	5.3	7.4	-2.1	5.4	-0.2	5.6
Sample size	59	48		34	30		38	28	

SOURCE: MDRC calculations from UI earnings records.

NOTE: A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

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Recent Publications on MDRC Projects

Note: For works not published by MDRC, the publisher's name is shown in parentheses. A complete publications list is available from MDRC and on its Web site (www.mdrc.org), which also contains copies of MDRC's publications.

Reforming Welfare and Making Work Pay

ReWORKing Welfare: Technical Assistance for States and Localities

A multifaceted effort to assist states and localities in designing and implementing their welfare reform programs. The project includes a series of "how-to" guides, conferences, briefings, and customized, in-depth technical assistance.

After AFDC: Welfare-to-Work Choices and Challenges for States. 1997. Dan Bloom.

Changing to a Work First Strategy: Lessons from Los Angeles County's GAIN Program for Welfare Recipients. 1997. Evan Weissman.

Work First: How to Implement an Employment-Focused Approach to Welfare Reform. 1997. Amy Brown.

Business Partnerships: How to Involve Employers in Welfare Reform. 1998. Amy Brown, Maria Buck, Erik Skinner.

Learnfare: How to Implement a Mandatory Stay-in-School Program for Teenage Parents on Welfare. 1998. David Long, Johannes Bos.

Promoting Participation: How to Increase Involvement in Welfare-to-Work Activities. 1999. Gayle Hamilton, Susan Scrivener.

Encouraging Work, Reducing Poverty: The Impact of Work Incentive Programs. 2000. Gordon Berlin.

Steady Work and Better Jobs: How to Help Low-Income Parents Sustain Employment and Advance in the Workforce. 2000. Julie Strawn, Karin Martinson.

Project on Devolution and Urban Change

A multi-year study in four major urban counties — Cuyahoga County, Ohio (which includes the city of Cleveland), Los Angeles, Miami-Dade, and Philadelphia — that examines how welfare reforms are being implemented and affect poor people, their neighborhoods, and the institutions that serve them.

Big Cities and Welfare Reform: Early Implementation and Ethnographic Findings from the Project on Devolution and Urban Change. 1999. Janet Quint, Kathryn Edin, Maria Buck, Barbara Fink, Yolanda Padilla, Olis Simmons-Hewitt, Mary Valmont.

Food Security and Hunger in Poor, Mother-Headed Families in Four U.S. Cities. 2000. Denise Polit, Andrew London, John Martinez.

Focusing on Fathers

Parents' Fair Share Demonstration

A demonstration for unemployed noncustodial parents (usually fathers) of children on welfare. PFS aims to improve the men's employment and earnings, reduce child poverty by increasing child support payments, and assist the fathers in playing a broader constructive role in their children's lives.

Low-Income Parents and the Parents' Fair Share Demonstration. 1996. Earl Johnson, Fred Doolittle.

Working with Low-Income Cases: Lessons for the Child Support Enforcement System from Parents' Fair Share. 1998. Fred Doolittle, Suzanne Lynn.

Building Opportunities, Enforcing Obligations: Implementation and Interim Impacts of Parents' Fair Share. 1998. Fred Doolittle, Virginia Knox, Cynthia Miller, Sharon Rowser.

Fathers' Fair Share: Helping Poor Men Manage Child Support and Fatherhood (Russell Sage Foundation). 1999. Earl Johnson, Ann Levine, Fred Doolittle.

Parenting and Providing: The Impact of Parents' Fair Share on Paternal Involvement. 2000. Virginia Knox, Cindy Redcross.

Working and Earning: The Impact of Parents' Fair Share on Low-Income Fathers' Employment. 2000. John M. Martinez, Cynthia Miller.

The Responsible Fatherhood Curriculum. 2000. Eileen Hayes, with Kay Sherwood.

Financial Incentives

Encouraging Work, Reducing Poverty: The Impact of Work Incentive Programs. 2000. Gordon Berlin.

Minnesota Family Investment Program

An evaluation of Minnesota's pilot welfare reform initiative, which aims to encourage work, alleviate poverty, and reduce welfare dependence.

MFIP: An Early Report on Minnesota's Approach to Welfare Reform. 1995. Virginia Knox, Amy Brown, Winston Lin.

Making Welfare Work and Work Pay: Implementation and 18-Month Impacts of the Minnesota Family Investment Program. 1997. Cynthia Miller, Virginia Knox, Patricia Auspos, Jo Anna Hunter-Manns, Alan Orenstein.

Reforming Welfare and Rewarding Work: Final Report on the Minnesota Family Investment Program. 2000: *Volume 1: Effects on Adults*. Cynthia Miller, Virginia Knox, Lisa Gennetian, Martey Doodoo, Jo Anna Hunter, Cindy Redcross.
Volume 2: Effects on Children. Lisa Gennetian, Cynthia Miller.

Reforming Welfare and Rewarding Work: A Summary of the Final Report on the Minnesota Family Investment Program. 2000. Virginia Knox, Cynthia Miller, Lisa Gennetian.

New Hope Project

A test of a community-based, work-focused antipoverty program and welfare alternative operating in Milwaukee.

The New Hope Offer: Participants in the New Hope Demonstration Discuss Work, Family, and Self-Sufficiency. 1996. Dudley Benoit.

Creating New Hope: Implementation of a Program to Reduce Poverty and Reform Welfare. 1997. Thomas Brock, Fred Doolittle, Veronica Fellerath, Michael Wiseman.

Who Got New Hope? 1997. Michael Wiseman.

An Early Look at Community Service Jobs in the New Hope Demonstration. 1998. Susan Poglinco, Julian Brash, Robert Granger.

New Hope for People with Low Incomes: Two-Year Results of a Program to Reduce Poverty and Reform Welfare. 1999. Johannes Bos, Aletha Huston, Robert Granger, Greg Duncan, Thomas Brock, Vonnie McLoyd.

Canada's Self-Sufficiency Project

A test of the effectiveness of a temporary earnings supplement on the employment and welfare receipt of public assistance recipients. Reports on the Self-Sufficiency Project are available from: Social Research and Demonstration Corporation (SRDC), 275 Slater St., Suite 900, Ottawa, Ontario K1P 5H9, Canada. Tel.: 613-237-4311; Fax: 613-237-5045. In the United States, the reports are also available from MDRC.

Creating an Alternative to Welfare: First-Year Findings on the Implementation, Welfare Impacts, and Costs of the Self-Sufficiency Project (Social Research and Demonstration Corporation [SRDC]). 1995. Tod Mijanovich, David Long.

The Struggle for Self-Sufficiency: Participants in the Self-Sufficiency Project Talk About Work, Welfare, and Their Futures (SRDC). 1995. Wendy Bancroft, Sheila Currie Vernon.

Do Financial Incentives Encourage Welfare Recipients to Work? Initial 18-Month Findings from the Self-Sufficiency Project (SRDC). 1996. David Card, Philip Robins.

When Work Pays Better Than Welfare: A Summary of the Self-Sufficiency Project's Implementation, Focus Group, and Initial 18-Month Impact Reports (SRDC). 1996.

How Important Are "Entry Effects" in Financial Incentive Programs for Welfare Recipients? Experimental Evidence from the Self-Sufficiency Project (SRDC). 1997. David Card, Philip Robins, Winston Lin.

Do Work Incentives Have Unintended Consequences? Measuring "Entry Effects" in the Self-Sufficiency Project (SRDC). 1998. Gordon Berlin, Wendy Bancroft, David Card, Winston Lin, Philip Robins.

When Financial Incentives Encourage Work: Complete 18-Month Findings from the Self-Sufficiency Project (SRDC). 1998. Winston Lin, Philip Robins, David Card, Kristen Harknett, Susanna Lui-Gurr.

Does SSP Plus Increase Employment? The Effect of Adding Services to the Self-Sufficiency Project's Financial Incentives (SRDC). 1999. Gail Quets, Philip Robins, Elsie Pan, Charles Michalopoulos, David Card.

When Financial Work Incentives Pay for Themselves: Early Findings from the Self-Sufficiency Project's Applicant Study (SRDC). 1999. Charles Michalopoulos, Philip Robins, David Card.

The Self-Sufficiency Project at 36 Months: Effects of a Financial Work Incentive on Employment and Income (SRDC). 2000. Charles Michalopoulos, David Card, Lisa Gennetian, Kristen Harknett, Philip K. Robins.

The Self-Sufficiency Project at 36 Months: Effects on Children of a Program That Increased Parental Employment and Income (SRDC). 2000. Pamela Morris, Charles Michalopoulos.

Time Limits

Cross-State Study of Time-Limited Welfare

An examination of the implementation of some of the first state-initiated time-limited welfare programs.

Implementing Time-Limited Welfare: Early Experiences in Three States. 1995. Dan Bloom, David Butler.

The View from the Field: As Time Limits Approach, Welfare Recipients and Staff Talk About Their Attitudes and Expectations. 1997. Amy Brown, Dan Bloom, David Butler.

Welfare Time Limits: An Interim Report Card. 1999. Dan Bloom.

Connecticut's Jobs First Program

An evaluation of Connecticut's statewide time-limited welfare program, which includes financial work incentives and requirements to participate in employment-related services aimed at rapid job placement. This study provides some of the earliest information on the effects of time limits in major urban areas.

Early Data on the Implementation of Connecticut's Jobs First Program. 1997. Dan Bloom, Mary Andes.

Jobs First: Early Implementation of Connecticut's Welfare Reform Initiative. 1998. Dan Bloom, Mary Andes, Claudia Nicholson.

Connecticut Post-Time Limit Tracking Study: Three-Month Survey Results. 1998. Jo Anna Hunter-Manns, Dan Bloom, Richard Hendra, Johanna Walter.

Connecticut Post-Time Limit Tracking Study: Six-Month Survey Results. 1999. Jo Anna Hunter-Manns, Dan Bloom.

Jobs First: Implementation and Early Impacts of Connecticut's Welfare Reform Initiative. 2000. Dan Bloom, Laura Melton, Charles Michalopoulos, Susan Scrivener, Johanna Walter.

Florida's Family Transition Program

An evaluation of Florida's initial time-limited welfare program, which includes services, requirements, and financial work incentives intended to reduce long-term welfare receipt and help welfare recipients find and keep jobs.

The Family Transition Program: An Early Implementation Report on Florida's Time-Limited Welfare Initiative. 1995. Dan Bloom.

The Family Transition Program: Implementation and Early Impacts of Florida's Initial Time-Limited Welfare Program. 1997. Dan Bloom, James Kemple, Robin Rogers-Dillon.

The Family Transition Program: Implementation and Interim Impacts of Florida's Initial Time-Limited Welfare Program. 1998. Dan Bloom, Mary Farrell, James Kemple, Nandita Verma.

The Family Transition Program: Implementation and Three-Year Impacts of Florida's Initial Time-Limited Welfare Program. 1999. Dan Bloom, Mary Farrell, James Kemple, Nandita Verma.

Vermont's Welfare Restructuring Project

An evaluation of Vermont's statewide welfare reform program, which includes a work requirement after a certain period of welfare receipt, and financial work incentives.

WRP: Implementation and Early Impacts of Vermont's Welfare Restructuring Project. 1998. Dan Bloom, Charles Michalopoulos, Johanna Walter, Patricia Auspos.

Forty-Two Month Impacts of Vermont's Welfare Restructuring Project. 1999. Richard Hendra, Charles Michalopoulos.

WRP: Key Findings from the Forty-Two-Month Client Survey. 2000. Dan Bloom, Richard Hendra, Charles Michalopoulos.

Mandatory Welfare Employment Programs

National Evaluation of Welfare-to-Work Strategies

Conceived and sponsored by the U.S. Department of Health and Human Services, with support from the U.S. Department of Education, this is the largest-scale evaluation ever conducted of different strategies for moving people from welfare to employment.

Adult Education for People on AFDC: A Synthesis of Research (U.S. Department of Education [ED]/U.S. Department of Health and Human Services [HHS]). 1995. Edward Pauly.

Early Findings on Program Impacts in Three Sites (HHS/ED). 1995. Stephen Freedman, Daniel Friedlander.

Five Years After: The Long-Term Effects of Welfare-to-Work Programs (Russell Sage Foundation). 1995. Daniel Friedlander, Gary Burtless.

Monthly Participation Rates in Three Sites and Factors Affecting Participation Levels in Welfare-to-Work Programs (HHS/ED). 1995. Gayle Hamilton.

Changing to a Work First Strategy: Lessons from Los Angeles County's GAIN Program for Welfare Recipients. 1997. Evan Weissman.

Evaluating Two Welfare-to-Work Program Approaches: Two-Year Findings on the Labor Force Attachment and Human Capital Development Programs in Three Sites (HHS/ED). 1997. Gayle Hamilton, Thomas Brock, Mary Farrell, Daniel Friedlander, Kristen Harknett.

Work First: How to Implement an Employment-Focused Approach to Welfare Reform. 1997. Amy Brown.

Implementation, Participation Patterns, Costs, and Two-Year Impacts of the Portland (Oregon) Welfare-to-Work Program (HHS/ED). 1998. Susan Scrivener, Gayle Hamilton, Mary Farrell, Stephen Freedman, Daniel Friedlander, Marisa Mitchell, Jodi Nudelman, Christine Schwartz.

Do Mandatory Welfare-to-Work Programs Affect the Well-Being of Children? A Synthesis of Child Research Conducted as Part of the National Evaluation of Welfare-to-Work Strategies (HHS/ED). 2000. Gayle Hamilton.

Evaluating Alternative Welfare-to-Work Approaches: Two-Year Impacts for Eleven Programs (HHS/ED). 2000. Stephen Freedman, Daniel Friedlander, Gayle Hamilton, JoAnn Rock, Marisa Mitchell, Jodi Nudelman, Amanda Schweder, Laura Storto.

Impacts on Young Children and Their Families Two Years After Enrollment: Findings from the Child Outcomes Study (HHS/ED). 2000. Sharon McGroder, Martha Zaslow, Kristin Moore, Suzanne LeMenestrel.

Los Angeles's Jobs-First GAIN Program

An evaluation of Los Angeles's refocused GAIN (welfare-to-work) program, which emphasizes rapid employment. This is the first in-depth study of a full-scale "work first" program in one of the nation's largest urban areas.

Changing to a Work First Strategy: Lessons from Los Angeles County's GAIN Program for Welfare Recipients. 1997. Evan Weissman.

The Los Angeles Jobs-First GAIN Evaluation: Preliminary Findings on Participation Patterns and First-Year Impacts. 1998. Stephen Freedman, Marisa Mitchell, David Navarro.

The Los Angeles Jobs-First GAIN Evaluation: First-Year Findings on Participation Patterns and Impacts. 1999. Stephen Freedman, Marisa Mitchell, David Navarro.

The Los Angeles Jobs-First GAIN Evaluation: Final Report on a Work First Program in a Major Urban Center. 2000. Stephen Freedman, Jean Knab, Lisa Gennetian, David Navarro.

Teen Parents on Welfare

Teenage Parent Programs: A Synthesis of the Long-Term Effects of the New Chance Demonstration, Ohio's Learning, Earning, and Parenting (LEAP) Program, and the Teenage Parent Demonstration (TPD). 1998. Robert Granger, Rachel Cytron.

Ohio's LEAP Program

An evaluation of Ohio's Learning, Earning, and Parenting (LEAP) Program, which uses financial incentives to encourage teenage parents on welfare to stay in or return to school.

LEAP: Final Report on Ohio's Welfare Initiative to Improve School Attendance Among Teenage Parents. 1997. Johannes Bos, Veronica Fellerath.

New Chance Demonstration

A test of a comprehensive program of services that seeks to improve the economic status and general well-being of a group of highly disadvantaged young women and their children.

New Chance: Final Report on a Comprehensive Program for Young Mothers in Poverty and Their Children. 1997. Janet Quint, Johannes Bos, Denise Polit.

Parenting Behavior in a Sample of Young Mothers in Poverty: Results of the New Chance Observational Study. 1998. Martha Zaslow, Carolyn Eldred, editors.

Other

Can They All Work? A Study of the Employment Potential of Welfare Recipients in a Welfare-to-Work Program. 1995. James Riccio, Stephen Freedman.

Florida's Project Independence: Benefits, Costs, and Two-Year Impacts of Florida's JOBS Program. 1995. James Kemple, Daniel Friedlander, Veronica Fellerath.

From Welfare to Work Among Lone Parents in Britain: Lessons for America. 1996. James Riccio.

Employment and Community Initiatives

Connections to Work Project

A study of local efforts to increase competition in the choice of providers of employment services for welfare recipients and other low-income populations. The project also provides assistance to cutting-edge local initiatives aimed at helping such people access and secure jobs.

Tulsa's IndEx Program: A Business-Led Initiative for Welfare Reform and Economic Development. 1997. Maria Buck.

Washington Works: Sustaining a Vision of Welfare Reform Based on Personal Change, Work Preparation, and Employer Involvement. 1998. Susan Gooden.

Cost Analysis Step by Step: A How-to Guide for Planners and Providers of Welfare-to-Work and Other Employment and Training Programs. 1998. David Greenberg, Ute Appenzeller.

Designing and Administering a Wage-Paying Community Service Employment Program Under TANF: Some Considerations and Choices. 1999. Kay Sherwood.

San Francisco Works: Toward an Employer-Led Approach to Welfare Reform and Workforce Development. 2000. Steven Bliss.

Jobs-Plus Initiative

A multi-site effort to greatly increase employment among public housing residents.

A Research Framework for Evaluating Jobs-Plus, a Saturation and Place-Based Employment Initiative for Public Housing Residents. 1998. James Riccio.

Mobilizing Public Housing Communities for Work: Origins and Early Accomplishments of the Jobs-Plus Demonstration. 1999. James Riccio.

Building a Convincing Test of a Public Housing Employment Program Using Non-Experimental Methods: Planning for the Jobs-Plus Demonstration. 1999. Howard Bloom.

Section 3 Public Housing Study

An examination of the effectiveness of Section 3 of the 1968 Housing and Urban Development Act in affording employment opportunities for public housing residents.

Lessons from the Field on the Implementation of Section 3 (U.S. Department of Housing and Urban Development). 1996. Maxine Bailey, Suzanne Lynn.

Canada's Earnings Supplement Project

A test of an innovative financial incentive intended to expedite the reemployment of displaced workers and encourage full-year work by seasonal or part-year workers, thereby also reducing receipt of Unemployment Insurance.

Implementing the Earnings Supplement Project: A Test of a Re-employment Incentive (Social Research and Demonstration Corporation). 1997. Howard Bloom, Barbara Fink, Susanna Lui-Gurr, Wendy Bancroft, Doug Tattrie.

Testing a Re-employment Incentive for Displaced Workers: The Earnings Supplement Project. 1999. Howard Bloom, Saul Schwartz, Susanna Lui-Gurr, Suk-Won Lee.

Education Reform

Career Academies

The largest and most comprehensive evaluation of a school-to-work initiative, this study examines a promising approach to high school restructuring and the school-to-work transition.

Career Academies: Early Implementation Lessons from a 10-Site Evaluation. 1996. James Kemple, JoAnn Leah Rock.

Career Academies: Communities of Support for Students and Teachers — Emerging Findings from a 10-Site Evaluation. 1997. James Kemple.

Career Academies: Building Career Awareness and Work-Based Learning Activities Through Employer Partnerships. 1999. James Kemple, Susan Poglinco, Jason Snipes.

Career Academies: Impacts on Students' Engagement and Performance in High School. 2000. James Kemple, Jason Snipes.

Project GRAD

This evaluation examines Project GRAD, an education initiative targeted at urban schools and combining a number of proven or promising reforms.

Building the Foundation for Improved Student Performance: The Pre-Curricular Phase of Project GRAD Newark. 2000. Sandra Ham, Fred C. Doolittle, Glee Ivory Holton.

LILAA Initiative

This study of the Literacy in Libraries Across America (LILAA) initiative explores the efforts of five adult literacy programs in public libraries to improve learner persistence.

So I Made Up My Mind: Introducing a Study of Adult Learner Persistence in Library Literacy Programs. 2000. John T. Comings, Sondra Cuban.

Project Transition

A demonstration program that tested a combination of school-based strategies to facilitate students' transition from middle school to high school.

Project Transition: Testing an Intervention to Help High School Freshmen Succeed. 1999. Janet Quint, Cynthia Miller, Jennifer Pastor, Rachel Cytron.

Equity 2000

Equity 2000 is a nationwide initiative sponsored by the College Board to improve low-income students' access to college. The MDRC paper examines the implementation of Equity 2000 in Milwaukee Public Schools.

Getting to the Right Algebra: The Equity 2000 Initiative in Milwaukee Public Schools. 1999. Sandra Ham, Erica Walker.

School-to-Work Project

A study of innovative programs that help students make the transition from school to work or careers.

Home-Grown Lessons: Innovative Programs Linking School and Work (Jossey-Bass Publishers). 1995. Edward Pauly, Hilary Kopp, Joshua Haimson.

Home-Grown Progress: The Evolution of Innovative School-to-Work Programs. 1997. Rachel Pedraza, Edward Pauly, Hilary Kopp.

MDRC Working Papers on Research Methodology

A new series of papers that explore alternative methods of examining the implementation and impacts of programs and policies.

Building a Convincing Test of a Public Housing Employment Program Using Non-Experimental Methods: Planning for the Jobs-Plus Demonstration. 1999. Howard Bloom.

Estimating Program Impacts on Student Achievement Using "Short" Interrupted Time Series. 1999. Howard Bloom.

Using Cluster Random Assignment to Measure Program Impacts: Statistical Implications for the Evaluation of Education Programs. 1999. Howard Bloom, Johannes Bos, Suk-Won Lee.

About MDRC

The Manpower Demonstration Research Corporation (MDRC) is a nonprofit, nonpartisan social policy research organization. We are dedicated to learning what works to improve the well-being of low-income people. Through our research and the active communication of our findings, we seek to enhance the effectiveness of social policies and programs. MDRC was founded in 1974 and is located in New York City and San Francisco.

MDRC's current projects focus on welfare and economic security, education, and employment and community initiatives. Complementing our evaluations of a wide range of welfare reforms are new studies of supports for the working poor and emerging analyses of how programs affect children's development and their families' well-being. In the field of education, we are testing reforms aimed at improving the performance of public schools, especially in urban areas. Finally, our community projects are using innovative approaches to increase employment in low-income neighborhoods.

Our projects are a mix of demonstrations — field tests of promising program models — and evaluations of government and community initiatives, and we employ a wide range of methods such as large-scale studies to determine a program's effects, surveys, case studies, and ethnographies of individuals and families. We share the findings and lessons from our work — including best practices for program operators — with a broad audience within the policy and practitioner community, as well as the general public and the media.

Over the past quarter century, MDRC has worked in almost every state, all of the nation's largest cities, and Canada. We conduct our projects in partnership with state and local governments, the federal government, public school systems, community organizations, and numerous private philanthropies.

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