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Off Welfare and Into Work
A Report Series of the Postemployment Services Demonstration

**THE EFFECTIVENESS OF THE POSTEMPLOYMENT
SERVICES DEMONSTRATION:
PRELIMINARY FINDINGS**

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EXECUTIVE SUMMARY

Sustained employment is the primary route to self-sufficiency. Recent welfare reforms focus on intensifying efforts to move individuals from welfare into work. As states impose work requirements and limitations on a larger group of welfare recipients, more individuals who are less job ready are entering and will continue to enter the labor market. Given this fact, as well as evidence that many individuals who leave welfare for work return to welfare fairly quickly, it is important to identify strategies that help welfare recipients keep their jobs or, if necessary, quickly find new ones.

The Postemployment Services Demonstration (PESD), which was funded by the Administration for Children and Families (ACF) of the U.S. Department of Health and Human Services (DHHS), was the first large-scale field program that used extended case management services and temporary financial support to promote job retention and reemployment among newly employed welfare recipients. This report examines early measures of the effectiveness of the PESD programs in promoting job retention, sustaining employment, and reducing welfare dependency during clients' first year after program enrollment. In general, the preliminary findings suggest that the PESD programs had, at best, small or modest success in increasing employment and earnings and reducing welfare dependency among newly employed welfare recipients.

PESD demonstration grants were awarded to four states in 1993, and programs to provide job retention and reemployment services were established in four sites: (1) Chicago, Illinois; (2) Portland, Oregon; (3) Riverside, California; and (4) San Antonio, Texas. Each PESD program enrolled between 800 and 1,500 welfare recipients who had recently found jobs. Through extended case management services, staff members maintained contact with clients after they had obtained jobs and were positioned to help identify and address problems before they resulted in job loss. Case managers were available to provide clients with services such as ongoing monitoring, counseling, and support; job search assistance; mediation and resolution of benefits issues; and help in accessing services and benefits (for example, child care subsidies, health care, and temporary financial assistance).

To test the effectiveness of the PESD program, staff at each site randomly selected between 400 and 500 newly employed welfare recipients (including those who left welfare as a result of their employment) to participate in the PESD program. The rest became part of a control group receiving regular services available to employed welfare recipients in the state, mostly through the Job Opportunities and Basic Skills Training (JOBS) program. The analysis presented here--which compares the experiences of program and control group members--is based on survey data for a representative subsample of 1,200 demonstration enrollees, evenly divided across the sites and between the program and control groups. We focus on this subsample because, at the time of the analysis, we did not have complete administrative records data for the full sample. Administrative records data will be available for and used in the final impact analysis to be conducted during spring 1998. Consequently, the findings in this report should be considered only as suggestive of what we might expect to find with the full sample and should be used with caution. The urgency of providing information in a time of rapidly changing welfare policy justifies this early analysis.

To measure the extent to which the PESD programs were effective at promoting job retention and supporting the employment pathways that welfare recipients take toward self-sufficiency, we examined three key questions in our analysis of impacts:

1. Were the programs successful in keeping people in their jobs longer?
2. Did the programs increase employment and earnings during the first year of followup?
3. Did the programs reduce AFDC and food stamp benefits during the first year of followup?

Overall, we find small or modest effects in three of the four sites: one site was modestly successful, with consistent patterns of increased job retention, increases in employment and earnings, and significant reductions in welfare dependency among those in the program group compared with those in the control group. Two other sites exhibited effects similar to those of the first site, although they were smaller and generally not significant. In the fourth site, we actually observe opposite (although insignificant) effects--lower earnings and increases in welfare.

Three primary factors influence the magnitude of the estimated program effects and should be considered in interpreting program impacts: (1) the experimental and evolving nature of the programs, (2) the extent to which case management services matched clients' diverse needs, and (3) the services already available to those in the control group.

First, the four PESD programs were fairly experimental, with each program learning and evolving as it went along. Since PESD was the first large-scale program of job retention services set in the context of state welfare programs, it had no service delivery models on which to build. Consequently, case managers had to learn over time which specific types of services to provide, how to deliver services, over what period of time to deliver services, and (sometimes) how and to what extent to integrate PESD service delivery with the JOBS program. For example, certain services (such as direct intervention with employers and the use of the Earned Income Tax Credit) were not as much in demand as expected. In contrast, case managers spent a substantial amount of time on issues they had not anticipated (for example, the resolution of client benefit eligibility and payment errors). Furthermore, many clients lost their jobs fairly quickly and required assistance over a longer than expected time period.

Second, since case management services are more appropriate for some clients than for others, the services delivered may not always have matched the diverse needs of all clients. The program guidelines required that case managers establish and maintain some contact with all clients who were enrolled in the programs, regardless of their needs. The process of contacting clients and informing them about PESD services often was challenging and time-consuming, especially given that PESD was a new program and that it was separated from the rest of the welfare system. Moreover, case managers received little guidance about how to serve clients with different types and levels of need. Given these factors, case managers often spent a large part of their time trying to maintain regular contact with most clients, regardless of clients' need. Over time, this effort imposed constraints on

case managers' time and may have made it difficult to provide services to the clients who most needed assistance. A systematic attempt to distinguish up front which clients most needed intensive case management services and which clients could make it on their own might have resulted in services that were better tailored to meet clients' diverse needs.

Third, the program context and services already available to those in the control group also influenced the magnitude of estimated program effects. Typically, estimated impacts are largest when a program provides new services that are not already available and when those in the control group have access to very few services. To the extent that those in the control group are receiving services similar to services those in the program group are receiving, or are receiving alternative types of supportive services, then measured program impacts are likely to be diluted. In the two sites (Chicago and San Antonio) where we observe modest increases in earnings and decreases in welfare, relatively limited case management services were available to control group members through the JOBS program. In the other two sites, where the control group had access to some similar services, we find smaller effects and effects that move in the opposite direction. In some sense, failure to find the expected program impacts in the latter two sites where control group members had access to some similar services may reinforce the value of making available job search and other services for employed welfare recipients.

Interpreted in light of these three factors, the evidence of modest preliminary effects of the program in three sites gives some reason for optimism about the usefulness and importance of providing job retention services to welfare recipients who have recently found jobs. Other programs that are attempting to set up job retention programs can learn from PESD service delivery. Our early analysis of the PESD programs suggests that:

- Some clients are able to sustain employment on their own and will need only little or short-term assistance to meet specific needs. Others, however, face multiple barriers and will need ongoing assistance; programs should attempt to target these clients for ongoing case management support.
- Simplifying service delivery mechanisms may help make the transition from welfare to work smoother for many clients and free case management resources for those clients who need it most.

I. INTRODUCTION

Sustained employment is the primary route to self-sufficiency. The focus of recent welfare reform efforts has been to explore different avenues for getting individuals off welfare and into work. This started with the Job Opportunities and Basic Skills Training (JOBS) program established under the Family Support Act (FSA) of 1988, which attempted to promote work by increasing investment in education and training. State welfare reform initiatives under waivers to the Aid to Families with Dependent Children (AFDC) program, as well as to the JOBS program, focused on increasing work among welfare recipients by increasing direct incentives to work or making work mandatory for more individuals. The current welfare reform law, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, goes a step further to promote employment. PRWORA replaces AFDC and provides states with block grants to help families under Temporary Assistance to Needy Families (TANF). This law reinforces the work mandates established by various states and requires most able-bodied welfare recipients to find employment within two years of welfare receipt or lose welfare benefits. The law also imposes a limit of five years of welfare receipt over an individual's lifetime.

These efforts to increase work among welfare recipients, combined with the general strength of the economy, have enabled many welfare recipients to *find* employment. What is less clear, however, is whether, and for how long, these individuals can *keep* their jobs. Previous studies of welfare dynamics show that many individuals who exit welfare via work return to welfare, and many do so fairly quickly (Blank 1989; Gritz and MaCurdy 1991; Harris 1991; Pavetti 1992; and Gleason, Rangarajan, and Schochet, forthcoming). In general, research shows that between 25 and 40 percent of welfare recipients who leave AFDC because of employment lose their jobs and return to AFDC within one year. Rates of job loss and the risk of return to AFDC are particularly high among those

with severe employment handicaps, such as those who have dropped out of high school or those living in impoverished neighborhoods with multiple risk factors (Pavetti 1992; and Berg, Olson, and Conrad 1991).

Research studies to date have only been able to follow welfare recipients who have found jobs without the pressure of strict work requirements and time limits. The provisions of TANF as implemented by states should increase the attachment of these individuals to their jobs. As states impose work requirements and time limits on a larger group of welfare recipients, however, more individuals who are less job ready will continue to enter the labor market. On the basis of what we know from those who have attempted to leave welfare under less rigid state mandates, these less job ready welfare recipients who enter the labor market under the new mandates of state laws are more likely to need assistance to help them keep their jobs or find new jobs quickly if they lose their jobs.

The Postemployment Services Demonstration (PESD) is the first large-scale demonstration program that examines the effectiveness of providing job retention services to newly employed welfare recipients. In 1993, on the basis of a competitive procurement process, four states were awarded grants under cooperative agreements with the U.S. Department of Health and Human Services (DHHS) to set up demonstration programs to provide additional case management services to newly employed welfare recipients. The major goals of these programs were to promote job retention, provide rapid reemployment for those who lost jobs, and, consequently, reduce welfare dependency. The four states received waivers to their AFDC programs and from their JOBS rules in order to provide job search and other assistance to facilitate job retention among all individuals enrolled in the demonstration programs, regardless of their JOBS or AFDC status.

The demonstration includes an evaluation of the PESD programs, with two main objectives: (1) to obtain a better understanding of the experiences of individuals after they become employed and

the factors that contribute to job loss or job stability, and (2) to examine whether the services provided by the programs can help individuals retain their jobs for longer or regain employment more quickly upon job loss. This report focuses on the second objective and provides an early look at the effectiveness of the programs in promoting employment and reducing welfare dependency. A synthesis report (to be produced in early 1998) will connect the findings from the previous PESD reports to draw lessons about program implementation, make recommendations for improving the operation of the program, and offer suggestions for programs thinking about providing job retention services in other settings. A final impact analysis report (to be produced in 1998) will examine how effectively PESD programs increase employment and reduce welfare dependency over a longer-term period of two years. The final impact report also will examine the costs and the cost-effectiveness of implementing the PESD programs.¹

II. PESD PROGRAMS

The PESD programs were implemented in four sites: (1) Chicago, Illinois; (2) Portland, Oregon; (3) Riverside, California; and (4) San Antonio, Texas. During a two-and-a-half-year period between spring 1994 and fall 1996, the four states operated demonstration programs under grants from the Administration for Children and Families (ACF) of DHHS. In the four demonstration sites, welfare recipients who had participated in the states' JOBS program and found employment during a 12- to 18-month period between March 1994 and December 1995 were identified and enrolled in the demonstration. Each PESD program enrolled between 800 and 1,500 welfare recipients who had recently found jobs during this period. Between one-half and one-third of the individuals in each site

¹A complete list of project reports completed so far is included at the end of this report.

were selected at random to participate in the program; the rest became part of a control group receiving regular services available to employed welfare recipients in the state.²

A. PESD Program Guidelines

DHHS established key guidelines for the design and delivery of demonstration services. These included the following:

- ***Serve nonexempt JOBS participants.*** The PESD programs were to serve AFDC recipients in the JOBS program who obtained employment (primarily individuals required to participate in JOBS activities instead of volunteers exempt from such requirements). Guidelines provided that services should continue after participants left AFDC, even if the recognized case closure reason was not increased earnings.
- ***Focus on job retention and reemployment.*** PESD services were to focus on two objectives: (1) helping individuals keep the initial job they obtained, and (2) helping individuals quickly find another job if they lost the initial one. Demonstration guidelines required that job clubs and job search services provided under the JOBS program also be made available for extended periods to all demonstration participants who lost their jobs, regardless of their AFDC status.³ Recognizing that initial jobs might not always be a path to stable self-sufficiency, DHHS specified that PESD services should include resources to develop new job options for participants who lost their jobs or who wanted to obtain better ones.
- ***Extend case management services.*** The cornerstone of PESD was case management services. DHHS specified that demonstration sites should assign PESD participants to case managers, who would maintain regular contact with them, provide encouragement and counseling, ensure access to transitional Medicaid and child care, arrange other needed support services, and intervene as necessary with employers or other parties. Case managers were to identify emerging problems that might affect employment and to intervene as early as possible to promote job retention. In addition, they were

²The term “employed welfare recipients” refers to individuals who found jobs while on welfare. Some of these individuals may have gone off welfare upon employment or shortly thereafter, whereas others may have continued to receive welfare while employed.

³Under federal regulations at the time, JOBS case management and related support services could be provided for only 90 days from the time an employed welfare recipient became ineligible for AFDC; in reality, JOBS services often were provided for a much shorter period of time or not at all.

expected to help participants apply for and obtain the Earned Income Tax Credit (EITC), especially in the form of an advance payment with each paycheck.

- ***Adopt a flexible service delivery approach.*** Demonstration guidelines stressed the importance of tailoring case manager involvement and the particular services delivered to the needs of individual participants. It was expected that some clients would need more intensive and long-lasting help, while others might need little or no assistance.

The case management services and financial support services were directly or indirectly expected to promote job retention and reemployment. It was also anticipated that these direct, individualized efforts in promoting employment would translate into reductions in welfare receipt or the amounts of benefits received.

Although the four sites built their PESD programs on a common framework provided by the demonstration design guidelines, the specific manner in which each site implemented the guidelines and delivered the services varied. The four sponsoring agencies: (1) defined different target populations; (2) had different program contexts with different levels of preexisting services for newly employed welfare recipients; and (3) provided the available postemployment services to participants in different ways. To provide some context for interpreting the effectiveness of the programs, we briefly describe each of these program design features.

B. Target Population

The four sites targeted slightly different groups of welfare recipients for enrollment into the demonstration. The programs in Chicago and Riverside targeted all welfare recipients who had been referred to or participated in their JOBS program and had recently found a job that was reported to the JOBS case management staff. The programs in Portland and San Antonio were somewhat more selective. The Portland program targeted JOBS clients who had been assigned to the two job

placement centers and subsequently found a job.⁴ The San Antonio PESD program primarily served Level 1 clients, who had higher education levels and were assessed as most job ready.⁵

The programs served all clients regardless of whether they had full-time or part-time jobs.⁶ Although the programs did not exclusively rule out teenage parents, few teenage parents were enrolled in the programs.⁷ The Riverside and Portland programs stuck more closely to the requirements to serve nonexempt JOBS clients. All Riverside clients were nonexempt JOBS participants, and more than 95 percent of Portland clients were nonexempt JOBS participants. In contrast, between 60 and 75 percent of clients in Chicago and San Antonio were nonexempt JOBS participants.

C. Program Context and Preexisting Services

The four sites varied widely in the generosity of their welfare programs, the work incentives they provided for welfare recipients, and the resources they had available for newly employed welfare recipients. For instance, at the time of the demonstration, California had the sixth most generous

⁴These clients either were directly assigned to the job placement center (if they were deemed job ready at the time of their JOBS assessment) or were assigned to the placement center after completing other JOBS components, such as education or training.

⁵The San Antonio program served “Level 1” clients who (1) had a high school diploma or General Educational Development (GED) certificate, (2) had worked in at least 12 of the past 24 months, or (3) had a certificate from a training program.

⁶The Riverside program required individuals to have started jobs involving at least 15 hours per week, and the San Antonio program included individuals who were working at least 10 hours per week.

⁷For instance, the Illinois Department of Public Aid already operated a separate Young Parent Services (YPS) program to provide teenage parents with intensive ongoing services, and it chose to focus its PESD program on adults. Similarly, targeting the more job-ready individuals probably ruled out many teenagers in Portland and San Antonio.

welfare program in the country (\$607 maximum benefits for a family of three). By comparison, the maximum grant in Texas of \$184 for a family of three was half the national median.

Two of the four sites already provided added incentives for all welfare recipients to work. Illinois had received a waiver to disregard indefinitely two-thirds of earnings in AFDC benefit calculation. In addition to the high benefit levels in California, which allowed many individuals to work and retain welfare benefits, the state also had a waiver to maintain the \$30 and one-third of earnings disregard indefinitely. These waivers meant that more individuals in these two sites could continue to receive welfare while employed. In contrast, the standard disregard policy, combined with very low benefits in San Antonio, led nearly all recipients who found a full-time job to become immediately ineligible for AFDC and even led those with fewer than full-time hours to lose eligibility four months later when the disregard ended. AFDC recipients in Portland received benefits substantially above the national median; however, they faced standard earnings disregard policies when they began working and this led to a relatively large drop in welfare receipt approximately four months after job start.

Control group members in the four sites could not receive PESD services but, in theory, could receive job retention services for 90 days after job start under the JOBS program. In practice, regular JOBS services to welfare recipients who began employment were fairly limited. To some degree, however, these services varied across the sites and control group members in Portland were more likely to have received follow-up services compared with those in the other sites. In Portland, the caseloads of placement center case managers serving JOBS participants were relatively modest and, because these staff members had had prior contact with clients in the placement center, they provided some continuing counseling to their clients after the clients began work. Services to newly employed JOBS participants were more limited in the other sites. Although, in theory, JOBS case management services were available for 90 days after job start, large caseloads of unemployed

welfare recipients prevented case managers in these sites from serving employed clients to any substantial extent. In Riverside, the strong JOBS program actively offered job search and other services to those on assistance. Because most welfare recipients in Riverside who found jobs continued to remain eligible for AFDC, they could avail themselves of these JOBS services if they lost their jobs.

The availability of supportive services such as health care, child care, and temporary financial assistance also varied from site to site. Transitional medical coverage for one year was available in all sites for clients who left welfare because they found a job. Beyond that year, each site had some program in place to serve medically needy or low-income individuals.

One-year transitional child care subsidies were generally available to AFDC recipients and former recipients in two sites: Oregon and Illinois. In Oregon, former participants who exhausted transitional benefits after a year could obtain help with child care costs fairly easily through Oregon's Employment-Related Day Care program. In Illinois, where subsidies were generally available to former participants, accessing the benefits was somewhat more difficult. For example, individuals in Illinois who had exhausted their year of transitional benefits tended to encounter relatively long waiting lists for subsidized child care slots. Subsidies for child care were available to a lesser extent in the other two sites. For example, AFDC recipients in Riverside could, in principle, receive a disregard of child care expenses in amounts prescribed by Title IV-A. However, very few actually received it. San Antonio experienced child care funding constraints, and, while there were some subsidized child care slots for those who did not receive transitional benefits, these programs generally had long waiting lists.

Through the JOBS program, each of the sites also offered limited work-related expense allowances soon after job start for employed welfare recipients. These payments, however, were not universal and typically were made when the client initiated the request.

D. PESD Services

Although the four PESD programs received broad federal guidelines regarding the provision of services, program staff had to decide how to set up and implement the program. The sites set up the postemployment services unit as an extension of the JOBS program and hired case managers to provide retention services. The PESD programs emphasized more sustained and intensive case management than was possible under the regular JOBS program. In all sites but one, PESD case managers were part of the state JOBS program and had the same access to databases and JOBS program services and the same authority to issue supportive service payments as regular JOBS case managers. Case managers in these three sites served all PESD clients, including those on and off AFDC. In the fourth site (Portland), PESD case managers were hired as part of a special unit to provide retention services to clients. They were stationed in each of the two placement centers and worked closely with JOBS staff members. In this site, PESD case managers handled all clients' general needs and provided job search assistance to those not receiving AFDC. JOBS staff members provided job search assistance to clients who were receiving AFDC.

Despite some site-by-site variation in how the services were delivered, PESD case managers provided five key services to program participants:⁸

⁸For a more detailed description of program design and services at each of the four sites, see Haimson, Hershey, and Rangarajan (1995). See Haimson and Hershey (1997) for an in-depth discussion of the use of postemployment services.

1. ***Counseling and Support.*** At all of the sites, individual counseling and support was the most commonly delivered PESD service and the one that staff and clients valued most. Case managers provided individual counseling on issues such as money management and budgeting, contingency planning, workplace behavior, and dealing with unsupportive family or friends. Staff members also provided encouragement and moral support to clients and gave them opportunities to discuss their frustrations and problems. Between 60 and 80 percent of PESD clients in each of the four sites received counseling and support services during the six months after PESD enrollment.
2. ***Job Search Assistance.*** PESD case managers and other staff members provided assistance to clients who had lost jobs or who wanted better ones. The job search services ranged from individualized job search assistance from PESD case managers (including specific job leads and general guidance on job search methods) to more structured job search activities available through the JOBS program (including workshops and referral services). About two-fifths of clients, on average, received job search assistance during the six months after enrollment.
3. ***Resolution of Benefits Issues.*** Case managers helped clients apply for and resolve eligibility or benefit problems concerning transitional Medicaid and child care, AFDC, and food stamps. They also helped clients access child care funding subsidies, particularly in the Chicago site. In addition, they provided information on how to obtain the EITC. The proportion of clients who received help resolving benefits during the first six months after enrollment varied across sites (from a low of 24 percent in Riverside to a high of 65 percent in San Antonio).
4. ***Service Referrals.*** Clients needed assistance finding services, including health care or child care providers, referrals to skill training or education programs, and referrals to legal aid or specialized individual or family counseling. Between one- and two-fifths of clients in the four sites received service referrals during the six months after enrollment.
5. ***Support Service Payments for Work-Related Expenses.*** The programs liberalized their agencies' policies on payment of transitional work-related expenses, allowing clients to obtain more frequent payments, larger payments, or payments for a wider array of expenses than normally allowed under JOBS rules. These payments typically covered temporary expenses associated with employment, job search, and emergencies that can affect employment. Such assistance was expected to prevent small emergencies or crises (such as having a car break down or not being able to afford suitable work clothes) from leading to job loss. The extent to which clients received work expense payments varied across the sites. In Portland and Chicago, about three-fifths received at least one work expense payment during the six months after enrollment. In Riverside, half received at least one payment, and, in San Antonio, less than one-fifth received payment.

The caseloads per case manager rose over time. By the end of the enrollment period, case managers had been assigned, on average, between 100 and 170 clients each, depending on the site. More than half of these clients were active or needed ongoing attention from the case managers at any given time. Case managers felt the strain of growing caseloads over time, as the period during which individual clients needed ongoing PESD services continued for longer than expected. In general, at the time of planning, programs had expected that services to individual clients would be most intensive during the first few months after job start and would decline rapidly in later months, with clients needing little to no services after some time. However, many clients experienced ongoing problems and many lost jobs within a short period of time; these clients required ongoing reemployment and retention services. Consequently, the number of clients served did not decline as rapidly as expected during the later months of the demonstration.⁹

Sites varied to some degree in their schedule for maintaining ongoing contact with the clients. Of the four sites, the Chicago program included a fairly intensive schedule for ongoing contact with new clients as well as with clients who lost jobs, and case managers had to report periodically to their supervisor on these contacts. The other sites also had specific schedules, but the schedules sometimes were less intense or were less closely adhered to (especially if the client did not want to maintain contact with the case manager). In all sites, contact schedules grew harder to maintain over time as the caseloads grew larger and more clients than expected required ongoing services.

⁹An average of between 40 and 70 percent of clients across the sites were active during any given quarter of the follow-up year; that is, they received at least one telephone or in-person contact that included services (a service contact) during a given three-month period. Active clients received, on average, between two and four service contacts during each three-month period. The number of active clients, as well as the estimated number of service contacts per active client, remained relatively constant during much of the follow-up period.

With no other models of similar programs to follow, the states had to be creative about the types of postemployment services they would provide and how to deliver them. They had to adapt and change aspects of service delivery as case managers learned about clients' needs and learned clients' reactions to their efforts to provide services.¹⁰ The formative nature of these programs, as well as their evolution over time as case managers had a better sense of clients' needs and adjusted to growing caseloads, must be kept in mind while interpreting program impacts.

III. ANALYSIS QUESTIONS AND METHODS

Using a case management approach, the PESD programs sought to promote job retention and support the employment pathways that welfare recipients take toward self-sufficiency. The programs aimed to promote sustained employment and thereby increase earnings and reduce welfare dependency in a manner that would ease clients' transition from welfare to work. To measure the extent to which the PESD programs met these objectives, we examined three key questions in our analysis of impacts:

1. Were the programs successful in keeping people in their jobs longer?
2. Did the programs increase employment and earnings during the first year of followup?
3. Did the programs reduce AFDC and food stamp benefits during the first year of followup?

¹⁰For example, some of the envisioned services did not work out. Program staff in all sites had anticipated that the case managers would maintain contact with clients' employers and resolve workplace conflicts or other workplace issues. This component of case management, however, turned out to be minor largely because of clients' preferences. On the other hand, case managers, particularly in Chicago and Portland, spent a large portion of their time correcting welfare benefit payment errors or dealing with child care payments.

The extent to which we are likely to observe impacts on these outcomes depends on the extent to which the PESD program staff promoted these outcomes, as well as on the extent to which control group members received services that might affect these outcomes. For example, if PESD case managers assisted or persuaded those who lost jobs to find jobs quickly, and no such assistance or persuasion was available to control group members, then we would expect to observe employment impacts. If, however, control group members who lost jobs and returned to welfare were pushed by JOBS case managers to find other jobs quickly, then observed program impacts would be much smaller. Similarly, to the extent that PESD case managers may have helped their clients obtain all the welfare benefits that would facilitate their transition from welfare to work, the programs may not have had any effect on welfare receipt.

Because a random-assignment design was chosen to evaluate the programs' effectiveness, we estimate program impacts by comparing mean outcome levels for program group members with those for control group members during the first year following random assignment. Because random assignment ensures the creation of two groups of individuals that differ only in their access to the program, any resulting differences between the two groups at a later time can be attributed to the incremental services offered by the program. In most of the tables in the report, we present control group means and estimated impacts (which represent the difference between the program and control group means).¹¹

¹¹These impacts are regression-adjusted to take into account any observed preexisting differences between the program and control groups that might have arisen out of chance. The program group mean can be obtained by adding the estimated impact to the control group mean. For example, if 80 percent of control group members received AFDC benefits during a given period, and the estimated impact is -8, this implies that $(80 + (-8))$, or 72, percent of program group members received AFDC during the same period. Similarly, if the control group mean for AFDC benefit amounts is \$2,800 and the estimated impact is \$250, then the program group mean for AFDC benefit amounts is $(\$2,800 + \$250)$, or \$3,050.

The preliminary findings in this report are based primarily on survey data collected for a random sample of about 300 individuals in each site approximately one year after program enrollment. Each site enrolled between 800 and 1,500 individuals in the demonstration, and we are collecting administrative records data for the full sample for the final impact analysis. Since we did not have complete administrative records data at the time of this analysis, we relied on the survey data to estimate program impacts. Furthermore, the survey data contain detailed information on employment and earnings that are not available in the administrative records. The survey sample is representative of the full sample; however, due to its relatively small size, this analysis is somewhat limited in its ability to detect statistically significant impacts. Therefore, the preliminary findings reported here should be used cautiously and considered as only indicative of the impacts we might expect to identify in the final analysis. The final impact analysis will be based on administrative records obtained for the full sample enrolled in the demonstration and will further examine the issues explored in this report.¹² Appendix A contains details on the sample sizes and data sources, the rationale for the decision to rely on the survey data rather than on the administrative records data for this analysis, the characteristics of the survey sample, and the analysis methods used to estimate program impacts.

¹²Using administrative records data for those sample members for whom we had data, we calculated preliminary impact estimates for the year following sample intake. The estimates based on these partial administrative records are displayed in Appendix Tables C.1-C.3. By and large, we find that administrative records estimates for employment and earnings are generally consistent with the estimates based on the survey data. However, the welfare impacts for the two samples tend to be somewhat more different. In the discussion of impact estimates, we note instances where estimates from the two sources differ substantively.

IV. IMPACTS ON EMPLOYMENT, EARNINGS, AND WELFARE

Overall, we find some indications that one program was modestly successful in promoting employment and reducing welfare, and two sites had somewhat similar, although smaller, effects.¹³ In the fourth site, we observe no move toward self-sufficiency, and, in fact, we find that those in the program group, on average, have lower earnings compared with those in the control group. (This finding is puzzling, and we suspect that it is partly the result of programmatic differences and the program context in this site.) Taken as a whole, and interpreted in light of the fact that these programs were a first attempt to provide postemployment services to welfare recipients, these findings give some reason for optimism about the usefulness of providing job retention services to welfare recipients who have recently found jobs. Our final impact analysis based on a larger sample may shed light on the groups of welfare recipients among whom these types of programs may have larger impacts.

A. Were the Programs Successful in Helping People Keep Their Jobs Longer?

The primary goal of the programs was to provide services that would enable individuals to hold their jobs for longer periods of time. Individuals are likely to face the greatest challenges to employment soon after finding a job, when they must deal with a variety of transition issues. Case managers tried to contact clients soon after they had found a job, and attempted to assess the support or services that clients needed to prevent job loss. The first question that naturally arises is to what extent the programs were successful in helping individuals maintain their jobs for longer periods of time. Overall, we find that two sites may have had small and modest (though not statistically significant) effects in increasing job tenure, while no effects were found in the other two sites.

¹³Few of the differences are statistically significant. This is due in part to the modest program effects and in part to the small size of the survey sample.

To estimate the length of time sample members maintained their initial jobs, we took three steps. First, we examined the percentage of sample members who reported having a job around the time of random assignment (a “starting job”). Since newly employed AFDC recipients were assigned randomly to the PESD program group or a control group soon after they were identified as having found a job, most sample members (both program and control group members) should have reported a starting job.¹⁴ On finding that not all sample members reported a starting job, we then tried to understand the reasons that some sample members may not have reported a starting job and the implication of these reasons for the impact estimates. Finally, we had to decide on how to account for those sample members who did not report starting jobs when we estimate program impacts on job tenure.

We found that only about 80 percent of the sample members across the sites reported a starting job in the self-reported survey. There are three reasons that some individuals may not have reported a starting job: (1) they found a job but never started it; (2) the job lasted or was expected to last less than two weeks and, thus, was not reported in the survey because of the way the survey question was asked; or (3) the job lasted such a short time that they did not recall it when the survey was conducted a year later. In order to understand why 20 percent of the sample members did not report a starting job, we examined the administrative earnings data for these individuals. We found that nearly 70 percent of those who did not report a starting job actually did earn income during the quarter when they were randomly assigned, suggesting that the nonreporting of starting jobs most likely reflects recall errors.¹⁵

¹⁴We allowed for a three-month window on either side of the random-assignment date, since we were concerned that some sample members might not correctly recall their job starting date.

¹⁵These administrative data do not include those sample members who only earned unreported
(continued...)

We also found that in three of four sites (all except Portland), a larger percentage of those in the program group reported having starting jobs compared with those in the control group (between five and seven percentage points). Program group members may be more likely to report a starting job, either because participating in PESD and working with case managers served as a trigger for them to better remember the jobs or because the PESD program and its services actually helped them hold their jobs longer (thus enabling the program group members to recall them more successfully). Each reason presents a different implication for the effects of the program in promoting job retention. For example, if more program group members remembered their starting jobs because they held the jobs for a longer period of time, this suggests that the programs may have been effective in promoting job retention. However, if program group members remembered their starting jobs only because the program acted as a trigger to help them remember, then this has no implication for the effectiveness of job retention services.

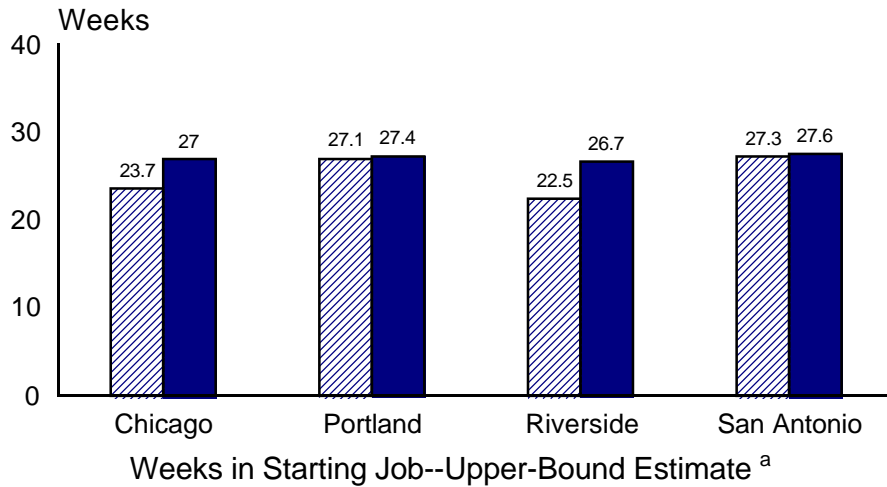
Since we cannot easily determine the real reason for these differences, we estimated the effectiveness of the PESD programs in promoting job retention using two different approaches: one to provide an upper-bound impact estimate of weeks spent in the starting job, the other to provide a more conservative (or lower-bound) impact estimate of weeks spent in the starting job. The upper-bound impact estimate assumes that all nonreporting of starting jobs is a result of recall error due to short jobs. To obtain this estimate, we calculated the amount of time sample members spent in their starting jobs assuming that the nonreporters held starting jobs that lasted, on average, two weeks. Based on this assumption, control group members, on average, spent between 22 and 27 weeks in their starting job (Figure 1, Graph A). In two of the four sites (Chicago and Riverside),

¹⁵(...continued)
income (for example, from odd jobs such as baby-sitting and yard work).

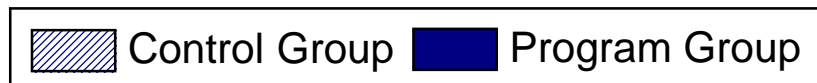
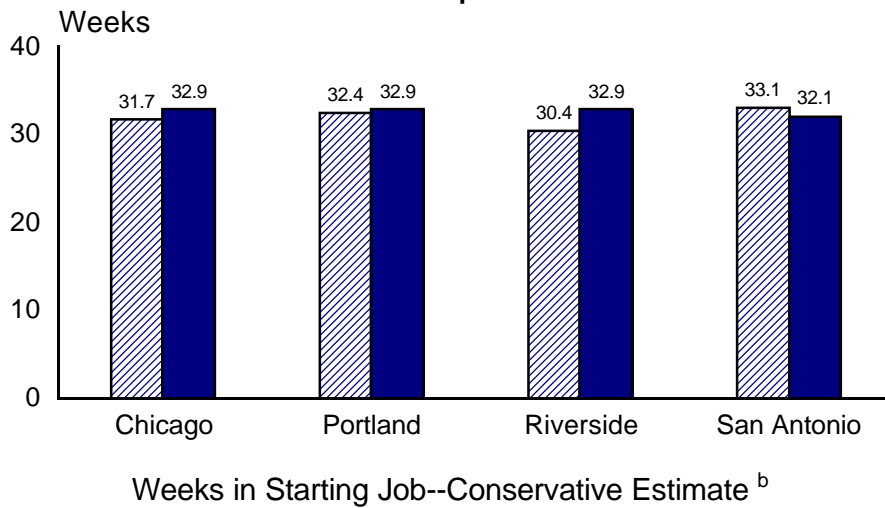
FIGURE 1

RANGE OF IMPACTS ON WEEKS IN THE STARTING JOB

Graph A



Graph B



SOURCE: PESD follow-up surveys conducted an average of 15 months after sample intake.

NOTE: Estimates are regression-adjusted.

* Program-control group differences are significantly different from zero at the 10 percent level, two-tailed test.

** Program-control group differences are significantly different from zero at the 5 percent level, two-tailed test.

^aCalculated based on the assumption that those individuals who did not report a starting job actually did have a starting job, and one that lasted an average of two weeks.

^bCalculated based on the assumption that those individuals who did not report a starting job actually did have a starting job, and one that lasted the average length of time as the starting jobs that were reported.

program group members spent three to four weeks longer in their starting job (a 14 to 19 percent increase) than the control group members. In the other two sites, there were no differences between the two groups. To the extent that the two-week assumption underestimates the time actually spent in the nonreported jobs, these estimates will overstate the programs' impact on employment length.

The more conservative impact estimate is based on a comparison of job tenure only for those who reported having a job. This estimate implicitly assumes that starting job nonreporters had jobs that lasted, on average, the same length of time as the jobs held by those who did report a starting job (or, at a minimum, about five months).¹⁶ Based on this estimate, control group members, on average, spent between 30 and 33 weeks in their starting jobs (Figure 1, Graph B). We find, then, that the differences in job tenure among those in the program group and those in the control group are much smaller based on this conservative assumption. For example, in Chicago and Riverside, program group members experienced only a 4 to 8 percent increase in starting job tenure, compared with a 14 to 19 percent increase when we assume a two-week job spell.

B. Did the Programs Increase Employment and Earnings During the First Year After Followup?

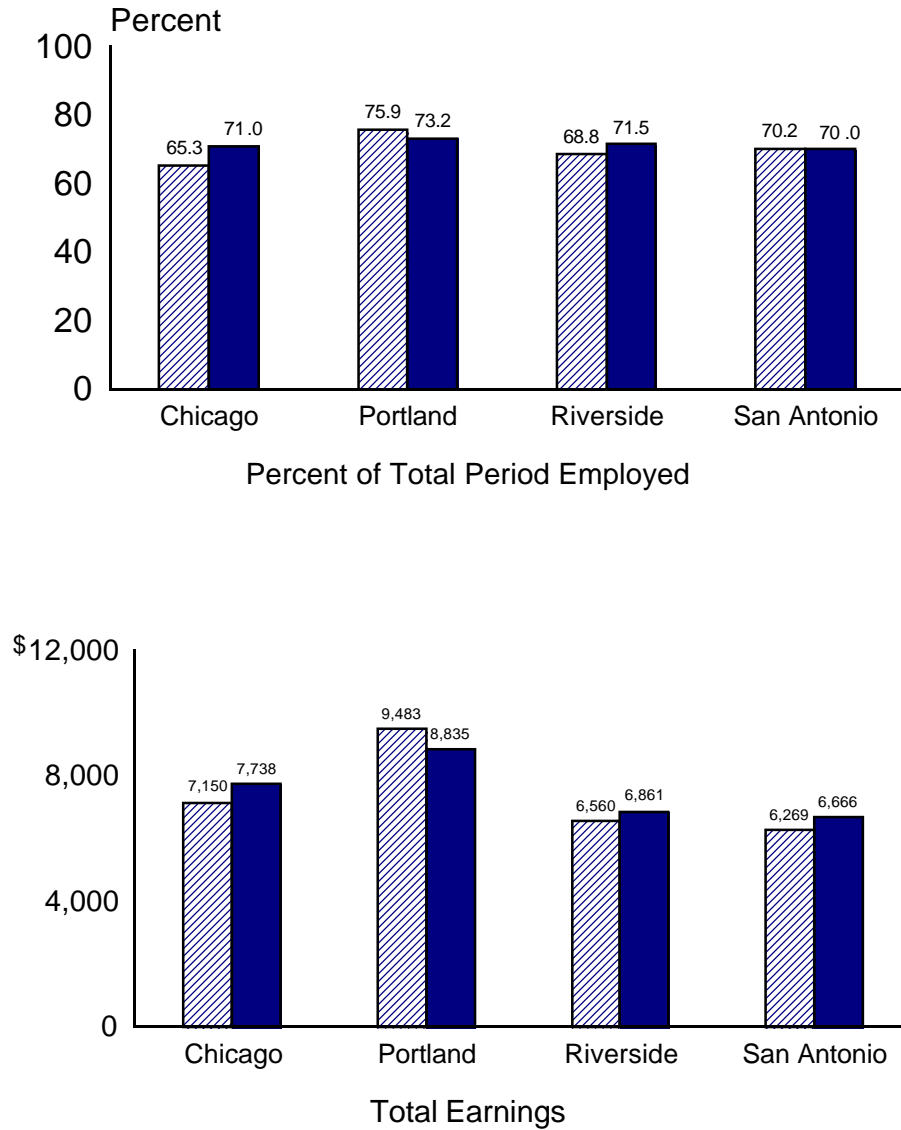
In addition to helping individuals stay in their jobs longer, the programs also tried to promote employment and earnings by helping those who lost jobs find other ones quickly. To assess this, we examined the extent and patterns of employment and earnings over the first follow-up year.



We find that sample members in the four sites were employed two-thirds to three-quarters of the weeks during the follow-up period (Figure 2). Those in the program group in Chicago

¹⁶This impact estimate assumes that the only reason that more program than control group members reported starting jobs is that the PESD program served as a trigger to help program group members better recall their jobs. Because we expect people who had jobs that lasted five months to remember their jobs, we view this calculation as providing a lower-bound estimate of program effects on job retention.

FIGURE 2

EMPLOYMENT AND EARNINGS OUTCOMES DURING THE FIRST YEAR AFTER INTAKE



 Control Group  Program Group

SOURCE: PESD follow-up surveys conducted an average of 15 months after sample intake.

NOTE: Estimates are regression-adjusted.

* Program-control group differences are significantly different from zero at the 10 percent level, two-tailed test.

** Program-control group differences are significantly different from zero at the 5 percent level, two-tailed test.

experienced a nearly six percentage point increase in the amount of time employed during the year compared with those in the control group (a nine percent increase over the control group mean). Program group members in Riverside experienced small increases in employment compared with those in the control group, whereas those in Portland and San Antonio experienced small decreases in employment. The magnitudes of the differences in employment in these three sites were fairly small, and none were statistically significant.

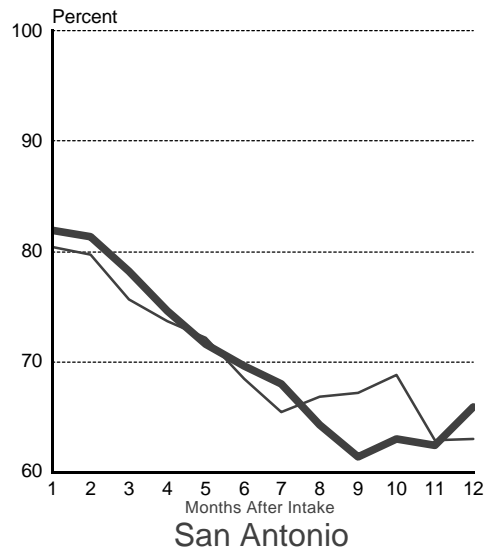
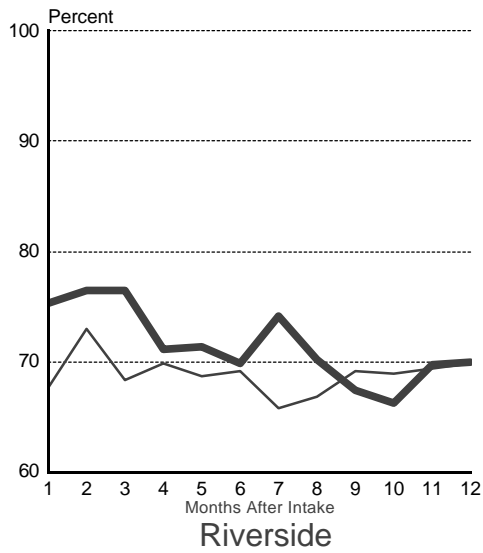
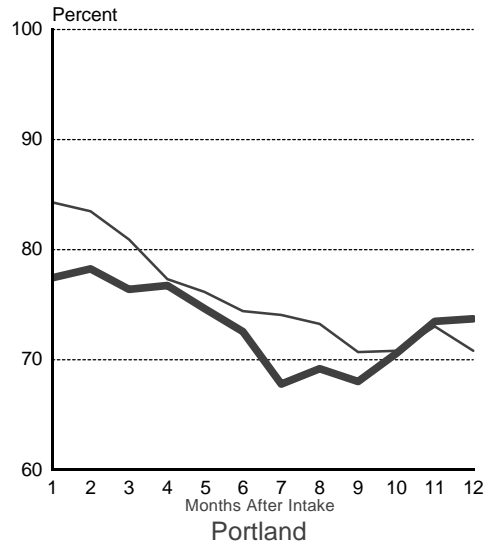
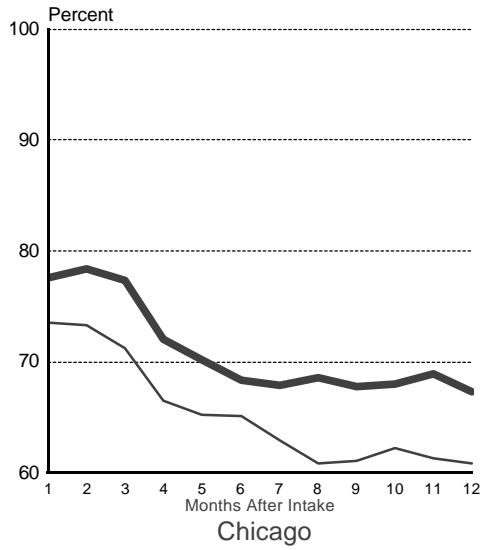
In all sites except Portland, we observe modest positive effects of the program on earnings (although none are significant).¹⁷ For example, those in the program group in these three sites experienced earnings gains of \$300 to nearly \$600 over the year, which translates to about a five to eight percent increase in annual earnings (Figure 2). The earnings increases in Chicago and Riverside are driven largely by employment increases among those in the program group. In San Antonio, the increase reflects the greater number of hours worked by program group members compared with hours worked by control group members (not shown). The impact in Portland continues the anomalous pattern for that site.

Figures 3 and 4 show patterns of monthly employment and earnings, respectively. In all sites, we observe a reduction in employment over the year among sample members in both the program and control groups. For example, approximately 70 to 85 percent of the control group members in the four sites reported being employed in the first month of followup; only 60 to 75 percent were employed 12 months after followup (Figure 3). Reductions in earnings over time were smaller than the reductions in employment (Figure 4); this fact suggests that those with low wages and/or low

¹⁷The estimates based on the available administrative records data for the larger sample are consistent with these findings.

FIGURE 3

PERCENT EMPLOYED,
BY MONTHS AFTER INTAKE



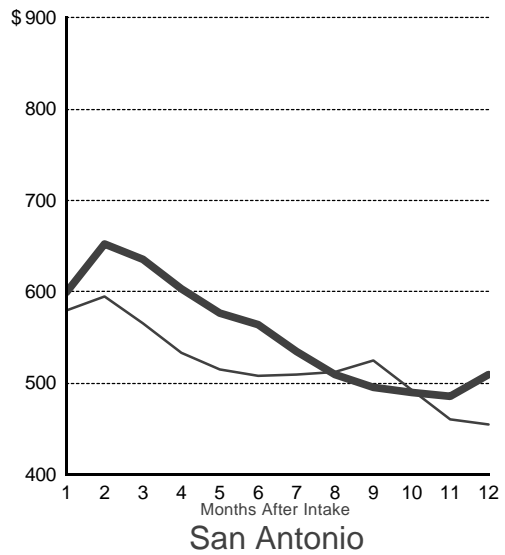
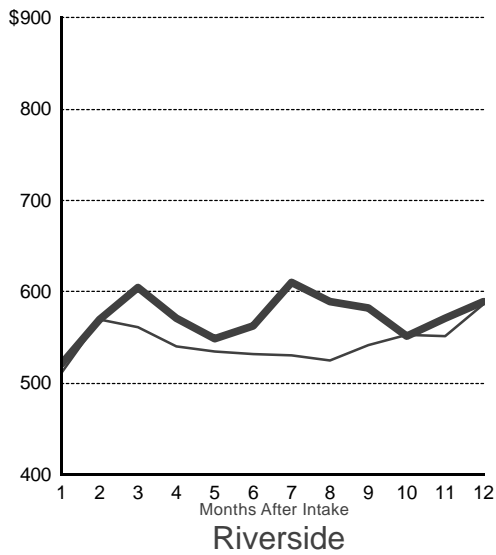
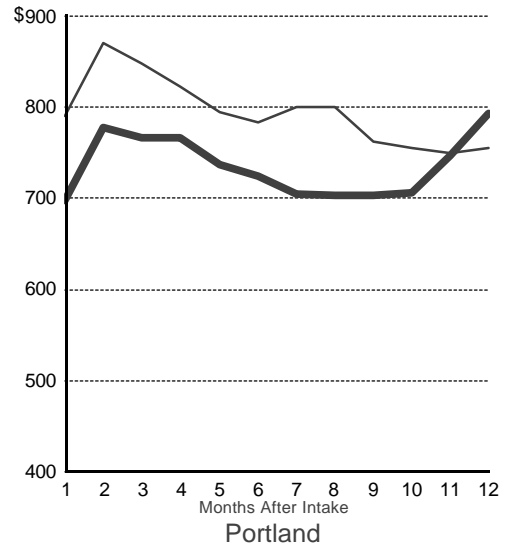
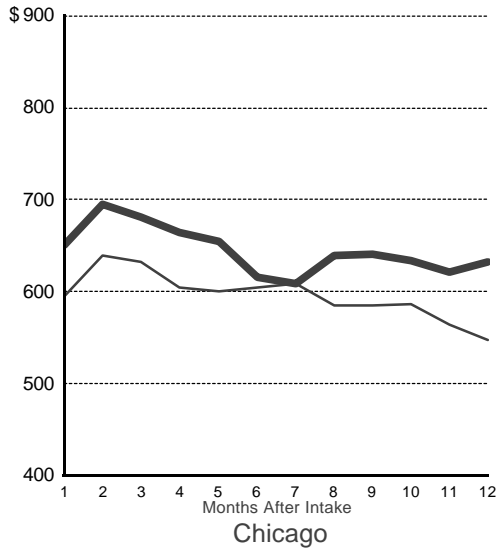
— Program Group — Control Group

SOURCE: PESD follow-up surveys conducted an average of 15 months after sample intake.

NOTE: Estimates are regression-adjusted. See Appendix Table B.1 for monthly impact estimates and the associated p-values that show the level of significance of the impacts.

FIGURE 4

AVERAGE MONTHLY EARNINGS,
BY MONTHS AFTER INTAKE



Program Group
 Control Group

SOURCE: PESD follow-up surveys conducted an average of 15 months after sample intake.

NOTE: Estimates are regression-adjusted. See Appendix Table B.1 for monthly impact estimates and the associated p-values that show the level of significance of the impacts.

hours were more likely to leave employment, and the higher wage earners (or those who received raises) and/or those who worked high hours were more likely to stay employed.

Program effects on employment and earnings over the follow-up year reflect fairly closely the overall program effects in aggregate employment and earnings. Chicago had the most consistent patterns of employment and earnings impacts over the first follow-up year. In this site, throughout the year following program entry, between 60 and 73 percent of those in the control group were employed in any given month. The employment rate for those in the program group was 3 to 8 percentage points higher in each month of the year (Figure 3), which represents a 5 to 12 percent increase in the employment rates of those in the program group, compared with those in the control group.¹⁸ The employment impacts in Riverside and San Antonio show greater variance. For example, in Riverside, we observe an increase in employment of seven to eight percentage points in some months and a decrease of two to three percentage points in other months. In both Riverside and San Antonio, however, we find positive earnings impacts (especially during the early months after random assignment). In Portland, consistent with the earlier findings, we observe negative effects on employment and earnings during most of the follow-up period.

Most of the jobs sample members held were in service, sales, and administrative support positions. Fewer than 10 percent of the jobs were in professional occupations. Between 6 and 14 percent of the jobs were in production and construction (Table 1). We do not observe many differences in the occupational distribution of jobs held by sample members in the program and control groups.

¹⁸Although these impacts are not statistically significant, the magnitude and consistency of patterns of impacts lead us to believe that the lack of statistical significance is likely due to the relatively small sample sizes in our survey sample.

TABLE 1
DESCRIPTION OF JOB CHARACTERISTICS AT THE TIME OF THE SURVEY
(Percentage)

	Chicago		Portland		Riverside		San Antonio	
	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a
Occupation								
Manager/professional/technical	6.0	0.3	7.4	1.0	4.9	3.5	7.0	1.4
Sales	16.0	3.4	12.2	-1.9	13.9	2.2	17.5	3.3
Secretarial	1.3	0.8	1.4	0.8	2.8	0.0	2.1	1.8
Other administrative support	28.7	-1.6	36.5	0.3	25.7	-4.0	30.0	-0.1
Food and beverage preparation	6.0	-3.9	7.4	-2.2	10.4	-2.7	7.0	-1.8
Health services	14.0	2.7	11.5	1.4	7.6	1.5	19.6	-4.7
Private household/protective services	8.7	-1.1	1.4	1.2	6.9	2.2	1.4	0.6
Other services	7.3	1.0	6.1	1.6	9.7	-2.0	8.4	-1.9
Mechanical/construction/ production	11.3	-1.6	14.2	-3.2	14.6	0.6	6.3	1.5
Other	0.7	0.0	2.0	0.6	3.5	0.0	0.7	0.2
Benefits Offered on the Job								
Health insurance	44.8	12.5**	61.6	-3.2	37.4	-4.3	39.0	17.7**
Paid sick leave	30.5	14.8**	44.1	-1.7	31.8	-1.4	30.9	8.7
Paid vacation	48.7	12.7**	63.0	4.7	35.8	1.2	44.6	6.4
Child care assistance	4.1	0.9	6.3	-2.9	4.3	-1.4	3.6	6.4**
Flexible hours	31.3	11.1	47.6	-11.2**	34.0	0.0	39.3	-0.8
Transportation	8.7	-1.0	4.1	3.0	4.2	7.7**	2.8	2.4
Retirement benefits	26.7	12.2**	46.4	-10.2*	26.5	-6.0	26.0	6.1
Job Benefits Used (Among Those Offered the Benefit)								
Health insurance	25.0	15.7**	50.3	-0.3	44.2	5.8	27.8	12.7
Flexible hours	80.0	4.6	76.8	2.5	75.0	-0.5	71.7	6.5
Shift Worked								
Day/afternoon shift	67.3	-3.2	74.8	-4.9	75.7	-6.5	68.8	9.1
Evening/graveyard shift	16.7	5.4	11.6	0.6	10.4	2.2	10.4	-3.9
Variable shift	16.0	-2.2	13.6	4.4	13.9	4.3	20.8	-5.2
Sample Size	155	305	156	315	152	302	150	312

SOURCE: PESD survey data.

NOTE: These data pertain to the job held at the time of the survey or the job held just prior to the interview.

^aThe estimated impact represents the difference between the program and control group means.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

In terms of job quality, as measured by earnings and benefits, sample members in both groups in Portland generally had better jobs (jobs that offered higher pay and more benefits), and sample members in Riverside held jobs that offered relatively low wages and the fewest benefits. About half the sample members across all sites held jobs that offered health insurance. Program group members in Chicago and San Antonio were more likely than control group members to hold jobs that offered health insurance.

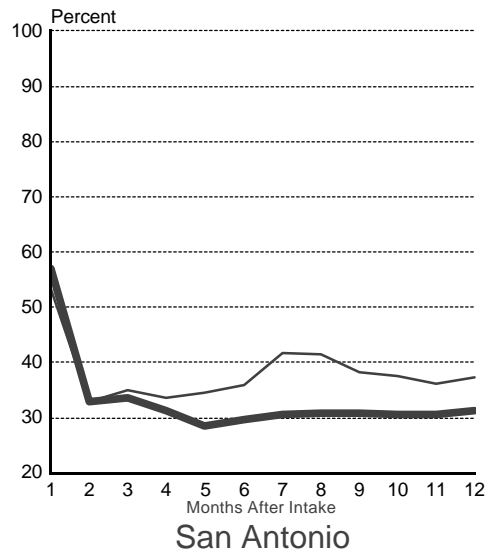
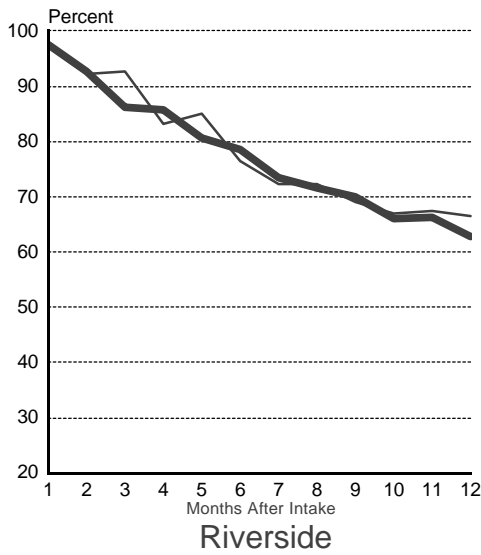
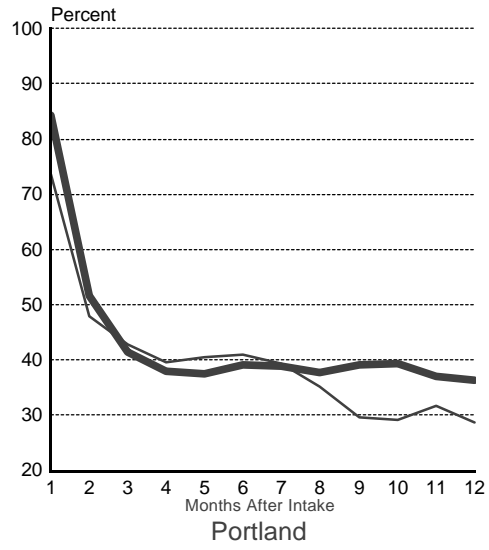
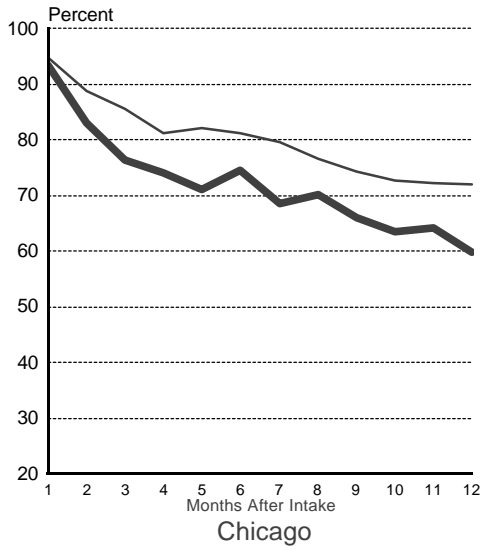
C. Did the Programs Lead to Reductions in Welfare Receipt During the First Year Following Intake?

To assess the extent to which sample members' reliance on welfare may have decreased over time as employment and earnings increased, we examined AFDC and food stamp receipt and benefit amounts over the follow-up year. To understand the differences in welfare experiences between program and control group members during this year, it is useful, first, to review the context in which these differences occurred.

In all sites during the months following random assignment, we observe a sharp reduction in the number of both program and control group members receiving welfare and a decrease in the average benefit amounts (Figures 5 and 6). The levels of benefit amounts and the decrease in welfare receipt varied across the sites, primarily reflecting the state policies regarding AFDC benefit generosity and earnings disregard. For example, levels of AFDC receipt stayed high in Riverside, where there were high benefits and an indefinite earnings disregard program; hence, most individuals who found jobs continued to receive welfare. Similarly, the two-thirds earnings disregard rules in Chicago led most individuals in that site who found jobs to continue to receive welfare. In these two sites, we observe a gradual reduction in welfare receipt during the first few months after job start

FIGURE 5

PERCENT RECEIVING AFDC,
BY MONTHS AFTER INTAKE



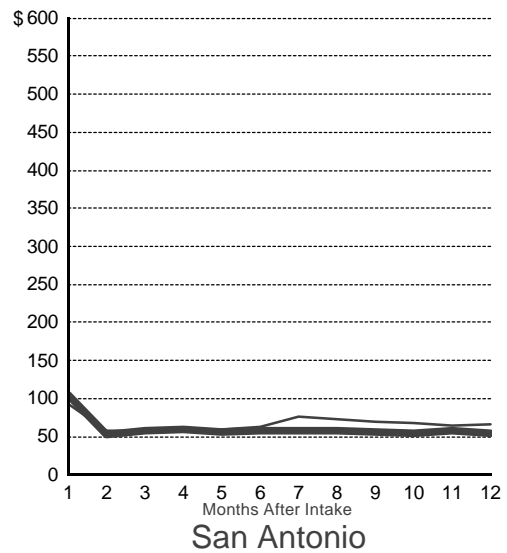
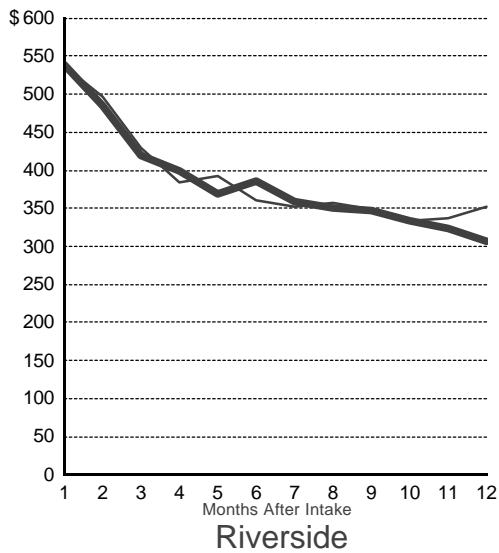
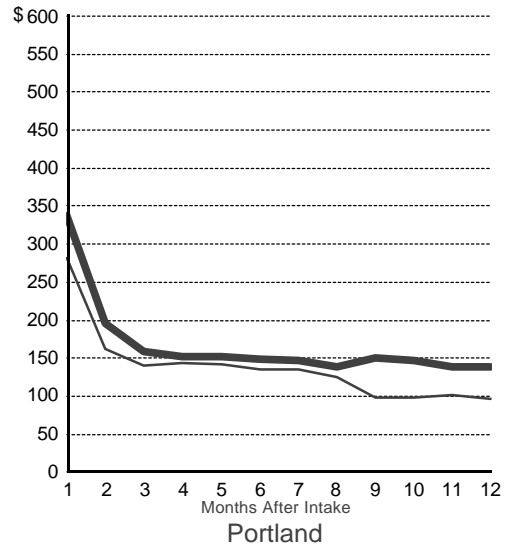
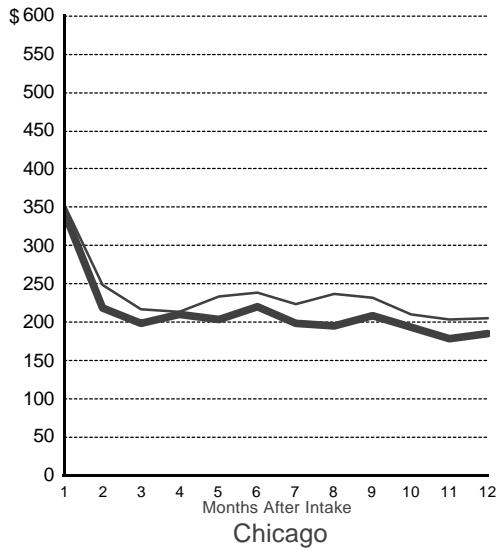
SOURCE: PESD follow-up surveys conducted an average of 15 months after sample intake.

NOTES: Estimates are regression-adjusted. See Appendix Table B.2 for monthly impact estimates and the associated p-values that show the level of significance of the impacts.

Site-by-site variation in the proportion of sample members receiving AFDC primarily reflects differences in state policies and program context related to AFDC benefits and earnings disregard.

FIGURE 6

AVERAGE MONTHLY AFDC RECEIPT,
BY MONTHS AFTER INTAKE



— Program Group — Control Group

SOURCE: PESD follow-up surveys conducted an average of 15 months after sample intake.

NOTES: Estimates are regression-adjusted. See Appendix Table B.2 for monthly impact estimates and the associated p-values that show the level of significance of the impacts.

Site-by-site variation in the level of AFDC benefit amounts for sample members primarily reflects state policies and program context related to AFDC benefits and earnings disregard.

but a large number of individuals (60 to 70 percent) continued to receive AFDC 12 months after intake.

In contrast, levels of AFDC receipt fall off fairly rapidly for employed welfare individuals in San Antonio and Portland, and we see a sharp decline in benefit receipt among sample members in these two sites during the first few months after intake. San Antonio and Portland both had the standard \$30 and one-third of earnings disregard for the first four months. Moreover, Texas is such a low-benefit state that an individual earning full-time minimum wage would almost immediately not be eligible for welfare. In these two sites, benefit receipt drops during the first few months and then stays fairly constant at a low level (between 30 and 40 percent) in the subsequent months.

In light of the welfare context in each of the sites, we now examine the impact of PESD on welfare receipt and benefit amounts during the year after intake. Table 2 shows AFDC and food stamp benefits for the entire one-year period after intake, while monthly patterns of AFDC receipt and benefits and food stamp receipt and benefits are shown in Figures 5 through 8, respectively. In terms of welfare receipt, we find evidence of significant program effects in both Chicago (a relatively high-benefit welfare site) and San Antonio (a relatively low-benefit one). In both sites, those in the program group had significantly lower rates of AFDC and food stamp receipt in the year following random assignment than those in the control group. For instance, Chicago and San Antonio program group members were likely to be receiving AFDC and food stamps for about 10 percent less time (or one less month) over the year than those in the control group. Program group members in these two sites also received smaller AFDC and food stamp benefit amounts--a 7 to 10 percent reduction in benefit amounts. Monthly patterns of impacts are consistent with overall impacts. They show, for example, that program group members in both sites consistently

TABLE 2

AFDC AND FOOD STAMP BENEFITS DURING THE FIRST YEAR AFTER INTAKE

	Chicago		Portland		Riverside		San Antonio	
	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a
AFDC								
Percentage of Total Period Receiving AFDC	80.1	-7.9**	39.9	3.8	77.3	-0.4	38.1	-4.8
Average Number of Months Receiving AFDC Among Those Who Received It	10.0	-0.9**	5.5	0.1	9.4	-0.2	6.5	-1.4**
Average Monthly Benefits (Dollars)	235	-21	139	31**	392	-8	67	-6
Food Stamps								
Percentage of Total Period Receiving Food Stamps	82.5	-7.3**	73.6	1.5	71.0	6.2	87.6	-8.9**
Average Number of Months Receiving Food Stamps Among Those Who Received Them	10.0	-0.8**	8.9	0.2	9.2	-0.6	10.7	-0.9**
Average Monthly Food Stamp Benefits (Dollars)	192	-19*	152	6	125	-9	218	-16
Sample Size	155	305	156	315	150	302	150	312

SOURCE: PESD data.

NOTE: Estimated impacts are regression-adjusted. Appendix Table B.4 presents the p-values that show the level of significance of the estimated impacts.

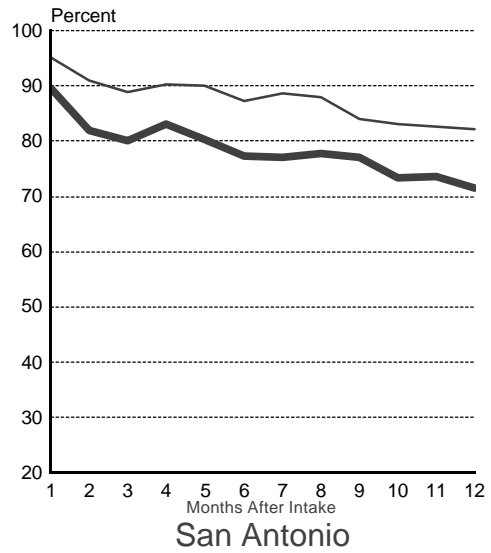
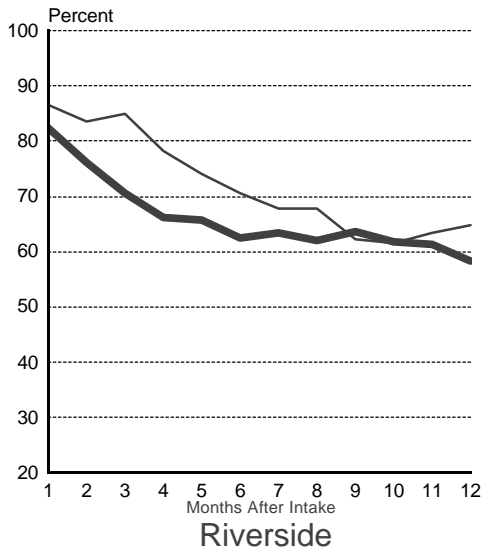
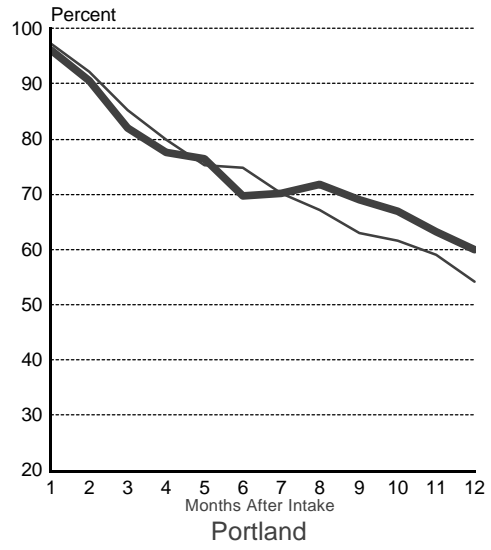
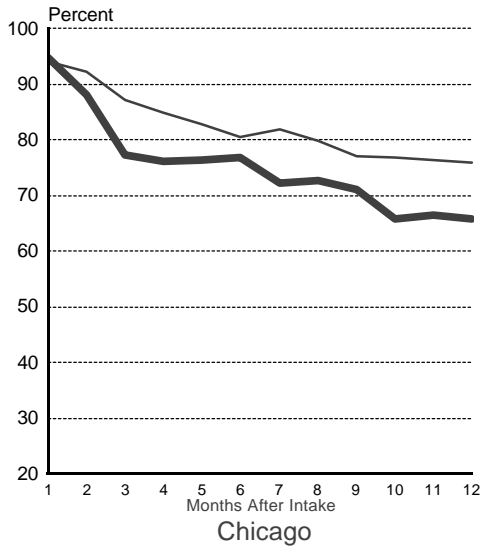
^aThe estimated impact represents the difference between the program and control group means.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

FIGURE 7

PERCENT RECEIVING FOOD STAMPS,
BY MONTHS AFTER INTAKE

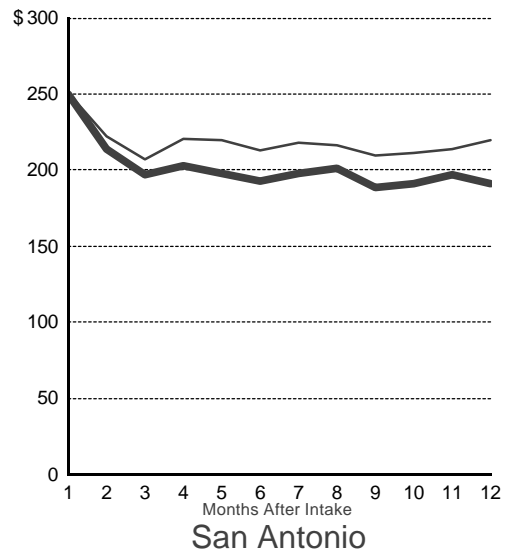
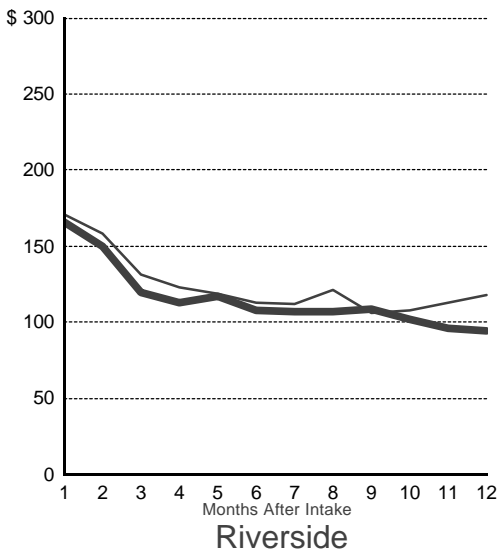
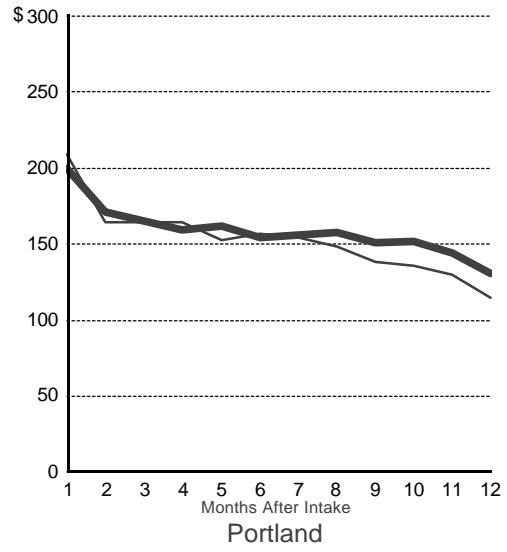
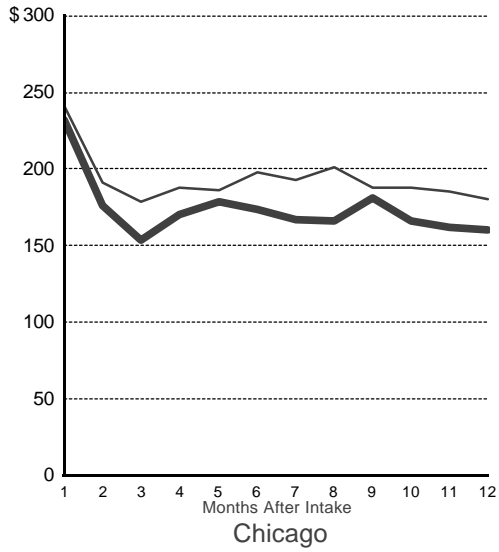


SOURCE: PESD follow-up surveys conducted an average of 15 months after sample intake.

NOTE: Estimates are regression-adjusted. See Appendix Table B.3 for monthly impact estimates and the associated p-values that show the level of significance of the impacts.

FIGURE 8

AVERAGE MONTHLY FOOD STAMPS RECEIPT,
BY MONTHS AFTER INTAKE



— Program Group — Control Group

SOURCE: PESD follow-up surveys conducted an average of 15 months after sample intake.

NOTE: Estimates are regression-adjusted. See Appendix Table B.3 for monthly impact estimates and the associated p-values that show the level of significance of the impacts.

experienced about a 10 percent reduction in food stamp benefits during most of the months following intake.

Results are more mixed in the Riverside site, where we do not observe any differences in the AFDC receipt of program or control group members, but we do observe modest reductions in food stamp receipt among program group members. Finally, in Portland, we actually see significant increases in AFDC benefit receipt over the follow-up period. Program group members in Portland were more likely than control group members to receive AFDC and food stamp benefits throughout most of the year, especially during the later months.¹⁹

To understand how the programs supported movement toward self-sufficiency and out of poverty, we examined the components of total income over the follow-up year (Table 3). For welfare recipients who found jobs, just over half of their income was from earnings, and the rest was divided between AFDC and food stamps.²⁰ In three of the four programs, sample members experienced an increase in the proportion of their total income from earnings and a reduction in the proportion from welfare over the follow-up period; in Chicago, this effect is statistically significant.

¹⁹For the Riverside and Portland sites, the impact estimates for welfare receipt that are based on the available administrative records for the full sample differ somewhat from those based on the survey sample. In Riverside, where nearly complete records data are available for eight months, we observe increases in AFDC and food stamp receipt and benefit amounts among program group members compared with control group members. This finding may reflect the efforts of the Riverside case managers to secure benefits for their PESD clients. In contrast, in Portland, where records data are much less complete, we observe a modest reduction in the receipt of AFDC and food stamp benefits, a result contrary to what we observed using the survey sample. These differing results serve as a reminder that the results presented in this report are preliminary and should be viewed with some caution.

²⁰Sample members received another seven percent, on average, from other sources, including Supplemental Security Income, Unemployment Insurance, help from family/friends, and child support. Unfortunately, we do not have information for the entire year on income obtained from these sources.

TABLE 3
TOTAL INCOME AND ITS SOURCES OVER THE FOLLOW-UP YEAR

	Chicago		Portland		Riverside		San Antonio	
	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a	Control Group Mean	Estimated Impact ^a
Total Income (Dollars)	12,260	153	12,982	-205	12,760	93	9,696	126
Earnings (Dollars)	7,151	588	9,483	-648	6,560	301	6,269	397
Unearned Income (Dollars)								
AFDC	2,815	-253	1,664	375**	4,701	-96	807	-78
Food stamps	2,309	-228*	1,826	77	1,498	-112	2,620	-194*
Proportional Contribution of Various Sources to Total Income (Percentage)								
Earnings	50.3	6.1*	65.3	-2.1	47.5	1.7	57.5	3.2
AFDC	27.4	-3.2	16.9	2.5	39.9	-1.0	11.2	-1.4
Food stamps	22.3	-2.9*	17.8	-0.4	12.5	-0.7	31.2	-1.8
Income as Percentage of Poverty Level (Percentage)		^b						
Less than 75	49.2	-8.0	34.0	5.1	40.1	0.7	62.2	5.5
75 to 99	18.2	7.1	24.0	-2.8	26.0	1.0	25.8	-3.4
100 or higher	32.6	0.9	41.9	-2.3	33.9	-1.6	12.0	-2.1
(Average)	(83.1)	(1.9)	(92.3)	(0.5)	(87.6)	(-1.6)	64.3	-0.2
Sample Size	155	305	156	315	152	302	150	312

SOURCE: PESD survey data.

NOTES: Estimated impacts are regression-adjusted. Appendix Table B.4 presents the p-values that show the level of significance of the estimated impacts.

Estimated impacts based on available administrative records data for the full sample are not provided for these measures. Given their aggregate nature, using incomplete data to calculate estimates for these measures would not be appropriate.

^aThe estimated impact represents the difference between the program and control group means.

^bThe distribution of income as a percentage of the poverty level is statistically significant at the .10 level, two-tailed test.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

V. INTERPRETATION AND CONCLUSIONS

The preliminary analysis of the interim impacts of PESD suggests that one site was modestly successful in promoting employment and reducing welfare, and two sites had somewhat similar (although smaller) effects. In the fourth site, we actually observe opposite results--lower employment and increases in welfare receipt. Even though these results are based on a subsample of demonstration enrollees, combined with our examination of the incomplete administrative records data (at the time of the analysis), we expect only small or modest effects at best in the final analysis.

What do we make of these modest effects, and why do some programs appear more successful than others? Three factors should be considered in interpreting program impacts: (1) the experimental and evolving nature of the PESD programs, (2) the extent to which case management services matched clients' diverse needs, and (3) the services already available to those in the control group.

The four PESD programs were fairly experimental in nature, with each program evolving and learning as it went along. PESD was the first large-scale program of job retention services set in the context of the state welfare program. None of the programs had any prior service delivery models on which they could build or from which they could benefit. Consequently, the PESD programs evolved throughout the demonstration period, as program staff worked toward full implementation. Gradually, case managers learned how to determine what specific types of services to provide, how to deliver the services, over what period of time to deliver services, and, in some cases, how and to what extent to integrate PESD service delivery with the JOBS program and connect it to the welfare system.

As case managers started delivering services, they found that several of the planned services could not be delivered. For example, case managers had hoped to resolve workplace conflicts by

talking directly to employers and thereby promoting job retention. However, most clients did not want case managers to intervene directly with employers, and this aspect of service delivery never took place. Similarly, the programs had anticipated that case managers would promote the use of the advance payment option of the EITC. PESD staff did provide information around tax time on obtaining the EITC; however, little was done to promote the advance payment option of EITC, a feature that could have enabled some clients to have more take-home pay each month. Nearly half the sample members received EITC (significantly more program group members than control group members--49 versus 42 percent), but less than one in five of these used the advance payment option.

In contrast, case managers spent a substantial amount of time on issues they had not anticipated. For example, case managers spent a lot of time attempting to resolve benefit eligibility and payment errors for clients; they helped correct income maintenance errors, helped obtain eligibility for transitional child care, and resolved child care payment errors. Case managers spent some time early on learning how to work with other agency staff to resolve these issues. To the extent that these services may not have the same immediate effect on employment as other services might (such as direct intervention with clients' employers to prevent job loss or job search assistance), the effects of these services may become more evident in the longer term.

In terms of the period over which services were delivered, the original intention (at the time of program planning) was that case management contacts and services would be regular and intensive during the early stages of clients' participation but much less so over time. This would keep the average caseload size manageable at any given point. However, many clients lost their jobs fairly quickly and required services over a longer than expected time period. Case managers needed to work actively with such clients on an ongoing basis to help them find and keep jobs and resolve crises or difficulties that arose after the first few months after job start. As caseloads increased over

time, case managers may not always have been able to provide services to all clients who might have needed assistance.

Since case management services are more appropriate for some clients than for others, the services delivered may not always have matched the diverse needs of all clients. The program guidelines required case managers to establish and maintain some contact with all clients who were enrolled in the programs, regardless of their needs. The process of contacting clients and informing them about PESD services often was challenging and time-consuming, especially since it was a new program and was separated from the rest of the welfare system. Case managers were given fairly wide latitude with respect to service delivery and received little guidance on how to serve clients with different types and levels of need, including those with few or no needs and those with multiple needs. Although case managers in each site implemented the program guidelines somewhat differently, they often spent some time trying to maintain regular contact with most clients, regardless of clients' needs. In some instances, case managers spent a lot of time with a small number of clients with severe needs or attempted to maintain contact even with clients who indicated they did not want any services. Over time, these practices imposed constraints on case managers' time and may have made it difficult to provide services to the clients who most needed assistance. A systematic attempt to distinguish up front which clients most needed intensive case management services and which could make it on their own might have resulted in services that were better tailored to meet clients' diverse needs.

The program context and services already available to those in the control group also influenced the magnitude of estimated program effects. Typically, estimated impacts are largest when a program provides new services that are not already available in the existing system, and those in the control group have access to very few services. To the extent that those in the control group

are receiving services similar to what those in the program group are receiving, or are receiving alternative types of supportive services, measured program impacts are likely to be diluted. In the two sites (Chicago and San Antonio) where we observe modest increases in earnings and decreases in welfare, relatively limited case management services were available to control group members through the JOBS program. In the other two sites (Riverside and Portland), where control group members had access to some similar services, we find smaller effects and effects that move in the opposite direction. In some sense, failure to find the expected program impacts in Riverside and Portland, where control group members had access to some services, may actually reinforce the value of making available job search and other services for employed welfare recipients.

We suspect that the lack of welfare impacts in Riverside may be due to the strong JOBS program in that state--the Greater Avenues for Independence (GAIN) program.²¹ The GAIN intervention has been nationally recognized as a program that makes a strong push to help individuals find jobs and get off welfare. The strong work orientation and constant push by GAIN case managers to find employment for clients could have led some control group individuals to leave welfare even without having a job. For example, PESD clients in the Riverside focus groups frequently complained about the strong work orientation of the GAIN caseworkers and talked about how much more they liked the friendlier approach of the PESD case managers. This approach of the PESD case managers might have led some clients who would otherwise have gone off welfare to remain on it. Moreover, the philosophies of the two programs could have differed somewhat, with the GAIN caseworkers focusing on reducing welfare dependency, while the PESD case managers focused on helping

²¹As mentioned previously, estimates based on available administrative records data for the full sample show that welfare benefits in Riverside were actually higher for program group members than control group members one year after intake and significantly so in more than half of the months.

employed clients stay employed and accessing all the benefits that would facilitate their transition from welfare to work.

The findings in Portland, where earnings decreased and welfare benefits increased among program group members relative to those in the control group, are the most puzzling. The findings from our process analysis do suggest that Portland may have offered a greater extent of similar services to control group members than the other sites did.²² Specifically, according to program staff, control group members were somewhat likely to avail themselves of the postemployment services available under the regular JOBS program because they had had previous contact with the job placement case managers. Furthermore, all sample members (both program and control group) had access to the resource room in the placement office where they could look for job postings and use a computer to update their resumes.

While the availability of these services could have resulted in no impact, it does not explain why program group members did *worse* compared with those in the control group.²³ A few features peculiar to the Portland PESD program may be partially responsible for these findings. First, Portland was the only site where PESD case managers did not fully assume the position of JOBS program case manager; instead, they shared responsibility with the JOBS case managers. It is possible, then, that clients who did not want program staff to contact them could more easily slip through the cracks as a result of the shared responsibility. Second, differences in how program and

²²See Haimson, Hershey, and Rangarajan (1995) for a detailed description of program implementation in the four sites, as well as the types of services available to those in the program group and control group in each site.

²³In Portland, the estimated impacts from the partial administrative records data differ more from the estimated impacts based on the survey data than in any other site. This suggests that these negative impacts in Portland may be related to the sample and data used in this study and should be interpreted with some caution.

control group case files were processed and transmitted may have affected how quickly some sample members received services. In Portland, case files of all welfare recipients were sent from the placement center back to the branch offices three months after a client had exited welfare. If a client went back on welfare, the PESD case managers were supposed to receive the case files. Sometimes (especially early in the demonstration, when no process had been established) it took a long time for the files to reach the PESD case managers. In contrast, control group members who went back on welfare were sent to a two-week job search program and then back to the placement center if they did not find a job within this period. As a result, some control group members may have found new jobs or received job search services fairly quickly upon job loss. Finally, of the four sites, the Portland program, because of its decentralized, self-directed team approach to managing the PESD program, allowed the greatest amount of flexibility and imposed the least amount of supervisory structure or oversight on the program. The case managers in Portland were as dedicated as the case managers in the other three sites; however, this lower level of structure, combined with the more complex interactions with JOBS and branch office staff, may have undermined the case managers' efforts. Despite these speculations, the negative effects in Portland are puzzling. These findings may be due to some of the programmatic differences just discussed, or they might be an artifact of the sample and data used in this study.

Since these programs were a first attempt to provide postemployment services, even the evidence of modest preliminary effects of the programs in three sites gives some reason for optimism about the usefulness and importance of providing job retention services to welfare recipients who have recently found jobs. Given some of the inconsistencies in the estimates based on the survey with those based on the administrative records data, however, we are less confident that the findings in Portland and Riverside will be sustained over time. The analysis in the final report will use

complete data for the full sample and will allow us to draw more definitive conclusions, as well as discuss the implications of targeting services in subgroups of the population. Other programs that set up services to promote job retention may build on the experiences of PESD service delivery and may have more success in promoting employment.

As the new welfare law is implemented, and more individuals with less experience enter the labor market, states will have to develop strategies to improve job retention. Through the final impact analysis, we will be better equipped to more fully explore questions relating to the effectiveness of case management services in promoting job retention. Nevertheless, in this preliminary analysis, we identify several key issues that will provide a framework for programs that are thinking about providing job retention services:

- ***Some clients are able to sustain employment on their own and will need only little or short-term assistance to meet specific needs. Others, however, face multiple barriers and will need ongoing assistance; programs should attempt to target these clients for ongoing case management support.*** In the context of a strong economy, about 60 percent of those who found jobs were able to sustain employment over the follow-up year; most of them likely needed only little or short-term assistance. Over time, however, more clients who are less job ready are likely to enter the labor market, as the strong work requirements of the new welfare law get implemented. While time limits may motivate some of these individuals to hold on to their jobs, it is likely that many will need some supportive services to enable them to stay employed. Programs will need to determine up front which clients most need ongoing support from a job retention specialist and which clients need only short-term assistance.
- ***Simplifying service delivery mechanisms may ease the transition from welfare to work for many clients and free case management resources for those clients who need it most.*** By changing procedures so that clients can more easily access services and by integrating functions across agencies so that duplication and delays can be eliminated, program staff may be better equipped to meet clients' needs efficiently. For example, simplifying access to child care resources or subsidies may be desirable in some states where these procedures are complicated. States may also want to provide immediate job search assistance to clients who have lost jobs, instead of waiting for individuals to come back onto welfare so that they can "qualify" for job search services. These types of system changes can reduce the administrative steps case managers and job retention specialists must take to provide certain services, effectively giving them more time to concentrate on providing other services and meeting other client needs.

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APPENDIX A

SAMPLE, DATA, AND ANALYSIS METHODS

This appendix lays out the framework for the interim impact analysis. It includes a discussion of the sample sizes and data sources used in the impact analysis, the characteristics of the survey sample, and the methods used to estimate program impacts.

A. STUDY SAMPLE SIZES AND DATA SOURCES

During a 12- to 18-month period between March 1994 and December 1995, at each of the four sites, between 775 and 1,545 newly employed welfare recipients were identified and enrolled in the Postemployment Services Demonstration (PESD). Between 400 and 500 from each site were randomly selected to participate in the PESD program; the rest became part of a control group receiving regular services available to employed welfare recipients in the state (Table A.1). At the time of this interim analysis, two sources of data relating to these sample members were available: (1) a partially complete set of administrative records for the full sample, and (2) a complete set of survey data for a representative subsample. Each data source has distinct advantages and disadvantages that influenced our decision about the data and sample to use in this analysis.

1. Administrative Records Data for PESD Sample Members

Data on wages and welfare receipt from state administrative records are being collected for all sample members for a two- to two-and-a-half-year period following random assignment. These data include quarterly earnings, as well as monthly AFDC and food stamp receipt and benefit amounts. The primary advantage of administrative records data is that they are potentially available for all sample members over the entire follow-up period. Furthermore, administrative records data, especially welfare data, provide accurate information at baseline and on benefit receipt and amounts for each month following random assignment. The administrative wage records data include fairly

TABLE A.1
THE STUDY SAMPLE AND SURVEY SUBSAMPLE

Sample Sizes	Chicago	Portland	Riverside	San Antonio ^a	Total
Sample Enrolled in Demonstration	1,545	804	1,506	778	4,633
Program Group	552	425	500	386	1,863
Control Group	993	379	1,006	392	2,770
Survey Sample					
Attempted Follow-Up Survey	428	424	428	425	1,705
Program Group	214	211	214	211	850
Control Group	214	213	214	214	855
Completed Follow-Up Survey	306	315	302	313	1,236
Program Group	155	156	150	150	611
Control Group	151	159	152	163	625
Response Rates (Percentage)	71.5	74.3	70.6	73.6	72.5
Program Group	72.4	73.9	70.1	71.1	71.9
Control Group	70.6	74.6	71.0	76.2	73.0

^a A total of 1,240 sample members were enrolled in the San Antonio site. About 37 percent of the sample (462 of the early enrollees) are not being included in our impact study because of problems with random assignment early in the demonstration.

accurate information on covered earnings. The main drawback of these data is that they do not contain the rich detail of employment information that is usually available in survey data. At the time that we conducted this analysis, however, we did not have a complete set of administrative records data for all our sample members--we had received only six to nine months of complete wage record and AFDC data for most sample members (Table A.2).¹

2. Survey Data for a Representative PESD Subsample

Employment, earnings, and welfare data for a representative subsample of program and control group members were collected through a telephone survey conducted by Mathematica Policy Research, Inc. Approximately 300 sample members from each site were randomly selected from among those who had enrolled between July 1994 and August 1995 and were included in the survey sub-sample (Table A.1).² The survey was conducted approximately a year following random assignment and asked respondents to recall their employment and other experiences during that year. The key advantage of the survey data is that, compared with the records data, these data provide a richer array of employment information on the jobs held by sample members during the one-year follow-up period, including job start and end dates, hours worked in each job, average earnings, hourly wages, occupation, and benefits for each job. Furthermore, these data are consistently available for a one-year follow-up period for all individuals for whom we have survey data. The primary drawback of the survey data is that they are conducted with only a subset of the sample

¹For Chicago and Riverside, we had records data for almost all of the sample for the first nine months. For Portland and San Antonio, where program intake was slower than initially anticipated, we had records data for only about half of the sample for the first six months.

²We completed follow-up surveys by telephone with more than 70 percent (1,236 out of 1,705) of the individuals we attempted to survey (Table A.1).

TABLE A.2

THE STUDY SAMPLE: AVAILABLE ADMINISTRATIVE RECORDS DATA

	Chicago		Portland		Riverside		San Antonio	
	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group
Total Sample Enrolled in Demonstration	552	993	425	379	500	1,006	386	392
Available Employment and Earnings Data								
Quarter 1	552	991	405	355	500	1,006	323	329
Quarter 2	494	889	318	279	500	1,006	220	215
Quarter 3	355	636	235	216	403	825	141	142
Quarter 4	288	518	152	154	304	641	82	84
Available Welfare Data								
Month 1	551	993	425	379	499	1,005	383	390
Month 2	551	993	425	379	499	1,005	383	390
Month 3	551	993	425	379	499	1,005	383	390
Month 4	551	993	421	373	499	1,005	363	365
Month 5	551	993	403	354	499	1,005	335	340
Month 6	551	993	373	325	499	1,005	300	301
Month 7	547	978	351	301	499	1,005	254	251
Month 8	493	891	317	277	499	1,005	228	223
Month 9	446	817	284	255	472	950	207	203
Month 10	394	722	263	238	442	895	177	177
Month 11	354	637	235	213	402	824	144	145
Month 12	335	594	212	192	376	771	123	125

SOURCE: Administrative records data.

members and the relatively small sample size hinders our ability to detect significant program impacts. Furthermore, survey data are also likely to contain omissions or inaccuracies in the self-reported data due to recall error.

3. Sample and Data Used in the Interim Analysis

After considering the relative advantages and disadvantages of the two data sources, we chose to base the interim impact analysis primarily on the subsample for whom we have survey data. The earnings outcomes are based on the survey responses, and welfare responses are based on administrative records supplemented with data from the survey. We used data from state administrative records for the months that they are available and supplemented them with the self-reported measures of welfare receipt for months for which we do not have these data from the records.³

We chose to use the survey sample and data, rather than the partial administrative records sample, for the following two reasons. First, complete administrative records were not available for the full sample at the time we conducted this analysis, and we had to rely on a partial sample in any case. Rather than choose the very early cohort for whom we were more likely to have a one-year follow-up period from administrative sources, we chose to focus on the more random, but representative, survey sample. Second, we wanted to take advantage of the richer employment data available in the survey sample. Since our final impact analysis will be based on administrative records data, we wanted to analyze the rich survey data in this interim report.⁴

³For the later months where we use data on receipt from the survey, we impute the benefit amounts as the average value of benefits received in the earlier months as reported in the welfare records data.

⁴We conducted additional surveys to double the total sample size from 600 to 1,200 cases, or
(continued...)

The survey sample, however, is relatively small, limiting our ability to detect statistically significant impacts. Consequently, the preliminary findings presented here should be considered as only indicative or suggestive of the impacts we might expect to identify using the full sample of administrative records and should be viewed with caution.

The final analysis of PESD impacts will be based on the complete set of administrative records for the full sample. Appendix C of this report uses the partial records data that we had at the time of this analysis to calculate preliminary impact estimates for the year following sample intake (Appendix Tables C.1-C.3). By and large, we find that administrative records estimates for employment and earnings are generally consistent with the estimates based on the survey data. However, the welfare impacts for the two samples tend to differ more. In the discussion of impact estimates, we note instances where estimates from the two sources differ substantively.

B. CHARACTERISTICS OF THE SURVEY SAMPLE

Background and demographic information on sample members was obtained from the survey and from various sources of administrative records data. The average sample member was just under 30 years old and had two children when she entered the demonstration (Table A.3).⁵ The youngest child of the sample member, on average, was five years old, and the mothers were, on average, just over 20 years old when they had their first child.

Two of the sites--San Antonio and Portland--generally targeted only clients who were more job ready based on their level of educational attainment, prior job experience, and/or participation in job

⁴(...continued)
about 300 cases from each site, in order to be able to use it to generate some impact estimates.

⁵A small fraction (between two and nine percent) of the sample enrolled in the demonstration were men who were single parents and took care of their children. AFDC-UP (unemployed parent) cases were not enrolled in the demonstration.

TABLE A.3
CHARACTERISTICS OF THE SURVEY SAMPLE
(Percentage)

	Chicago		Portland		Riverside		San Antonio	
	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group
Mean Age (in Years)	29.2	29.5	29.6	30.1	31.0	32.1	29.2	29.8
Race/Ethnicity								
Hispanic	6.6	11.6	2.5	3.2	34.0	34.5	62.7	67.8
Black, non-Hispanic	87.4	76.8	27.7	21.3	18.0	12.8	21.7	20.1
White, non-Hispanic	5.3	11.0	69.2	71.0	44.7	50.7	15.5	12.1
Other, non-Hispanic	0.7	0.7	0.6	4.5	3.3	2.0	0.0	0.0
Female	97.4	98.1	95.6	93.0	92.1	91.3	98.8	98.0
Lived with Both Parents While Growing Up	54.0	65.2	59.2	64.5	68.2	67.3	59.9	58.7
Mean Number of Children in the Household	2.1	2.0	1.8	1.6	1.8	2.0	2.0	2.1
Mean Age of the Youngest Child (in Years)	4.7	4.9	4.5	5.1	5.3	6.0	4.4	4.8
Mean Age of Respondent at Birth of First Child	20.4	21.2	22.1	22.9	22.0	21.9	21.3	21.51
Highest Grade Completed								
Less than high school	37.7	43.2	20.1	23.2	37.8	31.3	13.0	7.4
High school/GED	44.4	36.8	61.6	66.5	44.6	52.1	59.0	64.4
More than high school/GED	17.9	20.0	18.2	10.3	17.6	16.7	28.0	28.2
Received Welfare Most of Time While Growing Up	25.5	24.7	11.0	12.9	8.8	10.4	14.0	18.3
JOBS Mandatory	72.2	76.0	95.6	94.8	100.0	100.0	66.5	70.0
Had Earnings in at Least 2 Quarters in Year Prior to Job Start	28.5	26.6	35.9	34.6	23.7	21.3	33.6	36.6
Received AFDC All of the Previous Year	78.7	82.8	58.6	60.0	96.8	97.6	43.8	57.1**
Sample Size	151	155	159	156	152	150	163	150

SOURCE: Postemployment Services Demonstration survey data and administrative records data.

*Program-control group differences are significantly different from zero at the 10 percent level, two-tailed test.

**Program-control group differences are significantly different from zero at the 5 percent level, two-tailed test.

placement activities. The other two sites--Chicago and Riverside--targeted all clients, including those who were less job ready.⁶ Consistent with the populations targeted, educational attainment of individuals varied considerably across the sites. For instance, only 10 to 20 percent of the sample members in San Antonio and Portland did not have a high school diploma or GED, compared with 35 to 40 percent in Chicago and Riverside.

Earnings from employment also varied by site, reflecting differences in work experience among the target populations. For instance, about a third of the sample members in San Antonio and Portland had some earnings in at least two quarters in the year prior to intake, compared with over a quarter of the sample in Chicago and over a fifth in Riverside. AFDC receipt in the year prior to intake also varied substantially, reflecting both the target population and welfare benefit generosity in each site. For instance, although about 45 to 60 percent of San Antonio and Portland sample members had received AFDC during all of the previous year, about 80 to 95 percent of the Chicago and Riverside samples had received AFDC during all of that same year.

Along with our examination of the characteristics of the survey sample, we compared the survey sample with the full sample (Table A.4). In general, we found very few differences between the characteristics of those sample members who completed the survey and the characteristics of the full sample.

C. ANALYSIS METHODS

The evaluation of the PESD was designed to support a rigorous assessment of the programs' effectiveness in promoting job retention and reducing welfare dependency among newly employed welfare recipients. A random assignment design was chosen to evaluate the programs' effectiveness.

⁶The Riverside program focused exclusively on JOBS nonexempt clients.

TABLE A.4

CHARACTERISTICS OF RESPONDENTS TO THE FOLLOW-UP SURVEY COMPARED TO FULL SAMPLE
(Percentage)

	Chicago		Portland		Riverside		San Antonio	
	Completed Survey	Full Sample	Completed Survey	Full Sample	Completed Survey	Full Sample	Completed Survey	Full Sample
Mean Age	29.3	29.5	29.8	29.5	31.6	31.9	29.5	29.5
Race/Ethnicity								
Hispanic	9.2	10.3	2.9	2.1	34.2	33.4	65.2	67.6
Black, non-Hispanic	82.0	82.3	24.5	25.9	15.4	17.1	21.0	19.0
White, non-Hispanic	8.2	7.1	70.1	68.1	47.7	47.2	13.9	13.1
Other, non-Hispanic	0.7	0.4	2.6	3.9	2.7	2.4	0.0	0.3
Female	97.8	97.8	94.3	94.0	91.7	87.8**	98.4	97.5
Mean Number of Children in the Household	2.0	2.1	1.7	1.8	1.9	1.9	2.0	2.0
Mean Age of the Youngest Child in Years	4.8	5.1	4.8	4.8	5.7	6.1*	4.6	4.7
Mean Age at Birth of First Child	20.8	20.6	22.5	22.0	21.9	22.2	21.4	21.4
Highest Grade Completed								
Less than high school	40.5	42.8	21.7	25.3	34.6	36.6	10.3	12.9
High school/GED	40.5	40.0	64.0	63.2	48.3	46.2	61.6	60.9
More than high school/GED	19.0	17.3	14.3	11.5	17.1	17.2	28.1	26.2
Mandatory JOBS Status	74.1	75.2	95.2	96.4	100.0	100.0	68.2	68.4
Earnings in at Least 2 Quarters in Year Prior to Job Start	27.5	27.7	35.2	34.2	22.5	21.0	35.0	32.8
Period of Enrollment				**				
1994	50.3	46.5	48.9	34.6	51.3	54.4	0.0	0.0
1995	49.7	53.5	51.1	65.4	48.7	45.6	100.0	100.0
Sample Size	306	1,545	315	804	302	1,506	313	778

SOURCE: Postemployment Services Demonstration data.

*Significantly different from zero at the 10 percent level, two-tailed test.

**Significantly different from zero at the 5 percent level, two-tailed test.

Random assignment ensures the creation of two groups of individuals that differ only in their access to the program. Consequently, any resulting differences between the two groups at a later point in time can be attributed to the effects of the program compared with services that are otherwise available in the absence of the program.

The standard method for measuring the effectiveness of a program with a random-assignment design is to compare the value of outcomes for those in the program group with those in the control group. While a simple comparison of means is generally sufficient (as long as random assignment has been correctly implemented), means are often regression-adjusted to obtain more precise estimates of program impacts. This method controls for differences between the sample that can occur by chance despite the use of random assignment. Conventional tests of significance are then used to determine whether or not the impacts are “statistically” significant.

In our analysis, we provide regression-adjusted estimates of program impacts using a large set of baseline variables. In most of the tables in the report, we present both the control group means and the estimated impacts. The estimated impacts represent the difference between the program and control group means.⁷ However, the small survey sample sizes used in this interim analysis (about 300 per site) are inadequate to detect even modest-sized impacts at customary levels of statistical significance.⁸ To overcome this drawback, we considered pooling the data across the sites, but decided against it for two reasons: (1) the program contexts were very different in the four sites (for

⁷The program group mean for a given outcome can be obtained by adding the estimated impact to the control group mean. For example, if 80.0 percent of control group members received AFDC benefits during a given period, and the estimated impact is -8.0, then 72.0 percent of treatment group members received AFDC during the same period. Similarly, if the control group mean for AFDC benefit amounts is \$2,800 and the estimated impact is \$250, then the program group mean for AFDC benefit amounts is \$3,050.

⁸For instance, to detect a 10 percent impact on employment at an 80 percent power level, we would ideally need 800 to 900 sample members per site.

example, the AFDC benefit levels differed markedly); and (2) to some extent, the program structures were different across the four sites. These two factors make it difficult to clearly interpret the results of pooled models.⁹

Because the small sample sizes make it difficult for us to detect statistically significant impacts, we also focus on the magnitude of the estimated impacts and the patterns of consistency in impacts (both consistency over time and consistency across different outcomes) in our interpretation of the results. For instance, among two sets of estimated impacts that are both statistically insignificant, we have more confidence in an outcome that shows consistent and modest impacts over time than in one that may have large positive impacts in some periods and large negative impacts in others. Until larger sample sizes are available, however, the results in this report must be viewed only as suggestive and, consequently, used with caution.

⁹Nevertheless, we did calculate pooled estimates for employment outcomes using administrative records data for the survey sample. The pooled estimates across the four sites are fairly consistent with the site-by-site estimates. We observe a positive, although not significant increase in both average monthly earnings and total earnings. However, when data are pooled across all sites but Portland, the estimates for the employment outcomes are both positive and significant.

APPENDIX B

**MONTHLY IMPACT ESTIMATES AND P-VALUES
FOR EMPLOYMENT, EARNINGS, AND WELFARE RECEIPT,
BASED ON SURVEY DATA**

TABLE B.1

IMPACTS ON EMPLOYMENT AND EARNINGS BY MONTHS AFTER SAMPLE INTAKE, SURVEY SAMPLE
(Impacts Correspond with Those Displayed Graphically in Figures 3 and 4)

	Chicago			Portland			Riverside			San Antonio		
	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value
Percent of Sample Members Employed												
Month 1	73.5	4.1	0.43	84.3	-6.9	0.13	67.7	7.5	0.16	80.4	1.5	0.74
Month 2	73.3	5.2	0.32	83.5	-5.2	0.26	73.0	3.5	0.50	79.7	1.6	0.73
Month 3	71.2	6.1	0.25	80.9	-4.5	0.34	68.3	8.2	0.12	75.7	2.6	0.60
Month 4	66.5	5.6	0.31	77.3	-0.6	0.91	69.8	1.3	0.80	73.7	0.9	0.86
Month 5	65.2	5.0	0.38	76.2	-1.5	0.76	68.7	2.7	0.62	72.2	-0.6	0.92
Month 6	65.1	3.1	0.58	74.4	-1.7	0.74	69.1	0.8	0.89	68.4	1.2	0.83
Month 7	62.9	5.1	0.38	74.1	-6.3	0.25	65.8	8.3	0.13	65.4	2.6	0.64
Month 8	60.9	7.7	0.18	73.3	-4.1	0.45	66.8	3.4	0.55	66.8	-2.5	0.65
Month 9	61.1	6.7	0.24	70.7	-2.7	0.63	69.1	-1.7	0.76	67.2	-5.8	0.30
Month 10	62.2	5.8	0.32	70.8	-0.2	0.97	68.9	-2.7	0.63	68.8	-5.8	0.31
Month 11	61.3	7.7	0.19	73.0	0.5	0.92	69.4	0.3	0.95	62.9	-0.5	0.94
Month 12	60.9	6.5	0.27	70.8	2.8	0.60	69.8	0.1	0.98	63.0	2.9	0.65
Monthly Earnings												
Month 1	595	57	0.35	791	-93	0.12	511	10	0.85	580	19	0.69
Month 2	639	56	0.36	870	-93	0.15	569	0	1.00	595	57	0.24
Month 3	632	48	0.43	848	-82	0.20	561	44	0.47	565	71	0.15
Month 4	604	61	0.34	822	-55	0.39	540	30	0.60	533	70	0.16
Month 5	601	54	0.41	795	-58	0.38	534	15	0.79	515	62	0.23
Month 6	604	12	0.86	784	-59	0.38	532	31	0.60	508	56	0.29
Month 7	609	0	1.00	800	-94	0.17	531	79	0.19	510	25	0.63
Month 8	585	55	0.40	800	-98	0.16	525	65	0.29	512	-3	0.95
Month 9	585	56	0.41	763	-61	0.38	541	41	0.51	525	-30	0.58
Month 10	586	48	0.47	756	-50	0.48	553	-2	0.98	492	-2	0.96
Month 11	564	57	0.38	750	-3	0.96	551	20	0.74	460	26	0.62
Month 12	547	85	0.19	756	37	0.60	587	2	0.97	454	55	0.35
Sample Size	155	306		156	314		150	302		150	313	

SOURCE: PESD survey data.

NOTE: Estimated impacts are regression-adjusted.

^aThe estimated impact represents the difference between the program and control group means.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

TABLE B.2

IMPACTS ON AFDC RECEIPT AND BENEFIT AMOUNTS BY MONTHS AFTER SAMPLE INTAKE, SURVEY SAMPLE
(Impacts Correspond with Those Displayed Graphically in Figures 5 and 6)

	Chicago			Portland			Riverside			San Antonio		
	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value
Percent of Sample Members Receiving AFDC												
Month 1	94.8	-1.5	0.58	73.7	10.7	0.05**	97.2	0.2	0.90	53.2	3.8	0.51
Month 2	88.7	-5.6	0.16	47.9	3.8	0.54	92.2	0.4	0.89	33.0	-0.0	0.99
Month 3	85.5	-9.2	0.03**	42.9	-1.4	0.82	92.7	-6.6	0.14	35.1	-1.6	0.78
Month 4	81.2	-7.1	0.14	39.7	-1.6	0.77	83.1	2.7	0.65	33.5	-2.2	0.69
Month 5	82.2	-11.2	0.02**	40.5	-3.0	0.59	85.0	-4.5	0.52	34.5	-6.0	0.27
Month 6	81.3	-6.7	0.16	41.0	-1.8	0.75	76.4	2.2	0.73	35.9	-6.2	0.26
Month 7	79.5	-11.1	0.03**	39.3	-0.3	0.95	72.2	1.2	0.81	41.7	-11.1	0.05**
Month 8	76.6	-6.4	0.21	35.1	2.5	0.64	72.2	-0.8	0.88	41.4	-10.7	0.06*
Month 9	74.2	-8.2	0.12	29.7	9.5	0.09*	69.0	1.0	0.86	38.2	-7.4	0.19
Month 10	72.6	-9.0	0.09*	29.1	10.2	0.06*	67.0	-0.9	0.87	37.6	-7.0	0.21
Month 11	72.3	-8.1	0.13	31.7	5.3	0.33	67.4	-1.2	0.82	36.1	-5.4	0.34
Month 12	71.9	-12.1	0.03**	28.7	7.7	0.16	66.4	-3.7	0.52	37.3	-6.0	0.29
Monthly AFDC Benefits in Dollars												
Month 1	354	-10	0.68	282	55	0.01**	538	1	0.95	93	11	0.32
Month 2	249	-31	0.15	163	33	0.16	497	-12	0.61	57	-4	0.71
Month 3	217	-19	0.35	141	18	0.44	430	-10	0.68	58	-1	0.96
Month 4	213	-2	0.92	143	9	0.71	384	16	0.57	57	3	0.80
Month 5	234	-30	0.16	142	10	0.67	392	-23	0.44	59	-2	0.82
Month 6	239	-18	0.46	136	12	0.59	361	24	0.44	62	-5	0.63
Month 7	223	-25	0.25	136	12	0.61	353	7	0.83	77	-19	0.08*
Month 8	237	-42	0.06*	125	14	0.54	357	-7	0.82	73	-16	0.13
Month 9	232	-24	0.34	99	51	0.02**	349	-1	0.96	69	-13	0.21
Month 10	210	-17	0.46	99	48	0.03**	333	1	0.98	68	-13	0.21
Month 11	204	-24	0.25	102	37	0.10*	337	-13	0.70	65	-8	0.45
Month 12	205	-20	0.40	97	41	0.07*	352	-47	0.17	66	-12	0.26
Sample Size	155	305		156	315		150	302		150	312	

SOURCE: PESD survey and administrative records data.

NOTE: Estimated impacts are regression-adjusted.

^aThe estimated impact represents the difference between the program and control group means.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

TABLE B.3

IMPACTS ON FOOD STAMP RECEIPT AND BENEFIT AMOUNTS BY MONTHS AFTER INTAKE, SURVEY SAMPLE
(Impacts Correspond with Those Displayed Graphically in Figures 7 and 8)

	Chicago			Portland			Riverside			San Antonio		
	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value
Percent of Sample Members Receiving Food Stamps												
Month 1	94.2	0.5	0.85	97.3	-1.3	0.56	86.6	-4.3	0.32	95.2	-5.6	0.07*
Month 2	92.2	-4.2	0.21	92.1	-1.5	0.66	83.6	-7.5	0.11	90.9	-9.1	0.02**
Month 3	87.2	-9.9	0.02**	85.2	-3.2	0.44	85.0	-14.3	0.01**	88.8	-8.7	0.04**
Month 4	84.9	-8.7	0.06*	80.0	-2.4	0.61	78.3	-12.1	0.05**	90.2	-7.1	0.07*
Month 5	82.8	-6.4	0.16	75.3	1.2	0.81	74.1	-8.5	0.17	90.0	-9.8	0.02**
Month 6	80.5	-3.7	0.43	74.9	-5.1	0.32	70.7	-8.3	0.19	87.3	-10.2	0.02**
Month 7	81.8	-9.6	0.05**	70.1	-0.0	1.00	67.8	-4.5	0.48	88.7	-11.6	0.01**
Month 8	79.9	-7.1	0.16	67.1	4.7	0.37	67.8	-5.7	0.38	88.0	-10.2	0.02**
Month 9	77.0	-6.0	0.24	62.9	6.0	0.26	62.2	1.5	0.81	84.1	-7.1	0.12
Month 10	76.9	-11.0	0.04**	61.5	5.5	0.31	61.5	0.2	0.97	83.2	-9.9	0.04**
Month 11	76.3	-9.7	0.06*	59.0	4.2	0.44	63.3	-2.0	0.76	82.6	-9.0	0.06*
Month 12	76.0	-10.2	0.05**	54.3	5.6	0.31	64.8	-6.6	0.36	82.2	-10.6	0.03**
Food Stamps Benefit Amounts in Dollars												
Month 1	241	-9	0.42	209	-10	0.37	171	-4	0.67	251	-1	0.94
Month 2	191	-15	0.26	164	7	0.53	158	-8	0.46	222	-9	0.49
Month 3	179	-25	0.07*	164	2	0.88	131	-11	0.28	207	-9	0.44
Month 4	188	-18	0.22	164	-4	0.72	123	-10	0.34	221	-18	0.12
Month 5	186	-7	0.63	153	9	0.43	119	-2	0.83	220	-22	0.09*
Month 6	198	-24	0.12	157	-3	0.81	113	-5	0.62	213	-20	0.14
Month 7	193	-26	0.10*	154	2	0.90	112	-5	0.63	218	-20	0.15
Month 8	201	-35	0.02**	148	10	0.43	121	-14	0.22	216	-15	0.29
Month 9	188	-7	0.69	138	13	0.35	106	3	0.80	210	-20	0.15
Month 10	188	-23	0.16	136	16	0.24	108	-6	0.58	211	-20	0.16
Month 11	185	-24	0.13	130	14	0.31	113	-18	0.13	214	-17	0.24
Month 12	180	-19	0.24	115	16	0.26	118	-24	0.05**	220	-29	0.05**
Sample Size	155	305		155	311		150	302		150	312	

SOURCE: PESD survey and administrative records data.

NOTE: Estimated impacts are regression-adjusted.

^aThe estimated impact represents the difference between the program and control group means.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

TABLE B.4

AFDC BENEFITS, FOOD STAMP BENEFITS, AND INCOME DURING THE FIRST YEAR AFTER INTAKE:
P-VALUES SHOWING THE LEVEL OF SIGNIFICANCE OF THE IMPACTS FROM TABLES 2 AND 3

	Chicago	Portland	Riverside	San Antonio
	P-Values	P-Values	P-Values	P-Values
AFDC				
Percentage of Total Period Receiving AFDC	0.03**	0.32	0.92	0.24
Average Number of Months Receiving AFDC Among Those Who Received It	0.04**	0.82	0.72	0.01**
Average Monthly Benefits (Dollars)	0.15	0.05**	0.71	0.41
Food Stamps				
Percentage of Total Period Receiving Food Stamps	0.03**	0.68	0.15	0.00**
Average Number of Months Receiving Food Stamps Among Those Who Received It	0.04**	0.60	0.17	0.01**
Average Monthly Food Stamp Benefits (Dollars)	0.07*	0.49	0.25	0.10*
Income				
Total Income (Dollars)	0.78	0.70	0.86	0.77
Earnings (Dollars)	0.36	0.33	0.61	0.43
Unearned Income (Dollars)				
AFDC	0.16	0.04**	0.71	0.41
Food stamps	0.07*	0.48	0.25	0.09*
Proportional Contribution of Various Sources to Total Income (Percentage)				
Earnings	0.08*	0.53	0.59	0.34
AFDC	0.13	0.24	0.68	0.36
Food stamps	0.08*	0.82	0.48	0.39
Distribution of Income as Percentage of Poverty Level (Percentage)				
Average percentage	0.09*	0.88	0.92	0.68
	0.66	0.92	0.73	0.95
Sample Size	305	315	302	312

SOURCE: PESD survey and administrative records data.

NOTE: Estimated impacts are regression-adjusted.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

APPENDIX C

**IMPACT ESTIMATES AND P-VALUES
FOR EMPLOYMENT, EARNINGS, AND WELFARE RECEIPT,
BASED ON PARTIAL ADMINISTRATIVE RECORDS DATA**

TABLE C.1

IMPACTS ON EMPLOYMENT AND EARNINGS, WITH CORRESPONDING SAMPLE SIZES,
USING PARTIALLY COMPLETE ADMINISTRATIVE RECORDS DATA FOR FULL SAMPLE

	Chicago			Portland			Riverside			San Antonio		
	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value
Percentage of Sample Members Employed												
Quarter 1	81.0	1.1	0.60	67.5	0.3	0.92	67.9	0.5	0.85	92.3	-1.4	0.52
Quarter 2	67.4	1.7	0.52	70.8	-0.4	0.91	60.2	3.9	0.14	79.6	1.7	0.66
Quarter 3	57.3	0.6	0.86	69.9	3.3	0.44	54.7	3.0	0.32	81.3	-3.5	0.48
Quarter 4	61.0	3.1	0.39	71.0	-2.9	0.60	53.8	5.2	0.13	64.5	4.8	0.54
Monthly Earnings												
Quarter 1	1,583	30	0.71	1,406	-147	0.14	1,160	-24	0.74	1,465	104	0.25
Quarter 2	1,432	124	0.18	1,595	24	0.86	1,266	69	0.44	1,458	203	0.11
Quarter 3	1,401	-56	0.62	1,840	-112	0.50	1,229	81	0.43	1,404	108	0.48
Quarter 4	1,546	-30	0.83	1,839	-3	0.99	1,259	102	0.41	1,413	74	0.75
Sample Sizes												
	Chicago		Portland		Riverside		San Antonio					
	Control Sample	Full Sample	Control Sample	Full Sample	Control Sample	Full Sample	Control Sample	Full Sample				
Quarter 1	991	1,543	355	760	1,006	1,506	329	652				
Quarter 2	889	1,383	279	597	1,006	1,506	215	435				
Quarter 3	636	991	216	451	825	1,228	142	283				
Quarter 4	518	806	154	306	641	945	84	166				

SOURCE: Administrative records data.

NOTE: Estimated impacts are regression-adjusted.

^aThe estimated impact represents the difference between the program and control group means.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

TABLE C.2

IMPACTS ON AFDC RECEIPT AND BENEFIT AMOUNTS, WITH CORRESPONDING SAMPLE SIZES,
USING PARTIALLY COMPLETE ADMINISTRATIVE RECORDS DATA FOR FULL SAMPLE

	Chicago			Portland			Riverside			San Antonio		
	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value
Percent of Sample Members Receiving AFDC												
Month 1	94.5	0.5	0.69	80.5	4.4	0.12	96.5	1.5	0.17	55.8	-0.3	0.93
Month 2	86.8	-1.7	0.34	54.4	2.7	0.45	92.1	2.9	0.07*	34.9	-0.5	0.89
Month 3	82.3	-4.1	0.04**	47.3	-2.8	0.44	86.5	2.7	0.21	32.1	-2.6	0.44
Month 4	78.2	-2.7	0.21	44.9	-4.8	0.16	80.6	4.4	0.07*	32.8	-3.8	0.27
Month 5	78.0	-4.6	0.03**	46.1	-7.4	0.04**	77.3	2.5	0.35	32.2	-5.1	0.15
Month 6	78.0	-4.6	0.03**	42.0	-6.5	0.08*	72.0	5.9	0.03**	31.5	-3.7	0.33
Month 7	76.0	-6.9	0.00**	38.6	-1.0	0.80	68.1	6.3	0.02**	34.4	-5.8	0.17
Month 8	73.6	-4.3	0.08*	36.5	-0.3	0.94	65.8	6.6	0.02**	36.1	-7.9	0.08*
Month 9	71.7	-5.1	0.05**	34.2	0.2	0.97	63.9	5.7	0.05**	36.2	-9.9	0.04**
Month 10	69.6	-3.8	0.17	35.1	0.1	0.97	62.1	3.8	0.18	33.2	-8.8	0.08*
Month 11	69.5	-2.7	0.35	36.0	-3.5	0.44	59.4	5.6	0.07*	31.9	-7.1	0.19
Month 12	68.6	-3.8	0.22	33.7	0.4	0.93	58.4	4.5	0.15	33.8	-1.4	0.82
Monthly AFDC Benefits												
Month 1	347	35	0.00**	325	19	0.15	536	8	0.32	99	0	0.96
Month 2	233	5	0.63	190	21	0.15	494	8	0.47	60	-2	0.76
Month 3	213	-5	0.62	168	7	0.61	424	8	0.54	53	-6	0.34
Month 4	219	-8	0.42	168	-8	0.58	382	19	0.15	54	-4	0.57
Month 5	223	-19	0.04**	175	-24	0.11	368	12	0.37	54	-5	0.46
Month 6	234	-17	0.09*	154	-13	0.39	355	32	0.02**	54	-3	0.68
Month 7	225	-18	0.06*	136	4	0.79	340	19	0.18	64	-13	0.12
Month 8	225	-19	0.08*	130	6	0.70	331	20	0.18	64	-12	0.15
Month 9	220	-13	0.27	112	16	0.33	328	10	0.51	66	-18	0.05**
Month 10	207	-14	0.22	117	10	0.56	319	1	0.97	60	-16	0.08*
Month 11	211	-28	0.02**	118	2	0.92	294	15	0.36	57	-14	0.18
Month 12	203	-14	0.28	115	13	0.50	298	-4	0.83	60	-14	0.21

TABLE C.2 (continued)

	Sample Sizes							
	Chicago		Portland		Riverside		San Antonio	
	Control Sample	Full Sample	Control Sample	Full Sample	Control Sample	Full Sample	Control Sample	Full Sample
Month 1	993	1,544	379	804	1,005	1,504	390	773
Month 2	993	1,544	379	804	1,005	1,504	390	773
Month 3	993	1,544	379	804	1,005	1,504	390	773
Month 4	993	1,544	373	794	1,005	1,504	365	728
Month 5	993	1,544	354	757	1,005	1,504	340	675
Month 6	993	1,544	325	698	1,005	1,504	301	601
Month 7	978	1,525	301	652	1,005	1,504	251	505
Month 8	891	1,384	277	594	1,005	1,504	223	451
Month 9	817	1,263	255	539	950	1,422	203	410
Month 10	722	1,116	238	501	895	1,337	177	354
Month 11	637	991	213	448	824	1,226	145	289
Month 12	594	929	192	404	771	1,147	125	248

SOURCE: Administrative records data.

NOTE: Estimated impacts are regression-adjusted.

^aThe estimated impact represents the difference between the program and control group means.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

TABLE C.3

IMPACTS ON FOOD STAMP RECEIPT AND BENEFIT AMOUNTS, WITH CORRESPONDING SAMPLE SIZES,
USING PARTIALLY COMPLETE ADMINISTRATIVE RECORDS DATA FOR FULL SAMPLE

	Chicago			Portland			Riverside			San Antonio		
	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value	Control Group Mean	Estimated Impact ^a	P-Value
Percent of Sample Members Receiving Food Stamps												
Month 1	94.8	0.7	0.54	98.4	-2.2	0.07*	85.7	1.4	0.52	93.8	-1.7	0.37
Month 2	89.3	-1.9	0.27	91.9	-1.8	0.39	80.7	3.4	0.12	89.6	-1.7	0.47
Month 3	84.4	-4.2	0.03**	85.4	-2.2	0.40	76.6	3.5	0.17	89.5	-4.1	0.08*
Month 4	80.7	-2.9	0.17	80.8	-5.2	0.08*	72.5	3.5	0.21	88.9	-3.9	0.11
Month 5	79.6	-2.4	0.26	75.3	-3.3	0.30	68.2	4.4	0.10*	87.2	-5.0	0.07*
Month 6	79.7	-2.5	0.22	71.1	-2.7	0.43	64.7	5.1	0.07*	85.8	-5.9	0.05**
Month 7	79.1	-6.3	0.00**	68.5	0.1	0.97	61.9	5.9	0.04**	85.8	-5.6	0.09*
Month 8	76.6	-4.5	0.06*	64.9	1.0	0.79	60.4	5.8	0.04**	84.9	-5.1	0.15
Month 9	75.0	-3.7	0.14	62.9	-0.2	0.97	58.6	6.5	0.03**	85.4	-5.3	0.14
Month 10	73.5	-3.5	0.20	60.1	4.5	0.29	57.3	6.8	0.03**	84.7	-6.1	0.12
Month 11	73.3	-0.6	0.82	60.1	1.5	0.75	54.7	8.2	0.01**	82.2	-1.9	0.68
Month 12	72.4	-2.0	0.50	54.6	6.6	0.19	55.3	5.7	0.10*	81.5	-3.3	0.51
Food Stamps Benefit Amounts in Dollars												
Month 1	243	-1	0.89	216	-4	0.53	167	3	0.47	263	-1	0.92
Month 2	188	-4	0.57	176	0	0.95	151	6	0.23	222	6	0.45
Month 3	182	-11	0.09*	170	5	0.47	127	3	0.53	210	-4	0.61
Month 4	188	-4	0.58	173	-9	0.25	117	7	0.15	214	-9	0.25
Month 5	193	-7	0.33	164	-9	0.25	114	8	0.13	212	-10	0.22
Month 6	203	-11	0.12	155	-2	0.82	110	8	0.15	207	-7	0.45
Month 7	202	-16	0.03**	144	11	0.25	107	7	0.21	210	-7	0.53
Month 8	199	-18	0.02**	138	11	0.28	105	9	0.09*	210	-7	0.52
Month 9	194	-5	0.55	135	1	0.96	104	7	0.23	212	-17	0.14
Month 10	188	-10	0.23	131	15	0.18	101	5	0.40	214	-11	0.36
Month 11	194	-18	0.05**	131	5	0.67	100	1	0.91	209	4	0.78
Month 12	185	-10	0.28	119	10	0.44	99	-2	0.81	218	-7	0.63

TABLE C.3 (continued)

	Sample Sizes							
	Chicago		Portland		Riverside		San Antonio	
	Control Sample	Full Sample	Control Sample	Full Sample	Control Sample	Full Sample	Control Sample	Full Sample
Month 1	993	1,544	358	763	1,005	1,504	390	773
Month 2	993	1,544	356	762	1,005	1,504	390	773
Month 3	993	1,544	370	791	1,005	1,504	390	773
Month 4	993	1,544	365	783	1,005	1,504	365	728
Month 5	993	1,544	347	748	1,005	1,504	340	675
Month 6	993	1,544	320	691	1,005	1,504	301	601
Month 7	978	1,525	296	645	1,005	1,504	251	505
Month 8	891	1,384	273	587	1,005	1,504	223	451
Month 9	817	1,263	251	533	950	1,422	203	410
Month 10	722	1,116	235	496	895	1,337	177	354
Month 11	637	991	211	445	824	1,226	145	289
Month 12	594	929	190	401	771	1,147	125	248

SOURCE: Administrative records data.

NOTE: Estimated impacts are regression-adjusted.

^aThe estimated impact represents the difference between the program and control group means.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

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