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Report on a Meta-Analysis of Welfare-to-Work Programs

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EXECUTIVE SUMMARY

This report uses meta-analysis, a set of statistically based techniques for combining quantitative findings from different studies, to synthesize estimates of program effects from random assignment evaluations of welfare-to-work programs and to explore the factors that best explain differences in the programs' performance. The analysis is based on data extracted from the published evaluation reports and from official sources. All the programs included in the analysis targeted recipients of Aid to Families with Dependent Children (AFDC; now called Temporary Assistance for Needy Families, TANF[1]). The objective of the analysis is to establish the principal characteristics of welfare-to-work programs that were associated with differences in success, distinguishing between variations in the services received, differences in the characteristics of those who participated in each program, and variations in the socioeconomic environment in which the programs operated.

Meta-analysis is a powerful instrument for analyzing the combined impacts of comparable policy interventions, while controlling for a range of factors pertaining to these interventions or the environment in which they took place. However, like other statistical techniques, meta-analysis can be subject to data limitations that adversely affect its capacity to produce robust and reliable results. Multicollinearity of variables (resulting from small sample size), inconsistencies in the information provided in different evaluation reports, and omitted or misspecified variables are some of the data analysis risks that we sought to minimize by, for instance, verifying data entries and carefully considering the specification of the regression equations that are estimated. It would have been impossible, as well as impractical, to eradicate all risk of error in the analyses, much of which would have been beyond the control of this study and could be traced back to the original evaluations. In the light of such limitations, many of the

[1]Because most data used in this study were generated before AFDC was converted to the Temporary Assistance for Needy Families program, for convenience we use the AFDC acronym throughout this report.

conclusions that are reached are subjected to sensitivity tests. These tests were conducted to establish the robustness of the meta-analyses' key findings.

Separate meta-analyses of both voluntary and mandatory programs were conducted. Voluntary programs provide services (e.g., help in job search, training, and remedial education) for those who apply for them, and they sometimes provide financial incentives to encourage work. Mandatory programs are targeted at recipients of government transfer payments. They also provide employment-oriented services and sometimes provide financial work incentives, but differ from voluntary programs by requiring participation in the services by potentially subjecting individuals assigned to the program to fiscal sanctions (i.e., reductions in transfer payments) if they do not cooperate.

This study uses a unique database, assembled specifically for synthesizing findings from evaluations of welfare-to-work programs. The data used in the study are from 27 random assignment evaluations of mandatory welfare-to-work programs for AFDC applicants and recipients and four random assignment evaluations of voluntary welfare-to-work programs for AFDC recipients. The evaluations in the study sample were conducted similarly. AFDC applicants and recipients were randomly assigned to either a program group that participated in the welfare-to-work program being evaluated or to a control group, which was eligible to receive any services that existed prior to the introduction of the welfare-to-work program. Relying mainly on administrative data, various measures of outcomes (such as earnings and the percentage receiving AFDC) were computed for the members of the program and control groups over time. Once this follow-up information was available, each program effect was estimated as the difference in the mean outcome for the program group and the control group, a measure that is often referred to as the "program impact."

The database contains four measures of program impacts:

- average earnings,
- the percentage in employment,
- the average amount of AFDC received, and

- the percentage in receipt of AFDC.

Program impacts are available for up to twenty calendar quarters after random assignment, along with the levels of statistical significance for each of these impact measures. Findings from cost-benefit analyses are also included when available. In addition, the database contains the values of a number of explanatory variables. These include the characteristics of the program population (gender and ethnic mix, age distribution, family structure, education levels, and so forth), measures of program effects on participation in various activities (job search, basic education, vocational training, and work experience), whether each evaluated program tested financial incentive and time limits, program effects on sanctioning, and socioeconomic data for each of the program sites and for each of the evaluation years (site unemployment and poverty rates, the percentage of the workforce in manufacturing employment, median household income, and the maximum AFDC payment for which a family of three was eligible).

Because controls often receive services similar to those received by persons assigned to the evaluated programs, but from other sources, it is important to measure the net difference between the two groups in their receipt of services—that is, the program's impact on participation in services. These net differences indicate that a typical *mandatory* welfare-to-work program puts much more emphasis on increasing participation in relatively inexpensive activities, such as job search, than on increasing participation in more costly activities, such as basic education and vocational training. Nonetheless, it cost the government almost $2,000 (in year 2000 dollars) more per member of the program group to operate the evaluated mandatory programs than to run the programs serving controls. Voluntary programs typically put more emphasis on expensive services than mandatory programs do and, hence, are usually more costly to run.

The four impacts mentioned above were examined in four separate calendar quarters (the 3rd, 7th, 11th, and 15th after random assignment). Between 64 and 79 estimates were available for each impact measure during the two earlier quarters, and between 44 and 56 estimates were available during the later two quarters. The analysis suggests that welfare-to-work programs, on average, had the intended positive impact on the four indicators, although these averages were usually small. There was considerable variation among the individual programs, however, suggesting that some performed much better than others.

Much of the analysis was devoted to determining why some programs were more successful than others. Among the more important conclusions concerning *mandatory* welfare-to-work programs that were reached are the following (findings for voluntary programs are described later):

- Three program features appear to be positively related to the effectiveness of mandatory welfare-to-work interventions: increased participation in job search, the use of time limits, and the use of sanctions. The sanction impact is only important in the first couple of years after entry into a program.

- Financial incentives decrease impacts on whether AFDC is received and on the amount of AFDC that is received, but do not improve impacts on labor market outcomes.

- The evidence is somewhat mixed over whether increases in participation in basic education, vocational education, and work experience increase program effectiveness. However, in general the findings do not support putting additional resources into these activities.

- It is not clear whether the effectiveness of mandatory welfare-to-work programs has improved over time.

- Mandatory welfare-to-work programs appear to do better in strong labor markets than in weak ones.

- Because generous state AFDC programs (represented in the analysis by the size of the maximum AFDC payment for which a family of three is eligible) reduce incentives to leave the welfare rolls, it was anticipated that the relationship between AFDC generosity and program impacts on the receipt of AFDC would be negative. However, the evidence on this relationship is mixed, varying with the statistical procedures used to test the hypothesis.

- A typical mandatory welfare-to-work program appears to have a positive effect on all four program impact measures for five to seven years after random assignment, although the impacts begin to decline after two or three years.

- In general, mandatory welfare-to-work programs appear to be more effective in serving relatively more disadvantaged caseloads than more advantaged caseloads—for example, AFDC recipients (rather than applicants), program group members without recent employment experience (rather than program group members with recent employment experience), and long-term (rather than short-term) participants in AFDC. However, similar evidence of a differential impact for program group members with and without a high school diploma is lacking. Moreover, there is some evidence of a positive relationship between program impacts and the average age of persons in the caseload.

The findings listed above are based on weighted regressions in which the dependent variables are estimates of program impacts and the weights, as prescribed by meta-analysis, are the inverse of the standard errors of the impact estimates. The report provides evidence suggesting that these regressions can be used to assess whether it is likely that a particular mandatory welfare-to-work program is performing better or worse than an average mandatory program. Although this information is not as reliable as that provided by a full evaluation, it can serve as a partial substitute for such an evaluation.

The net operating costs of a typical mandatory welfare-to-work program (i.e., the cost to the government of providing program services, excluding income transfers, such as AFDC payments), were around $1,800 per program group member (in year 2000 dollars). These costs, most of which are incurred in the first few months after participants enter a program, are larger for programs that substantially increase participation in basic education and vocational education. Increases in participation in work experience do not seem to increase costs, perhaps because work experience participants are often assigned to agencies other than those operating the welfare-to-work programs, and whatever costs are involved may not get incorporated into estimates of net operating costs. Increases in participation in job search appear to result in very small increases in cost, while financial incentives appear fairly costly to administer. Increases in sanction rates engender considerable costs, presumably because of government expenditures required for administering and enforcing sanctions.

Benefit-cost analyses were conducted as part of the evaluation of many, but far from all, of the evaluations of the welfare-to-work programs in the database. Findings from these analyses indicate that the *net* benefits (i.e., benefits less costs) of a typical mandatory welfare-to-work program are surprisingly small. According to the findings, which attempt to capture total net benefits over several years (often five), society receives net benefits of around $500 per program group member from a typical mandatory welfare-to-work program; savings to the government are around $400 per program group member, on average; and those assigned to a typical program are barely affected. It is likely that the net benefits from a typical mandatory welfare-to-work program are actually even smaller than these estimates imply because, as shown in the report, benefit-cost analyses are less likely to be conducted for those programs with especially small impacts on earnings.

Unsurprisingly, the net benefits received by participants are higher for program group members in programs that offer financial incentives than for those assigned to programs that do not. However, the increases in participant net benefits are fully or nearly offset by reductions in government net benefits. Thus, the social cost of financial incentives appears to be small or negligible. Because the findings also suggest that they do little to increase employment or earnings, financial incentives that are provided through welfare-to-work programs are perhaps best viewed as simply transferring income from the government to low-wage welfare recipients who find jobs.

We used meta-analysis to systematically identify interventions with very high positive or negative impacts. In doing so, we defined "very high" positive or negative impacts as those at least one standard deviation above or below, respectively, the mean for all interventions. We did this for all four impact measures for quarters 3, 7, 11, and 15. We required interventions to have at least two quarterly outliers before classifying them as having had exceptionally high positive or negative outliers because we were interested in identifying programs that repeatedly, perhaps persistently, under- or overperformed. This was to avoid highlighting isolated instances of above-average positive or negative performance that may not be sustained over time. We conducted two types of analysis: (1) we compared each impact estimate in a given calendar quarter to the weighted mean of all impact estimates available for that quarter (Type A); (2) we used the weighted regressions and their explanatory variables to control for factors that influence the effectiveness of welfare-to-work interventions (Type B). We find, as expected, that interventions are more likely to produce Type A than Type B outliers. Additional analysis that may well be worth undertaking, but that would require information beyond that in our database, would be required to determine why interventions that produced Type B outliers were over- or underperforming.

As indicated by the fact that they produced multiple positive outliers for different impact measures, the GAIN evaluation interventions in California's Riverside and Butte Counties and the

NEWWS evaluation in Portland Oregon are among those that repeatedly overperformed. Other interventions with repeated positive outlier impacts include the California Work Pays Demonstration and the New York State Child Assistance Program. Both record more Type B than Type A outliers; that is, their status as overperforming interventions becomes more apparent after the factors that influence program impacts have been taken into account. Repeatedly underperforming programs include Minnesota's Family Investment Program (MFIP), Vermont's Welfare Restructuring Project (WRP), and GAIN's Tulare County intervention. MFIP especially underperformed with respect to reducing AFDC payments or the number of AFDC recipients, perhaps because it offered financial incentives.

Seven of the random assignment evaluations of mandatory welfare-to-work programs in our database provided sufficient information on child outcomes for analysis as part of this report. Even though the data severely limited in-depth analysis of program impacts for children, several findings are noteworthy. Overall, program impacts on children were small, but there is evidence that the considerable variation across programs in their estimated impacts on children is not entirely due to sampling error, but is partially attributable to systematic differences among the interventions. However, with the exception of impacts on the emotional and behavioral problems of children, we were unable to determine what these systematic differences might be.

There is no support in our data for the proposition that increasing the net incomes of welfare families improves child outcomes. However, the welfare-to-work programs that were examined did not produce large changes in the incomes of those assigned to them. When various program characteristics are controlled, impacts on emotional and behavioral problems are less positive for school-age children than for young children. Additionally, three program features appear to positively affect the impact of welfare-to-work interventions on children's behavioral and emotional outcomes: sanctions, participation in basic education, and participation in unpaid work. Two features of welfare-to-work programs exert negative influences on impacts on childhood behavioral or emotional impacts: financial incentives and time limits. Finally, increasing expenditures on welfare-to-work programs has a positive effect on their impacts on childhood behavioral and emotional problems.

There have been four evaluations of ten interventions that paid a stipend to AFDC recipients who volunteered for temporary jobs that were intended to help them learn work skills. These voluntary welfare-to-work interventions increased the earnings and decreased the AFDC payments of participants by modest, but nontrivial, amounts. However, there is fairly substantial variation in these impacts. A partial explanation for this variation appears to be that more expensive voluntary welfare-to-work programs produce larger impacts on earnings and AFDC payment amounts than less expensive programs. We found no evidence of a similar relationship between program costs and program impacts for mandatory welfare-to-work programs.

The research that is presented in this report suggests a number of conclusions about welfare-to-work programs. One that particularly stands out is that although there are a few welfare-to-work programs that may be worth emulating, most such programs by themselves are unlikely to reduce the size of welfare rolls by very much or to improve the lives of most program group members and their children very substantially. Thus, they must be coupled with other policies, such as earnings subsidies.

Report on a Meta-Analysis of Welfare-to-Work Programs

1. INTRODUCTION

The research presented in this report uses meta-analysis to conduct a statistical synthesis of findings from random assignment evaluations of welfare-to-work programs and to explore the factors that best explain differences in performance. The analysis is based on data extracted from the published evaluation reports and from official sources. All the programs included in the analysis were targeted on recipients of Aid to Families with Dependent Children (AFDC).[2] The objective of the analysis is to establish the principal characteristics of welfare-to-work programs that were associated with differences in success, distinguishing among variations in the services received, differences in the characteristics of those who participated in each program, and variations in the socioeconomic environment in which the programs operated.

In part, the origins of this report can be traced back to the greater use of the "1115 waiver authority" in the 1980s and, especially, the 1990s. Although available since the 1960s, waivers were increasingly applied for by U.S. states that wanted to experiment with their welfare provisions, including not only welfare-to-work programs, but also involving child support measures and Food Stamps and Medicaid provisions. The federal government became more receptive to the idea of welfare-to-work experimentation and increasingly granted state waivers, leading to a rapid rise in new welfare-to-work programs being tried and tested. In exchange, states were usually required to evaluate the policy changes they implemented, and the federal government increasingly required more rigorous evaluations that included the use of random assignment. However, it is important to recognize that a number of states did not have to be coaxed by the waiver process to undertake random assignment evaluations of the welfare-

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[2]Because most data used in this study are for years before AFDC was converted to the Temporary Assistance for Needy Families (TANF) program, for convenience we use the AFDC acronym throughout this report.

to-work programs, but did so voluntarily because of a desire to learn about the effectiveness of their innovations. Interestingly, the interest of both the states and the federal government in random assignment evaluations during the 1990s was stimulated by a series of successful random assignment evaluations undertaken in the early 1980s by MDRC, a New York City evaluation firm.

Thus, by the turn of the last century, there were a plethora of evaluations of welfare-to-work programs designed to promote work and reduce welfare caseloads, the results of which have been widely disseminated (Greenberg and Shroder, 2004; Friedlander, Greenberg, and Robins, 1997; Gueron and Pauly, 1991; Greenberg and Wiseman, 1992; and Walker, 1991). The evaluations measured the effects (usually called "impacts") of welfare-to-work programs on outcome indicators, such as the receipt of welfare, the employment status of welfare recipients, their earnings, and the amount of welfare benefit they received. Some, but not all, of these evaluations also estimated the overall costs and benefits of the evaluated programs. Some of the more recent evaluations have also measured program impacts on measures of the welfare of the children of program participants. These impact measures are all examined in the report.

While the evaluations were able to gauge the effectiveness of each welfare-to-work program, they were rarely able to determine reliably the program features that contributed to success or failure. For instance, social and environmental conditions affecting program sites were seldom taken into account, nor were the characteristics of programs. In fact, they did not need to be, because the evaluation designs used by many studies, based on random assignment of welfare recipients into experimental and control groups, guaranteed that individuals in the two groups shared environmental conditions and characteristics. In addition, evaluations often recorded impacts for only the first one, two, or three years after program implementation and were thus unable to assess the long-term performance and viability of interventions. Because their evaluation period was short-term, there was again less need to control for conditions that might have affected impacts over time.

Meta-analysis provides a statistically based means for assembling and distilling findings from collections of policy evaluations. The approach is based on a well-established statistical methodology. On the basis of a comprehensive, systematic review of available evidence, meta-analysis is a check against unwarranted generalizations and unfounded myths, and therefore can help lead to a more sophisticated understanding of the subtleties of policy impacts.

The remainder of this report first provides additional background information, including a discussion of previous statistical syntheses of welfare-to-work programs and a description of the specially constructed database of welfare-to-work evaluations that is used in the study. It then outlines the methodological principles of meta-analysis. This is followed by a discussion of findings from a formal meta-evaluation of the welfare-to-work programs in our sample. Finally, the findings are summarized and conclusions are drawn on their policy implications.

2. BACKGROUND

2.1 Previous Related Research

In a 1997 summary of training and employment program evaluations, Friedlander et al. suggested that welfare-to-work programs typically result in modest, but sometimes in substantial, positive effects on the employment and earnings of one-parent families headed by women. They also noted that the programs are often found to reduce the receipt of welfare and welfare payment levels of these families, but these effects are usually modest and tend to decrease over time. The evidence is less clear for two-parent families.

Friedlander et al. (1997) make a useful distinction between voluntary and mandatory programs. Voluntary programs provide services (e.g., help in job search, training, and remedial education) for those who apply for them and they sometimes provide financial incentives to work for individuals who apply for them. Mandatory programs are targeted at recipients of government transfer payments. They also provide employment-oriented services and sometimes provide financial work incentives, but they formally require participation in the services by potentially subjecting individuals assigned to the program to fiscal sanctions (i.e., reductions in transfer payments) if they do not cooperate.

The Friedlander et al. (1997) review also indicates that there is considerable variation in the effectiveness of different training and employment programs. As previously indicated, a key objective of the research described in this report is to examine the extent to which this variation is attributable to the characteristics of the programs themselves, the characteristics of participants in the programs, and the economic environment in which the programs are conducted.

In previous, closely related, work, we conducted a meta-analysis of 24 random-assignment evaluations of mandatory welfare-to-work programs that operated in over 50 sites between 1982 and 1996 and were targeted at one-parent families on AFDC. Published papers on this research (Ashworth et al., 2004, and Greenberg et al., forthcoming) highlighted the effects of the receipt of program services and participant and site characteristics on program impacts on earnings and the receipt of AFDC. For example, the findings suggest that higher levels of program sanctioning rates result in larger impacts on earnings and leaving the welfare rolls. They also imply that if a program can increase the number of participants who engage in job search, it will, as a result, have larger effects on their earnings and on their ability to leave the welfare rolls. On the other hand, increases in participation in basic or remedial education, vocational training, or work experience and the provision of financial work incentives do not appear to result in larger increases in earnings. Additional findings from the meta-analysis indicate that program impacts on earnings are larger for white than for nonwhite participants and for older participants than for younger participants. They also appear to be larger when unemployment rates are relatively low. In another published paper, Greenberg et al. (2004) examined how program impacts on earnings change over time and found that the effects of a typical welfare-to-work program appear to increase after random assignment for two or three years and then disappear after five or six years.

Of all the welfare-to-work programs that have been evaluated by random assignment in the United States, the two that operated in Riverside, California, and Portland, Oregon, produced the most dramatic impacts. As a result, these programs have become very well known. Greenberg et al. (forthcoming) examined the factors that contributed to Riverside and Portland's exceptional success. More specifically, they first measured the difference in impacts for these two programs and the impact for an average site. They then determined whether the estimated regression could explain a substantial proportion of these differences (it typically could). The findings suggest that only part of this success can be attributed to the design of the programs that operated in Riverside and Portland. The social and ethnic mix of the programs' participants and the economic conditions prevailing at Riverside and Portland at the time of evaluation were also important.

There have been several recent studies in addition to our own that have also attempted to unravel the factors that cause program effectiveness to vary across training, employment, and welfare-to-work programs. For example, the National Evaluation of Welfare-to-Work Strategies, which provides a comparative analysis of eleven welfare-to-work programs over a five-year period, was particularly concerned with comparing the effectiveness of employment-focused and education-focused programs (Hamilton et al., 2001). However, although this study compared impacts across different programs, it did not control for differential exogenous factors, such as variations in the mix of program participants or in economic conditions. It found that welfare-to-work programs that emphasize labor market attachment were more effective in reducing welfare spending, increasing earnings, and facilitating the return to employment of participants than programs that emphasized human capital development.

In a path-breaking re-analysis of random assignment evaluations of California's Greater Avenues for Independence (GAIN) program, Florida's Project Independence, and the National Evaluation of Welfare-to-Work Strategies, Bloom, Hill, and Riccio (2003) pooled the original survey data for over 69,000 members of program and control groups who were located in 59 different welfare offices. The resulting hierarchical linear analysis, which utilized unpublished qualitative data on the program delivery processes, found that the way in which welfare-to-work programs were delivered and the emphasis that was placed on getting the "work-first" message across strongly affected the second-year impacts of the programs included in the analysis. The results also indicated that welfare-to-work programs were less effective in environments with higher unemployment rates.

Greenberg, Michalopoulos, and Robins (2003) have recently completed a meta-analysis of the impacts of voluntary training programs on earnings. They systematically took account of differences in program design, the characteristics of program participants, and labor market conditions. Their analysis indicates that program effects are greatest for adult women participants (many of whom received welfare), modest for adult men, and negligible for youth. They also found race to be an important determinant of program impacts on earnings and that, at least for adults, more expensive programs were not more effective than otherwise similar less expensive programs.

2.2 The Database

All the studies listed above provide useful information. However, the database on which we based our previous research offered several distinct advantages over the data sources utilized in the other studies. In particular, the database:

1. Provided the widest coverage of mandatory welfare-to-work program evaluations by including all those available by the end of 2000 that used a random assignment design to assess programs that provided job search, training services, or financial incentives to encourage work to AFDC recipients. However, unlike Hamilton et al. (2001) and Bloom, Hill, and Riccio (2003), it utilized data that pertain to each evaluated program ("macro-level" data), not to individual members of program and control groups ("micro-level" data).

2. Recorded all the quarterly and annual program impact estimates that were published in various reports from these evaluations by the end of 2000.

3. Included variables that pertain to the receipt of program services, the characteristics of program participants, and the characteristics of the sites in which the programs operated.

The database that we previously used for the studies described above has been greatly updated and expanded for use in the research described in this report. First, data for new impact estimates for previously evaluated mandatory welfare-to-work programs have been added. Second, data from several more recently initiated random-assignment evaluations of mandatory welfare-to-work programs have also been incorporated into the database. Third, the database that we previously used contained information on only mandatory welfare-to-work programs. Random-assignment evaluations of voluntary programs that were targeted specifically at AFDC recipients have now been added. Fourth, indicators of the evaluated programs' impact on the well-being of the children of program participants, as well as outcome information on the children of controls, have been added to the database for those programs for which they are available. Fifth, the original database recorded the net effect of mandatory welfare-to-work programs on the proportion of program participants who were sanctioned (that is, it indicates the experimental-control difference in percentage sanctioned) and indicated whether each evaluated program provided financial work incentives. The information in the database about sanctions and financial incentives has been greatly expanded. For example, it now records the duration of the sanctions and whether the sanctions required the complete or only the partial withdrawal of AFDC benefits. For those evaluated programs that offered financial work incentives to welfare recipients, the database now indicates the dollar amount of the financial incentive that would be received by an individual with two children who has been in a full-time minimum wage job for two months. A similar calculation is available for an individual who has worked full-time for 13 months. All financial information that is recorded in the database—and, by implication, available for use in this report—has been inflated to year 2000 dollars.

Further information about the database and how it can be accessed is provided in Appendix A.

The random assignment welfare-to-work evaluations that are included in the database are listed in Table 1.[3] Some of the listed evaluations were conducted at more than one site. Moreover, some of these sites experimented with more than one type of welfare-to-work program (or intervention). In other words,

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[3]Four of the evaluations listed in Table 1 are excluded from the meta-analysis described in this report. Two of the evaluations were of a voluntary pilot program that was run in Canada, the Self-Sufficiency Project (SSP). Because of differences in the Canadian and U.S. welfare systems, as well as other differences between the two countries, in conducting a meta-analysis, data from the SSP evaluations should probably not be pooled with evaluation data from the United States, and we did not do so. Another excluded program is New York State's Comprehensive Employment Opportunities Support Centers Program. This voluntary program was simply unique; there was no other program to which it could be appropriately compared. The final excluded evaluation is of the Wisconsin Self-Sufficiency First/Pay for Performance Program, a mandatory program. This evaluation was subject

an evaluation may have reported the impacts of several interventions, undertaken at several sites. This is reflected in our database, which records the impacts for each site and intervention separately. For example, the National Evaluation of Welfare-to-Work Strategies (NEWWS) pertains to 11 interventions at seven sites.

The majority of the evaluations of mandatory welfare-to-work programs in our database were conducted in the 1990s. For 77 of the 116 interventions recorded in the database, random assignment commenced between 1990 and 1998. A further 38 interventions were evaluated in the 1980s, including a few for which the random assignment extended into the following decade. One evaluation, MDRC's study of the National Supported Work Demonstration program, was completed in the 1970s. Only one evaluation, Indiana's Welfare Reform program, was conducted after the introduction of TANF, which replaced AFDC[4] and took effect July 1, 1997.[5] TANF ended federal entitlement to assistance and created block grants to fund state expenditures on benefits, administration, and services to needy families. It also introduced time limits on the receipt of welfare assistance and changed work requirements for benefit recipients. [6]

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to a number of technical problems and, consequently, only limited confidence can be placed in the estimates of program effects that were produced by it.

[4]It also replaced the Job Opportunities and Basic Skills Training (JOBS) program and the Emergency Assistance (EA) program.

[5]Indiana's Welfare Reform program included the random assignment of two participant and control group cohorts. The assignment for the later cohort commenced in March 1998 and was completed in February 1999. In all other cases, the random assignment process had started and was completed before TANF took effect. Five of the evaluated state welfare reform programs listed in Table 1 continue to run under the same name at the time of writing (May 2005), although some may have modified their service contents. These are California's CALWORKS, Delaware's A Better Chance (ABC), Iowa's Family Investment Program (FIP), Minnesota's Family Investment Program (MFIP) and Virginia's Initiative for Employment, not Welfare (VIEW).

[6]The introduction of TANF in lieu of AFDC was intended to enhance the services available to welfare recipients and improve their effectiveness in placing recipients in jobs, increasing their earnings, and reducing their welfare dependency. It is conceivable, therefore, that the taking effect of TANF would influence some of our findings. As explained in more detail below, we conduct the meta-analysis for different calendar quarters after the random assignment of program and control groups. For those interventions that completed random assignment in the mid-1990s, impacts that were measured three or four years after random assignment, in fact, occurred after the date during which TANF took effect. It is impossible to determine with precision the number of persons assigned to the samples used in the evaluations in our database that may have been affected by TANF because program evaluations

All the evaluated welfare-to-work programs listed in Table 1 were intended to encourage employment and also, in most cases, to reduce dependency on welfare. The evaluations are divided between those that assessed mandatory programs and those that examined voluntary programs. There are a few evaluations listed in Table 1 that assessed programs that provided financial work incentives but not services. We classify those "pure" financial work incentive programs for which individuals had to apply as "voluntary" and those for which welfare recipients were made eligible, regardless of whether they applied, as "mandatory." Individuals assigned to "pure" financial work incentive programs that are classified as "mandatory" were obviously not subject to sanctions for refusal to participate in services, as no services were offered. However, the manner in which eligibility to participate in the program was determined was similar to that of mandatory programs that did provide services.

The older experiments listed in Table1 (for example, SWIM in San Diego and the Employment Initiatives program in Baltimore) tended to be of demonstration programs, which were run for the express purpose of seeing how well they functioned. The study sites volunteered for this purpose and may not have been very representative. For example, funding levels may have been high and the staff exceptionally motivated. However, there is little evidence on whether the findings were distorted by such factors. Most of the more recent random assignment evaluations (for example, the Virginia Independence Program and the Indiana Welfare Reform program) resulted because a state desired to implement a new program and as a condition of obtaining federal waivers was required to evaluate the intervention using random assignment. These evaluations often took place when state AFDC programs were undergoing

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typically do not provide data about the number of individuals randomly assigned at each point in time during the random assignment process. However, assuming a steady process of random assignment with a similar number of individuals assigned in each calendar quarter, we estimate that, in the case of the 3rd quarter impact measurements in our database, roughly 1 percent were taken during or after 1997. This increased to around 6 percent for Quarter 7, 16 percent for Quarter 11, and 42 percent for Quarter 15. (We count an impact measurement as having taken place during or after 1997 if the evaluation data for at least half the sample population pertain to that period. See also footnote 13.) Our analysis captures some of the potential effect of TANF by including an independent variable that measures for each evaluation the years between its mid-point of random assignment and the mid-point of random assignment of the earliest of the evaluation, included in our database.

many changes. Consequently, staff was probably less motivated and had less time to focus on the innovations being evaluated than was the case in the earlier evaluations.

2.3 Components of the Current Research

The research described in this report attempts to exploit the greater diversity of programs and greater number of impact estimates that are available in the updated and expanded database. For example, we examine voluntary welfare-to-work programs, as well as those that are mandatory. In our prior work, we were only able to study the latter. In all, 27 evaluations of mandatory welfare-to-work programs and four evaluations of voluntary welfare-to-work programs, which together cover nearly 100 interventions, are used in the analysis.

A key objective of the research described in this report, like the original analyses that were described earlier, is to explore whether and how program impacts are affected by various program, participant, or site characteristics. To determine how robust our conclusions are, we subject much of our statistical analysis to sensitivity tests. These tests will be described when they are presented.

Our previous analysis was limited to program impacts among one-parent families. In this report, we examine program impacts on two-parent families, as well as those on one-parent families. Only two measures of program impacts—earnings and the percentage of program group members receiving AFDC—were examined in our earlier research. In addition to utilizing the updated and expanded database to reexamine program impacts on earnings and AFDC receipt, the research presented in this report includes an analysis of additional measures of program effects, including impacts on employment status and the amount of AFDC benefits received. The estimates of net program benefits, which were obtained from the cost-benefit analyses that were part of many (but not all) of the welfare-to-work evaluations listed in Table 1, are also examined. In addition, an analysis is conducted of the various measures of the effects of welfare-to-work programs on child well-being, which, as previously mentioned, have been added to the database.

The database contains up to 20 calendar quarters of impact estimates for the evaluated programs (although fewer quarters of estimates were available from many evaluations). Using these estimates, we examine how the impacts of welfare-to-work programs vary over time. The additional data allow for a longer-term follow-up of impacts. As previously mentioned, we also explored this issue in our earlier research. However, the updated database contains substantial numbers of additional quarters of measured impacts, especially from the later calendar quarters, that were not previously available to us, permitting somewhat more precise estimates of how program effects change over time.

Current understanding of what constitutes a "successful" welfare-to-work program or a "failed" one is mainly based on simple comparisons of the impacts of selected welfare-to-work programs that rarely attempt to standardize for differences in participant or site characteristics that exist across programs. In this report, we attempt to identify especially successful and unsuccessful programs *after controlling* for the effects of measurable program features and target group and site characteristics. The objective of this analysis is to identify welfare-to-work programs that are highly successful or unsuccessful, *ceteris paribus.* In other words, the goal is to distinguish programs that still record *positive* or *negative* impacts even after accounting for factors that might be expected to increase or decrease impacts, such as a program design, and advantageous or disadvantageous labor market conditions and target group characteristics. Once identified, one can speculate as to what accounts for the remaining over- or underperformance of these programs.

It has been recognized that welfare reform and welfare-to-work programs might affect different population subgroups differentially (Walters, 2001). Thus, a few studies have begun to explore the effects of, for example, different amounts of education or differences in ethnic origin on program impacts (Michalopoulos, Schwartz, and Adams-Ciardullo, 2001 and Harknett, 2001). In this report, we add to this research by presenting comparisons of the separate impact estimates for subgroups contained in the database.

3. META-ANALYSIS

This section provides a brief description of the meta-analysis methods that we use to accomplish the goals discussed above. Meta-analysis provides a set of statistical tools that allow one to determine whether the variation in impact estimates from evaluations of welfare-to-work programs is statistically significant and, if it is, to examine the sources of this variation. For example, it can be used to determine whether some of the variation is due to differences in the mix of program target groups, economic conditions in the places and the time periods in which the evaluations took place, or the types of services provided by the programs. Good descriptions of meta-analysis are available in Hedges (1984), Rosenthal (1991), Cooper and Hedges (1994), and Lipsey and Wilson (2001).

Separate meta-analyses of the mandatory and voluntary welfare-to-work programs, were conducted. The motivation of individuals entering these two types of programs would be expected to differ. In addition, as a result of differences in the evaluation design that is typically used, a higher proportion of those assigned to the program group of voluntary programs than those assigned to the program group of mandatory programs typically receive program services and financial work incentive payments.

The alternative to using meta-analysis (or a related statistical method such as the hierarchical linear approach utilized by Bloom, Hill, and Riccio [2003]) to synthesize the evaluations of welfare-to-work interventions is a narrative review. Both approaches rely on comparisons among evaluated programs. It is important to recognize that, even if these comparisons are limited to programs that were evaluated through random assignment (as they are in this study), the comparisons themselves are nonexperimental in character and thus may be subject to bias.

Although both meta-analysis and narrative synthesis rely on available information from evaluation reports, and as a result and as discussed later, are subject to numerous limitations, meta-analysis offers a number of advantages. Possibly most important, it imposes discipline on drawing conclusions about why some programs are more successful than others by formally testing whether apparent relationships between estimated program impacts and program, client, and environmental characteristics are statistically significant. Moreover, it can focus on one of these characteristics, while statistically holding others constant. In addition, given a set of evaluations that are methodologically solid (for example, based on random assignment) narrative synthesis typically gives equal weight to each, regardless of the statistical significance of the estimates of program impacts or the size of the sample upon which they are based. As discussed in the following section, meta-analysis uses a more sophisticated approach.

3.1 Weighting

In conducting a meta-analysis of program impacts, it is essential take account of the fact that the impact estimates for the individual programs are based on different sample sizes and, hence, have different levels of statistical precision. The reason for taking account of different levels of statistical precision is suggested by the following formal statistical model, which explains variation in a specific program impact, such as on earnings, employment, AFDC receipt, or child outcomes:

$$E_i = E_i^* + e_i, \text{ where } i = 1, 2, 3, \ldots, n$$

where E_i is the estimated effect or impact of a welfare-to-work intervention, E^*_i is the “true” effect (obtained if the entire target population had been evaluated), n is the number of interventions for which impact estimates are available, and e_i is the error due to estimation on a sample smaller than the population. It is assumed that e_i has a mean of zero and a variance of v_i.

To provide an estimate of the mean effect that takes account of the fact that v_i varies across intervention impact estimates, a weighted mean can be calculated, the weight being the inverse of v_i, $1/v_i$. The reason for weighting by the inverse of the variance of the estimates of program impacts is intuitive. In evaluations, estimates of impacts from policy interventions are usually obtained by using samples from the intervention’s target population. One subset of persons from this population who are assigned to the program is compared to another subset of persons from the same population who are not assigned. As a result of sampling from the target population, the impact estimates are subject to sampling error. The variance of an estimated impact (which typically becomes smaller as the size of the underlying sample increases) indicates the size of the sampling error. In general, a smaller variance implies a smaller sampling error and, hence, that an impact estimate is statistically more reliable. Because all estimates of intervention impacts are not equally reliable, they should not be treated the same. By using the inverse of the variance of the effect estimates as a weight, estimates that are obtained from larger samples and, therefore, are more reliable, contribute more to various statistical analyses than estimates that are less reliable.[7]

We use such weights throughout in conducting the statistical analysis presented in this report. Typically, however, the evaluations used in this study did not report the exact value of the variance of the impact estimates, but instead reported that estimates of impacts were not statistically significant or were significant at the 1-, 5-, or 10-percent levels. Thus, the standard errors had to be imputed, except for those relatively rare instances when exact standard errors were provided. Once the standard errors were imputed, the variance could be computed as their square.

For impacts that are measured as proportions (e.g., the impact on the percentage of program group members who are employed or receiving AFDC), the imputation of the standard errors was done as follows:

$$\sigma^2 = \sqrt{(P_t(1-P_t)/N_t) + (P_c(1-P_c)/N_c)}\ ,$$

where σ^2 is the standard error of the program impact, P_t is the proportion receiving AFDC in the treatment group, N_t is the number of people in the treatment group, P_c is the proportion receiving AFDC in the control group, and N_c = the number of people in the control group.

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[7]There is an alternative weighting scheme that attempts to take account of factors that cause variation in program impacts that were not measured (e.g., the quality of leadership at program sites or local attitudes towards welfare recipients), as well as sampling variation (see Raudenbush, 1994). This method is laborious to implement and we do not use it here.

For impacts that are measured as a continuous variable (e.g., earnings or the amount of AFDC received), imputation of the standard error is considerably more complex. First, for impacts that were significant at the 5- or 10-percent levels, it was assumed that the p-value was distributed at the midpoint of the possible range, i.e., if 0.1>p>0.05, p was assumed to equal 0.075; and if 0.05>p>0.01, p was assumed to equal 0.03. Second, cases for which impacts were significant at the 1-percent levels have an unbounded t-value and cases for which impacts were not significant can have extremely small standard errors. For these cases, we used the following procedure: (1) we multiplied each of the standard errors imputed as described above for impacts that were significant at the 5- or 10-percent levels by the square root of the sample on which the impact estimate was based; (2) we computed the average of the values derived in (1); (3) for cases in which impacts were significant at the 1-percent level or were not significant, we imputed the standard error by dividing the constant derived in (2) by the square root of the sample size on which the impact estimate was based.

No measures of statistical significance are available for the cost-benefit estimates of program effectiveness because such estimates are a composite of separate impact estimates. Thus, in our analysis of these measures, we weight by the square root of the total sample used in the evaluation (i.e., by the square root of $N_t + N_c$), rather than by $1/v_i$. However, for impact estimates for which both measures are available, the simple correlation between them is quite high, around .85 to .9.

3.2 Steps in Conducting the Statistical Analysis of the Program Effect Estimates

Descriptive Analysis. The first step in performing the meta-analysis was to conduct a descriptive analysis of program impacts. Thus, we present statistics for the means and medians of the impact estimates, their standard deviations, and their minimum and maximum values. Both weighted and unweighted means are reported. These statistics provide an overall picture of the size of the effects of welfare-to-work programs and how they vary.

Regression Analysis. The next step consists of using regression analysis to explain the variation among the program effect estimates. This analysis is limited to the evaluations of the mandatory programs, as there are an insufficient number of observations for the voluntary programs to conduct a regression analysis. The analysis performed for the voluntary programs is described below. For the reasons discussed above, we focus on regressions that are weighted by $1/v_i$. However, given the problems in computing $1/v_i$, we also estimated unweighted regressions for comparison purposes. It may be useful to point out that the R-squared in both the unweighted and weighted regression must be less than one because the program impact estimates are subject to sampling error. This would be true even if *all* the systematic sources of variation in the program impact estimates could be taken into account.

To examine how the impacts of welfare-to-work interventions change over time, we pooled impact measures across the twenty post-random-assignment calendar quarters in our database. Otherwise, however, we estimated separate regressions for intervention impacts measures in four different post-random-assignment calendar quarters, the 3rd, 7th, 11th, and 15th.[8] There are three reasons we did this. First, we can determine whether the importance of certain explanatory variables changes over time. For example, one might anticipate that job search would have a stronger influence on earnings during the early post-random-assignment quarters than later calendar quarters and that the opposite might be true of vocational training. Second, an evaluation of a welfare-to-work intervention usually reports impact estimates for several different calendar quarters. These impact estimates are not statistically independent of one another. Moreover, more quarters of impact estimates are available for some evaluated programs than for others. Thus, pooling across quarters would inappropriately give more weight to some

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[8]There were a few evaluations that did not report impact estimates for the quarters of interest, but did report them for nearby quarters—for example, for quarter 6 or 8 or 9, but not quarter 7. These values were included in conducting the analysis in order to maximize the number of quarterly observations on which the calculations are based. In addition, there were a few evaluations that reported program effects on annual earnings and annual AFDC receipts, but did not provide quarterly estimates of these impacts. In these instances, the annual estimates were divided by four and assigned to the quarter of interest that occurred during the year over which the annual impacts were measured.

evaluations than to others. Estimating separate regressions for different quarters helps circumvent these problems. Third, we conducted Chow tests of several of the impact measures to see if different regression models were needed for different calendar quarters. The tests resoundingly rejected the hypothesis that the coefficient vector for calendar quarters 1–10 are the same as that for quarters 11–20. Although less strongly, they also rejected the hypotheses that the regression models were the same for quarters 1–5 as for quarters 6–10 and the same for quarters 11–15 as for quarters 16–20. These results imply that although impact estimates might be pooled across a few adjacent or nearly adjacent calendar quarters, separate regressions should be estimated for quarters that are far apart.

In estimating separate regressions for quarters 3, 7, 11, and 15, we conducted additional Chow tests to determine whether different regression models are required for program impacts that are estimated for one-parent families and for those that were estimated for two-parent families or for impacts for programs that provided services and for impacts for programs that only provided financial work incentives. This time, the tests strongly and consistently indicated that the coefficient vectors did not significantly differ for these different groups and, hence, that the impact estimates could be pooled across the groups.

In estimating the regressions, we needed measures of the difference that welfare reform programs made in terms of the type and range of services provided as explanatory variables. For this purpose, we used the difference in participation rates in various activities (job search, basic education, work experience, and so forth) between those assigned to programs and those assigned to the control groups. Thus, we obtain measures that quantify the “net effect” of the introduction of a welfare reform program relative to the traditional program. These measures have an advantage over other effect indicators, such as stated policies or declared program intentions, in that they reflect what actually occurred. In addition, they take account of program nonparticipation, including caseload attrition due to unassisted return to work or leaving the welfare rolls on one’s own volition. As relative or “net” effect indicators, they also take account of variations in the intensity of service provision between different programs and program sites.

There is a possibility, however, that the measures of program participation rates that are used as explanatory variables in the regressions are endogenously determined. This could occur, for example, if programs that have a client population of individuals who are mostly job ready (e.g., high school graduates with considerable previous work experience) tend to stress job search, while programs with large fractions of clients who are not job ready tend to emphasize basic education. Similarly, programs that are located at sites with low unemployment rates might tend to emphasize job search and those with high unemployment rates might make more use of vocational training. Under these circumstances, program participation rates would, in part, reflect client and site characteristics, causing estimates of the relation between these measures and program impacts to be biased. It should be borne in mind, however, that the regressions control directly for client and site characteristics. Moreover, as discussed above, the program participation rates that we actually use in the regressions are measured in terms of the degree to which each program *changes* the pre-program regime—that is, the difference between the program group and the control group. Although program designs may reflect the characteristics of the available client population or local environmental conditions, it is not apparent that *changes* in how programs are run would be affected by client and site characteristics, assuming that these characteristics remain fairly stable.

Homogeneity Tests. For the 7th and the 11th calendar quarters, the database contains ten impact estimates for programs that placed welfare recipients who volunteered to participate into temporary jobs that paid them a stipend while they learned work skills. This is an insufficient number to conduct a regression analysis of the sort we conducted with the mandatory programs. Thus, we have conducted formal tests of homogeneity instead. We also conducted tests of homogeneity of the measures of the effects of welfare-to-work programs on child well-being, because sample size is also limited. In this case, we have a variety of different measures for each of three different age groups, but relatively few estimates for most measures. The homogeneity tests allowed us to see whether the estimated impacts differer significantly from one another (e.g., whether impacts for expensive interventions differ from impacts for inexpensive interventions).

A homogeneity test relies on the Q statistic, where Q is the weighted sum of squares of the estimated impacts, E_i, about the weighted mean effect, $\overline{E}$, and where (as before) the weights are the inverse of the variance of the estimated impacts (Lipsey and Wilson, 2001, pp. 215–216). Thus, the formula for Q is

$$Q = \Sigma\, 1/v_i\, (E_i - \overline{E})^2$$

Q is distributed as a chi-square with the degrees of freedom one less than the number of program effect estimates. If Q is below the critical chi-square value, then the distribution in the program effect estimates around their mean is no greater than that expected from sampling error alone. If the null test of homogeneity is rejected (i.e., Q exceeds the critical value), this implies that there are differences among the program effect estimates that are due to systematic factors (e.g., differences in program or target group characteristics), not just sampling error alone.

To analyze the voluntary welfare-to-work programs and the child well-being impact measures, we first pooled all the available program impact estimates and then, using the test described above, determined whether they are distributed homogeneously. In those cases when they are not, we then divided the impact estimates into subgroups on the basis of various potential explanatory factors (e.g., differences in net government operational costs, services provided, client characteristics, or site environmental characteristics) and repeated the homogeneity test. If the impact estimates for the subgroups are more homogeneous than those for the full set of observations, then this suggests an explanation for at least some of the divergence in the impact estimates.

4. FINDINGS OF THE DESCRIPTIVE ANALYSIS

As noted earlier, the analyses in this report focus on four indicators of program impacts: increases in the earnings received by members of the program group, increases in the percentage of those in the program group in employment, decreases in the amount of AFDC payments that those in the program group received, and decreases in the percentage of those in the program group in receipt of AFDC payments. Table 2 presents basic descriptive statistics for these four indicators, measured at the 3rd, 7th, 11th and 15th quarter after random assignment for mandatory welfare-to-work interventions. Both weighted and unweighted estimates are shown. However, unless we specifically indicate otherwise, in discussing Table 2 we focus on the weighted estimates.

Some caution is required in comparing statistics for different quarters because the number of evaluations and, therefore, the composition of the evaluations upon which the statistics are based, changes.[9] This is illustrated by the increasing importance over time since random assignment of the weighted mean impacts relative to their median counterparts. With the exception of the impact measuring the percentage employed, by the 15th quarter the means are higher than their corresponding medians. The results for later quarters, in particular the 15th quarter, are, thus, based on a greater proportion of relatively high-impact programs than appears to have been the case during earlier quarters.

The most striking result in Table 2 is the modest sizes of the impacts, whether measured as means or medians. For example, the weighted impacts on quarterly earnings are all around $100 or less than $500 annually (in year 2000 dollars) and the weighted impacts on AFDC payments tend to be even smaller. However, the standard deviations of the mean impacts are quite large, suggesting that some of the evaluated interventions were much more effective than others. Thus, it is useful to explore the reasons why success differs among welfare-to-work programs. Much of the rest of this report is devoted to such an exploration.

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[9]In addition, the number of individuals in the evaluation sample populations that move out of state increases over time. This causes problems in making comparisons over time because most evaluations of welfare-to-work programs rely on state-gathered administrative data. Thus, program impacts cannot usually be estimated for persons moving out of state. Consequently, if program impacts for persons moving out of state differ from those remaining in-state, the evaluation findings will be increasingly distorted over time. Furthermore, because both program and control group members who move out of state, do not show up in state administrative data, they are

4.1 Earnings

The largest number of observations is available for impacts on the earnings of the program group. Individuals taking part in traditional welfare programs (the control group) earn, on average, $675 (weighted) in quarter 3. This rises to nearly $1,360 (weighted) in quarter 15 (in year 2000 dollars). Similarly, mean impacts—that is, the difference between the control groups' and the program groups' earnings—average $74 in quarter 3 and $115 in quarter 15. Program group members therefore earn, on average, around 10 percent more per quarter than control group members. The proportion of extra earnings, however, declines somewhat in later quarters as mean impacts rise less from one quarter to the next than the mean control group earnings do. In fact, the unweighted mean impact declines between the 11th and the 15th quarter, while mean earnings for the control group continue to increase. Large standard deviations highlight the variability of both weighted and unweighted impacts among the evaluated interventions.

4.2 Percentage Employed

Around a third of control group members are employed in each quarter, although this fraction increases somewhat over time. Welfare-to-work programs appear to increase employment among those assigned to them by about three percentage points. However, while this is an 11.3 percent increase over the control groups' mean employment rate in the third quarter after random assignment, it is only a 7.5 percent increase in the 15th quarter. Again, high standard deviations indicate considerable variation among programs. The weighted and unweighted mean employment figures for controls are fairly similar, as are the weighted and unweighted impact estimates.

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usually treated in evaluations of welfare-to-work evaluations as neither receiving AFDC nor working. To the extent this is not the case, the impact estimates will be further distorted, and this distortion will increase over time.

4.3 Amount of AFDC Payment

The weighted amount of AFDC payments received between the 3rd and the 15th quarter by members of the control groups of welfare-to-work programs declines, on average, from $1,033 in quarter 3 to under $460 in quarter 15 as individuals increasingly leave the AFDC rolls without the intervention of a welfare-to-work program. It will be recalled that a reduction in AFDC payment is recorded as a positive impact; that is, positive values for the impact indicate a reduction in the receipt of AFDC. Thus, in the 3rd quarter, as control group members receive $1,033 on average, individuals assigned to the program group receive approximately $38 less (i.e., about $995). In the 15th quarter, the control mean is only $458, or less than half of the mean recorded for the 3rd quarter, while the weighted mean impact reaches $75, or about twice the amount recorded for a typical site after three quarters. AFDC payments to program group members in quarter 15, therefore, average around $383.

As a proportion of control group AFDC payments, mean impacts increase from less than 4 percent ($37.8/$1,032.8) in the 3rd quarter to over 16 percent ($75.1/$458.0) in the 15th quarter. However, the mean and median impacts change differentially between quarters. For example, the median impact value declines from $89 in the 11th quarter to $41 in the 15th quarter. Thus, the increase in the mean impact between these quarters may be affected by the greater presence of a number of very high-impact programs among the declining total of observations available in the final quarter.

4.4 Percentage Participating in AFDC

A similar trend can be observed for the percentage of individuals still receiving AFDC payments after random assignment. For the control group, this proportion decreases from nearly 81 percent in the 3rd quarter to 41 percent in the 15th quarter; that is, it is approximately halved. The additional reduction in the receipt of AFDC due to welfare-to-work programs averages 1.5 percentage points in the 3rd quarter and 4.4 percentage points in the 15th quarter. Hence, as the AFDC caseload among those randomly assigned in welfare-to-work experiments declines, both the relative and the absolute program impact increases. The decline in the median impact from the 11th to the 15th quarter again suggests that the greater mean impact of welfare-to-work programs in the later quarters after random assignment at least in part reflects the increasing importance of high-impact programs in the remaining sample of evaluated interventions.

Overall, the descriptive statistics from Table 2 suggest that, on average, welfare-to-work programs had the intended positive impact on all four indicators and that these positive impacts were maintained in all four quarters we have examined. However, the programs' absolute and relative impacts appear to be sustained longer with respect to AFDC payments and AFDC receipt than earnings and employment. In all instances, standard deviations matched, or indeed exceeded, the mean impact values, suggesting considerable variation among individual programs. For later quarters, as the number of evaluations declines, their composition also changes, with the inclusion of a greater proportion of high-impact interventions.

4.5 Description of the Tested Interventions, Target Populations, and Sites

As mentioned earlier, much of the analysis in this report relies on estimating regressions to examine the relation between the four impact measures described above and measures of program design, the characteristics of the target population, and social and economic conditions at the sites of the evaluated programs. A list of variables that are in the database and thus could potentially be used as explanatory variables in these regressions appears in Table 3, along with their means and standard deviations.

These means and standard deviations pertain to the subset of 79 observations for which estimates of impacts on earnings were available in the 7th calendar quarter. As indicated by Table 2, the sample size and hence the sample composition varies by impact measure and by quarter. In addition, the values of the site socioeconomic condition measures are specific to the year during which the impacts were measured. Hence, the means and standard deviations of the variables listed in Table 3 also vary to some extent by impact measure and quarter. However, the values that appear in the table are representative of the values for the other impact measures and quarters.

As indicated by Table 3, some of the variables listed are not available for every observation. As discussed below, we attempted to minimize this problem by selecting explanatory variables for the regressions that have relatively few missing values. When an explanatory variable was nevertheless missing in running the regressions, we used its mean value. Later we report the results of sensitivity tests in which we compare our findings with those from regressions in which observations with missing values are dropped.

A key indicator of program design is how it affects the receipt of the services it provides. Because controls often receive services similar to those received by persons assigned to the evaluated programs, but from outside the program, it is important to measure the net difference between the two groups in their receipt of services—that is, the program's impact on participation in services. The measures of impacts on participation that are reported in Table 3 typically indicate whether participation has occurred by around a year after random assignment, although some evaluations record participation impacts later than that. The data indicate that a typical mandatory welfare-to-work intervention in our sample put much more emphasis on increasing participation in relatively inexpensive activities, such as job search, than on increasing participation in more costly activities, such as basic education and vocational training. Nonetheless, it costs the government almost $2,000 (in year 2000 dollars) more to operate the evaluated programs than to run the programs serving controls.

Arguably, the singularly greatest contribution of the evaluated mandatory welfare-to-work interventions was to increase participation in job search activities by an average of 21 percentage points. The programs' net contributions to other activities, including those aimed at promoting human resource development, were considerably smaller, increasing participation in basic education by an average of just seven percentage points and in vocational training and work experience by less than three percentage points. Indeed, some individual programs with a work-first emphasis actually had a negative impact on participation in these activities. The mandatory nature of the programs covered by Table 3 is exemplified by the six-percentage-point average net increase in sanctions that resulted from them.

About 15 percent of the 79 interventions that comprise the sample for Table 3 tested time limits and nearly one-third tested financial incentives. Nearly half of the latter interventions ("pure" financial incentive programs) were designed to test financial incentives alone. The mean financial incentive amount of $82.75 that appears in Table 3 is computed by averaging over *all* 79 interventions those that provided financial incentives and those that did not. Thus, the interventions that did provide financial incentives paid about $250, on average, to a single mother with two children during her 13^{th} month in a full-time job.

The mid-point of the random assignment of the earliest evaluations of mandatory programs listed in Table 1 occurred in 1983. The mid-point of random assignment of the typical mandatory welfare-to-work intervention in our sample took place about eight years later.

In a typical evaluated intervention, the average age of family heads in the target population was 31, and about one-quarter were under 25. The number of children in these families was about two. Around half the families had at least one child less than six years of age. On average, 36 percent of the target population was black, 41 percent was white, and 17 percent was Hispanic. Just over half of the family heads in the target population for a typical evaluation had obtained a high school degree or diploma, and this varied little across the evaluated intervention. Finally, slightly less than half the family heads had been employed during the year before random assignment, with some variation across sites.

Unemployment rates, which serve as indicators of the availability of jobs, averaged 6.4 percent across the sample of interventions in Table 3 but, as indicated by a standard deviation of 2.3, varied considerably. An alternative measure of the availability of jobs is the annual percentage change in manufacturing employment, which was just over 1 percent, and, as the standard deviation of 4.5 implies, was often negative. Poverty rates, which are indicative of a range of factors reflecting both individual characteristics (e.g., lone parenthood, lower educational attainment) and area characteristics (lower job availability in deprived areas, less commercial investment, greater risk of segregation), averaged 14.6

percent. Annual median household income, which averaged $40,237 (in year 2000 dollars) across the evaluation sites, provides an alternate measure of local living standards. Manufacturing employment accounted for 13 percent of total employment at the sites, on average.

Two measures of the characteristics of the AFDC programs at the evaluation sites appear in Table 3. The first indicates the generosity of AFDC payments across the program sites. Averaged across the interventions, single mothers with two children and no other income were eligible for a monthly payment of $603 (in year 2000 dollars). The standard deviation of just under $200 of the maximum AFDC payment confirms the considerable state-to-state variation in generosity. The second measure attempts to capture the "toughness" of sanctions at the sites as exemplified by either specifying a minimum sanction length at the first sanction (the alternative is to sanction until compliance) or terminating full family AFDC benefits during the first sanction (the alternative is a partial reduction in benefits). Only 6 percent of the sites had at least one of these provisions.

5. HYPOTHESES TESTED IN THE REGRESSION ANALYSES

A number of hypotheses are possible about the relation between the intervention impact estimates and the variables described in the previous section. We consider some of these hypotheses in this section. As will be seen, in a number of instances there are plausible contradictory hypotheses, one of which implies a negative relationship between a given explanatory variable and intervention impacts and the other of which implies a positive relationship.

5.1 Intervention Characteristics

It is, of course, difficult, if not impossible, to capture the essence of a welfare-to-work intervention with a few quantitative measures. However, the best available measures are probably the net participation and sanction rate estimates that appear in Table 3. In general, we anticipated that an increase in any activity, holding program effects on other activities constant, would be positively related to program impacts, if the activity were at all effective. However, some training activities, such as basic education, work experience, and (especially) vocational training, require a number of weeks to complete. If these activities are not very effective in increasing earnings, but those participating in them believe that they will be, they may hold some individuals on the welfare rolls and out of the labor market longer than would otherwise be the case. Thus, net participation rates for basic education and vocational training could be negatively related to the impacts of welfare-to-work interventions.

Sanctioning would be expected to have an indirect effect on welfare-to-work intervention impacts by increasing participation in program services. These indirect effects should, in principle, be captured by the measures of net participation in program activities. However, sanctions may also have positive direct effects on program impacts. In those instances in which an AFDC grant is entirely eliminated families are terminated from the welfare rolls and, as a consequence, some of the heads of these families will presumably seek employment. Even when the grant is only partially reduced, the reduction may cause some individuals to decide to leave the AFDC rolls and seek employment.

We expected that financial incentives would be positively related to program impacts on employment and earnings. However, because these incentives usually raise the earnings level at which families can continue to receive AFDC, we also anticipated that they would be negatively related to intervention impacts on whether AFDC was received. Moreover, it is also conceivable that greater earnings disregards reduce the work effort of AFDC recipients who would work even in their absence, as these persons are able to maintain their standard of living while working fewer hours (see Blank, Card, and Robins, 2000). Whenever this is the case, total income would remain roughly the same and earnings disregards would reduce earnings.

Although few members of the treatment groups who were assigned to the interventions that tested time limits actually reached these limits during the earlier calendar quarters after random assignment, the very existence of time limits may create pressures to leave the welfare rolls and replace transfer payments with earnings. For instance, the lengths of different welfare spells are summed under most time limit provisions to determine whether a family has reached the limit. Thus, to the extent possible, families may wish to conserve months on welfare for those times when they need financial aid the most. Consequently, we anticipated that time limits would be positively related to intervention impacts in all the calendar quarters we examine, but especially the later quarters as members of welfare-to-work program groups begin to approach them.

The purpose of the number of years since 1982 variable that appears in Table 3 is to test whether welfare-to-work programs have improved over time because more has been learned about running them effectively. If so, the relation between this variable and the program impact estimates would be positive. As previously discussed, however, older random assignment evaluations tended to be of demonstration programs, while more recent evaluations were typically of new welfare reform programs that states desired to implement. This difference could have caused the estimated relation to be negative.

5.2 Characteristics of the Target Population

Two opposing hypotheses can be formulated about the relationship between program impacts and the extent to which the caseload in a welfare-to-work program faces disadvantages or barriers in obtaining employment and, hence, in increasing earnings and leaving the AFDC rolls. On the one hand, program impacts may be smaller the more disadvantaged the participant caseload, because persons in such a caseload will have greater difficulty in obtaining employment. On the other hand, members of more advantaged caseloads may be better able to obtain employment on their own, without the aid of a welfare-to-work program, while such an intervention may be needed to help those with barriers to employment overcome them. If so, impacts will be larger the more disadvantaged the caseload is. To illustrate, impacts could be larger for program group members with recent work experience because it is easier for these persons than for program group members with a long-term welfare dependency to find jobs. The contrary possibility is that such individuals may be better able than persons with a long-term welfare dependency to succeed in the labor market on their own without help from a program. If so, the impacts of the interventions will be smaller as the percentage of the target population that worked during the year prior to random assignment becomes larger.

5.3 Socioeconomic Conditions at the Sites

The impacts of welfare-to-work programs on earnings and welfare receipt are likely to be influenced by the socioeconomic conditions that prevailed at the times and places the programs operated. The evaluations that are included in our database measured program impacts under a wide variety of socioeconomic conditions. Although each individual study estimated training effects over only a few years, taken together, they cover a time span of nearly two decades. Moreover, the evaluated programs operated in varied communities. For example, the welfare-to-work programs included in this study include programs from high-benefit states, such as California and Connecticut, alongside states with relatively low AFDC benefit levels, such as Arkansas, Virginia, and Florida. However, although this means that the study captures a range of benefit regimes and associated work incentive conditions, the included programs are not necessarily nationally representative.

In theory, measures of the availability of jobs at the evaluation sites (e.g., the unemployment rate or the annual percentage change in manufacturing employment) could be either positively or negatively related to the impacts of welfare-to-work interventions. On the one hand, if jobs are scarce, then those who are assigned to a welfare-to-work program may enjoy a competitive advantage over similar persons who enter the program when jobs are difficult to find. If so, the relationship between the unemployment rate and program impacts would be positive and that between the change in manufacturing employment and program impacts negative. On the other hand, a welfare-to-work program may be most helpful when jobs are plentiful. If so, the relationship between the unemployment rate and program impacts would be negative and that between the change in manufacturing employment and impacts positive. Finally, there could be a quadratic relationship between the two measures of job availability and program impacts. This would occur if, when there are few job openings, there is little that a welfare-to-work program can accomplish; but when jobs are in abundance, welfare recipients can readily obtain employment without the aid of a program. Under these circumstances, program impacts would be greatest when job availability is between these two extremes.

Similar reasoning suggests that the relationship between the poverty rate or median household income and the impacts of welfare-to-work initiatives could be either positive or negative. On the one hand, a welfare-to-work program could be especially helpful to persons who reside in areas with limited economic opportunities, providing them an advantage over similar persons who do not receive services from such a program, resulting in a positive relation between impacts and poverty rates and a negative relation between impacts and median household income. On the other hand, neither persons assigned to such a program nor similar persons who are not assigned may have decent employment opportunities in highly disadvantaged areas. Thus, a welfare-to-work program may have relatively little impact in such areas. In addition, if a job is obtained, it should pay less where poverty rates are high or median household income is low. This suggests that the poverty rate at the evaluation sites should be negatively related to program impacts on earnings and median household income should be positively related.

The generosity of welfare payments at the evaluation sites (which is represented in the analysis by the size of the maximum AFDC payment for which a family of three is eligible) is expected to reduce the impact of a welfare-to-work program on the receipt of AFDC by reducing the incentive of some welfare recipients to leave the rolls. In addition, the earnings level at which families can continue to receive AFDC increases with the generosity of welfare payments, making it more feasible to remain on the welfare rolls while working.

Welfare payment generosity could either reduce or increase the impact of a welfare-to-work intervention on the amount of AFDC payments. If individuals are more likely to remain on the welfare rolls when the system is more generous, this will, of course, also increase the amount of AFDC that is paid out. Once individuals leave the AFDC rolls, however, the reductions in transfer payments will be greater.

5.4 Selecting Explanatory Variables for the Regression Analysis

Because the number of impact estimates that are available in each quarter that we analyze is limited, especially in the 11th and 15th quarters, multicollinearity was a serious potential problem in conducting the regression analysis. Thus, it was necessary to restrict the explanatory variables to a subset of those appearing in Table 3.

We used the following strategy to do this. First, with one minor exception (discussed below), we use the same regression model throughout, rather than "tailoring" the set of explanatory variables to each impact measure. Relying in part on the hypotheses discussed above, the variables were mainly selected for conceptual reasons. However, the number of missing values was also considered. It was also necessary to drop variables that were highly correlated with other variables that were in the model in order to minimize multicollinearity.

Second, and most important, because policy makers have control over the design of welfare-to-work programs, but little control over most contextual factors at the program sites, and thus would presumably be more interested in how the former affects program impacts than the latter, we attempt to capture the characteristics of the intervention being evaluated as completely as possible. With this in mind, the regressions include all but three of the intervention characteristic variables listed in Table 3. We did not include the net cost of operating the evaluated welfare-to-work programs because it is missing for about a third of our observations and it tends to be collinear with the participation measures (e.g., programs that substantially increase basic and vocational education tend to be more expensive). A preliminary examination suggested, however, that the net cost of welfare-to-work programs is virtually unrelated to their impacts. To conserve the number of explanatory variables, we also did not distinguish between pure incentive programs and those that provided both financial incentives and services and we did not include variables that measured the size of the financial incentive package. A preliminary investigation suggested that the pure financial incentive programs may be less effective than mixed programs, holding other factors constant, but this result was rarely statistically significant. The preliminary analysis also included a measure of the amount of the financial incentive that would be received by an AFDC recipient who found a minimum wage job that she or he kept for over a year. The coefficient on this measure was small and never approached statistical significance, indicating that the generosity of the financial incentive did not matter.

Third, because we use a large number of intervention characteristics variables and were concerned about the number of available observations and multicollinearity, the regressions include a minimal number of variables representing target population and site characteristics. While being parsimonious, however, we attempted to control for socioeconomic conditions as best as possible so we could isolate the true effects of program characteristics on program impacts.

Specifically, we included the following three socioeconomic contextual variables in all the regressions: the average age of the target population, the percentage of the target population employed in the year prior to random assignment, and the annual percentage change in manufacturing employment. In addition, the regressions on impacts on earnings and employment include the poverty rate, and the regressions on impacts on AFDC payments and the receipt of AFDC include the maximum AFDC payment available to a family of three. The first variable is most likely to capture the state of the labor market at the evaluation sites and the second the generosity of the welfare system.

Hypotheses concerning all the included variables were discussed earlier and need not be repeated here. It is important to point out, however, that the included socioeconomic variables proxy many of the variables that were left out. For example, the hypotheses that we developed concerning the annual percentage change in manufacturing employment and the unemployment rate are similar, as are those pertaining to the poverty rate and median household income. Moreover, the poverty rate also captures the racial composition and the educational level of the target population to a considerable degree. For example, using the same set of observations as those on which Table 3 is based, the simple correlation between the poverty rate and the percentage of the target population that is white is -.68 and the simple correlation between the poverty rate and the percent of the target population with a high school degree or equivalent is -.59. Similarly, being employed in the year prior to random assignment is highly correlated with length of time on AFDC (-.66), the percentage of the target population that is white (.52), and the percentage of the target population with a high school degree or equivalent (.52). In addition, the average age of the target population is highly correlated with number of children in the families in the target population (.78) and the percentage of families having a child under six (-.66). Most of the sites that had "tough" sanctions were also testing time limits (.64).[10] Thus, only one of these variables could be used in the regressions, and we chose the latter.

When explanatory variables were highly correlated with one another, we usually selected the one with the fewest missing values for inclusion in the estimated regression equations. If we had selected the variables we ended up excluding in their place, some of the conclusions drawn from the regressions would have differed. For example, instead of emphasizing the effects of time limits on program impacts, we would have discussed how impacts were influenced by strong sanctions.

5.5 Omitted and Misspecified Variables

Like virtually all nonexperimental empirical work that estimates relationships, this study is potentially subject to biases resulting from omitted variables. Such biases result if an omitted factor is correlated with both the dependent variable and at least one of the included explanatory variables. Ideally, therefore, the hypotheses discussed above should be tested holding everything constant that may affect both the dependent variables and the explanatory variables. As noted in the previous section, one limitation in doing this is multicollinearity. However, controlling for all potential influences is also both impossible because information is not available about all the factors that may be germane, and impractical because there are such a wide variety of possibilities. For example, the data needed to measure staff morale, cooperation among organizational units, employer attitudes towards welfare recipients, and the

[10]We also examined a dummy variable that equaled one if an intervention imposed more rigorous sanctions on the program group than on the control group and zero otherwise. Just over 20 percent of the evaluated interventions did so. This variable was even more highly correlated with the testing of time limits than the dummy variable discussed in the text.

quality of leadership at welfare-to-work program sites do not exist. A number of the more recent welfare-to-work experiments (e.g., Minnesota's MFIP, Delaware's ABC program, Iowa's FIP, and Virginia's VIEW) tested a wide variety of provisions, including changes in rules affecting assets, rules pertaining to exemptions from participating in welfare-to-work requirements, sanctions for not complying with nonwork requirements (such as cooperating with child support and ensuring that children receive immunizations and attend school), family caps that did not allow family benefits to increase with the birth of an additional child, and requirements for minors with children of their own. Whether these provisions are correlated with *both* the program impacts *and* the explanatory variables that we focus on in this study is unclear. However, even if they are, it is difficult to construct measures of some of them because of their complexity (e.g., requirements for minor parents), while others are specific to only one or two of the experiments in our database (e.g., family caps).

Biases may be caused by misspecified explanatory variables, as well as omitted variables. To the extent variables are misspecified or measured with error, their coefficient estimates are generally biased towards zero.

One example of explanatory variables that may be misspecified to some degree are the measures of participation in program activities that are commonly available in reports on the evaluations of welfare-to-work programs and that are used in this study. To illustrate, welfare recipients are usually counted as participants in a particular program activity such as job search or basic education if they take part in these activities for as little as one day. Intensity of participation is not measured. Nor is the order in which services are provided. For instances, so-called "work-first" programs require participants seek jobs first and provide them with education and training only if they fail to find employment, while programs that emphasize human capital development provide education and training first and job search afterward. However, work-first programs usually have a greater measured impact on participation in job search than those that emphasize human capital development, while the latter have a greater impact on participation in basic education and vocational training. Such considerations are not captured by the measures of program participation produced by most welfare-to-work evaluations.

There are other difficulties in appropriately specifying various explanatory variables. For example, available estimates of site environmental variables often do not correspond to the exact geographic area in which program target populations live and work, although we attempted to make as close a match as possible. Although we have information about the percentage of program target populations that were employed prior to random assignment, it might be more useful to know the average number of months of prior employment, but the required data are not available. More generally, we are limited to aggregate information about the characteristics of members of program and control groups. It would be better to have information at the individual level—for example, the number of weeks each individual in the evaluation sample population worked prior to random assignment.[11] It is especially difficult to construct a variable, or even a set of variables, that adequately captures the use of sanctioning by welfare-to-work programs because sanctioning is so multidimensional, including for example how easy it is to be reinstated after being sanctioned, the extent to which families who are sanctioned are followed up, the services available to those who are sanctioned, and the number of times individuals are sanctioned. As previously discussed, we did construct measures of several dimensions of sanctioning for use in this study, but because of multicollinearity we were restricted to a simple measure of program impact on the percentage of program group households that were sanctioned.

6. BASIC REGRESSION FINDINGS

The regressions that examine the hypotheses discussed above are reported in Tables 4 through 7. The regressions for each of the four impact measure are contained in a separate table. Each table reports

[11]As previously mentioned, Bloom, Hill, and Riccio (2003) were able to utilize such data in conducting a hierarchical linear analysis of a subset of welfare-to-work evaluations.

regressions for the 3rd, 7th, 11th, and 15th calendar quarters. Although it is useful and frequently interesting to compare the regressions across the impact measures and calendar quarters, and we do so below, it is important to keep in mind that sample composition varies among the impact measures and quarters and this may account for some of the differences in findings.

The regression for each impact and quarter was computed twice, once with only the nine intervention characteristic measures and once with the four socioeconomic contextual measures also included. The F-test for the first of these computations indicates whether the coefficients on the nine intervention characteristic variables are jointly statistically significant, while the F-test for the second set of computations indicates whether the coefficients on the four socioeconomic contextual characteristic variables are jointly significant when these variables are added to the original nine. The F-tests indicate that, with one exception,[12] the first set of coefficients are always jointly highly significant at conventional levels, but that the second set of coefficients frequently are not jointly significant, even when some of the individual coefficients are significant. This suggests that differences in design among programs contribute importantly to why some interventions are more effective or less effective than others, but that once program characteristics are taken into account, differences among target populations and site characteristics often do not play an important role. However, there are important exceptions to the latter conclusion, as suggested by the fact that the F-test for the coefficients on the contextual variables is sometimes statistically significant and, even when it is not, some of the individual coefficients are significant.

Because two regressions are reported for each of four different calendar quarters and four different impact measures, the findings in Tables 4–7 are complex to interpret. In presenting the results, we treat all four impacts as positive values, including impacts on AFDC receipt and payments, although,

[12]The exception occurs for the regressions on the 15th quarter earnings impact. No individual coefficient is statistically significant in either of these regressions.

unlike the earnings and the employment impacts, they record reductions rather than increases in participation and financial receipts. The reporting of all four impacts as positive values results in their meta-regression coefficients having the same sign if an explanatory variable has similar effects on different impact measures. This should aid in the interpretation of the regression results.

In the discussion of the regression results, we first consider each explanatory variable separately. This is followed by a brief overall summary of key findings.

6.1 Sanctions

Increasing the sanction rate appears to have a positive effect on program impacts, especially those on whether AFDC is received and on the amount of AFDC received. Although some of the coefficients on sanctioning are negative, these negative coefficients are never statistically significant, while the positive coefficients are often significant. The coefficients on sanctions are usually largest in the 7th calendar quarter, when they are always positive and highly statistically significant at conventional levels. After that the importance of sanctioning appears to fade, suggesting that sanctions help get welfare recipients off the welfare rolls and into jobs initially, but do not necessarily keep them there over the longer term.

6.2 Job Search

Tables 4–7 indicate that increasing the use of job search has a positive effect on the impact of welfare-to-work programs, regardless of how the impact is measured. The coefficients on the job search variable are almost always positive and are usually statistically significant. Moreover, the contribution of job search does not seem to diminish with time since random assignment.

6.3 Basic Education

Tables 4 and 5 imply that increasing participation in basic education does not improve labor market outcomes. The coefficients on the basic education variable are sometimes positive and sometimes negative in Tables 4 and 5, but they are small relative to the coefficients on job search and statistically significant in only one regression. Although Table 6 suggests that increasing participation in basic education may decrease the amount of AFDC payments the welfare-to-work program group receives, Table 7 implies that this does not reduce the welfare rolls, except possibly in the 15th quarter. It is difficult to reconcile the divergent implications of Tables 6 and 7; but, in general, there appears to be little evidence in support of making basic education a major component of welfare-to-work programs.

6.4 Vocational Education

The coefficients on vocational education are statistically insignificant at conventional levels more often than not. Moreover, they are typically negative in sign, even when significant. The major exceptions occur in the regressions on impacts on AFDC payments during the 7th and 11th quarters. There, the coefficients on vocational training are both positive and highly significant, but only in the regressions that contain the socioeconomic contextual variables. These coefficients are not very robust, however. For example, if the variable for maximum AFDC payments is dropped from the regression specification, they become nonsignificant and shrink greatly in size, with the 11th quarter coefficient becoming negative. We conclude that increasing participation in vocational training does not exert a positive influence on the impacts of welfare-to-work programs and may even have a negative effect.

6.5 Work Experience

The coefficients on the work experience variable are seldom statistically significant and, when they are significant, they are usually only marginally so. The major exceptions occur for the 7th quarter impacts in the regressions on the receipt of AFDC and the amount of AFDC received, when the coefficients are negative. Overall, the evidence does not seem to indicate that program impacts improve very much, if at all, with an increase in participation in work experience.

6.6 Financial Incentives

Tables 6 and 7 indicate that including financial incentives in a welfare-to-work program exerts a negative influence on its impacts on the receipt of AFDC and on the amount of AFDC received. These negative coefficients are highly statistically significant through the 11th quarter and are often large relative to the mean impacts (see Table 2). However, they appear to diminish over time as individuals leave the welfare rolls and no longer qualify for financial incentive payments. This finding is unsurprising as financial incentives typically operate by increasing the earnings disregarded in computing AFDC benefits. What is surprising is that the financial incentive coefficients are usually negative in sign and occasionally statistically significant in the labor market regressions reported in Tables 4 and 5. As noted earlier, this may be the result of earnings disregards reducing the work effort of some employed AFDC recipients who decide to work fewer hours while maintaining their overall income. The objective of financial incentives is, of course, to encourage employment and thereby increase earnings, but they do not appear to do so.

6.7 Time Limits

The coefficients on the dummy variable for time limits are almost always positive in sign and they are often statistically significant. Moreover, they appear to grow through the 11th quarter (about three years after random assignment) as AFDC recipients approach the time limits or even reach them. Thus, either through threat or direct implementation, time limits seem to increase the impacts of mandatory welfare-to-work interventions. However, as indicated by Table 4, they do not seem to have much effect on earnings, except possibly in the 11th quarter.

6.8 Number of Years since 1982

The purpose of this variable is to determine whether policy makers have learned from past experiences so that newer programs are more successful than older ones. Unfortunately, a clear conclusion cannot be drawn. The coefficients on the years since 1982 variable are always positive in the regressions on labor market impacts (see Tables 4 and 5), but they are rarely statistically significant. They are more often statistically significant in the regressions on the AFDC impacts (see Tables 6 and 7), but are about as likely to be negative as positive.[13]

6.9 One-Parent Families versus Two-Parent Families

All evaluations of welfare-to-work programs assess their effects on one-parent families as this group constitutes over 90 percent of all families who received AFDC. Some also include two-parent families in the evaluation, and when they do, one- and two-parent families are usually evaluated separately. Thus, a dummy variable was included in the regression specification to distinguish between impacts estimated for the two types of families, with impacts for two-parent families assigned a value of one and impacts for one-parent families assigned a value of zero. The findings are difficult to interpret because the coefficients on this variable are positive about as often as they are negative, and a subset of both the positive and negative coefficients are statistically significant.

6.10 Average Age of the Target Group

Young welfare recipients, many of whom are teenagers or in their early twenties, may face greater disadvantages in the labor market than older recipients both because of their age and because their children tend to be younger. Thus, caseload age may be positively associated with impacts of welfare-to-

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[13]To examine whether the findings were affected by the implementation of TANF, which began in 1997, we created a dummy variable that equaled one if an impact was estimated in 1997 or thereafter for most of the evaluation sample and zero if it was not. This variable was used as an additional explanatory variable in the regression both with and without the years since 1982 variable, with which it is highly collinear, also included. (The regressions that included the variable are not reported.) Regardless of whether the years since 1982 measure is included in the regression, the variable was positive and statistically significant in the 15th quarter in the earnings and percentage employed regressions and negative and statistically significant in the 11th quarter percentage participating in AFDC regression. Otherwise, it was never statistically significant. However, even when it was significant, it had little effect on the other regression coefficients. We conclude from this exercise that our results are quite robust to the introduction of TANF.

work interventions. It could instead be negatively related, however, if the programs help younger recipients overcome barriers that would otherwise exist, but older recipients can find jobs on their own. In addition, older recipients tend to have more children.

As it turns out, the coefficients on the average age of the target group are positive much more often than they are negative. Moreover, none of the negative coefficients are statistically significant, while a few of the positive coefficients are significant. Thus, there is weak evidence that program impacts are larger when the average age of the target group is greater.

6.11 Percentage of the Target Group Employed in the Year Prior to Random Assignment

AFDC recipients with recent employment experience, most of whom have been on the welfare rolls for relatively short periods of time, are much more likely to find employment readily and leave the welfare rolls than their counterparts who have little or no work experience and are long-term recipients. Moreover, as pointed out earlier, such individuals also tend to have more education and are more likely to be white, which may also make it easier for them to obtain a job.

However, if these more job-ready recipients can find employment on their own, without the aid of welfare-to-work programs, but less job-ready recipients need the help of such programs, then the relation between our measure of recent employment and program impacts may be negative. As it turns out, except for the 3rd calendar quarter, the coefficients on the recent employment variable are always negative. However, most of these negative coefficients are not statistically significant. Thus, there is only weak evidence that welfare-to-work programs do more to aid recipients without recent employment experience than recipients who can more readily obtain employment on their own.

6.12 Annual Percentage Change in Local Manufacturing Employment

This variable reflects the state of the labor markets at the intervention sites at the time the evaluations were conducted by indicating whether manufacturing jobs were being added or lost. The coefficients on the variable are usually positive. Furthermore, they are often statistically significant, and they are only significant when they are also positive. This suggests that welfare-to-work programs work best when there are job openings.[14] The fact that the coefficients in the earnings and employment regressions are only positive and statistically significant in the 3rd and 7th calendar quarters suggests that the availability of jobs is most important during the first couple of years after AFDC recipients are assigned to welfare-to-work programs.

6.13 Poverty Rate

The poverty rate is only included in the earnings and employment regressions and was intended to capture both the availability of jobs at the places and times the evaluated interventions operated and the quality of those jobs that were available. As noted earlier, a high poverty rate is also indicative of target populations that are less likely to have a high school degree and more likely to be nonwhite. With only one exception, the coefficients on this variable are negative, suggesting that earnings and employment are lower when there are fewer jobs available,[15] those that are available are unattractive, and program target populations are more disadvantaged. However, the coefficients are statistically significant only in the 7th quarter.

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[14]As previously mentioned, it can be argued that when job availability is very low, there is little that a welfare-to-work program can accomplish; but when jobs are in abundance, welfare recipients can readily obtain employment without the aid of a program. This argument implies that program impacts will be greatest when conditions are between these two extremes. In unreported regressions, we tested this hypothesis by adding the square of the annual percentage change in manufacturing employment to the regressions presented in Tables 4 and 5 that include the linear value of the change in manufacturing employment. Except for the employment regression in the 7th quarter, when the coefficients on both terms of the quadratic were highly statistically significant, there was little support for the hypothesis. The overall fit of the remaining seven regressions was reduced (i.e., the F-value fell and the adjusted R-squared often was reduced), and the coefficient on the squared poverty rate term in these regressions never approached statistical significance.

[15]We also estimated regressions for earnings and employment that included the quadratic of the poverty rate. Neither the coefficient on the linear poverty rate term nor the coefficient on the squared poverty rate term ever approached statistical significance.

6.14 Maximum AFDC Payments

We suggested earlier that the generosity of AFDC (as represented by the maximum payment for which a single mother with two children and no earnings is eligible) could be either positively or negatively related to program impacts on AFDC payment amounts. Table 6 indicates that this relationship is positive and, except for the 15th quarter, highly statistically significant. However, we expected AFDC generosity to be negatively associated with program impacts on the size of the welfare rolls, both because program break-even levels are higher under more generous programs and because welfare recipients may be more hesitant to leave the rolls when benefits are more generous. Table 7 suggests that this hypothesis finds weak support in the 3rd and 15th quarters,when the coefficients on maximum AFDC payments are negative but insignificant, but not in the 7th and 11th quarters,when the coefficients are both positive and statistically significant.

6.15 Summary of Key Findings

Conclusions based on the regressions results discussed above are summarized below. We examine the robustness of these conclusions in Section 7; until then, they should be considered tentative.

- Three program features appear to be positively related to the effectiveness of welfare-to-work interventions: increased participation in job search, the use of time limits, and the use of sanctions. The latter relationship is only important in the first couple of years after entry into a program.
- Financial incentives decrease impacts on whether AFDC is received and on the amount of AFDC that is received, but do not improve impacts on labor market outcomes.
- The evidence is somewhat mixed on hether increases in participation in basic education, vocational education, and work experience increase program effectiveness. In general, the findings do not support putting additional resources into these activities.
- It is not clear whether the effectiveness of welfare-to-work programs has improved over time.
- Welfare-to-work programs appear to do better in strong labor markets than in weak ones, especially in the first year or two after individuals enter these programs.
- Impacts on the size of AFDC payments and (possibly) the receipt of AFDC are larger in locations where AFDC systems are more generous.

- Based on the regression results described above, it is not clear whether the welfare-to-work interventions tend to be more effective or less effective in serving relatively more disadvantaged caseloads than more advantaged caseloads. The typically negative relation between program impacts and recent job experience suggests that they are more effective in serving a relatively disadvantaged caseload, but the generally positive relation between impacts and caseload age and the negative association between impacts and the poverty rate implies the opposite. However, none of these relationships is very often statistically significant. In Section 12, we directly compare subgroups of relatively disadvantaged AFDC recipients with subgroups that are relatively less disadvantaged and find some evidence that welfare-to-work program impacts tend to be larger for the former than for the latter, although this evidence is still not entirely conclusive.

7. SENSITIVITY ANALYSES

It is important to determine whether the list of key findings that appears above is robust. In this section, we examine whether these findings change when the assumptions that underlie the regressions reported in the previous section are modified. Specifically, we tested the sensitivity of the findings to each of the following modifications:

1. Running unweighted regressions. This is especially important for regressions on impacts that are measured as continuous variables (i.e., earnings and the amount of AFDC payments) because, as explained earlier, some strong assumptions were needed to derive the weights used in computing the weighted regressions for these impacts.

2. Excluding the observations with the two highest and two lowest impact estimates from those used to run the regressions. This sensitivity test indicates whether the findings were influenced by extreme values.

3. Dropping observations with missing values for any of the explanatory variables from the regressions. In the regressions reported in the previous section, we instead used the sample mean of a variable when it was missing for an observation.[16]

4. When the same control group was used in estimating the impacts of two different interventions at the same site, each impact estimate was given only half the weight they were given in computing

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[16]There are a number of additional ways in which missing values might be treated in the regression analysis, although the two methods we examine would be expected to produce findings that differ the most from one another. For example, the missing values might have been imputed. We did not do so because the computational requirements are quite time consuming. Yet another approach is to treat missing values on explanatory variables as "list-wise." In other words, the correlation between two variables is made up by each available pair-wise observation between the two variables. In earlier work (Ashworth et al., 2004), we used this method and found that our findings differed little from those that used the mean when a value was missing.

the previously reported regressions. The purpose of this exercise was to see if the findings are sensitive to possible dependency between the two impact estimates.

To minimize the number of regressions involved, we limited these sensitivity tests to the 7^{th} quarter after random assignment and to regressions that included the full list of explanatory variables. The 7^{th} quarter allows us to maximize sample size, while also examining a period well after most individuals who were assigned to the evaluated programs are no longer receiving program services.

The results from the sensitivity tests for each impact estimate appear in Tables 8 through 11. For purposes of comparison, the first column repeats the 7^{th} quarter regressions that appear in Tables 4–7. The remaining four columns contain regressions based on each of the four modifications described above.

With a few exceptions, which are discussed in the following paragraph, the key findings from the earlier regressions appear quite robust, although the magnitudes of some of the individual coefficients on the different variables sometimes change substantially. The adjusted R-squared for the unweighted regressions are much smaller than those for the weighted regressions and the coefficients are considerably less likely to be statistically significant. However, the coefficients are generally of the same sign and even of the same order of magnitude as the original estimates, especially when the latter are statistically significant. The coefficients in the fourth column, in which observations containing missing information are dropped, vary the most from the original estimates. These differences probably occur both because the observations used for the two computations differ considerably from one another and because the smaller sample size on which the estimates in the fourth column are based causes them to be less precise. Thus, there are fewer significant coefficients in the fourth column than in the first. Nonetheless, most of our earlier conclusions are still supported.

Turning now to our individual conclusions, Table 10 provides some indication that increases in participation in basic education may increase program impacts on the amount of AFDC that is paid out, which is contrary to the third bullet point that appears at the end of the previous section. However, the sensitivity tests do not suggest that the three remaining impacts increase as basic education increases. Furthermore, the sensitivity analyses provide additional support for our earlier contention that the significant positive relation between increases in participation in vocational education and impacts on AFDC payments found in the first column is not robust. The fourth columns of Table 9 and 11 exhibit negative (but insignificant) relations between program impacts and the annual percentage change in manufacturing employment, thereby weakening our conclusion that welfare-to-work programs are most effective when labor market conditions are good. However, the relationship continues to be positive in the fourth columns of Tables 8 and 10, supporting the conclusion. Moreover, the other sensitivity tests are also consistent with the conclusion. Perhaps the strongest case for changing one of the original conclusions in light of the sensitivity analysis concerns the relations between AFDC generosity and program impacts on the receipt of AFDC and the size of AFDC payments. Our original estimates indicate that these relationships are positive, while three of the sensitivity tests suggest that they are negative. For previously discussed reasons, a negative relationship between AFDC generosity and program impacts on the receipt of AFDC seems more plausible than a negative one.

8. THE PREDICTIVE ABILITY OF THE REGRESSIONS

The regressions that were reported earlier can be assessed in terms of their predictive ability. We do this in two ways:

First, we reestimate the 7th quarter regressions reported in Tables 4–7, leaving out a randomly selected observation. We then see how successfully the regressions can predict the impact of the omitted observation. This test is repeated for ten randomly selected observations for each of the four impact measures. The selection of the ten observations is conducted by independently generating random numbers for each of the four impact measures. Thus, 40 separate predictive regressions were estimated in all.

Second, we reestimate the 7th quarter regressions reported in Tables 4–7, leaving out the five observations that resulted from the three most recently evaluated mandatory welfare-to-work programs in our database.[17] We then see how well the impacts of these newer programs are predicted on the basis of findings from the older welfare-to-work programs. This is important because it indicates how reliably evaluations of existing programs can predict the impacts of future programs.

In assessing both sets of predictions, it is important to recognize that their accuracy can be determined only by comparing them to *estimates* of program impacts; they cannot be compared to the "true" impact of a welfare-to-work intervention. As previously discussed, because each estimated impact is subject to sampling error, it is unlikely to measure the "true" effect exactly. Indeed, unless the standard error of the estimated impact is small, it could diverge considerably from the "true" value. Consequently, it is quite possible that a predicted impact and an estimated impact differ, but the former is actually closer to the "true" impact than the latter. There is no way to know for sure.

Table 12 compares 7th quarter estimated impacts with 7th quarter predicted impacts. The first panel presents the comparison for the ten randomly selected observations and the second panel reports the comparison for the five observations drawn from the three most recent evaluations. The ten observations in the first panel differ for each of the four impact measures because they were selected by four independent random draws, but the five observations in the second panel remain constant across the impact measures.

It is evident that many of the individual predicted impacts in Table 12 vary considerably from their estimated counterparts. Indeed, sometimes they even have a different sign. Moreover, for the ten randomly selected interventions, the average of the absolute value of the difference, which is shown in the bottom row of the panel for each impact measure, is larger than the average estimated impact for all four impact measures. The predictive performance of the regressions is considerably better in the case of the

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[17]Specifically, these evaluations consist of the Connecticut Jobs First Program, which was evaluated at two sites (New Haven and Hartford), the Los Angeles Jobs First GAIN Program, in which separate evaluations were conducted for one-parent and two-parent families, and the second random assignment evaluation of the Indiana Welfare Reform Program.

five most recent interventions in terms of this comparison, however. This suggests that the regressions can provide useful information about how well future welfare-to-work programs are likely to function, even though a prediction for a specific individual program is likely to be subject to considerable error.

The divergence between the estimated and predicted impacts occurs for two reasons. First, as already discussed, the estimated impacts are subject to sampling error. Second, the regressions on which the predictions are based undoubtedly fail to capture some of the systematic factors that cause the "true" impacts of welfare-to-work programs to vary. This is really an omitted variables problem. Unfortunately, it is not possible to determine the relative importance of these two sources of divergence. However, by averaging across the observations, both sources of divergence tend to wash out to some extent. Thus, except for the pair of figures appearing in the bottom right-hand corner of Table 12, the averages for the estimated and predicted impacts that are presented at the bottom of each panel are quite similar to one another. It is also interesting to note that the predicted impacts suggest that the five most recent interventions should have substantially larger impacts, on average, than the ten randomly selected interventions (with the exception of the impact on the receipt of AFDC), and the estimated impacts indicate that this is indeed the case.

Assuming, as seems likely, that the omitted variables are an important source of the differences between the estimated and predicted impacts, the results in Table 12 suggest that although it is possible to predict impacts for groups of welfare-to-work programs with reasonable accuracy, predictions of the impacts of individual interventions will often be subject to considerable error. Yet the fact that a number of the coefficients on which the predictions are based are statistically significant implies that it is possible to say something useful about a "typical" (i.e., average) welfare-to-work program, information that is of considerable value at the national policy level. However, because it is likely that most welfare-to-work programs differ from a "typical" program in various ways that are difficult to measure, few individual programs are probably sufficiently "typical" that their impacts can be predicted with precision.

Nonetheless, the regression findings are still of use for predictive purposes. For example, local welfare officials might want to know if it is likely that a proposed welfare-to-work program will do better than already existing programs. Alternatively, they may want an idea of whether it is likely that their current welfare-to-work program is performing better than a "typical" welfare-to-work program, but not have to conduct a full evaluation to find out. The regressions can potentially provide useful information about such issues, providing the values pertaining to the local program are available for each of the explanatory variables used in the regressions. For example, if local administrators who already had a welfare-to-work program planned to increase the participation in job search by (say) 10 percentage points, decrease participation in basic education by (say) 5 percentage points, and introduce financial incentives and leave everything unchanged, they could then use the regressions to predict the combined impact of these changes.

To illustrate, the weighted average for 7th quarter earnings impacts in Table 2 is \$99, but the predicted earnings for Case 1 in Table 12 are less than this amount. This implies that this intervention performed worst than a typical program. The fact that the estimated impact for Case 1 is also below the average impact indicates that this conclusion is probably correct. The predicted impact for Case 2, on the other hand, is well above \$99, suggesting that this program performs better than a typical welfare-to-work program, a conclusion that is again verified by comparing the estimated impact with the impact for a typical welfare-to-work program. Conclusions based on the regressions about whether individual programs are performing better or worse than typical programs are not confirmed every time, of course—for example, they are not confirmed for earnings impacts for Cases 7 and 8—but as the following tabulation demonstrates, in most cases they are.

Percentage of Confirmed Predictions of Whether Impacts Are Larger or Smaller than the Impacts of a "Typical" Program

	Randomly Selected Interventions	Most Recent Intervention
Earnings	80%	40%
Percentage Employed	80	80
Average AFDC Payments	60	60
Percentage Receiving AFDC payments	60	40

An important reason why a specific welfare-to-work program may perform better or worse than a "typical" program is because potentially important factors (for example, leadership and staff morale) could not be measured and included in the regressions. Thus, using the regressions to compare a particular program with a "typical" program can only be viewed as suggestive. However, by comparing the explanatory variable values for a local program with those for a "typical" program (see Table 3), an administrator can obtain pertinent information about how the former differs from the latter. Such a comparison may suggest, for example, that participation in job search might be usefully increased, while participation in basic education is decreased.

9. PROGRAM IMPACTS OVER TIME

In this section, we look at how long program impacts last. Most of the studies listed in Table 1 measured program impacts for several calendar quarters after random assignment took place. This allowed us to investigate how impacts might change over time across a range of program evaluations. A variable was constructed for this purpose that equals one for program impact estimates that pertain to the first post-random-assignment quarter, two for estimates that pertain to the second post-random-assignment quarter, and so forth. We refer to this variable as the "quarters since random assignment" variable.

The findings described earlier suggest that job search and sanctions make key contributions to the effectiveness of welfare-to-work programs. It seems possible that such programs may give the experimental group a competitive advantage in the labor market at first, but that this advantage will diminish over time as the control group catches up. Thus, we expected that while the relation between the quarters since random assignment variable and the program impact measures might initially be positive, it would eventually turn negative.[18]

To investigate this possibility, we computed regressions with two time variables: the quarters since random assignment variable in its original form (number of quarters) and the square of the quarters since random assignment. We expected the coefficients on the first variable to be positive and the coefficient on the second to be negative.

The coefficients are reported in Table 13. All the quarters of impact estimates that were provided by each mandatory evaluation in our database were included in computing the regressions. The regressions were estimated both with and without the inclusion of other explanatory variables. The purpose of including the other explanatory variables in the regressions is to determine whether the relation between impacts and quarters since random assignment changes after controlling for other factors. The variables we use to control for other factors are identical to those used in the regressions reported in Tables 4 through 7.

Both time variables are statistically significant at the 1-percent level in all eight regressions, regardless of whether control variables are included in the regression or not. However, unsurprisingly, the inclusion of the control variables considerably increases the explanatory power of the regression model, as indicated by the increased value of the adjusted R-squared. This increase is particularly apparent with respect to the amount of AFDC payment, where the adjusted R-squared increases from .048 to .484 as a

[18]However, an argument is frequently made that while basic education and vocational education are costly in the short term, they payoff in the longer term by equipping welfare recipients with additional human capital resources. If true, this would mean that the impacts of welfare-to-work programs might continue to increase over time. However, as discussed earlier, we found little evidence that basic education and vocational education are, in fact, effective in the longer term.

result of inclusion of the control variables. Nevertheless, this has no effect on the statistical significance of the two time variables, although the size of the coefficients on both variables increases a bit.

The linear variable for the number of quarters is positively associated with all four impacts, indicating that, indeed, program impacts increase over time. However, the coefficient on the squared time variable is negative. In other words, while it is correct to say that program impacts increase with time, this is only true initially as, after some point, they begin to shrink.

As indicated by the penultimate row in Table 13, the coefficients in the regressions including control variables imply that the impacts of a typical welfare-to-work program begins to diminish between the eighth and fourteenth quarter, depending on the impact indicator. After about two years (8.4 quarters), the average program's employment impact peaks and begins to decline, followed about a year later by the amount of AFDC (10.7 quarters) and earnings (11.9 quarters) impacts. The decline in the impact on the percentage receiving AFDC is the last to set in, doing so after approximately three and a half years (13.6 quarters).

The regressions that exclude the control variables imply that the impact peaks occur somewhat later, although the difference between the two types of regression is less than one calendar quarter with respect to earnings and the percentage employed. However, this rises to three and four quarters, respectively, for the AFDC payment and percentage receipt impacts.

The regressions also imply that the impacts of a typical welfare-to-work program disappear five to seven years after random assignment.[19] Again, except for the percentage employed, the regressions that exclude the control variables predict that this point occurs somewhat later. Some caution is required in interpreting these findings because they are based on extrapolation somewhat beyond the sample range of five years. In addition, as previously discussed, welfare-to-work interventions with large positive impacts

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[19]Taken literally, the regressions predict that after five to seven years, impacts for a typical welfare-to-work program would become negative, which is implausible. This period is beyond the five-year range of the data, however. Moreover, the functional form of the regressions imposes this result.

tend to be evaluated for a longer period of time than less successful interventions. This could cause our estimates of how long program impacts last to be exaggerated. An investigation of this possibility in earlier research, which was limited to earnings impacts (Greenberg et al., 2004), suggests that the exaggeration is only around half of one year.

In sum, the regressions show that a typical welfare-to-work program has a positive effect on all four program performance measures for five to seven years after random assignment, although the impacts begin to decline after two or three years.

10. ANALYSIS OF OUTLIERS

As previously documented, welfare-to-work interventions vary in their effectiveness. In this section, we look more closely at interventions that stand out because they achieve especially high or especially low impacts. We refer to these interventions as “outlier interventions” and the atypically large or small impacts they produce as “outliers.” Somewhat arbitrarily, we define impacts that are outliers as those that are at least one standard deviation higher or lower than the mean impact of the interventions in our database. We identify outliers for all four impact measures. As in previous analyses, we focus on four selected quarters after random assignment: quarters 3, 7, 11, and 15.

Using our definition of outliers, impacts that are at least one standard deviation above or below the mean of all the impact estimates available in a given quarter, we identify outliers in two ways. First, we simply compare each impact estimate in a given calendar quarter to the weighted mean of all the impact estimates available for that quarter. We refer to these outliers as Type A outliers. Second, we identify outliers by regressing the impacts on the same independent, explanatory variables that we used previously. In other words, we use the weighted regressions that appear in Tables 4–7 and contain 13 explanatory variables to control for factors that affect the effectiveness of welfare-to-work programs. We refer to these outliers as Type B outliers.

10.1 The Prevalence of Outlier Programs and Outlier Estimates

Table 14 summarizes details about the prevalence of outliers among the quarterly estimates of the four impact measures. Across the four quarters, the evaluations of welfare-to-work programs included in our database recorded between 232 (percentage receiving AFDC) and 259 (earnings) quarterly impact estimates. Of these, between 71 (earnings) and 125 (percentage employed) are designated as outliers. In relative terms, between about a quarter (earnings) and a half (percentage employed and percentage receiving AFDC) of the impact estimates are classified as outliers.

Between one-third (earnings) and two-fifths (AFDC payment and percentage employed) of the outliers are only Type A outliers. About half as many impact estimates are designated as only Type B outliers—that is, they become outliers only after controlling for explanatory factors. The remaining outliers are of both types. Thus, Type B outliers (those listed in the last two columns of Table 14) account for around 60 percent of all the impact estimates designated as outliers.

These figures demonstrate that controlling for independent influences on program impacts substantially reduces the number designated as outliers. Nonetheless, there are considerable numbers of Type B outliers. There are two reasons for this. First, because of limitations on the availability of the data, as discussed in considerable detail earlier, we could not control for all the factors that may cause impact estimates to vary systematically from one another. Second, as also previously discussed, sampling error causes impact estimates to vary across evaluated programs, but regression analysis does not control for this. Thus, the "true" impacts of some interventions may not deviate very much from the mean for our sample of interventions, even when their estimated impacts differ considerably.

In Table 15 we show the number of interventions for which outliers for a given type of impact occur in more than one quarter. We refer to such interventions as "multiple outlier interventions." Because the statistics in Table 15 are based on four quarters of impact estimates, however, and not all evaluations estimated impacts for all four quarters, not all interventions have the same chance of having multiple outliers. Nevertheless, in focusing on interventions with multiple outliers, we aim to reduce the risk of highlighting exceptionally high or low impacts that perhaps reflect isolated instances instead of interventions that consistently under- or overperformed. Moreover, by considering multiple outlier interventions, we are able to identify programs that over- or underperform two or more years after random assignment, thereby highlighting those that may have required time to "settle in" before showing an impact. The latter might be the case for programs that focused on developing human capital instead of emphasizing immediate job placement.

Over the four quarters of data, earnings impacts are recorded for 99 interventions, employment percentage impacts for 87 interventions, AFDC payment impacts for 90, and AFDC percentage receipt impacts for 84 interventions. Only a minority of these interventions produced multiple outliers. Seventeen of the 99 interventions with earnings impacts contain multiple outliers that are either Type A outliers, Type B outliers, or both. The same is true for 42 of the 87 interventions with percentage employed impact estimates; 27 of the 90 interventions with AFDC payment impact estimates; and 32 of the 84 interventions with percentage receiving AFDC impact estimates. The fourth column in Table 15 indicates the number of outlier impacts accounted for by these interventions. A comparison of this column with the third column in Table 14 indicates that the interventions with multiple outliers account for most of the impact estimates designated as outliers.

About half of the interventions with multiple outliers produced two or more impact estimates classified as Type B outliers, although this rises to over 60 percent in the case of the impact measure for the percentage in receipt of AFDC (20 of 32). Thus, after controlling for external and program-specific influences, far fewer interventions can be classified as multiple outlier interventions. Moreover, interventions with multiple Type B earnings outliers account for just under half of all Type B earnings outliers (23 of 14+34; see Table 14) and between 60 and 70 percent of the outliers for the other three impact measures.

10.2 Separating Positive and Negative Outliers

So far, we have presented summary results for both positive and negative outliers. We now turn to identifying the individual interventions with multiple outliers, distinguishing between those with multiple positive outliers (Table 16) and those with multiple negative outliers (Table 17). These tables show the number of quarterly outliers for interventions with multiple outliers both before and after regression adjustments that control for factors affecting program impacts (Type A and Type B outliers, respectively). Single Type B outliers are reported in parentheses if multiple Type A outliers occur for the same impact measure, and vice versa.

Positive Outliers. Table 16 indicates that 22 interventions targeted at one-parent families and 10 interventions targeted at two-parent families have multiple positive outliers for one or more of the impact measures. Together, these 32 interventions account for 210 outliers. Interventions implemented under the GAIN program or assessed as part of the NEWWS evaluation are more likely to contain multiple outliers for several, rather than just one or two, impact measures than most other interventions.

Two-parent interventions account for only about one-fifth of the interventions in our overall sample (see Table 3) but comprise around one-third of the interventions listed in Table 16. This likely reflects the much smaller samples usually used in evaluating interventions targeted at two-parent families than used to evaluate interventions targeted at one-parent families. Smaller samples increase the statistical variance and, as a result, raise the likelihood of impacts being classified as outliers.

Most interventions that produce multiple positive Type A outliers also produce multiple positive Type B outliers, although, for a majority of interventions, there are fewer Type B outliers than Type A outliers. However, there are numerous exceptions. For example, one-parent interventions that were assessed as part of the California Work Pays Demonstration Program (CWPD) or the New York State Child Assistance Program (CAP) tend to be associated with more Type B outliers than Type A outliers. Riverside, GAIN and some of the interventions assessed as part of the NEWWS evaluation, in contrast, lose outliers as a result of regression adjustment. Most interventions that are targeted at two-parent families tend to retain the same number of multiple outliers before and after regression adjustment.

Negative Outliers. Eighteen one-parent family interventions and ten two-parent family interventions produced multiple negative outliers. These 28 interventions account for 164 outliers in total, a smaller number than in the case of positive outliers. Slightly fewer than half the interventions listed in Table 17 (13 of 28) provided financial incentives. In contrast, in the overall sample, about a third of the interventions offered these incentives (see Table 3).

Among the one-parent interventions, negative outliers less frequently stretch across more than two impact measures than do positive outliers. Moreover, although there are a few exceptions, the negative outliers tend to occur for either the AFDC impact measures or the labor market impact measures but not both. One-parent interventions with multiple negative outliers thus underperformed either in terms of increasing employment and earnings or in terms of reducing reliance on AFDC, but in stark contrast to interventions with multiple positive outliers, they rarely underperformed on both accounts. This pattern is absent in the case of interventions targeted at two-parent families.

Comparing Interventions with Multiple Positive and Negative Outliers. As indicated by Table 16, besides GAIN's Riverside intervention and the NEWWS evaluation's Portland program, which are already celebrated cases of high-impact one-parent family interventions, other especially notable high performers among interventions targeted at one-parent families include the New York State Child Assistance Program (CAP) and the NEWWS evaluation's Labor Force Attachment (LFA) interventions in Grand Rapids, Michigan, and Riverside, California. Among interventions targeted at two-parent families, repeated high-impact performers include the GAIN program in Butte County and the California Work Pays Demonstration sites in Alameda County and San Bernardino County.

By contrast, as indicated by Table 17, Minnesota's Family Investment Program (MFIP), Vermont's Welfare Restructuring Project (WRP), and GAIN's Tulare intervention underperformed for both one-parent and two-parent families. In addition, Baltimore's Employment Initiative generated negative outliers for two-parent families, but not for one-parent families.

Most interventions appear in either Table 16 or in Table 17 but not both. Thus, they either produce positive or negative impact outliers. Of course, evaluations that encompass multiple interventions, such as GAIN or NEWWS, might have positive outliers for some interventions and negative outliers for others. However, the same interventions record both positive and negative outliers in only four instances. First, Minnesota's Family Investment Program (MFIP) in urban Minnesota achieved positive employment outliers, but negative AFDC payment and AFDC receipt outliers. This may reflect the presence of financial incentives as part of the MFIP treatment. Second, New York State's Child Assistance Program (CAP) in Niagara County produced atypically large increases in earnings, but was less effective in reducing the percentage of program group members receiving AFDC. This may again reflect the provision of financial incentives. However, the regression adjustment has the interesting effect of increasing the positive earnings outliers from one to two, but decreasing the negative AFDC receipt outliers from two to one. Third, GAIN's two-parent family program in San Diego shows positive AFDC payment outliers, albeit only before regression adjustment, but negative employment impact outliers.

The fourth and final case of concurrent positive and negative outliers also represents the most complex and perplexing instance. Vermont's incentives-only variant of its Welfare Restructuring Project (WRP) resulted in exceptionally large increases in the earnings of two-parent AFDC recipients, but substantially underperformed in terms of the other three impact indicators. Financial incentives may have been responsible for retaining a high proportion of program group members on AFDC, but the program's underperformance in terms of the employment indicator is more difficult to explain. This said, it may reflect the fact that the program's financial incentive structure slightly penalized program group members during their first few months of employment, but substantially rewarded them after that.[20] It is conceivable that, for this reason, the program initially discouraged and delayed employment, leading to negative employment and AFDC impact outliers. However, the reasons why the program had substantially better than average impacts on earnings is unclear.

10.3 What Causes Type B Outliers to Occur?

We noted earlier that Type B outliers occur both because it is not possible through regression analysis to control for all the factors that cause impacts to vary across interventions and because sampling error causes some impacts estimates to be exceptionally large or small. However, the relative importance of these factors cannot be measured. Moreover, almost by definition, uncertainty is inevitable about the role of factors omitted from the regression equations. Thus, one can only speculate as to why a specific intervention produces impacts that are outliers. This section contains such speculation for a few of the interventions listed in Tables 16 and 17. Useful lessons may be learned by a more in-depth study of the interventions listed in these tables than we are able to provide here.

Because of limited information in the evaluation reports used to construct our database, it has not been possible to use regression analysis to control for how welfare-to-work programs deliver their services, a factor that other authors suggest plays a significant role in determining program impacts (Bloom et al., 2003). For example, in GAIN's Riverside intervention and in the NEWWS evaluation's Riverside and Grand Rapids Labor Force Attachment programs, the staffs placed particular emphasis on placing program group members into employment as quickly as possible. As shown in Table16, these programs all produced exceptionally large positive impacts. Leadership and direction provided to program staff by program management, which have been particularly associated with GAIN's Riverside

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[20]We estimate that a family of three would be $12 worse off per month under WRP during the first year of employment than under Vermont's traditional welfare program. After that, they would be $168 better off.

intervention, may also be important. The NEWWS Portland program may have benefited from a feature that allowed program group members to wait for a good job before accepting employment and from arrangements that provided for close cooperation between the welfare agency and various partner organizations. Other interventions made potentially important changes in their AFDC programs that may well have been to the detriment of their performance. For example, Minnesota's Family Investment Program provided financial incentives that increased the earnings level at which families can continue to receive AFDC, which may explain why the impacts of this program on AFDC payments and the receipt of AFDC were exceptionally low. Although the regressions included a dummy variable that was intended to control for the provision of financial incentives, it may have inadequately done so in the case of the Minnesota program.

Sampling error is most likely to be important when the sample used to estimate impacts is small. In evaluating welfare-to-work programs, much smaller samples are usually used to measure the impacts of interventions targeted at two-parent families than at one-parent families, reflecting the smaller number of two-parent families participating in AFDC. For example, 2,823 one-parent families but only 337 two-parent families were used in evaluating the Employment Initiative Demonstration in Baltimore. The corresponding figures for the GAIN program in Alameda County were 1,205 one-parent families and 182 two-parent families. This may help explain why negative outliers are associated with Baltimore and Alameda's two-parent interventions but not the one-parent interventions. However, the GAIN program in San Diego County produced similar findings, yet the impact estimates for two-parent families were based on a large sample of 3,277 observations.

11. ANALYSIS OF BENEFIT-COST FINDINGS

To determine whether welfare-to-work programs are socially efficient, evaluations cannot be limited only to effects on persons in the program. They must take account of effects on society, which includes persons not in the program. If a program has a positive earnings effect on those in the program, then the effect on society may be positive or negative, depending on the costs incurred by the government in operating the program. The usual way of taking account of societal effects is through benefit-cost analysis. An illustration of the framework that is typically used in conducting benefit-cost studies of welfare-to-work programs appears in Table 18.[21]

Costs and benefits will be shown in dollar amounts in Tables 19 and 20 as resulting from program impacts on output produced while participating in work experience, earnings, tax payments, AFDC payments, payments from other government transfer programs, and net operating costs. The plus and minus signs indicate whether each amount is expected to be a benefit (+) or cost (-) from the perspectives of four groups: persons assigned to the program, nonassignees (i.e., all persons outside the program group, including taxpayers who pay the cost of operating the program), the government, and the whole of society (those assigned plus those not assigned). As indicated, it is usually assumed that benefits and costs to nonassignees and the government are identical except that the former benefits from the output that individuals produce while assigned to work experience programs and the government does not. As can be seen, benefits and costs to society are simply the algebraic sum of benefits and costs to those assigned and those not assigned to a program. Hence, the framework implies that if a welfare-to-work program causes a decline in transfer payments received by program group members (for example, in AFDC receipts and food stamps), then this decline should be regarded as a cost to program group members (albeit one that may be offset by earnings increases); as a savings or benefit to taxpayers; and as neither a benefit nor a cost to society, but simply as a transfer of income from one segment of society to another.

One goal of benefit-cost analyses of welfare-to-work programs, as the last row of Table 18 suggests, is to determine whether the program being evaluated has positive or negative *net* benefits (i.e., benefits less costs) from each of the four perspectives represented by the four columns. The societal perspective is usually viewed by economists as the appropriate one for assessing the efficiency of social

[21]For a detailed description of issues involved with conducting benefit-cost analyses of welfare-to-work programs, see Chapter 11 of Boardman et al., 2001.

programs. Policymakers, however, often focus on the government perspective, that is, on whether the program increases or decreases government budgetary requirements.

11.1 Descriptive Analysis

Table 19 provides summary information on net benefits from each of the four perspectives. Estimates of the government's net operating costs are also presented. The estimates are measured in terms of costs and benefits per individual assigned to the evaluated interventions. The estimates in Table 19 are for the 49 benefit-cost studies included in our database. One set of estimates is used from each study. Because most evaluations of welfare-to-work programs conduct only one benefit-cost study, summing annual or quarterly estimates and projections of benefits and costs over several years (most often five), only one set of estimates is usually available. If more than one estimate was available, however, we use the most recent.

Both unweighted and weighted means are provided in Table 19. However, because net benefit estimates are a composite from several different impact estimates, and also incorporate information from other sources, standard errors do not exist for them. Yet the estimates are subject to sampling error, and, weighting is appropriate. As mentioned earlier in the report, the weights we use are the square roots of the number of observation in the total evaluation sample.

Except for the program group, the median net benefit estimates in Table 19 are somewhat larger than their corresponding means, indicating that some underperforming interventions are pulling down the average. In addition, the weighted values are larger than their unweighted counterparts, implying that evaluations with larger samples tend to produce larger net benefit estimates than those with smaller samples. It is not apparent why this should be the case. Net benefits received by nonassignees are somewhat larger than those received by the government, although not by very much. This is unsurprising because, as shown in Table 18, the computations of net benefits from these two perspectives are identical, except that nonassignees are credited with the value of output produced in work experience programs and the government is not. In most welfare-to-work programs, the value of such output is small, and in many it does not exist.

Keeping in mind that, unlike the estimates of impacts on earnings and AFDC payments reported in Table 2, the mean and median net benefit estimates in Table 19 are intended to capture the total effects of interventions over several years, not just effects for a single quarter, they are surprisingly small from all four perspectives. The largest are a little over $500 and the smallest a couple hundred dollars below zero. The weighted medians, which tend to be the largest values, indicate that society receives net benefits of around $500 from a typical welfare-to-work program, savings to the government are around $400, and the welfare of program group members are barely affected.

It is likely that the net benefits of a typical welfare-to-work program are actually even smaller than the estimates in Table 19 imply, because benefit-cost analyses are less likely to be conducted for those programs with especially small impacts. For example, the weighted means of the estimated earnings impacts in the 3rd and 7th quarters for our sample of studies were over twice as large for those interventions for which benefit-cost analyses were conducted as for those for which they were not. The gap is less striking for the unweighted averages but still substantial.

Thus, it seems apparent that the net benefits from a typical welfare-to-work intervention are modest indeed. However, as the standard deviations and the minimum and maximum values reported in Table 19 make clear, the variation across the 49 studies for which benefit-cost analyses were conducted is enormous. Thus, according to the benefit-cost findings, there were some impressively successful welfare-to-work programs and a few spectacular failures.

As is the case with the impact estimates presented in Table 2, this variation is due both to true differences among programs and to sampling error. Unlike the impact analysis, however, it also results because benefit-cost analyses of welfare-to-work programs are based on a large number of assumptions (see Boardman et al., 2001, Chapter 11, for a detailed discussion) and different evaluators make somewhat different assumptions. Although a discussion of these assumptions is beyond the scope of this report, this last factor suggests that some of the individual cost-benefit estimates may be subject to considerable error and these errors may differ in unsystematic ways across the studies.

11.2 Regression Analysis

To determine the factors that might influence the size of the net benefits from welfare-to-work, we conducted a regression analysis with the net benefit estimates from all four perspectives as dependent variables and the variables used in the previously discussed regressions on impacts as explanatory variables. The regressions were weighted by the square root of the sample size. However, we also estimated (unreported) unweighted regressions, which turned out to be very similar. The regression estimates are presented in Table 20. Because of inaccuracies and inconsistencies in the estimates of net benefits, because benefit-cost analyses are conducted for a somewhat unrepresentative subset of all evaluations, and because it was not possible to use the weighting scheme recommended in the meta-analysis literature, findings from these regressions should be considered as exploratory and viewed with considerable caution.

Perhaps, at least in part, because of these shortcomings, relatively few of the coefficients in Table 20 are statistically significant. However, there may be other reasons as well. For example, as we saw earlier, greater use of job search increases the earnings of participants, but it also reduces their transfer benefits. Thus, as implied by the first two columns of Table 20, the net effect of job search on benefits received by those assigned to welfare-to-work programs (the program group) is small and statistically insignificant. Similar reasoning suggests that the negative, but insignificant, coefficients on sanctioning in the first two columns may result because increases in sanction rates cause program group members to lose more in transfer benefits than they gain in earnings. It is more difficult to interpret the negative, marginally statistically significant, and fairly large effect of increases in participation in vocational education on the net benefits received by program group members. However, some of our earlier reported regressions implied that there was a negative relationship between vocational education and earnings.

Some of the other findings in Table 20 are straightforward to interpret. For example, two-parent families appear to benefit substantially less than one-parent families from assignment to welfare-to-work programs, and this translates into lower benefits to society from requiring two-parent families to participate in these programs.[22] The regressions on net benefits received by nonassignees and the government imply that these benefits are larger when AFDC payments are more generous. This is consistent with some of the earlier reported regressions that suggest that the impacts of welfare-to-work programs on welfare payments increase with the generosity of the AFDC system. Recall, however, that sensitivity tests indicated that this result is not very robust.

Unsurprisingly, net benefits are higher for those assigned to programs that offer financial incentives than for programs that do not provide these incentives, and smaller for nonassignees and the government. These effects are large and highly statistically significant. The two columns on the right of Table 20 indicate that the increases in net benefits to the program group are offset or nearly so by the reductions in nonassignee and government benefits. Thus, the social cost of financial incentives appears to be small or negligible. Because our earlier findings suggest that they do little to increase employment or earnings, financial incentives that are provided through welfare-to-work programs are perhaps best viewed as simply transferring income from the government to low-wage welfare recipients who find jobs. Their small social costs suggests that they are nonetheless efficient in this respect, as some research has suggested that it typically costs taxpayers about $1.50 to $2.00 to transfer one dollar to low-income persons (for example, see Gramlich, 1990, pp. 123–127, and Browning and Johnson, 1984). However, the results shown in Table 20 do not take account of potential social costs resulting from distortions in the labor supply and investment behavior of taxpayers, which are caused by transfer programs.

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[22]The California Work Pays Demonstration, which produced positive outliers for two-parent AFDC families, was not included in the regressions reported in Table 20 because the evaluation of this program did not include a benefit-cost analysis.

In interpreting some of the remaining regression coefficients in Table 20, it is helpful to be aware of how increases in participation in various services affect the net operating costs of welfare-to-work programs. Operating costs are the cost to the government of providing program services, but do not include transfers of income, such as AFDC payments. Table 19 indicates that these costs were around \$1,800 (in year 2000 dollars), on average, although the median value is considerably smaller. A weighted regression on net operating costs appears below (with the standard errors in parentheses):

Regression Model of Program Impacts on Net Operating Costs		
Constant	\$132.764 (455.47)	
Intervention impact on % sanctioned	110.711 (26.52)	***
Intervention impact on % participated in job search	4.623 (13.54)	
Intervention impact on % participated in basic education	104.051 (18.83)	***
Intervention impact on % participated in vocational education	48.174 (50.72)	
Intervention impact on % participated in work experience	-9.611 (35.54)	
Intervention included financial incentive=1	520.460 (493.07)	
Adjusted R-squared	.549	

These results imply that program net costs increase by over \$100 per program group member for every percentage-point increase in participation in basic education and by nearly \$50 for every percentage-point increase in vocational education, although the latter estimate is statistically imprecise. Increases in participation in work experience do not seem to increase costs. This may be because work experience participants are often assigned to agencies other than those operating welfare-to-work experience and whatever operating costs are involved may not get incorporated into the net cost estimates. Consistent with usual views on the topic, increases in participation in job search appear to result in very small increases in cost, although the estimate is very imprecise. Although the coefficient is also imprecisely estimated, financial incentives appear fairly costly to administer. Finally, the estimates also indicate that a one-percentage-point increase in the sanction rate cost over $100, presumably because of agency expenditures required for administering and enforcing sanctions.

Turning back to Table 20, it is not surprising that because basic education is costly to provide, it significantly reduces the net benefits to nonassignees and the government. Our earlier results suggested that basic education has a more or less negligible net effect on the earnings and transfer receipts of program group members. Thus, there are few benefits to offset these costs. There is also some indication in Table 20 that net benefits to non-assignees and the government are reduced by increasing participation in vocational education, although this result is statistically insignificant and disappears once the socioeconomic contextual variables are added to the regression model. Although sanctions are somewhat costly, our previous results indicate that they also reduce AFDC payments. The findings in the middle columns of Table 20 suggest that this reduction in AFDC benefits may more than offset increases in costs that result from sanctions. Table 20 also suggests that increases in participation in job search do not increase net benefits received by nonassignees and the government, and may even decrease them a bit. This finding cannot be easily reconciled with our previously discussed results, which indicate that job search reduces transfer payments and is inexpensive to provide, but may result from some of the limitations of the regressions on net benefits mentioned earlier.

12. ANALYSIS OF SUBGROUPS

A minority of evaluations of random assignment welfare-to-work programs report impacts on subgroups of those assigned and not assigned to programs. In this section, we summarize and analyze the impacts for the four pairs of subgroups most commonly recorded in welfare-to-work evaluations:

1. Participants in AFDC who, at the time of random assignment, were either *applicants* for AFDC or already *recipients* of AFDC.
2. AFDC participants who had been *in employment sometime during the year prior to random assignment* or who had not been employed during that time.

3. AFDC participants who, at the time of random assignment, had obtained a *high school diploma* or General Education Degree or who had not.

4. *Long-term AFDC participants* who, at the time of random assignment, had received AFDC for two or more years or *short-term participants* who had received AFDC for less than two year or were new AFDC applicants.

While some evaluations report separate impacts for both AFDC applicants and AFDC recipients, a small number of evaluated welfare-to-work programs specifically targeted only one of these subgroups. We include both types of evaluations in our analysis. Our analysis of subgroups is limited to evaluations of the effects of welfare-to-work programs on one-parent families, as impacts are very rarely reported for subgroups of two-parent households.

Table 21 lists the evaluations that report specific subgroup impacts, including programs solely targeted at applicants or recipients of AFDC. The characteristics that distinguish the four subgroup pairs that appear in the table are all known or believed to influence the chances of AFDC participants finding employment and leaving welfare. For instance, chances of leaving the welfare rolls typically decline with the duration of AFDC participation. This may result from eroding occupational and social skills that are rated highly by employers or simply because employers doubt that long-term welfare recipients have much of a commitment to the work force. Hence, long-term recipients of AFDC would be expected to be more difficult to place into work than short-term recipients. Similarly, AFDC applicants are more likely to leave welfare after just a short time than existing recipients of AFDC. AFDC participants with recent employment experience might find it easier to gain employment and to leave welfare than AFDC participants who have not worked for a year or more. Employers not only value recent work experience as a demonstration of acquired or retained occupational skills, but also as an indication of the AFDC recipient’s willingness and ability to hold down a job. Skills and educational qualifications, of course, also matter. In principle, employment chances are augmented by higher levels of education. AFDC participants with a high school diploma may spend less time on welfare and more time in employment than participants without a high school diploma. Their more advanced level of education should make them more attractive to employers. We anticipate that program impacts will be smaller for those subgroups that are able to find employment without the aid of welfare-to-work interventions (i.e., short-term AFDC recipients, AFDC applicants, and AFDC participants with recent work experience or with a high school diploma than for subgroups requiring assistance from interventions (i.e., long-term AFDC recipients, AFDC recipients, and AFDC recipients without recent work experience or a high school diploma).

12.1 Caveats

The evaluations listed in Table 21 report separate impact estimates for subgroups. As indicated by the table, because the reporting of subgroup impact estimates is selective and irregular, the number of estimates that are available for the meta-analysis varies among subgroups and among types of impact measures. As before, below we report impacts for the 3rd, 7th, 11th and 15th quarter after random assignment.

Evaluation reports typically contain less comprehensive data for subgroups than for the total target population participating in a welfare-to-work experiment. They frequently cover fewer calendar quarters or the data are limited to annual impacts.[23] Moreover, the evaluation reports do not provide the same detail, if they provide any at all, on the characteristics of subgroups or their rates of participation in program services. In the absence of specific data about the subgroups, this information is taken from data provided for the overall evaluation sample.

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[23]Estimates of quarterly impacts can be obtained from annual data in the case of *monetary* impacts (i.e., earnings and the amount of AFDC payments) by dividing the annual impacts by four. For conceptual reasons and because there is no pattern apparent from evaluations that do provide quarterly data, we do not apportion annual to quarterly impacts in the case of *percentage* impacts (i.e., percentage employed and participation in AFDC). As a result, there are often fewer quarterly impact estimates available for the analyses of percentage impacts than for the analyses of monetary impacts. As in the main analyses, in order to boost the number of observations, impacts from quarters adjacent to the one in question have been included in the analysis where the latter quarter is not available. For instance, if no quarter 3 impact is available, the quarter 2 or quarter 4 impact is used, whenever it is present. The later quarter receives preference over the earlier quarter, if both are present.

As explained in Section 3, sample size is used in imputing standard errors when they are not otherwise available in order to weight program impacts. Because we do not have data on the size of individual subgroup samples in our database, we use the size of the overall evaluation sample for this purpose. Values for individual subgroups are, of course, smaller than those for the evaluation sample as a whole. It is important to bear this mind in interpreting the results of the meta-analysis.

12.2 Three Types of Analyses

Three types of analysis of subgroup impacts are reported. The first two analyses use only subgroup impacts; this analysis is referred to as the "pure-subgroup analysis." Dummy variables, coded 0 and 1, are added to the data to indicate the subgroup within a subgroup pair to which each impact estimate refers. The third analysis includes both subgroup and main group impacts and is referred to as the "mixed-group analysis."

Pure-subgroup analyses. In the first pure-subgroup analysis, mean impacts are calculated for each subgroup. This is done for all four quarters and for all four impact measures, using the impact estimates available in each quarter.

The second pure-subgroup analysis computes the difference between the mean impacts for each pair of subgroups, using regression analysis to adjust these differences for a range of explanatory control variables. In other words, the regression-adjusted differences can be viewed as an attempt to control for the fact that the two subgroups in each pair may differ from one another in numerous ways, such as in age and in the services they received. This exercise is limited to impacts from the 7th quarter after random assignment, the quarter that usually contains the largest number of observations.

The regression analysis follows the same principles outlined earlier for the main group analyses. In fact, all the subgroup regressions take the independent variables of the main group analyses as their starting point, but also include the subgroup dummy variable. Because fewer impact estimates are available for the subgroup analyses than for the main-group analyses, however, multicollinearity between explanatory variables is more serious. This reduces the number of explanatory variables that can be use in the meta-analysis of subgroups relative to those employed in the main-group analyses. Thus, control variables that are highly correlated with one another are dropped following two basic principles. First, variables describing program characteristics are the last to be removed, because they describe program conditions that administrators are able to influence and are, therefore, of particular practical interest. Second, some variables that are highly correlated with others are removed, while retaining as many explanatory variables as possible. For instance, if one variable is highly correlated with another two, but the latter two are not highly correlated with one another, the former variable is dropped and the latter two are retained.

Mixed-group analysis. The mixed-group analysis integrates subgroup and all other one-parent impacts into one data file. As before, impact estimates that pertain to pure subgroups (e.g., AFDC recipients *or* AFDC applicants) are coded 0 and 1, respectively. In addition, when separate impact estimates for subgroups are not reported for an evaluated intervention, impact estimates that pertain to individuals from both subgroup categories (e.g., recipients *and* applicants) are coded within this range as appropriate. For instance, an impact for a program with 30 percent of the program group being AFDC applicants and the remainder recipients is coded as 0.3 in constructing the subgroup variable used for comparing recipients with applicants. Evaluations that do not report the proportion of subgroup members within their total sample are excluded from the mixed-group analysis. As before, the regression seeks to reduce multicollinearity among independent variables, while retaining as many variables that describe program characteristics as possible.

12.3 Findings for the Comparison of Unadjusted Means for the Pure Subgroups

Table 22 presents weighted impact means for the four pairs of pure subgroups, four impact measures, and four quarters.

Applicants and recipients. The simple means comparisons of the pure subgroup of AFDC applicants and recipients that appear in Table 22 indicate that impacts for recipients are larger than those for applicants with respect to the percentage receiving AFDC and the percentage employed in each of the four reported quarters. The results are less consistent with respect to the amount of AFDC payment and the amount of earnings. The latter is higher for recipients than applicants in the early quarters, but this is reversed in the later quarters, when fewer evaluations are available for analysis. The relative sizes of the amount of AFDC payment impacts alternate between the two subgroups from quarter to quarter. Moreover, the impact means for the percentage receiving AFDC are negative for both subgroups in the 11th quarter. This is unusual, but the number of available impact estimates is small. This highlights the sensitivity of the analyses to changes in the number and the composition of the impact estimates that are available.

Employed and not employed in year prior to random assignment. Relatively few evaluations estimate separate impacts for program group members who were, and were not, employed in the year prior to random assignment, thereby reducing the robustness of comparisons between these two subgroups. The number of observations that are available for each subgroup reaches double-digit figures in only two instances. In both cases, the mean impact is larger for welfare-to-work program group members without employment in the previous year than for those with previous employment.

Program group members with and without a high school diploma. The analysis of the mean impacts of welfare-to-work program group members with and without a high school diploma also suffers from a scarcity of observations. This said, comparisons in the four instances in which there are at least 20 observations all suggest a greater impact of welfare-to-work programs for program group members with high school degrees.

Long-term and short-term participants in AFDC. Comparisons of impact means for long-term and short-term participants in AFDC are again severely curtailed by the small number of observations available for all but the 7th quarter. Both the AFDC payment and the earnings data for the 7th quarter suggest a greater positive impact for long-term AFDC participants than for short-term participants. Although earnings impacts are greater for short-term AFDC participants in the 3rd and 11th quarters, many fewer observations are available for analysis in these two quarters.

Summary of findings from the comparison of unadjusted means. The comparison of these impact means is hampered by the small number of observations available for analysis. In many instances, findings are inconsistent from one quarter to the next or from one impact measure to the next. It is, therefore, difficult to identify generalizable patterns across all four subgroups and impacts. Nevertheless, with only one exception, whenever ten or more observations are available for each of the subgroup pairs, impacts are more positive for the more disadvantaged subgroup—that is, for AFDC recipients (rather than applicants), for program group members without recent employment experience (rather than program group members with recent employment experience) and for long-term (rather than short-term) participants in AFDC. The sole exception occurs in the comparison of welfare-to-work program group members with and without a high school diploma. Program impacts average higher for the former than the latter.

12.4 Findings for Quarter 7 Differences in Means between Subgroups

The pattern of relatively larger impacts for the more disadvantaged subgroup in each pair becomes more apparent when the analysis focuses on the 7^{th} quarter. Table 23 shows the differences in weighted mean impacts for the pure subgroups without regression adjustment and the differences in weighted mean impacts for the pure subgroups and for the mixed groups after regression adjustment. The full regression results are reported in Tables A.1 through Table A.4 in the Appendix. Positive differences indicate greater mean impacts among the first listed subgroup in each pair (i.e., the relatively more advantaged subgroup) and negative differences imply the converse.

As explained above, the mixed-group analyses add data from evaluations for which the proportion of welfare-to-work program group members belonging to subgroups is known, but impact estimates for separate subgroups were not estimated, to data for the pure subgroups. This addition substantially increases the number of observations available for analysis, particularly for the comparisons between the subgroups of program group members who were and were not employed during the year prior to random assignment and program group members who had and did not have a high school diploma.

Applicants and recipients of AFDC. With the exception of the amount of AFDC payments, the results reported in Table 23 indicate that a consistently greater program impact is achieved for AFDC recipients (indicated by the negative sign) than for AFDC applicants. The differences are statistically significant for all three types of analyses with respect to impacts on employment. The mixed-group analysis also suggests that welfare-to-work programs perform significantly better for recipients than for applicants with respect to their impacts on AFDC payments and earnings. However, this is not the case with respect to participation in AFDC, which is only statistically significant in the pure-subgroup analyses.

Employed or not employed in year prior to random assignment. The comparisons reveal consistently greater impacts for program group members who have not been recently employed than for those who have. However, statistical significance only occurs for earnings and the percentage employed in the pure-subgroup analyses and for the amount of AFDC payments in the mixed-group analysis.

Program group members with and without high school diplomas. Findings are more mixed in the comparison between the subgroup of welfare-to-work program group members with a high school diploma and the subgroup of program group members without a high school diploma. Whereas welfare-to-work programs produce greater impacts among program group members without high school diplomas in terms of their participation in AFDC, the reverse is true for the other program impacts. However, with only one exception (earnings in the regression-adjusted pure-subgroup analysis), none of the differences in means are statistically significant.

Long-term and short-term participants in AFDC. Long-term participants in AFDC achieve significantly greater impacts in quarter 7 than short-term participants in all three types of analysis for three of the four impacts—namely, AFDC payments, earnings, and the percentage employed. Moreover, the level of statistical significance of these differences is at the 1-percent level. This consistently high level of significance sets the meta-analysis results for the subgroup of long-term and short-term participants apart from those of any of the other subgroups. Only with respect to participation in AFDC are the comparisons of means statistically not significant for the pure subgroups, although statistical significance is achieved for the mixed groups.

Summary of findings from the comparison of quarter 7 impact differences. The comparison of mean impacts between subgroups seven quarters after random assignment most clearly reveals the strong and positively greater effects of welfare-to-work programs on long-term participants in AFDC than on short-term recipients. At the other extreme, the subgroup analyses found little evidence of a statistically significant difference in the performance of welfare-to-work programs for program group members with and without high school diploma.

The evidence is more diverse for the remaining two subgroups. First, there is evidence that welfare-to-work programs benefit program group members without recent employment experience more than those with recent employment experience. However, this finding is statistically significant in less than half of the comparisons, and it is never significant in all three types of analyses, suggesting once again that the regression findings are highly sensitive to changes in the availability of impact estimates.

Second, the analyses suggest that welfare-to-work programs achieve greater impacts for AFDC recipients than applicants. But again, although this finding is replicated across the four impact measures and the three types of analyses, statistical significance is achieved across all three types of analysis in just one instance, for the impact on the percentage employed.

When statistical significance of differences can be established, however, it is evident that welfare-to-work programs are more likely to benefit individuals from more disadvantaged subgroups than individuals from less disadvantaged subgroups. More specifically, AFDC recipients, program group members not employed in the previous year, and long-term participants in AFDC are more likely to gain from being assigned to welfare-to-work programs than their more advantaged counterparts. Similar evidence of a differential impact for program group members with and without high school diploma is lacking.

Comparing the results of the pure-group and the mixed-group analyses. Methodologically, the pure-subgroup analyses are the more appropriate ones, as they use impact estimates separately for each subgroup. Were it not for their small numbers, pure subgroups would provide the more appropriate analysis framework. However, because the mixed-group analysis is based on a larger number of cases, including those cases for which impacts were measured separately by subgroup, its results should be more robust. Moreover, it is possible to include a larger number of explanatory control variables in comparisons of subgroup differences.

Despite their differences, the pure-group and the mixed-group analyses produced remarkably similar results. With just two exceptions (among 48 mean estimates), the estimated impacts were in the same direction. Although there was less agreement between the two analyses concerning the statistical significance of impacts, all three of the differences between impacts for short-term and long-term AFDC recipients are highly statistically significant.

13. ANALYSIS OF CHILD OUTCOMES

Longitudinal studies have found that welfare use directly affects the economics of the household and the behavior of the parents receiving it. This has serious implications for the children in those households (Duncan and Brooks-Gunn, 1997). Specifically, factors such as the level of employment, the level of earnings, how much welfare improves overall income, the level of stress, and the schedule and conventionality of working hours can indirectly affect child well-being (Huston, 2002). Figure 1 depicts a conceptual model of how welfare policies can affect child outcomes.

Although employment for single parents on AFDC is a key goal of current welfare policy, the effects of maternal employment on child outcomes are not clear. When maternal employment does appreciably increase income, the effects on children are positive, but employment may not increase income if there is a loss of cash transfers and in-kind services (Bloom and Michalopoulos, 2001). Also, increasing a family's material resources by increasing maternal employment can change the amount of time parents spend with children, as well as relationships among family members. For example, increased employment can lead to less supervision for children, which can be particularly problematic for adolescents in low-income neighborhoods. Research shows that in such cases, adolescents displayed increased behavior problems and lower educational attainment (Huston, 2002).

While there is debate about the effects of income on child well-being (Mayer, 1997), there is evidence that improving a family's overall financial and material resources during a child's early years has significant, positive benefits on that child's cognitive abilities, educational attainment, and employment status later in life (Duncan and Brooks-Gunn, 1997). However, it is possible that other factors are affecting these outcomes. For example, working families with higher incomes use formal child care centers more than low-income families, who are more likely to use informal child care arrangements with members of their extended family, and studies have shown that formal child care arrangements produce better outcomes for children (NICHD, 1997). Also, low income is correlated with higher parenting stress and the use of harsher punishments, which is associated with children's diminished socioemotional well-being (Conger and Elder, 1994; McLoyd, 1998). Indeed, the receipt of temporary cash assistance alone has not been shown to have much relationship with child outcomes after controlling for other demographic, human capital, and environmental factors (Yoshikawa, 1999).

The problem with the interpretation of these findings is that the data are largely correlational in nature. In order to control for other endogenous variables, such as parental characteristics, experimental studies are needed. Huston (2002) refers to the relationship between welfare, financial resources, family systems, and child development as a "black box" in which researchers need to find causal links and pathways. The best way to discover these links is through random assignment experiments.

The design of random assignment experiments enables researchers to control for unmeasured differences, such as the individual characteristics of the parents and children and their family histories, while determining the effects of the tested interventions on children. When measured, child outcomes are generally broken down into three categories: academic/cognitive, behavioral/emotional, and health/safety. Seven of the random assignment evaluations of mandatory welfare-to-work programs in our database provided sufficient information on child outcomes for analysis as part of this report. The nature and respective findings of each of these programs are discussed briefly below, with particular emphasis on their impacts on child outcomes.

13.1 Evaluations that Measured Child Outcomes

Connecticut's Jobs First Program. This demonstration project was one of the first to focus on strict time limits for its recipients. In addition to time limits, however, it also offered financial work incentives and an intensive focus on quick job placement. While Jobs First attained its goal of replacing welfare with work among the treatment group, material hardship stayed high for both the control and treatment groups. Jobs First had mixed effects on children. Children between the ages of 5 and 8 exhibited more positive behavior and fewer behavioral problems. Likewise, adolescents in the treatment group were less likely to be convicted of a crime; however, their overall school achievement was significantly less than adolescents in the control group (Bloom et al., 2002).

Florida's Family Transition Program (FTP). This demonstration project focused primarily on time limits for welfare receipt. It also offered financial incentives. The overall effects on adults showed that the time limits increased employment and earnings, but did not significantly increase overall family income. Further, by the time of the four-year follow-up, all of these effects had disappeared. For children aged 5 to 12, there were no significant effects in the academic/cognitive area, but there were weak decreases in behavioral/emotional problems and weak increases in health and safety. For adolescents, school suspensions significantly increased (Morris et al., 2001).

Los Angeles Jobs First Greater Avenues for Independence (LA GAIN). The overarching goal of LA GAIN was to convert an education-first welfare-to-work program to a work-first focused program. It was successful for single-parent families; by two years after random assignment, the intervention caused employment rates to increase substantially, welfare participation to fall, and earnings to increase.

However, little to no systematic effects on children were found, although there was a slight increase in behavioral problems and academic achievement among a small group of preschoolers (Freedman et al., 2000).

The National Evaluation of Welfare-to-Work Strategies (NEWWS). This demonstration project included mothers who were 19 years of age or older with children who were 3 years old or above. Child outcome data were collected in four of its seven sites, including three sites in which mothers could be assigned to one of two treatment groups: education-first or work-first. The impacts on children from each group were generally weak, and when they did occur, they were typically positive in some sites and negative in others. The research suggests that even though the mothers in NEWWS had increased employment, their overall income levels did not increase much because of lost welfare benefits. This may help explain the weak and inconsistent impacts on children (Morris et al., 2001).

The Minnesota Family Investment Program (MFIP). MFIP provided families with strong financial work incentives by providing cash supplements and subsidizing child and health care. Additionally, there were no time limits placed on the receipt of welfare benefits. As a result, employment and overall income increased throughout the final three-year follow-up assessment, while poverty decreased among the families assigned to welfare-to-work programs. Impacts on children were mixed, however. There were very weak or no impacts on children under 5; positive impacts on behavioral/emotional and academic/cognitive measures for children age 5 to 12 (Morris et al., 2001), and negative impacts on externalizing behaviors such as smoking, drinking, and substance abuse for adolescents. Further, there was a significant decrease in injuries and accidents for children (Gennetian et al., 2002).

Vermont's Welfare Restructuring Project (WRP). WRP was an intensive demonstration project that aimed to promote work and decrease dependency on welfare. Time limits were enforced, and if members of the program group did not find employment, they were placed in minimum-wage jobs. Failure to comply with WRP work requirements resulted in the state taking over the welfare grant.

Financial incentives were offered to encourage work. Although WRP increased employment and reduced cash assistance, income and material hardship did not change, because the increased income generated through employment was offset by the reduction in cash assistance. Program effects on children were minimal. Absenteeism for children aged 10–13 decreased, but adolescents were much more likely to get in trouble with the police (Scrivener et al., 2002).

Iowa's Welfare Reform. The goal of Iowa's welfare reform plan was to reduce the receipt of cash assistance by increasing employment through employment-oriented services, sanctions for noncompliance, and expanded earned income disregards. The program had mixed results. There was higher participation in job training and placement programs, and employment increased. However, outcomes were poor among AFDC applicants, as their earnings and incomes decreased substantially. Furthermore, the children of applicants had poorer school achievement outcomes (Fraker et al., 2002).

13.2 A Meta-Analysis of Program Effects on Children

This brief review of the mixed impacts of welfare-to-work interventions on children provides a basis for further investigation. Using information in our database on demonstration projects that examined child outcomes, it is possible to get a clearer picture of the possible systematic effects of welfare-to-work programs on children. We hypothesize that increasing the net income of members of the program group (as measured by program impacts on net benefits) is positively related to program impacts on child outcomes. As previously demonstrated, one way welfare-to-work programs increase the incomes of AFDC recipients who go to work is through financial incentives. Thus, we also hypothesize that when financial incentives are offered by an intervention, this will improve impacts on child outcomes. On the other hand, time limits and program-induced increases in sanction rates are likely to increase stress within a family and reduce family income. Hence, they may have negative effects on program impacts on children. We also investigate whether impacts on the percentage of program group members who participated in job search, basic education, vocational training, and unpaid work experience affect child outcomes. Participation in all of these activities may increase family stress. Moreover, mothers are taken out of the household while they are participating. Thus, we hypothesize that the impacts of welfare-to-work programs on children will be negatively affected to the extent that programs increase participation in these activities. Finally, we examine whether more costly welfare-to-work programs (as measured by the government's net operating cost) produce more positive impacts on children. A summary of the explanatory variables just considered is provided in Table 24 for each of the evaluations that measured program impacts on children.

The various child impacts that were measured by the evaluations include positive behavior, school achievement, suspension, expulsion, repeating a grade, behavioral or emotional problems, and/or child health. However, because different evaluations measured these impacts of welfare-to-work programs on children differently (see Appendix B for details of measurement by evaluation), it was necessary to make them comparable. In meta-analysis, this is usually done by converting impact estimates into an "effect size" measure. Using a method developed by Glass (1976), we do this by first subtracting the control group mean for each outcome from the treatment group mean for the outcome and then dividing the resulting impact estimate by the standard deviation of the control group means.

13.3 Findings

Table 25 presents unweighted and weighted descriptive statistics for the child outcomes. A positive effect size for an outcome measure indicates that the treatment group had a positive impact on the child outcome, and vice versa. Cohen (1988) suggests that effect sizes can be interpreted as follows: small effect size = .20, medium effect size = .50, and large effect size = .80. The means and medians indicate that the effect size of the impact was quite small for each respective outcome, as all are well below .20. However, the standard deviations and minimum and maximum values indicate that there is substantial variation among the evaluated programs.

The Q-statistic is used to test whether this variation is attributable to program differences or to sampling error. The results of these tests also appear on Table 25. All of the outcomes that were aggregated across age groups failed the test for homogeneity. Even when each age group is examined separately, all but two of the outcomes (young age suspension/expulsion and school-age positive behavior) far exceed the critical value of chi-squared for p =.05. These results indicate that the observed variability among the effect sizes is unlikely to have resulted from sampling error alone and, hence, that some of the variation results from systematic differences among the interventions.

Next, we use weighted regressions to explore different possible explanatory variables that may account for some of the variation in effect sizes among child impacts. To maximize sample size in computing the regressions, we pooled the effect size estimates for the three age groups, using dummy variables to control for differences in effect size across these groups. Multicollinearity was a serious problem in conducting the regression analyses. Many of the program characteristics were highly correlated (r >.50) with one another and other contextual variables, such as race and poverty rate. This, combined with a small sample size, made it necessary to restrict the explanatory variables severely. This limitation should be considered carefully when interpreting the regression results.

As previously discussed, the most logical possibilities were whether the program offered a financial incentive (see Model 1, below), enforced time limits, or increased the sanction rate (Model 2). We also examined whether program impacts on children were influenced by the net cost of the programs to the government and net program benefits to members of the program group (Model 1). Finally, we tested whether program impacts on the percentage of parents participating in job search, basic education, vocational training, or unpaid work on child outcomes had an effect on program impacts on children (Model 3).[24]

[24]We also investigated two additional potential sources of systematic differences in child impacts among programs by dividing the sample in two ways: Vermont versus other programs, and low-cost versus high-cost programs. However, these subgroups were nearly as heterogeneous as the combined groups. Thus, the null hypothesis that the variation in the effect sizes of child impacts is due entirely to sampling error is rejected for all the impact measures but young age suspension/expulsion and school-age positive behavior.

13.4 Regression Findings

The regressions include all the intervention characteristic variables listed in Table 24 but net program benefits to members of the program group, as this was never a significant predictor of any of the program impacts on children. We computed exploratory regressions on all the child impact measures except the two for which we had only four observations (positive behavior and health). However, statistically significant coefficients were obtained only for regressions that used the impact on the "behavioral/emotional problems" measure as a dependent variable. Thus, these are the only findings we report. These findings appear in Table 26 and are discussed below.

Model 1. Model 1 in Table 26 indicates that welfare-to-work programs have a less positive impact on the emotional and behavioral problems of school-age than younger children, the comparison group. The coefficient for adolescents is also negative, but not statistically significant. The coefficient for the financial incentives dummy variable is negative and statistically significant, indicating that including financial incentives in a welfare-to-work program negatively affects the program's impact on childhood emotional and behavioral outcomes even though the incentives increase the incomes of working AFDC recipients. This result is the opposite of what we hypothesized. On the other hand, the coefficient on the net cost variable is positive and highly statistically significant, suggesting that more expensive welfare-to-work programs have more positive effects on this impact than less costly interventions.

Model 2. Like Model 1, Model 2 indicates that welfare-to-work programs have less positive impacts on the emotional and behavioral outcomes of school-age children than they do on younger children. Again, adolescents have a negative, but statistically insignificant, coefficient. In support of our hypothesis, the coefficient on the dummy variable for time limits is negative and statistically significant, indicating that time limits have a negative effect on the impact of welfare-to-work programs on children's behavioral or emotional outcomes. In contrast to our hypothesis, however, increases in the use of sanctions appear to have a positive effect on this impact.

*Model 3***.** The school-age dummy variable is again statistically significant and negative in Model 3, adding further support to the finding that impacts for this age group are smaller than for younger children. The positive, statistically significant coefficient for participation in basic education and work experience indicates that increasing the use of these services has positive effects on program impacts on children's behavioral and emotional outcomes.

13.5 Summary of Key Findings

Conclusions based on the results discussed above are summarized below. These findings, while limited, provide some insight into how the characteristics of welfare-to-work interventions influence program impacts on children. The findings should be considered highly tentative because important contextual variables were not included as controls.[25] Further research on the factors that influence the impacts of welfare-to-work programs on children is clearly warranted.

- Overall, program impacts on children were small.
- There is evidence that the considerable variation across programs in their impacts on children is not entirely due to sampling error, but is partially attributable to systematic differences among the interventions. However, with the exception of impacts on the emotional and behavioral problems of children, we were unable to determine what these systematic differences might be.
- There is no support in our data that increasing the net income of welfare families improves child outcomes. However, the welfare-to-work programs that were examined did not produce large changes in the incomes of those assigned to them.
- When various program characteristics are controlled, impacts on emotional and behavioral problems are less positive for school-age children than for younger children.
- Three program features appear to positively affect the impact of welfare-to-work interventions on children's behavioral and emotional outcomes: sanctions, participation in basic education, and participation in unpaid work. These findings are inconsistent with what we predicted. However, it

[25]For an in-depth discussion of contextual factors such as child care arrangements and home environment that affect child outcomes in five welfare-to-work demonstration projects, see *Welfare Reform and Children: A Synthesis of Impacts in Five States,* (2005), Washington DC, Administration for Children and Families, which is available at http://www.acf.hhs.gov/programs/opre/welfare_employ/ch_outcomes/reports/welfare_reform_children/welfare_reform_toc.html.

may be worth noting that increasing the sanction rate was also found to have a positive effect on program impacts on getting program group members off AFDC and into jobs.

- Two features of welfare-to-work programs exert negative influences on impacts on childhood behavioral or emotional impacts: financial incentives and time limits. The result for financial incentives, while not supportive of our hypothesis, is consistent with the previously discussed finding that financial incentives decrease program impacts on AFDC participation and payments, while failing to increase their impacts on employment and earnings

- Increasing expenditures on welfare-to-work programs has a positive effect on their impacts on childhood behavioral and emotional problems.

14. VOLUNTARY PROGRAMS

There are four voluntary evaluations in our database (Supported Work, Homemaker Health Aide, Maine's Training Opportunities in the Private Sectors, and New Jersey's Grant Diversion Project) that at least superficially appear similar. They all evaluated programs that paid a stipend to AFDC recipients who volunteered for temporary jobs that were intended to help them learn work skills.

Summary information about the results of these evaluations appears in the first two columns of Table 27. Impact estimates were only available from all four evaluations for earnings and the amount of AFDC received. Ten impact estimates are available for the 7^{th} calendar quarter because one of the four evaluations (the Homemaker Health Aide evaluation) assessed somewhat different programs that were independently operated by six different states. One evaluation (Supported Work) did not estimate impacts for the 11^{th} quarter, so only nine observations are available for that quarter. Impact estimates are not available for the 15^{th} quarter, and we do not provide impact estimates for the 3^{rd} quarter because some members of the program groups were still actively participating in the programs then and some of the evaluations counted the stipends they were receiving as earnings.

The means and medians shown in Table 27 indicate that the evaluated voluntary programs increased the earnings of members of the program group by a modest amount in the 7^{th} quarter and by somewhat more in the 11^{th} quarter. Conversely, impacts on AFDC payments were larger in the 7^{th} quarter than in the 11^{th} quarter.

The standard deviations and maximum and minimums in the first two columns of Table 27 suggest that there is fairly substantial variation in these impacts. Whether this variation is due to systematic differences among the intervention or merely attributable to sampling error is investigated by Q-tests. Results from this investigation are shown in the second column of the table. The fact that the Q-test statistic is well below the critical value for a chi-squared for the 7th quarter earnings impacts implies that all the variation in these impacts can be attributed to sampling error. However, the Q-statistic is above the critical value for a chi-squared for the variation in 11th quarter earnings, suggesting that some of the variation in these impact results from systematic differences among the interventions. The hypothesis that the variation in the impacts in AFDC payment impacts is due entirely to sampling error is resoundingly rejected for both quarters.

We explored different possible explanations for variation in the impact estimates that is attributable to systematic differences among the interventions and, thus, is not a result of sampling error. The most promising explanation appears to be that, unlike mandatory welfare-to-work programs, more expensive voluntary welfare-to-work produce larger impacts than less expensive programs.

To examine this possibility, we used estimates of operating costs per program group member, which range between \$2,500 and \$16,000 (in year 2000 dollars) and refer to the early years of a program's operation, to divide the ten interventions into two subgroups of equal size: high-cost programs and low-cost programs. As indicated by Table 27, with the exception of the 7th quarter earnings impact, impacts were considerably higher for the high-cost programs than the low-cost programs. To investigate this further, we computed weighted regressions in which operating cost per program group member was the only explanatory variable. As expected, the coefficient on this variable is very small and statistically insignificant for the 7th quarter earnings impacts. The other three coefficients are statistically significant at the 5-percent level, however. More specifically, the results indicate that a \$100 increase in expenditures per program group member results in impacts on earnings that are almost \$5 larger in the 11th quarter and in impacts on AFDC payments that are about $5 larger in the 7th quarter and $2.50 higher in the 11th quarter.

The Q-test statistics in the right-hand column of Table 27 imply that, with the exception of the 7th quarter impacts on AFDC payments, the variation among the impacts for the low-cost interventions is due to sampling error. The Q-test statistics in the fourth column indicate that the variation among the earnings impacts for the high-cost interventions is also caused by sampling error, but that much of the variation among the AFDC payment impacts results from systematic differences among these interventions. With only five high-cost interventions with which to work, we were unsuccessful in discovering the sources of these systematic differences.

15. SUMMARY OF FINDINGS AND CONCLUSIONS

A key goal of this research has been to increase information about what sorts of welfare-to-work programs work best under different labor market conditions and for different types of welfare recipients. Although the methodology we used is somewhat technical, our goal was a practical one: to provide information to help policymakers make informed decisions in designing welfare-to-work programs.

We have analyzed 27 random-assignment evaluations of mandatory welfare-to-work programs and four random-assignment evaluations of voluntary welfare-to-work programs, covering nearly 100 welfare-to-work interventions, exploring what program features increase or decrease an intervention's effectiveness and cost-effectiveness. In the following sections, we summarize our key findings and present our conclusions.

15.1 Impacts of Mandatory Welfare-to-Work Interventions

Overall, mandatory welfare-to-work interventions had the desired impacts, although these were typically of modest size. Quarterly earnings impacts, for instance, averaged around $75 (in year 2000 dollars) in the 3rd quarter, rising to $115 in the 15th quarter. Over a year, the earnings for program group members from a typical welfare-to-work intervention, therefore, amounted to around $500 more than for control group members, about a 10 percent increase. However, there was considerable variation among the interventions. Similarly, the percentage of AFDC participants who were employed in a given quarter was around 10 percent (or three percentage points) higher among program group members in the mandatory reform program than among participants in traditional AFDC programs. Again, there was considerable variation among interventions impacts.

The amount of AFDC payments received by control families in the evaluations of mandatory welfare-to-work programs declined sharply between quarters, from an average of $1,033 in the 3rd quarter to $458 in the 15th quarter. The additional decreases attributable to reform programs rose from $38 to $75 over the same period. A very similar pattern was apparent for AFDC participation, which declined from 81 percent to 41 percent between the 3rd and the 15th quarter among control group members. It decreased by a further 1.5 percentage points in the 3rd quarter and 4.4 percentage points in the 15th quarter among persons assigned to the reform programs. However, the trends over time may be exaggerated by the inclusion in the analysis of a greater proportion of high-impact interventions in the later quarters.

15.2 What Makes Welfare-to-Work Interventions Successful?

Using meta-analysis, this study has been able to go beyond conventional comparisons of the impacts of selected mandatory welfare-to-work programs by identifying what makes a welfare-to-work program "successful" or "unsuccessful." In particular, we have been able to examine differences among program characteristics, the characteristics of program caseloads, and the characteristics of the local environment.

The analyses consistently indicated that sanctions and job search had strong, positive effects on program impacts. The imposition of time limits was also found to increase impacts, albeit not for earnings impacts. In contrast, activities intended to improve a program group member's human capital (e.g., basic or vocational education and work experience) did not have a consistent positive effect on program impacts. In fact, in some instances, their effects may have been negative. Financial incentives reduced program impacts on AFDC participation and payments, as might be expected, but, contrary to intentions, failed to have positive effects on program impacts on earnings and employment.

The impacts of mandatory welfare-to-work programs seem to be larger when labor markets are strong than when they are weak. It was not clear whether interventions were more effective for one-parent or two-parent families. We were also unable to draw firm conclusions as to whether the performance of welfare-to-work interventions has improved over time and, hence, whether program implementers have learned from experience and improved the content and administration of programs.

A series of sensitivity tests generally confirmed the results of our meta-analyses. Although the tests changed the size of some of the regression coefficients, the signs of the coefficients mostly remained the same, especially when they were statistically significant. A further analysis suggested that the regression models are useful for assessing whether a particular mandatory welfare-to-work program is performing better or worse than an average program.

15.3 Effectiveness among Subgroups

The impacts of mandatory welfare-to-work programs seem to be greater for more disadvantaged groups, particularly for long-term participants in AFDC (as compared to short-term participants). Other subgroups that particularly benefited from these interventions were program group members without employment experience during the year before random assignment (as compared to those with recent employment experience) and existing recipients of AFDC (as opposed to new applicants). However, in the latter two instances, the differences between the two subgroups that were compared were not always statistically significant. Moreover, no statistical significance at all could be established for differences in the impacts on program group members with and without a high school diploma. In addition, there was some evidence that program impacts were larger for older caseloads than for younger caseloads.

15.4 Program Impacts over Time

Regression analysis suggests that the impacts of mandatory welfare-to-work programs typically linger for between five and seven years, but begin to decline after two to three years. Employment was the first of the impacts to decline, followed by AFDC payments and earnings, while the impact on the percentage receiving AFDC was the last to decline.

15.5 Analysis of Outliers

A small number of mandatory interventions stand out for their very large or small impacts at multiple times during the evaluation follow-up period. Programs with multiple, exceptionally large impacts included the already celebrated welfare-to-work interventions in Riverside County, California, and Portland, Oregon. Alongside these are two lesser-known programs, the California Work Pays Demonstration and the New York State Child Assistance Program. The extent to which they outperformed other programs became particularly apparent after the analysis controlled for factors that affect program impacts.

Vermont's Welfare Restructuring Project and Minnesota's Family Investment Program were repeatedly among the lowest performing interventions, possibly because they provided financial incentives that tended to reduce program impacts on AFDC participation rates and payment amounts while having little positive effect on employment rates or on earnings.

15.6 Costs and Benefits

The net costs to the government of operating the evaluated mandatory welfare-to-work interventions averaged around $1,800 (in year 2000 dollars) per program group member, and most occurred during the early months of a program's implementation and evaluation period. Job search activities were, by far, the lowest cost contributors to governments' net operating costs, whereas basic and vocational education and the administration of sanctions and financial incentives all added substantially to net operating costs.

An analysis of benefit-cost data, which are available for 49 mandatory welfare-to-work interventions, indicated that the median net benefit to society from these interventions was around $500 per program group member (in year 2000 dollars) and the median net benefit to the government was approximately $400 per program group member. Program group members themselves seemed to gain little from being assigned to most welfare-to-work programs. As benefits and costs are measured over a number of years after random assignment, the fiscal gains from welfare-to-work programs appear small.

15.7 Child Outcomes

Even though welfare-to-work programs do not directly target child outcomes, it is likely that changes in family income and maternal employment can affect child development. To examine these relationships, seven evaluations of mandatory welfare-to-work programs that provided information on child outcomes were analyzed in this report. In general, effects on children were small and mixed. No clear widespread harm or benefits to children could be found. While several individual program evaluations have found that increasing the incomes of welfare recipients increases positive outcomes for children, we found no evidence of that in these data. However, the welfare-to-work programs that were examined did not produce large changes in the incomes of those assigned to them. Several program characteristics were found to affect program impacts on the behavioral and emotional outcomes among children, but not other outcomes among children. Sanctions, participation in basic education, participation in unpaid work, and program net operating costs were found to have positive effects on program impacts on children's behavioral and emotional outcomes, while financial incentives and time limits appear to have a negative effect. Finally, after controlling for various program characteristics, it was clear that these programs were less effective at affecting the behavioral and emotional outcomes of school-age children than of younger children.

Findings from this analysis are tentative because we were unable to control for important social and environmental indicators that may also affect child well-being. Indeed, many questions in the "black box" remain unanswered. Still and all, policy makers should pay particular attention to the negative effect that financial incentives had on impacts on behavioral and emotional outcomes for children, as well as the fact that financial incentives certainly did not improve other types of impacts. On the other hand, increasing sanctions did improve program impacts on children's behavioral and emotional well-being, as well as producing the desired effects on program group members.

15.8 Voluntary Programs

There were four evaluations of voluntary welfare-to-work interventions that paid a stipend to AFDC recipients who volunteered for temporary jobs intended to provide them with work experience and skills. These interventions had positive, but moderate, impacts on earnings and AFDC payments, and more costly interventions had larger impacts than less expensive interventions.

15.9 Conclusions: Lessons for Policy and Analysis

Several conclusions of relevance to policy-making emerge from our analysis.

First, the meta-analysis, especially the analysis of findings from benefit-cost studies, suggests that the effects of a typical welfare-to-work program are extremely modest. Although these programs are probably worth running, and, as discussed below, can be improved, by themselves they will do little to reduce the size of welfare rolls or improve the lives of most persons assigned to them. Thus, they must be coupled with other policies. Thus, they must be coupled with other policies.

Second, the meta-analysis clearly supports the focus of welfare-to-work programs on job search. Activities that emphasize human capital development appear less effective than job search activities and are much more costly to provide.

Third, welfare-to-work interventions seem to produce better results for more disadvantaged groups, but the evidence was consistent only for long-term recipients of AFDC. With respect to other subgroups, the evidence was somewhat ambiguous. The greater benefit of program assignment accruing to long-term AFDC recipients might, in fact, be the result of greater efforts to target these recipients by some interventions. Arguably, there is a case for extending welfare-to-work programs to other disadvantaged subgroups, particularly if further analysis shows that they are indeed served less well than other similarly disadvantaged subgroups.

Fourth, we also found that financial incentives, or at least the current structure of these incentives, have perverse effects on program impacts. Financial incentives tended to increase AFDC participation and payments. Importantly, they also may have had a negative effect on employment and earnings impacts, at least in the early quarters after random assignment. At best, they do not have the intended positive effects. Moreover, the use of financial incentives seems to be one of the factors that caused some specific interventions to markedly underperform. Furthermore, financial incentives appeared to have had no effect on most impacts of welfare-to-work programs on children and a negative effect on program impacts on behavioral and emotional outcomes for children. Thus, although financial incentives do transfer income to the working poor, they appear to fail to have their intended effects on program group members *and* their children, and they are costly to administer. Consequently, policy makers may want to reconsider offering financial incentives or at least the way in which they are offered. Perhaps earning subsidies that appreciably increase family income will produce the desired effects on parents and children.

Fifth, the analysis suggests that welfare-to-work programs are more likely to be effective in locations that are enjoying job growth. Thus, it may make sense to allocate additional resources to welfare-to-work programs when job growth is occurring.

Sixth, there is evidence that the impacts of mandatory welfare-to-work programs linger for five to seven years after random assignment. However, this evidence is not definitive, because most evaluations of welfare-to-work programs do not provide impact estimates for this many years. Better information on this important topic would be possible by extending the length of time over which impacts are estimated.

Finally, our analyses have found little evidence that the performance of welfare-to-work programs has been improving over time. This might indicate an absence of systematic policy learning. It is important that lessons from welfare-to-work programs are shared and that the results of analyses, such as the one reported here, are widely studied. In particular, we found that while the effects of most welfare-to-work programs are quite modest, a few stand out. However, our meta-analysis could determine only some of the factors contributing to these success stories. Other methods, such as more detailed surveys and focus groups, might be used to investigate these programs in more detail to attempt to unravel the sources of their success.

APPENDIX A
The Welfare-to-Work Program (Meta-Analysis) Database

The welfare-to-work program database is the largest and most comprehensive information source on evaluations of U.S. welfare-to-work programs that is currently available. A public use copy of it can be obtained from the Office of Planning, Research, and Evaluation (OPRE) of the U.S. Department of Health and Human Services, Administration for Children and Families. The database can be used in several different ways. For example, it allows researchers to conduct complex statistical studies of welfare-to-work programs. It also provides a readily accessible means for analysts to make simple comparisons among welfare-to-work programs. A *Users' Guide* that provides a detailed description of the database is also available from OPRE. In this report, we give only a brief description of the database.

The database is maintained in Microsoft Access. It contains detailed information on each of the random assignment evaluations listed in Table 1 in the main body of the report. In the database, evaluation studies are first classified by evaluation, then within each evaluation by site, and then within each site by program. For example, in the case of the very large, and complex, National Evaluation of Welfare-to-Work Strategies (NEWWS), there is one evaluation and there are seven sites. For three of these sites, there is one program in each; and for the remaining four sites, there are two programs in each. Thus, there are separate sets of program impact estimates for 11 interventions. In our analysis, we treat each as a separate observation. In addition, for each of the 11, there are separate sets of effect estimates for subgroups (for example, AFDC applicants and AFDC recipients, and persons with and without a high school degree). In some (but not most) of the research described in this report, each of the subgroup impact estimates is treated as a separate observation.

Although the NEWWS evaluation was limited to program impacts on one-parent families on AFDC, other evaluations estimated separate impacts for one- and two-parent AFDC families (one prominent example is the GAIN evaluation that was conducted at six sites in California during the late 1980s and early 1990s). When available, these separate impact estimates are both included in the database and treated in the analysis as separate observations (that is, as pertaining to separate interventions).

The database is divided into five "levels":

Level 1 lists the title of the evaluations, their evaluators, and the reports used in constructing the database.

Level 2 contains the relevant sampling information for each intervention, including the sample sizes and the characteristics of the sample population (gender, ethnicity, age/age group, education, number of children, welfare and employment experience prior to random assignment, and so forth).

Level 3 contains both annual and quarterly estimates of program impact measures, as well as their levels of statistical significance. Impacts are recorded for all the years (up to five) and all the quarters (up to 20) for which they are available. Whenever it was possible, the impacts were recorded separately by program site. Program impacts were collected for one-parent families and two-parent families separately, whenever they were available. In addition, whenever they were available, impacts were recorded for both the overall program target group and for program subgroups. The impact measures in Level 3 include:

- Average earnings during the quarter or year;
- Percentage ever employed during the quarter or year;
- Average AFDC payment amount during the quarter or year;
- Percentage ever in receipt of AFDC payments during the quarter or year;
- Net program benefits (that is, program benefits less program costs) from the perspectives of the experimental (or program) group, the control group, the government, and society as a whole;
- Assorted measures of program impacts on children (e.g., parent reports of school achievement, general health, behavior problems, and ever suspended or expelled from school) are recorded for three separate age groups (young, school age, and adolescents).

Level 4 records program participation statistics (for example, overall program participation rates; program sanction rates; and rates of participation in job search, basic education, vocational training, and work experience). Whenever it was available, this information was obtained separately for the program treatment group and the control group and separately for different program sites. Level 4 also records the gross and net cost per program group member to the government of operating each program and indicates whether each program tested financial incentives and time limits. Information is also provided on the dollar value of the financial incentives tested by the evaluated programs and on the characteristics of sanctioning under the program.

Level 5 contains socio-economic background data for each of the program sites during each of the evaluation years, including the unemployment and poverty rates, the percentage of the work force in manufacturing employment, the annual percentage change in manufacturing employment, median household income, and the maximum AFDC payment for which a family of three is eligible. Unlike the data for levels 1 through 4, which were extracted directly from the evaluation reports, the level 5 data were obtained from government sources, mainly various U.S. Census Bureau and Bureau of Labor Statistics Web sites.

All levels of the database are linked via unique identifiers for each of the evaluations and evaluation sites and are, therefore available for analysis. For reasons of comparability, all the financial data (including earnings, AFDC payments received, median household income, and maximum AFDC payments) have been inflated to year 2000 U.S. dollars.

APPENDIX B
Measurement of Child Outcomes

POSITIVE BEHAVIOR

- MFIP and Iowa: Positive behavior in the focal child is measured by summing parental responses to a 25-item behavior scale that measured three positive behaviors: autonomy, social competence, and compliance. Parents rated the child on a scale of 0 to 10, with 0 indicating that the behavior was not at all like their child and 10 indicating that it was completely like their child. Scores can range from 0–70, with higher scores reflecting more positive behaviors.

- Connecticut's Job First Program and Florida's Family Transition Program: Positive behavior is defined as the percentage of children who scored in the top 25th percentile on a seven-item positive behavior scale developed by Polit (1996) and completed by the focal child's mother. Responses ranged from 0 to 10, with 0 indicating that the behavior was not at all like their child and 10 indicating that it was completely like their child. Scores were summed.

SCHOOL ACHIEVEMENT

- MFIP: Parents reported their child's overall school performance, with 1 indicating not doing well at all and 5 indicating the child is doing "very well" in school.

- Connecticut, FTP, LA GAIN, Vermont, and Iowa: School achievement was defined as the percentage of children performing below average in school.

EVER SUSPENDED OR EXPELLED

- NEWWS, MFIP, LA GAIN, Vermont, and Iowa: The percentage of children suspended or expelled since last interview.

- Connecticut and FTP: The percentage of children suspended since last interview.

BEHAVIORAL OR EMOTIONAL PROBLEMS

- Connecticut and FTP (Young Age and School Age): The percentage of children with serious behavioral problems. This is defined as the percentage of children who scored in the top 25th percentile of the Behavioral Problems Index (BPI), a 28-item scale that measures externalizing and internalizing behavioral problems with responses ranging from 0 (not true of my child) to 2 (often true of child). Scores were summed with higher scores representing more behavior problems.

- Connecticut and FTP (Adolescents): The percentage of children ever found guilty of a crime since random assignment.

- LA GAIN and NEWWS: The percentage of children who ever had special physical, emotional, or mental condition that made their mothers' work or school difficult.

- MFIP and Iowa: The sum of parental responses to the 12-item externalizing scale of the BPI. The range of scores is from 0 to 22.

- Vermont: Parent-reported percentage of children having "trouble" with the police since the last interview.

EVER REPEATED A GRADE

- All evaluations but MFIP: Percentage of children who have repeated a grade since random assignment.

HEALTH

- Connecticut: The percentage of children in "good" health.

- MFIP: Parent responses from 1 (poor) to 5 (very good).

- FTP: The percentage of children in "poor" health.

- Iowa: The percentage of children in "fair" or "poor" health.

APPENDIX TABLES

Table A.1
Regression Models of the Impacts of Welfare-to-Work Programs
Results for AFDC Applicants and Recipients - 7 Quarters after Random Assignment
(Standard errors in parentheses)

	Participation in AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
	Pure Subgroups	Mixed Groups	Pure Subgroups	Mixed Groups	Pure Subgroups	Mixed Groups	Pure Subgroups	Mixed Groups
Constant	-3.314***	-6.852***	-32.707	***	74.605	-24.019	3.008*	2.274***
	(0.71)	(2.43)	(28.76)	(10.67)	(50.14)	(102.41)	(1.52)	(0.71)
Applicant = 1	-1.342**		13.156		-24.565		-1.291**	
	(0.50)		(14.95)		(29.35)		(0.50)	
Proportion of applicants (0 to 1)		0.659		-68.336***		-49.021**		1.562***
		(0.55)		(12.77)		(23.27)		(0.44)
Intervention impact on	0.063	0.116***		3.811***		7.035***	0.158***	0.199***
% sanctioned	(0.04)	(0.04)		(0.52)		(1.53)	(0.04)	(0.03)
Intervention impact on	0.154***	0.128***	1.724**	1.770***	4.078***	1.225*	0.020	0.050***
% participated in job search	(0.02)	(0.02)	(0.75)	(0.31)	(1.27)	(0.70)	(0.02)	(0.01)
Intervention impact on	0.112***	0.078**	3.509***	0.879		0.113	-0.043	-0.005
% participated in basic education	(0.04)	(0.03)	(1.00)	(0.61)		(1.28)	(0.04)	(0.03)
Intervention impact on	0.176*	0.130*	2.472	3.585***	4.622	-1.736	0.281***	0.115*
% participated in vocational education	(0.10)	(0.08)	(3.83)	(1.28)	(4.49)	(2.51)	(0.10)	(0.07)
Intervention impact on		0.004		-2.295**				
% participated in work experience		(0.05)		(1.02)				
Intervention included financial				-93.866***		-43.947***		
incentive=1				(9.14)		(16.25)		
Intervention included time limit=1	2.311***	2.652***	-5.139		157.348***		2.241***	1.151*
	(0.73)	(0.66)	(23.83)		(42.78)		(0.76)	(0.68)
Average age of target group		0.133*				6.006*		
		(0.08)				(3.28)		
% of target group with recent	0.004***	0.000					-0.010	-0.001*
employment	(0.00)	(0.00)					(0.27)	(0.00)
Annual % change in local	-0.033	-0.128**		6.845***		4.350		0.159***
manufacturing employment	(0.07)	0.059		(1.49)		(2.86)		(0.05)
Poverty rate					-9.099**	-7.513***	-0.125**	-0.016***
					(3.39)	1.795	(0.05)	(0.00)
Adjusted R-squared	0.299	0.0327	0.270	0.345	0.288	0.274	0.058	0.193
F-test for contribution of added variables	3.025**	4.354***	3.001**	4.880***	3.102**	4.397***	1.363	3.21***
Number of observations	39	70	28	60	27	82	48	84

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Table A.2
Regression Models of the Impacts of Welfare-to-Work Programs, 7 Quarters after Random Assignment
Results for AFDC Participants Employed and Not Employed in Year Prior to Random Assignment
(Standard errors in parentheses)

	Participation in AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group
Constant	11.103***	-4.678*	-19.114	-91.197**	-34.798	-96.335	16.488***	-5.097**
	(2.52)	(2.54)	(25.73)	(39.14)	(43.37)	(98.56)	(2.44)	(2.37)
Employed in previous year = 1	-0.252		-8.670		-58.960**		-2.203**	
	(0.80)		(12.30)		(25.89)		(0.76)	
Proportion employed in previous		-0.647		-27.634**		-32.553		-0.930
year (0 to 1)		(0.79)		(12.76)		(27.92)		(0.75)
Intervention impact on		0.155***	4.183***	2.301***	7.303***	6.074***		0.200***
% sanctioned		(0.05)	(0.79)	(0.46)	(1.68)	(1.23)		(0.03)
Intervention impact on	-0.217***	0.001	5.093***	3.561***	4.662***	2.823***	-0.306***	0.077***
% participated in job search	(0.06)	(0.02)	(0.73)	(0.32)	(1.35)	(0.70)	(0.05)	(0.02)
Intervention impact on		-0.073*		1.261***		-2.477**		0.005
% participated in basic education		(0.04)		(0.42)		(1.03)		(0.03)
Intervention impact on	-0.571**	0.066	-6.064***	-2.392**	2.670	4.479*		0.056
% participated in vocational education	(0.18)	(0.07)	(2.11)	(0.96)	(3.83)	(2.59)		(0.07)
Intervention impact on		-0.157***				1.462		0.026
% participated in work experience		(0.06)				(2.49)		(0.04)
Intervention included financial incentive=1		-7.115***		-171.466***	230.763***	-15.610		3.134***
		(1.01)		(12.28)	(49.77)	(32.97)		(0.81)
Intervention included time limit=1		3.148***		71.710***				1.777**
		(0.92)		(10.09)				(0.76)
Number of years since 1982	-0.099	-0.132						
	(0.11)	(0.11)						
Average age of target group		0.225**		2.779***		3.644		0.139*
		(0.09)		(1.24)		(3.24)		(0.07)
Annual % change in local		0.003	10.054	8.153***		5.764*	0.291**	0.139**
manufacturing employment		(0.09)	(2.84)	(1.32)		(3.04)	(0.10)	(0.06)
Poverty rate							-0.223**	
							(0.10)	
Max. amount of AFDC for family of 3		(0.004) *						
		(0.00)						
Adjusted R-squared	0.038	0.321	0.660	0.598	0.473	0.364	0.647	0.253
F-test for contribution of added variables	1.110	3.360***	9.913***	12.217***	6.207***	5.646***	6.040**	3.001***
Number of observations	12	61	24	69	30	74	12	60

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Table A.3
Regression Models of the Impacts of Welfare-to-Work Programs, 7 Quarters after Random Assignment
Results for Participants with and without High School Diploma
(Standard errors in parentheses)

	Participation in AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group
Constant	1.507*	-0.266	38.037**	-45.925***	-155.368**	5.849	-1.985**	-0.900
	(0.77)	(1.38)	(15.60)	(10.32)	(59.93)	(26.87)	(0.77)	(1.22)
Has high school diploma=1	-0.033		3.119		55.100*		1.038	
	(0.69)		(9.58)		(29.12)		(0.70)	
Proportion with high school diploma (0 to 1)		-0.118		2.202		15.342		0.680
		(0.68)		(9.56)		(26.82)		(0.70)
Intervention impact on % sanctioned	-0.051	0.096**	3.443***	2.764***	12.466***	6.680***	0.130	0.157***
	(0.07)	(0.04)	(0.59)	(0.44)	(2.03)	(1.18)	(0.07)	(0.04)
Intervention impact on % participated in job search	0.109***	0.010	3.905***	4.412***	6.318***	3.136***	0.237***	0.151***
	(0.03)	(0.02)	(0.49)	(0.30)	(1.49)	(0.74)	(0.03)	(0.02)
Intervention impact on % participated in basic education		-0.010		2.076***	-5.021**	-3.646**		-0.078**
		(0.03)		(0.55)	(2.17)	(1.41)		(0.03)
Intervention impact on % participated in vocational education		-0.073	-10.773***	-0.894	1.538	1.667		0.098
		(0.07)	(1.49)	(0.93)	(4.69)	(2.59)		(0.07)
Intervention impact on % participated in work experience		0.051						-0.055
		(0.05)						(0.04)
Intervention included financial incentive=1		-7.015***		-160.362***		2.964		2.346***
		(0.79)		(10.13)		(28.53)		(0.84)
Intervention included time limit=1		2.589***		72.235***		-6.734		0.424
		(0.79)		(9.56)		(22.37)		(0.84)
Years since 1982								
% of target group with recent employment		-0.009						-0.002
		(0.02)						(0.00)
Annual % change in local manufacturing employment				8.926***	27.334***			
				(1.54)	(6.96)			
Poverty rate								0.031
								(0.06)
Maximum amount of AFDC for family of 3		***						
		(0.00)						
Adjusted R-squared	-0.11	0.249	0.592	0.596	0.553	0.34	0.533	0.44
F-test for contribution of added variables	0.471	2.891***	8.614***	10.041***	5.331***	4.827***	7.076***	5.247***
Number of observations	17	58	22	50	22	53	17	55

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

TABLE A.4

Regression Models of the Impacts of Welfare-to-Work Programs, 7 Quarters after Random Assignment: Results for Long-Term and Short-Term Participants in AFDC

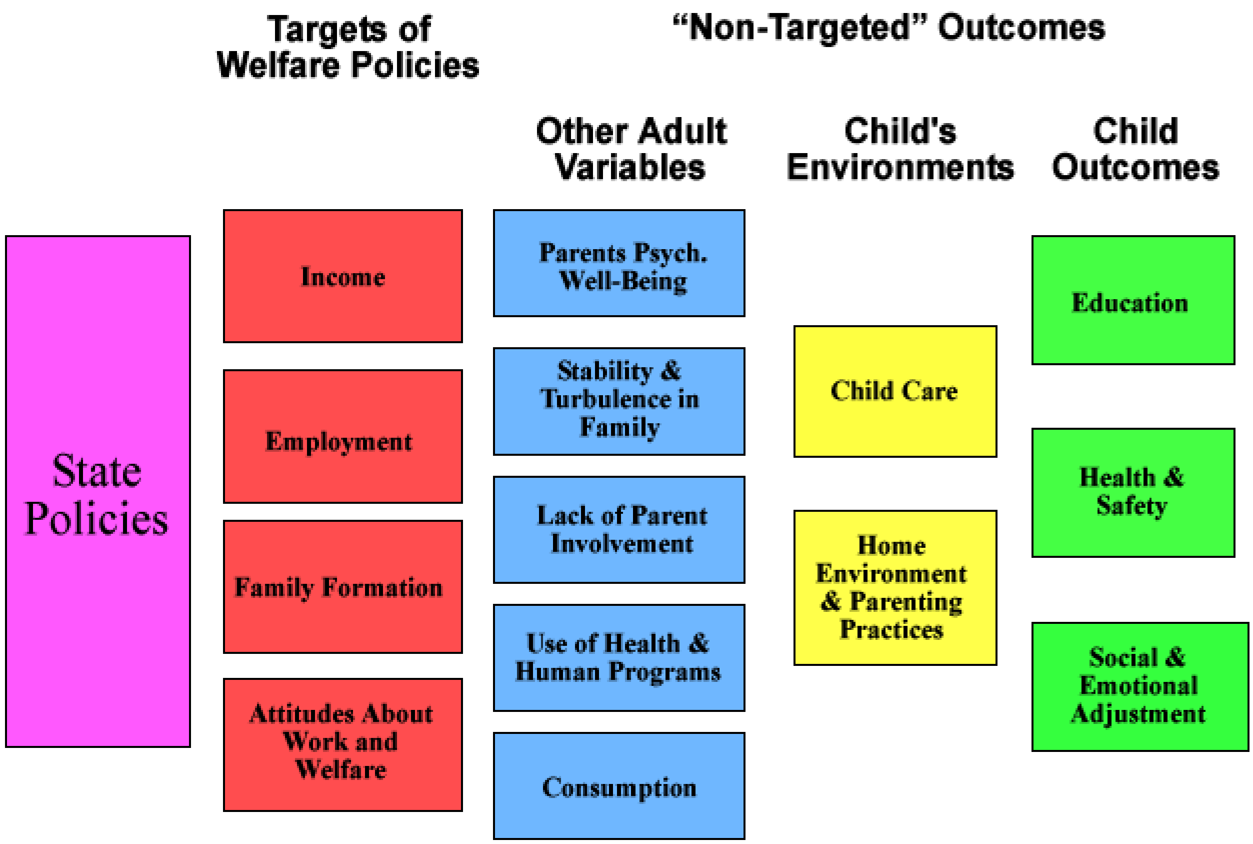
(Standard errors in parentheses)

	Participation in AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group	Pure Subgroup	Mixed Group
Constant	6.884***	-2.669	418.105***	17.215	-94.078	15.584	5.365***	2.103
	(1.41)	(2.04)	(105.84)	(11.04)	(68.15)	(33.08)	(1.48)	(1.45)
Long-term recipient = 1	0.651		29.659***		103.994***		6.305***	
	(0.93)		(10.77)		(24.37)		(1.00)	
Proportion long-term recipients (0 to 1)		2.204**		-42.433***		95.760***		5.408***
		(0.82)		(10.85)		(24.12)		(0.96)
Intervention impact on	-0.798***	0.973***		2.679***	4.746***	5.562**	0.252	0.591***
% sanctioned	(0.25)	(0.19)		(0.66)	(1.62)	(2.09)	(0.28)	(0.19)
Intervention impact on		0.031	5.696***	3.384***	6.660***	4.725***		
% participated in job search		(0.04)	(0.60)	(0.41)	(1.03)	(0.85)		
Intervention impact on			2.121***	-0.071		-4.144***		0.609*
% participated in basic education			(0.67)	(0.54)		(1.25)		(0.35)
Intervention impact on	17.198***			-6.580***	3.172	3.071	4.527**	
% participated in vocational education	(1.54)			(1.23)	(3.70)	(3.11)	(1.70)	
Intervention impact on		-0.296**	-3.006*			0.870	0.347*	-0.032
% participated in work experience		(0.12)	(1.71)			(4.45)	(0.18)	(0.16)
Intervention included financial			-62.002***	-25.556*	1.736	-59.514**		1.993*
incentive=1			(20.07)	(13.12)	(41.51)	(24.27)		(0.98)
Number of years since 1982		0.108						
		(0.17)						
Average age of target group			-14.250***					
			3.578					
Annual % change in local	-0.359*			4.290**		3.857	-1.491***	
manufacturing employment	(0.18)			(1.84)		(3.68)	(0.23)	
Poverty rate					-1.695	-4.537*		-0.378***
					(3.92)	(2.40)		(0.08)
Adjusted R-squared	0.568	-0.070	0.378	0.391	0.577	0.513	0.519	0.242
F-test for contribution of added variables	3.962	0.753	4.036***	5.034***	7.127***	6.615***	2.945	2.013
Number of observations	10	20	31	45	28	49	10	20

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

FIGURE 1

HOW WELFARE POLICIES MIGHT AFFECT CHILDREN



Source: *Welfare Reform and Children: A Synthesis of Impacts in Five States*. 2005. Washington DC: Administration for Children and Families. Retrieved March 17, 2005, from http://www.acf.hhs.gov/programs/opre/welfare_employ/ch_outcomes/reports/welfare_reform_children/welfare_reform_toc.html.

Table 1
U.S. Welfare-to-Work Evaluations Included in the Database

Program Title	Short Program Name	Evaluator/Author	Midpoint of Random Assignment	Financial Incentive Scheme
MANDATORY				
Greater Avenues for Independence Program	GAIN (California)	MDRC	1989	
Job Search and Work Experience in Cook County	Cook County	MDRC	1985	
Community Work Experience Demonstrations	West Virginia	MDRC	1983	
WORK Program	Arkansas	MDRC	1983	
Employment Initiatives	Baltimore	MDRC	1983	
Saturation Work Initiative Model	SWIM (San Diego)	MDRC	1985	
Employment Services Program	Virginia	MDRC	1984	
Project Independence (Florida's JOBS Program)	Florida	MDRC	1991	
Jobs First	Connecticut	MDRC	1996	√
Family Transition Program	FTP (Florida)	MDRC	1994	
Los Angeles Jobs-First GAIN Evaluation	Los Angeles	MDRC	1996	
San Diego Job Search and Work Experience Demonstration	San Diego	MDRC	1983	
National Evaluation of Welfare-to-Work Strategies	NEWWS	MDRC	1993	
Minnesota Family Investment Program	MFIP	MDRC	1994	√
Vermont's Welfare Restructuring Project.	Vermont	MDRC	1995	√
Teenage Parent Demonstration	Teenage Parents	Mathematica Policy Research (MPR)	1988	
Wisconsin Welfare Employment Experiment	Wisconsin	University of Wisconsin	1988	
Ohio Transitions to Independence Demonstration	Ohio	Abt Associates	1990	
Indiana Welfare Reform Program	Indiana	Abt Associates	1995	
Saturation Work Program	Philadelphia	PA Department of Public Welfare	1986	
To Strengthen Michigan Families	TSMF (Michigan)	Abt Associates	1993	
A Better Chance	ABC (Delaware)	Abt Associates	1996	
Virginia Independence Program	VIEW	MPR	1996	
Family Investment Program	FIP(Iowa)	MPR	1994	
Personal Responsibility and Employment Program	PREP (Colorado)	The Centers of the University of Colorado	1995	
Self-Sufficiency First/Pay for Performance Program	SSF/PFP (Wisconsin)	Institute for Research on Poverty, University of WI	1995	
California Work Pays Demonstration Program (financial incentive only)	CWPDP	UCLA School of Public Policy and Social Research	1993	√
Child Assistance Program	CAP (New York)	Abt Associates	1989	√
VOLUNTARY				
Supported Work	SW	MDRC	1976	
Homemaker Health Aide	HHA	Abt Associates	1984	
Training Opportunities in the Private Sector Program	TOPS (Maine)	MDRC	1984	
New Jersey Grant Diversion Project	NJGD	MDRC	1985	
New York State Comprehensive Employment Opportunities Support Centers Program	CEOSC	Abt	1989	
Self-Sufficiency Project	SSP (Canada)	MDRC	1994	√
Self-Sufficiency Project Plus	SSP+ Canada)	SRDC	1985	√

Table 2
Summary Statistics for the Mandatory Program Impact Measures

	Quarter since Random Assignment							
	3		7		11		15	
	Unweighted	**Weighted**	**Unweighted**	**Weighted**	**Unweighted**	**Weighted**	**Unweighted**	**Weighted**
EARNINGS (year 2000 $)								
Mean value for controls	757.7	675.2	1021.1	965.0	1129.8	1132.3	1406.5	1357.4
Mean impact	79.9	73.9	86.8	99.0	111.4	112.2	86.2	114.9
Standard Deviation	64.2	64.0	88.6	97.9	117.6	112.0	112.0	125.6
Median impact	135.5	88.0	164.8	102.1	145.1	103.2	192.1	87.5
Minimum	-672.7	-672.7	-916.8	-916.8	-352.9	-352.9	-543.0	-543.0
Maximum	413.9	413.9	496.7	496.7	467.0	467.0	428.9	428.9
Number of observations	79	79	79	79	56	56	45	45
PERCENTAGE EMPLOYED								
Mean value for controls	31.4	29.3	37.0	34.8	36.7	37.4	39.4	37.4
Mean impact	3.4	3.3	2.6	2.8	4.0	3.6	3.5	2.8
Standard deviation	2.8	2.5	3.3	2.9	3.6	3.5	2.5	2.4
Median impact	3.5	3.7	4.1	3.2	3.0	2.9	3.9	2.8
Minimum	-3.1	-3.1	-10.5	-10.5	-1.2	-1.2	-9.6	-9.6
Maximum	13.0	13.0	10.3	10.3	11.8	11.8	15.9	15.9
Number of observations	68	68	76	76	51	51	53	53
AFDC PAYMENTS (year 2000 $)								
Mean value for controls	1321.6	1032.8	1050.4	803.6	991.2	644.0	669.5	458.0
Mean impact	52.1	37.8	63.9	53.7	80.0	59.4	63.6	75.1
Standard deviation	50.2	38.8	50.3	43.6	73.8	54.7	55.3	69.4
Median impact	140.6	104.0	122.9	99.8	124.8	88.5	62.0	41.4
Minimum	-565.7	-565.7	-406.7	-406.7	-311.2	-311.2	-129.0	-129.0
Maximum	400.1	400.1	325.8	325.8	501.1	501.1	178.2	178.2
Number of observations	74	74	71	71	51	51	44	44
PERCENTAGE RECEIVING AFDC PAYMENTS								
Mean value for controls	75.6	80.9	58.9	60.6	53.5	51.1	42.7	41.0
Mean impact	0.9	1.5	1.0	2.1	1.8	2.6	2.6	4.4
Standard deviation	2.0	2.4	2.1	3.0	2.4	4.0	3.3	4.7
Median impact	4.5	3.6	5.1	4.3	5.3	4.8	3.9	3.2
Minimum	-9.0	-9.0	-15.9	-15.9	-12.7	-12.7	-7.3	-7.3
Maximum	9.3	9.3	11.1	11.1	13.6	13.6	11.5	11.5
Number of observations	64	64	75	75	48	48	45	45

Table 3
Means of Available Explanatory Variables

	Number of Observations	Mean	Standard Deviation
Characteristics of Interventions			
Program impacts on the percentage who:			
Were sanctioned	62	6.37	7.77
Participated in job search	66	21.12	13.95
Participated in basic education	66	7.31	11.90
Participated in vocational education	66	2.81	5.02
Participated in work experience	76	2.86	5.57
Percentage of interventions that included financial incentives	79	31.65	46.81
Percentage of interventions that only provided financial incentives	79	15.19	36.12
Monthly amount of financial incentive after 13 months in a job in year 2000 dollars	79	82.75	135.68
Percentage of interventions that included time limit	79	15.19	36.12
Percentage of interventions targeted at 2-parent families	79	20.25	40.45
Net cost to the government in year 2000 dollars	61	1,853.41	1,711.49
Number of years since 1982 to mid-point of random assignment	79	8.85	3.69
Characteristics of Target Population			
Average age of target group	71	31.02	3.92
Average number of children in family	62	1.94	0.33
Percentage of target population:			
Employed during year before random assignment	63	48.33	17.41
On AFDC for at least 2 years before random assignment	63	46.08	23.92
With either a high school degree or a GED	62	51.50	11.08
Who were under 25 years of age	58	26.64	21.93
With at least one child under 6 years of age	51	50.71	23.56
Who were white	77	41.24	23.36
Who were black	76	36.02	25.96
Who were Hispanic	70	17.24	14.73
Socioeconomic Conditions at Site			
Maximum monthly AFDC payment for a family of 3 in year 2000 dollars	79	602.67	199.43
Median annual household income in year 2000 dollars	79	40,237.38	6,670.37
Poverty rate	79	14.61	4.96
Unemployment rate	79	6.36	2.34
Percentage of workforce in manufacturing employment	79	12.99	5.16
Annual percentage change in manufacturing employment	79	1.11	4.49
Percentage of sites that at first sanction specified minimum length or terminated full family benefits	79	6.33	24.50

Table 4
Regression Models of the Impacts of Welfare-to-Work Programs on Earnings
(Standard errors in parentheses)

	Quarter since Random Assignment							
	3		7		11		15	
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted
Constant	11.644	-126.070	3.714	-5.523	-1.695	217.358	-62.258	-128.846
	(30.80)	(143.27)	(41.62)	(141.25)	(47.61)	(149.34)	(78.61)	(297.33)
Intervention impact on	1.340	2.426*	4.586***	6.337***	2.217	-0.652	0.085	0.713
% sanctioned	(1.17)	(1.44)	(1.43)	(1.92)	(1.53)	(2.20)	(2.01)	(3.15)
Intervention impact on	2.395***	1.924**	2.280***	1.711**	3.582***	3.486***	2.025	1.909
% participated in job search	(0.77)	(0.81)	(0.80)	(0.85)	(0.97)	(1.05)	(1.42)	(1.59)
Intervention impact on	-0.818	-1.859	-0.688	-1.873	0.967	1.373	-1.266	-1.795
% participated in basic education	(1.09)	(1.18)	(1.11)	(1.24)	(1.13)	(1.25)	(1.90)	(2.35)
Intervention impact on	-6.276**	-4.889*	-2.069	1.579	-6.791**	-4.542	6.403	7.335
% participated in vocational education	(2.58)	(2.75)	(2.63)	(2.88)	(3.09)	(3.36)	(4.88)	(6.01)
Intervention impact on	0.583	-1.238	0.919	-2.763	6.530*	6.487*	4.212	1.826
% participated in work experience	(2.08)	(2.25)	(2.45)	(2.74)	(3.32)	(3.74)	(5.24)	(7.06)
Intervention included financial incentive=1	-63.761***	-46.481**	-37.343*	-44.170*	-12.477	-20.880	27.623	16.641
	(19.53)	(21.31)	(21.13)	(23.06)	(23.02)	(25.70)	(34.93)	(39.02)
Intervention included time limit=1	-3.916	-5.517	10.019	27.207	43.370	68.010**	-5.058	5.527
	(23.63)	(25.47)	(25.88)	(31.26)	(27.88)	(32.95)	(38.84)	(46.82)
Number of years since 1982	4.431	0.497	2.933	-1.452	2.845	5.583	10.209	8.677
	(2.74)	(3.40)	(3.49)	(4.22)	(3.99)	(5.15)	(6.76)	(8.15)
Two-parent family target group=1	62.691*	40.910	35.280	38.581	-54.159	-3.799	38.792	18.250
	(34.25)	(40.40)	(33.15)	(39.22)	(34.60)	(40.94)	(50.73)	(72.87)
Average age of target group		4.694		5.413		-2.256		4.455
		(4.20)		(4.04)		(4.33)		(7.42)
% of target group with recent employment		0.425		-1.041		-3.147***		-0.424
		(0.85)		(0.87)		(0.91)		(1.53)
Annual % change in local		7.636***		7.693**		-2.617		-2.444
manufacturing employment		(2.82)		(3.15)		(3.36)		(4.90)
Poverty rate		0.943		-4.774**		-1.087		-2.037
		(2.19)		(2.26)		(2.72)		(5.10)
Adjusted R-squared	0.349	0.386	0.201	0.239	0.228	0.279	0.011	-0.075
F-test for regression	5.640***		3.179***		2.802***		1.054	
F-test for contribution of added variables		2.057*		1.855		1.825		0.299
Number of observations	79	79	79	79	56	56	45	45

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level

Table 5
Regression Models of the Impacts of Welfare-to-Work Programs on Percentage Employed
(Standard errors in parentheses)

	Quarter since Random Assignment							
	3		7		11		15	
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted
Constant	-2.059**	-3.983	-1.519	-5.193*	-0.761	9.641**	-3.330	-1.868
	(0.79)	(2.74)	(0.92)	(3.08)	(1.43)	(3.78)	(2.09)	(5.03)
Intervention impact on	0.009	0.060*	0.106***	0.180***	0.029	-0.083	-0.036	-0.030
% sanctioned	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.04)	(0.06)
Intervention impact on	0.129***	0.098***	0.084***	0.068***	0.124***	0.134***	0.148***	0.142***
% participated in job search	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
Intervention impact on	0.013	-0.040	0.035	-0.014	0.043	0.047	0.069**	0.048
% participated in basic education	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Intervention impact on	-0.372***	-0.314***	-0.162**	-0.047	-0.202**	-0.125	-0.004	0.006
% participated in vocational education	(0.06)	(0.07)	(0.06)	(0.07)	(0.08)	(0.08)	(0.12)	(0.13)
Intervention impact on	0.071*	0.025	0.074*	-0.037	0.087	0.091	-0.155	-0.201
% participated in work experience	(0.04)	(0.04)	(0.04)	(0.05)	(0.07)	(0.08)	(0.11)	(0.12)
Intervention included financial incentive=1	-1.189**	-0.741	-0.198	-0.097	-0.204	-0.786	-0.791	-0.718
	(0.48)	(0.51)	(0.52)	(0.56)	(0.63)	(0.67)	(0.87)	(0.82)
Intervention included time limit=1	0.038	-0.303	1.060	1.368	3.301***	3.283***	3.791***	3.788***
	(0.66)	(0.70)	(0.74)	(0.83)	(0.83)	(0.93)	(0.99)	(1.12)
Number of years since 1982	0.406***	0.206**	0.204***	0.051	0.120	0.228	0.304*	0.276
	(0.07)	(0.09)	(0.08)	(0.10)	(0.11)	(0.14)	(0.16)	(0.18)
Two-parent family target group=1	-0.966	-1.696**	-1.782***	-2.146***	0.457	2.197**	0.113	0.783
	(0.63)	(0.76)	(0.65)	(0.80)	(0.73)	(0.86)	(0.92)	(1.03)
Average age of target group		0.146*		0.247***		-0.154		0.069
		(0.08)		(0.09)		(0.10)		(0.12)
% of target group with recent employment		0.013		-0.014		-0.101***		-0.037
		(0.02)		(0.02)		(0.02)		(0.03)
Annual % change in local		0.270***		0.129*		-0.093		-0.060
manufacturing employment		(0.06)		(0.07)		(0.07)		(0.08)
Poverty rate		-0.046		-0.129**		-0.097		-0.092
		(0.05)		(0.05)		(0.06)		(0.08)
Adjusted R-squared	0.355	0.403	0.191	0.221	0.328	0.493	0.261	0.279
F-test for regression	5.102***		2.967***		3.715***		3.037***	
F-test for contribution of added variables		2.158*		1.636		4.330***		1.274
Number of observations	68	68	76	76	51	51	53	53

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Table 6
Regression Models of the Impacts of Welfare-to-Work Programs on Amount of AFDC Payment
(Standard errors in parentheses)

	Quarter Since Random Assignment							
	3		7		11		15	
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted
Constant	29.429*	119.895**	-20.133	-177.367***	-174.792***	-203.292***	-36.465	-33.129
	(16.66)	(59.75)	(18.87)	(58.87)	(26.77)	(65.24)	(28.12)	(100.44)
Intervention impact on	4.111***	3.261***	3.692***	5.842***	0.916*	3.290***	-0.125	0.672
% sanctioned	(0.56)	(0.66)	(0.58)	(0.83)	(0.53)	(1.01)	(0.84)	(1.17)
Intervention impact on	1.406***	0.973**	2.701***	1.093***	4.183***	1.945***	2.928***	1.437*
% participated in job search	(0.33)	(0.37)	(0.34)	(0.39)	(0.40)	(0.51)	(0.60)	(0.85)
Intervention impact on	1.673***	1.143*	2.069***	-0.947	3.151***	-0.557	1.700**	1.251
% participated in basic education	(0.51)	(0.60)	(0.51)	(0.64)	(0.54)	(0.74)	(0.68)	(0.90)
Intervention impact on	-3.393***	0.125	-1.988*	4.303***	-1.694	4.369***	-1.295	-1.376
% participated in vocational education	(1.08)	(1.28)	(1.05)	(1.28)	(1.19)	(1.46)	(1.90)	(2.36)
Intervention impact on	-1.640	-0.840	-1.294	-4.835***	1.865	-1.958	-1.897	-1.278
% participated in work experience	(1.11)	(1.22)	(1.04)	(1.30)	(1.28)	(1.61)	(2.18)	(2.97)
Intervention included financial incentive=1	-118.585***	-138.649***	-84.248***	-111.666***	-78.413***	-107.303***	-20.716	-26.539
	(8.82)	(9.87)	(8.38)	(9.80)	(9.93)	(11.98)	(16.43)	(19.77)
Intervention included time limit=1	18.760*	31.165***	32.241***	64.701***	43.899***	73.533***	38.507**	55.726***
	(10.43)	(11.19)	(11.23)	(13.10)	(11.36)	(13.68)	(15.69)	(18.70)
Number of years since 1982	-1.840	-0.250	0.558	-4.144*	13.353***	5.778**	5.826**	2.687
	(1.42)	(1.62)	(1.62)	(2.17)	(2.02)	(2.86)	(2.32)	(2.74)
Two-parent family target group=1	61.322**	74.592***	46.902**	4.649	7.911	-16.586	-97.158***	-81.327**
	(24.21)	(25.51)	(20.56)	(23.10)	(23.12)	(25.18)	(33.06)	(38.10)
Average age of target group		-3.213		5.065***		1.958		0.998
		(2.00)		(1.82)		(1.82)		(2.93)
% of target group with recent employment		-1.354***		-0.483		-1.009**		-0.709
		(0.31)		(0.36)		(0.39)		(0.62)
Annual % change in local		-0.572		6.925***		5.122***		4.672**
manufacturing employment		(1.21)		(1.54)		(1.50)		(1.88)
Maximum AFDC payment for a family of 3		0.116***		0.175***		0.247***		0.085
		(0.03)		(0.03)		(0.04)		(0.06)
Adjusted R-squared	0.376	0.376	0.329	0.407	0.387	0.472	0.473	0.589
F-test for regression	5.880***		4.819***		4.508***		5.283***	
F-test for contribution of added variables		1.017		2.994**		2.650**		3.416**
Number of observations	74	74	71	71	51	51	44	44

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Table 7
Regression Models of the Impacts of Welfare-to-Work Programs on Percentage Participating in AFDC
(Standard errors in parentheses)

	Quarter Since Random Assignment							
	3		7		11		15	
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted
Constant	3.877***	5.159**	2.810***	-1.755	1.846	4.321	-1.825	0.360
	(0.75)	(2.33)	(0.85)	(3.08)	(1.32)	(4.07)	(1.65)	(5.78)
Intervention impact on	0.075***	0.072**	0.117***	0.181***	0.103***	0.107*	0.060	0.071
% sanctioned	(0.02)	(0.03)	(0.03)	(0.05)	(0.04)	(0.06)	(0.05)	(0.08)
Intervention impact on	0.003	-0.001	0.057***	0.026	0.085***	0.053*	0.054	0.066*
% participated in job search	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)
Intervention impact on	-0.021	-0.011	-0.019	-0.095***	-0.008	-0.055	0.063*	0.056
% participated in basic education	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)
Intervention impact on	-0.085	-0.125**	0.002	0.131*	-0.155**	-0.078	-0.194*	-0.229*
% participated in vocational education	(0.05)	(0.06)	(0.06)	(0.07)	(0.08)	(0.08)	(0.10)	(0.13)
Intervention impact on	-0.074*	-0.056	-0.074	-0.157***	0.031	-0.042	0.136	0.074
% participated in work experience	(0.04)	(0.05)	(0.05)	(0.06)	(0.08)	(0.09)	(0.11)	(0.16)
Intervention included financial incentive=1	-6.006***	-6.262***	-5.424***	-5.52***7	-5.703***	-5.963***	-0.452	-0.197
	(0.46)	(0.50)	(0.55)	(0.63)	(0.70)	(0.82)	(0.84)	(1.10)
Intervention included time limit=1	1.121*	0.835	2.043***	3.301***	4.659***	6.194***	1.884*	2.974**
	(0.62)	(0.64)	(0.74)	(0.81)	(0.84)	(0.93)	(0.99)	(1.42)
Number of years since 1982	-0.109*	-0.112	-0.110	-0.249**	-0.032	-0.196	0.438***	0.328*
	(0.06)	(0.08)	(0.07)	(0.10)	(0.11)	(0.15)	(0.14)	(0.18)
Two-parent family target group=1	-1.664***	-1.783**	-2.660***	-3.722***	-5.044***	-4.601***	-4.843***	-3.638**
	(0.56)	(0.69)	(0.68)	(0.88)	(0.82)	(1.04)	(1.17)	(1.70)
Average age of target group		-0.052		0.159		0.025		0.057
		(0.07)		(0.10)		(0.11)		(0.16)
% of target group with recent employment		0.021		-0.021		-0.07***0		-0.045
		(0.01)		(0.02)		(0.02)		(0.04)
Annual % change in local		0.050		0.202***		0.190**		0.006
manufacturing employment		(0.06)		(0.07)		(0.09)		(0.10)
Maximum AFDC payment for a family of 3		-0.001		0.004**		0.004*		-0.002
		(0.00)		(0.00)		(0.00)		(0.00)
Adjusted R-squared	0.444	0.418	0.425	0.447	0.469	0.494	0.456	0.456
F-test for regression	6.593***		7.086***		5.605***		5.095***	
F-test for contribution of added variables		0.388		1.626		1.475		1.009
Number of observations	64	64	75	75	48	48	45	45

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Table 8
Sensitivity Tests of Regression Models of the Impacts on Earnings (7th Quarter)
(Standard errors in parentheses)

	[1]	[2]	[3]	[4]	[5]
Constant	-5.523	180.587	-54.502	92.961	-2.906
	(141.25)	(167.48)	(141.93)	(188.81)	(149.25)
Intervention impact on	6.337***	6.170***	6.608***	3.328	6.773***
% sanctioned	(1.92)	(2.29)	(1.94)	(2.85)	(2.13)
Intervention impact on	1.711**	1.510	1.610*	3.141**	1.701*
% participated in job search	(0.85)	(1.01)	(0.85)	(1.23)	(0.92)
Intervention impact on	-1.873	-0.686	-2.221*	0.053	-1.997
% participated in basic education	(1.24)	(1.47)	(1.25)	(1.68)	(1.33)
Intervention impact on	1.579	-0.370	0.937	-0.924	1.906
% participated in vocational education	(2.88)	(3.42)	(2.89)	(3.46)	(3.09)
Intervention impact on	-2.763	-3.945	-2.965	0.118	-3.153
% participated in work experience	(2.74)	(3.25)	(2.75)	(3.79)	(2.89)
Intervention included financial incentive=1	-44.170*	-29.174	-45.387*	-147.741**	-38.148
	(23.06)	(27.31)	(23.14)	(64.80)	(24.09)
Intervention included time limit=1	27.207	-10.960	27.091	109.571	32.577
	(31.26)	(36.96)	(31.32)	(66.05)	(33.49)
Number of years since 1982	-1.452	6.167	-2.703	9.926	-3.297
	(4.22)	(5.02)	(4.25)	(6.97)	(4.42)
Two-parent family target group=1	38.581	19.958	-3.928	6.894	36.847
	(39.22)	(48.52)	(41.12)	(64.82)	(40.66)
Average age of target group	5.413	1.674	6.990*	-0.260	6.219
	(4.04)	(4.80)	(4.07)	(5.45)	(4.29)
% of target group with recent employment	-1.041	-2.320**	-0.705	-1.258	-1.101
	(0.87)	(1.03)	(0.87)	(1.16)	(0.91)
Annual % change in local	7.693**	7.185*	7.382**	5.514	8.265**
manufacturing employment	(3.15)	(3.76)	(3.19)	(4.74)	(3.32)
Poverty rate	-4.774**	-9.846***	-4.486*	-6.291*	-5.310**
	(2.26)	(2.67)	(2.26)	(3.71)	(2.35)
Adjusted R-squared	0.239	0.089	0.287	0.381	0.238
Number of observations	79	79	75	53	79

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Legend:
[1] Original estimates,
[2] Unweighted estimates,
[3] Observations with two highest and lowest impacts omitted,
[4] Observations with missing data omitted,
[5] Observation using same controls given half weight.

Table 9
Sensitivity Tests of Regression Models of the Impacts on Percentage Employed (7th Quarter)
(Standard errors in parentheses)

	[1]	[2]	[3]	[4]	[5]
Constant	-5.193*	-1.326	-3.748	-4.578	-5.470
	(3.08)	(7.37)	(3.11)	(3.63)	(3.32)
Intervention impact on	0.180***	0.207**	0.159***	0.111**	0.217***
% sanctioned	(0.04)	(0.10)	(0.04)	(0.05)	(0.05)
Intervention impact on	0.068***	0.064	0.059***	0.100***	0.072***
% participated in job search	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)
Intervention impact on	-0.014	0.021	-0.024	0.028	-0.034
% participated in basic education	(0.03)	(0.06)	(0.03)	(0.03)	(0.03)
Intervention impact on	-0.047	-0.099	-0.011	-0.117	-0.008
% participated in vocational education	(0.07)	(0.16)	(0.07)	(0.08)	(0.08)
Intervention impact on	-0.037	-0.002	-0.009	0.048	-0.077
% participated in work experience	(0.05)	(0.12)	(0.05)	(0.06)	(0.06)
Intervention included financial incentive=1	-0.097	-0.083	-0.132	0.972	-0.099
	(0.56)	(1.41)	(0.56)	(1.30)	(0.59)
Intervention included time limit=1	1.368	2.321	0.824	1.861	1.894**
	(0.83)	(1.56)	(0.84)	(1.41)	(0.90)
Number of years since 1982	0.051	0.011	0.113	0.262**	-0.055
	(0.10)	(0.20)	(0.10)	(0.13)	(0.11)
Two-parent family target group=1	-2.146***	-1.451	-1.588*	-1.614	-1.728**
	(0.80)	(1.45)	(0.81)	(1.07)	(0.82)
Average age of target group	0.247***	0.163	0.197**	0.105	0.290***
	(0.09)	(0.21)	(0.09)	(0.10)	(0.10)
% of target group with recent employment	-0.014	-0.047	-0.022	-0.012	-0.016
	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)
Annual % change in local	0.129*	0.181	0.096	-0.021	0.180**
manufacturing employment	(0.07)	(0.14)	(0.07)	(0.09)	(0.08)
Poverty rate	-0.129**	-0.129	-0.125**	-0.033	-0.145***
	(0.05)	(0.10)	(0.05)	(0.08)	(0.05)
Adjusted R-squared	0.221	0.107	0.208	0.260	0.290
Number of observations	76	76	72	56	76

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.
Legend:
[1] Original estimates,
[2] Unweighted estimates,
[3] Observations with two highest and lowest impacts omitted,
[4] Observations with missing data omitted,
[5] Observation using same controls given half weight.

Table 10
Sensitivity Tests of Regression Models of the Impacts on Amount of AFDC Payments
(7th Quarter)
(Standard errors in parentheses)

	[1]	[2]	[3]	[4]	[5]
Constant	-177.367***	-94.451	-22.656	89.217	-126.368**
	(58.87)	(206.03)	(58.96)	(68.64)	(62.27)
Intervention impact on	5.842***	4.724	2.961***	0.887	6.208***
% sanctioned	(0.83)	(2.89)	(0.87)	(1.11)	(0.94)
Intervention impact on	1.093***	3.021**	1.801***	1.835***	1.690***
% participated in job search	(0.39)	(1.33)	(0.37)	(0.45)	(0.40)
Intervention impact on	-0.947	1.718	1.317**	1.497**	-0.024
% participated in basic education	(0.64)	(1.55)	(0.61)	(0.69)	(0.67)
Intervention impact on	4.303***	0.166	-1.385	-2.129	1.766
% participated in vocational education	(1.28)	(3.73)	(1.26)	(1.34)	(1.33)
Intervention impact on	-4.835***	-0.749	-2.033	-1.591	-5.838***
% participated in work experience	(1.30)	(3.23)	(1.33)	(1.47)	(1.40)
Intervention included financial incentive=1	-111.666***	-54.605	-82.015***	-353.025***	-76.981***
	(9.80)	(41.12)	(9.37)	(23.44)	(9.50)
Intervention included time limit=1	64.701***	25.285	31.286**	261.099***	48.564***
	(13.10)	(53.23)	(14.18)	(23.78)	(14.88)
Number of years since 1982	-4.144*	3.687	-1.185	5.419*	-7.036***
	(2.17)	(5.86)	(2.16)	(2.75)	(2.29)
Two-parent family target group=1	4.649	58.552	28.321	40.532	37.972
	(23.10)	(43.11)	(23.41)	(31.99)	(24.98)
Average age of target group	5.065***	1.519	3.943**	1.803	8.166***
	(1.82)	(5.71)	(1.81)	(2.07)	(1.91)
% of target group with recent employment	-0.483	-0.796	-0.769**	-0.911**	-0.503
	(0.36)	(1.16)	(0.38)	(0.43)	(0.40)
Annual % change in local	6.925***	2.225	5.124***	3.349*	7.261***
manufacturing employment	(1.54)	(3.60)	(1.54)	(1.84)	(1.63)
Maximum AFDC payment for a family of 3	0.175***	0.092	-2.420**	-6.988***	-2.353**
	(0.03)	(3.31)	(1.02)	(1.28)	(1.08)
Adjusted R-squared	0.407	0.160	0.405	0.733	0.324
Number of observations	71	71	67	56	71

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.
Legend:
[1] Original estimates,
[2] Unweighted estimates,
[3] Observations with two highest and lowest impacts omitted,
[4] Observations with missing data omitted,
[5] Observation using same controls given half weight.

Table 11
Sensitivity Tests of Regression Models of the Impacts on Percentage Participating in AFDC
(7th Quarter)
(Standard errors in parentheses)

	[1]	[2]	[3]	[4]	[5]
Constant	-1.755	-1.961	-1.259	5.095	-2.564
	(3.08)	(8.31)	(3.36)	(3.59)	(2.76)
Intervention impact on	0.181***	0.141	0.173***	0.014	0.190***
% sanctioned	(0.05)	(0.11)	(0.05)	(0.05)	(0.04)
Intervention impact on	0.026	0.053	0.031	0.016	0.042**
% participated in job search	(0.02)	(0.05)	(0.02)	(0.02)	(0.02)
Intervention impact on	-0.095***	-0.057	-0.058**	-0.019	-0.076***
% participated in basic education	(0.03)	(0.07)	(0.03)	(0.03)	(0.02)
Intervention impact on	0.131*	-0.021	0.067	-0.048	0.077
% participated in vocational education	(0.07)	(0.17)	(0.07)	(0.08)	(0.06)
Intervention impact on	-0.157***	-0.091	-0.180***	0.050	-0.183***
% participated in work experience	(0.06)	(0.13)	(0.06)	(0.07)	(0.05)
Intervention included financial incentive=1	-5.527***	-4.970***	-4.114***	-14.738***	-4.406***
	(0.63)	(1.67)	(0.65)	(1.28)	(0.54)
Intervention included time limit=1	3.301***	3.879**	3.157***	11.922***	3.034***
	(0.81)	(1.75)	(0.85)	(1.38)	(0.71)
Number of years since 1982	-0.249**	-0.165	-0.296***	0.262**	-0.331***
	(0.10)	(0.21)	(0.10)	(0.13)	(0.09)
Two-parent family target group=1	-3.722***	-4.133**	-2.269**	-1.338	-3.748***
	(0.88)	(1.76)	(0.99)	(0.98)	(0.75)
Average age of target group	0.159	0.145	0.218**	-0.009	0.265***
	(0.10)	(0.23)	(0.10)	(0.10)	(0.08)
% of target group with recent employment	-0.021	0.007	-0.022	-0.008	-0.009
	(0.02)	(0.05)	(0.02)	(0.02)	(0.02)
Annual % change in local	0.202***	0.030	0.176**	-0.030	0.196***
manufacturing employment	(0.07)	(0.15)	(0.07)	(0.09)	(0.06)
Maximum AFDC payment for a family of 3	0.004**	0.049	-0.008	-0.200**	-0.034
	(0.00)	(0.12)	(0.06)	(0.08)	(0.05)
Adjusted R-squared	0.447	0.311	0.398	0.691	0.382
Number of observations	75	75	70	57	75

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.
Legend:
[1] Original estimates,
[2] Unweighted estimates,
[3] Observations with two highest and lowest impacts omitted,
[4] Observations with missing data omitted,
[5] Observation using same controls given half weight.

Table 12
Comparison of Estimated Impacts and Regression-Predicted Impacts for the 7th Quarter

Randomly Selected Interventions

Case	Earnings: Estimated Impact	Earnings: Predicted Impact	Earnings: Absolute Difference	% Employed: Estimated Impact	% Employed: Predicted Impact	% Employed: Absolute Difference	Average AFDC Payments: Estimated Impact	Average AFDC Payments: Predicted Impact	Average AFDC Payments: Absolute Difference	% Receiving AFDC: Estimated Impact	% Receiving AFDC: Predicted Impact	% Receiving AFDC: Absolute Difference
1	2.57	89.06	86.49	2.08	1.79	0.29	14.23	-97.99	112.22	5.30	-3.99	9.29
2	404.90	142.21	262.69	3.80	6.35	2.55	-44.56	29.56	74.11	-0.20	0.92	1.12
3	-77.51	80.28	157.78	-2.70	1.18	3.88	9.01	253.61	244.60	5.40	-1.23	6.63
4	-3.96	36.27	40.23	-0.40	2.95	3.35	275.14	-26.28	301.42	0.50	3.59	3.09
5	211.26	214.00	2.74	5.30	0.69	4.61	9.97	38.75	28.78	-1.50	2.35	3.85
6	-53.38	-40.80	12.58	-4.80	-2.27	2.53	62.71	16.85	45.86	3.40	4.31	0.91
7	90.04	172.68	82.64	2.80	3.24	0.44	-2.79	178.27	181.06	-4.30	-2.65	1.65
8	150.08	-42.37	192.46	4.80	2.85	1.95	175.25	100.52	74.74	2.40	4.50	2.10
9	31.50	-24.68	56.18	1.00	2.74	1.74	108.99	86.39	22.60	5.30	3.56	1.74
10	-96.74	0.61	97.35	10.20	4.24	5.96	129.45	199.10	69.65	3.80	4.45	0.65
Average	65.88	62.72	99.11	2.21	2.37	2.73	73.74	77.88	115.50	2.01	1.58	3.10

Most Recent Interventions

Case	Earnings: Estimated Impact	Earnings: Predicted Impact	Earnings: Absolute Difference	% Employed: Estimated Impact	% Employed: Predicted Impact	% Employed: Absolute Difference	Average AFDC Payments: Estimated Impact	Average AFDC Payments: Predicted Impact	Average AFDC Payments: Absolute Difference	% Receiving AFDC: Estimated Impact	% Receiving AFDC: Predicted Impact	% Receiving AFDC: Absolute Difference
A	93.17	109.41	16.24	8.10	3.95	4.15	64.90	-20.49	85.40	5.40	-2.21	7.61
B	255.43	62.87	192.56	7.60	3.72	3.88	49.20	6.57	42.63	5.70	-1.40	7.10
C	211.26	248.60	37.35	6.30	6.53	0.23	129.45	200.13	70.69	4.70	5.84	1.14
D	277.54	321.02	43.48	7.20	3.38	3.82	181.23	188.14	6.91	4.60	2.98	1.62
E	-18.91	152.01	170.92	-0.10	5.31	5.41	50.83	62.92	12.09	0.80	2.64	1.84
Average	163.70	178.78	92.11	5.82	4.58	3.50	95.12	87.45	43.54	4.24	1.57	3.86

Table 13
The Impact of Welfare-to-Work over Time

	Control Variables							
	Earnings		% Employed		AFDC Payments		% Receiving AFDC	
	Excluded	Included	Excluded	Included	Excluded	Included	Excluded	Included
Constant	16.189***	-106.468***	1.895***	-3.362***	-11.119***	-155.903***	-0.460***	-1.261*
	(5.69)	(33.37)	(0.15)	(0.86)	(2.82)	(14.14)	(0.11)	(0.74)
Number of quarters	16.706***	18.207***	0.379***	0.323***	12.364***	13.124***	0.471***	0.564***
	(1.43)	(1.37)	(0.04)	(0.04)	(0.65)	(0.65)	(0.03)	(0.03)
Number of quarters squared	-0.679***	-0.766***	-0.021***	-0.019***	-0.473***	-0.614***	-0.014***	-0.021***
	(0.07)	(0.07)	(0.00)	(0.00)	(0.03)	(0.03)	(0.00)	(0.00)
Adjusted R-squared	0.078	0.361	0.043	0.369	0.048	0.484	0.112	0.464
Number of observations	857	857	875	875	857	857	906	906
Quarter of peak growth	12.3	11.9	8.9	8.4	13.1	10.7	17.1	13.6
Quarter at which zero impact reached	25.5	24.4	21.9	21.9	25.2	21.2	33.1	26.2

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Table 14
Number of Quarterly Impact Estimates That Are Outliers, Quarters 3, 7, 11 and 15

				Type of Outliers		
	Total	Not Outliers	Outliers	Only Type A	Only Type B	Both Types A & B
Earnings	259	188	71	23	14	34
Percentage employed	248	123	125	53	25	47
Mean AFDC payment	240	153	87	37	19	31
Percentage receiving AFDC	232	115	117	44	26	47

Legend: → Type A = Outlier before controlling for explanatory variables. Type B = Outlier after controlling for explanatory variables.

Table 15
Multiple Outlier Interventions, Quarters 3, 7, 11, and 15

		Interventions with Multiple Outliers			
		Number of Interventions with at Least Two Outliers		Number of Outliers Accounted for by Interventions with at Least Two Outliers	
	All Interventions	Any Type	Type B[a]	Any Type	Type B[a]
Earnings	99	17	9	42	23
Percentage employed	87	42	21	99	46
Mean AFDC payment	90	27	15	69	35
Percentage receiving AFDC	84	32	20	83	46

[a]Includes some cases that are also outliers under Type A.
Legend: → Type A = Outlier before controlling for explanatory variables. Type B = Outlier after controlling for explanatory variables.

TABLE 16
Welfare-To-Work Interventions with Multiple Positive Outliers, by Type and Impact

Program Title	Site/Intervention	Earnings		% Employed		AFDC Payments		% Receiving AFDC	
		Type A	Type B	Type A	Type B	Type A	Type B	Type A	Type B
One-Parent Family Programs									
Greater Avenues for Independence Program	Butte					(1)	2		
	Riverside	3	3	4	3	4	(1)	2	
WORK Program (Arkansas)	Jefferson & Pulaski S				2			3	(1)
Employment Initiatives (Baltimore)					3				
Saturation Work Initiative Model (San Diego)				3		2		2	
Jobs First (Connecticut)	Manchester			2				2	(1)
	New Haven			2	(1)			2	(1)
Family Transition Program (Florida)				2				2	(1)
Los Angeles Jobs-First GAIN Evaluation		2		3					
San Diego Job Search and Work Experience Demonstration	Job search & work experience	2	(1)						
National Evaluation of Welfare-to-Work Strategies (NEWWS)	Grand Rapids -LFA			2	(1)	2	2	3	2
	Riverside-LFA			2	(1)	3	(1)	3	
	Riverside: HCD					2		3	
	Portland (OR)	3	(1)	3	2	3		4	3
	Columbus-Integrated case mgt							4	(1)
Minnesota Family Investment Program	Urban—services & incentives			2	(1)				
Vermont Welfare Restructuring Project	Rutland—services & incentives			2	2				
California Work Pays Demonstration	Alameda	(1)	2						
	San Bernardino					(1)	2		
New York State Child Assistance Program	Monroe	(1)	3						3
	Niagara	(1)	2						2
	Suffolk								2
Two-Parent Family Programs									
Greater Avenues for Independence Program	Butte	3	3	2		(1)	2		2
	Los Angeles			4	4				
	Riverside			(1)	2	2	(1)		
	San Diego					2			
Saturation Work Initiative Model (San Diego)						3	(1)		
Los Angeles Jobs-First GAIN Evaluation		3		3		3			
San Diego Job Search and Work Experience Demonstration	Job search only							2	(1)
Vermont Welfare Restructuring Project	Combined areas—incentives only	2	2						
California Work Pays Demonstration	Alameda					3	3		
	San Bernardino	3	3	2	2	2	2		

Legend: → Type A = Outlier before controlling for explanatory variables. Type B = Outlier after controlling for explanatory variables.
LFA - Labor Force Attachment. HCD - Human Capital Development.
Note: Positive outliers are defined as one standard deviation or more above the mean impacts in quarters 3, 7, 11, and 15.

Table 17
Welfare-to-Work Interventions with Multiple Negative Outliers, by Type and Impact

Program Title	Site/Intervention	Earnings		% Employed		AFDC Payment		% Receiving AFDC	
		Type A	Type B	Type A	Type B	Type A	Type B	Type A	Type B
One-Parent Family Programs									
Greater Avenues for Independence Program (GAIN)	Butte							(1)	2
	Tulare			2	(1)		2		
Employment Services Program (Virginia)					2		2		2
San Diego Job Search and Work Experience Demonstration	Job search only				2				
Project Independence (Florida)	Duval	2	(1)						
National Evaluation of Welfare-to-Work Strategies (NEWWS)	Detroit			2					
	Oklahoma City			4					
Minnesota Family Investment Program (MFIP)	Urban - service & incentives					3	3	3	3
	Urban - incentives only	2				3	3	3	3
	Rural - services & incentives					2	2	2	2
Vermont Welfare Restructuring Project	Barre—Incentives only							2	2
	Burlington - incentives only			2	(1)				
	Newport - services & incentives			(1)	2				
	Newport—Incentives only			2	2				
Teenage Parent Demonstration	Newark			2					
Indiana Welfare Reform Program	Cohort—1998-99				2				
California Work Pays Demonstration	Los Angeles	2	(1)	2	(1)				
New York State Child Assistance Program	Niagara							2	(1)
Two-Parent Family Programs									
Greater Avenues for Independence Program (GAIN)	Alameda					(1)	2	2	2
	San Diego			2	(1)				
	Tulare	1	2				3	2	(1)
Employment Initiatives (Baltimore)		2	2	2		2	2		
The San Diego Job Search and Work Experience Demonstration	Job search & work experience			(1)	2				
	Job search only			2	2				
Minnesota Family Investment Program (MFIP)	Urban—services & incentives							2	2
	Urban + rural—services & incentives			3	(1)			3	3
Vermont Welfare Restructuring Project	Combined areas—services & incentives			2	2	2		4	2
	Combined areas—incentives only			3	2	3	2	4	1

Legend: → Type A = Outlier before controlling for explanatory variables. Type B = Outlier after controlling for explanatory variables.
LFA - Labor Force Attachment. HCD - Human Capital Development.
Note: Negative outliers are defined as one standard deviation or more below the mean impacts in quarters 3, 7, 11 and 15.

Table 18
Stylized Cost-Benefit Framework of Welfare-to-Work Programs

	Society (B + C) (A)	Program Group (B)	Nonassignees (C)	Government (D)
In-program output	+	0	+	0
Earnings	+	+	0	0
Tax payments by program group	0	-	+	+
AFDC payments	0	-	+	+
Other transfer payments	0	0	+	+
Net program operating costs	-	-	-	-
Net benefits of program (column sum)	?	?	?	?

Table 19
Net Benefit-Costs of Interventions over Observation Period (year 2000 dollars)

	Mean		Standard Deviation		Median				
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Minimum	Maximum	No. of Obs.
Perspective									
Program group	$121	$122	$3,002	$2,932	$-54	$56	$-6,559	$11,040	49
Government	-236	54	3088	2950	246	436	-9207	5287	49
Nonassignees	-188	88	3133	2992	436	570	-9372	5287	49
Society	76	355	2928	2785	438	507	-5904	5966	49
Net cost to government	1,967	1,773	1,861	1,738	1,346	1,346	-290	8,613	49

Note: The observation period varied between evaluations, typically covering several years and, most frequently, five years.

Table 20
Regression Models of the Impacts of the Benefit-Cost Outcomes from Alternative Perspectives
(Standard errors in parentheses)

Perspective	Program Group		Government		Nonassignees		Society	
Constant	-964.33	-10487.83	1957.22	4672.17	2130.83	4628.66	1513.32	-5620.21
	(1658.9)	(6826.2)	(1719.0)	(6664.3)	(1713.6)	(6711.4)	(1627.5)	(6023.3)
Intervention impact on	-99.42	-48.45	20.84	64.83	4.74	50.07	-28.22	67.47
% sanctioned	(75.0)	(85.4)	(77.7)	(83.4)	(77.5)	(83.9)	(73.6)	(75.3)
Intervention impact on	31.99	26.28	-1.21	-44.04	-8.73	-50.11	7.71	-35.98
% participated in job search	(31.3)	(34.7)	(32.4)	(33.9)	(32.3)	(34.1)	(30.7)	(30.6)
Intervention impact on	53.82	46.87	-104.85**	-177.54****	-106.96**	-175.82***	-39.22	-105.54**
% participated in basic education	(44.6)	(57.8)	(46.2)	(56.4)	(46.1)	(56.8)	(43.8)	(51.0)
Intervention impact on	-217.98*	-282.42*	-91.74	29.59	-106.44	9.77	-352.74***	-323.63**
% participated in vocational education	(123.0)	(139.4)	(127.5)	(136.1)	(127.1)	(137.0)	(120.7)	(123.0)
Intervention impact on	70.13	29.83	-38.91	-4.73	5.03	40.53	84.08	78.98
% participated in work experience	(89.4)	(94.7)	(92.6)	(92.4)	(92.4)	(93.1)	(87.7)	(83.5)
Intervention included financial	3735.60***	4620.01***	-3686.69***	-5462.32***	-3830.52***	-5560.00***	-88.86	-766.41
incentive=1	(1103.3)	(1263.2)	(1143.3)	(1233.3)	(1139.7)	(1242.0)	(1082.4)	(1114.7)
Intervention included time limit=1	1612.98	1068.73	727.29	602.75	563.18	434.46	1956.49	1188.55
	(1326.6)	(1371.1)	(1374.7)	(1338.6)	(1370.4)	(1348.0)	(1301.5)	(1209.8)
Number of years since 1982	32.67	-116.14	-51.00	-53.02	-46.66	-41.14	-48.66	-196.12
	(156.4)	(174.5)	(162.1)	(170.4)	(161.6)	(171.6)	(153.4)	(154.0)
Two-parent family target group=1	-1541.57	-3159.42**	135.70	-1000.40	232.61	-892.60	-1566.91	-4331.89***
	(1143.9)	(1501.5)	(1185.3)	(1465.9)	(1181.6)	(1476.2)	(1122.2)	(1324.9)
Average age of target group		334.09		-219.28		-215.89		130.52
		(216.5)		(211.4)		(212.9)		(191.1)
% of target group with recent		67.84*		7.93		9.43		83.12**
employment		(36.8)		(35.9)		(36.1)		(32.4)
Annual % change in local		90.88		77.92		53.26		142.51
manufacturing employment		(153.2)		(149.5)		(150.6)		(135.2)
Maximum AFDC payment for a		-4.2		8.0**		7.9**		2.7
family of 3		(3.2)		(3.2)		(3.2)		(2.9)
Adjusted R-squared	0.256	0.280	0.210	0.322	0.238	0.332	0.206	0.378
F-test for contribution of added variables	2.834**	1.324	2.422**	2.599*	2.662**	2.370*	2.385**	3.703
Number of observations	49	49	49	49	49	49	49	49

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Table 21
Welfare-to-Work Evaluations Reporting Subgroup Impacts and Included in the Database

Program Title	County	AFDC Applicants	AFDC Recipients	Employed in Year Before RA	Not Employed in Year Before RA	With High School Diplomas	Without High School Diplomas	Long-Term AFDC Participants	Short-Term AFDC Participants
GAIN	Alameda		O	X	X			X	X
	Butte			X	X			X	X
	Los Angeles		O	X	X			X	X
	Riverside			X	X			X	X
	San Diego			X	X			X	X
	Tulare			X	X			X	X
Job Search and Work Experience in Cook County		X	X	X	X				
Community Work Experience Demonstrations									
WORK Program (Arkansas)	Jefferson	X	X			X	X		
	Pulaski South	X	X						
	Combined Jefferson & Pulaski South	X	X					X	X
Employment Initiatives (Baltimore)		X	X	X	X				
Saturation Work Initiative Model (SWIM, San Diego)		X	X						
Employment Services Program (Virginia)		X	X		X	X			
Project Independence (Florida's JOBS Program)	All Florida counties			X	X	X	X	X	X
Jobs First (Connecticut)	Manchester & Newhaven	X	X	X	X			X	X
The Family Transition Program (FTP, Florida)				X	X		X		
The Los Angeles Jobs-First GAIN Evaluation		X		X	X	X	X	X	X
The San Diego Job Search and Work Experience Demonstration		O		X	X		X	X	
National Evaluation of Welfare-to-Work Strategies (NEWWS)	Atlanta			X	X	X	X	X	X
	Grand Rapids			X	X	X	X	X	X
	Riverside			X	X	X	X	X	X
	Portland (OR)			X	X	X	X	X	X
	Columbus			X	X	X	X	X	X
	Detroit			X	X	X	X	X	X
	Oklahoma City	O		X	X	X	X	X	X

(table continues)

TABLE 21, continued

Program Title	County	AFDC Applicants	AFDC Recipients	Employed in Year Before RA	Not Employed in Year Before RA	With High School Diplomas	Without High School Diplomas	Long-Term AFDC Participants	Short-Term AFDC Participants
Minnesota Family Investment Program	Urban	X	X						
	Rural		X						
Vermont Welfare Restructuring Project.	Barre	X	X						
	All research districts	X	X	X	X	X	X		
Teenage Parent Demonstration	Camden	O							
	Newark	O							
	Chicago	O							
To Strengthen Michigan Families (TSMF)	All sites	X	X						
	Kalamazoo	X	X						
	Madison Heights	X	X						
	McNichols/ Goddard	X	X						
	Schaefer/Six Mile	X	X						
Virginia Independence Program (VIEW)	Lynchburg	X	X						
	Prince William	X	X						
	Petersburg	X	X						
Family Investment Program (FIP, Iowa)		X	X	X	X				
California Work Pays Demonstration Program	Alameda	X	X						
	Los Angeles	X	X						
	San Bernardino	X	X						
	Combined	X	X						
New York State Child Assistance Program (CAP)			O						

Note: X - subgroup impacts available; O - program targeted at subgroup.

Table 22
Mean Impacts of Welfare-to-Work Programs for Selected Subgroups

		AFDC Applicant (A) versus AFDC Recipients (R)							
		Percentage Receiving AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
Quarter		A	R	A	R	A	R	A	R
3		0.811	1.204	73.756	48.975	-27.827	33.553	2.268	2.508
	N	29	18	23	13	20	11	33	19
7		0.172	1.74	48.67	55.288	36.215	58.684	1.774	2.711
	N	23	16	13	15	16	11	29	19
11		-0.423	-0.064	34.665	31.314	127.552	52.244	1.375	2.677
	N	8	13	6	9	10	11	14	13
15		0.836	2.011	28.32	79.434	101.932	-33.243	1.895	2.604
	N	6	9	6	7	1	6	10	9

		Employed (E) versus Not Employed in Year Prior to Random Assignment (NE)							
		Percentage Receiving AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
Quarter		E	NE	E	NE	E	NE	E	NE
3		4.076	0.311	55.941	56.038	89.748	87.033	2.655	0.838
	N	3	3	5	6	4	3	3	3
7		2.652	2.929	81.223	92.407	72.949	143.747	1.466	3.001
	N	6	6	12	12	15	15	6	6
11		1.4	2.8	69.028	89.55	191.084	172.011	6	2.3
	N	1	1	1	1	8	8	1	1
15		-0.53	2.772			155.597	21.189	5.733	6.998
	N	2	2	0	0	2	2	2	2

(table continues)

Table 22, continued

		High School Diploma (HSD) versus No High School Diploma (nHSD)							
		Percentage Receiving AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
Quarter		HSD	nHSD	HSD	nHSD	HSD	nHSD	HSD	nHSD
3		-4.031	2.39	88.937	77.651	126.832	91.6	4.031	3.158
	N	8	9	10	11	10	11	9	10
7		-3.914	3.069	89.56	88.987	118.84	103.384	3.914	3.095
	N	8	9	10	12	11	11	8	9
11			5.76				117.52		5.4
	N	0	1	0	0	0	2	0	1
15		-5.629	4.542			71.687	74.759	5.629	4.941
	N	2	3	0	0	2	3	2	3

		Long-Term AFDC Recipient (LT) versus Short-Term AFDC Recipients (ST)							
		Percentage Receiving AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
Quarter		LT	ST	LT	ST	LT	ST	LT	ST
3		-3.4	-2.639	52.944	-39.897	5.701	12.952	8.211	1.749
	N	2	2	4	4	3	4	3	3
7		-1.753	-1.524	113.179	81.577	154.903	44.266	8.513	2.269
	N	5	5	16	15	12	16	5	5
11		-7.67				161.971	308.949	5.96	
	N	2	0	0	0	7	5	2	0
15									
	N	0	0	0	0	0	0	0	0

Table 23
Differences between the Impacts of Welfare-to-Work Programs on Subgroups (7th Quarter)
(Standard errors in parentheses)

Subgroup	Impacts							
	Participation in AFDC		Amount of AFDC Payment		Earnings		Percentage Employed	
	Mean Difference	N	Mean Difference	N	Mean Difference	N	Mean Difference	N
AFDC Applicants - AFDC Recipients								
Unadjusted, pure subgroups	-1.568	39	-6.618	28	-22.469	27	-0.937**	48
	(0.47) ***		(4.07)		(28.62)		(0.43)	
Regression adjusted, pure subgroups	-1.342	39	13.156	28	-24.565	27	-1.291**	48
	(0.50) **		(14.95)		(29.35)		(0.50)	
Regression adjusted, mixed groups	-0.659	70	-68.336***	60	**	82	-1.562***	84
	(0.55)		(12.77)		(23.27)		(0.44)	
Employed - Not Employed in Year Prior to Random Assignment								
Unadjusted, pure subgroups	-0.277	12	-11.184	24	-70.798**	30	-1.535*	12
	(0.79)		(12.26)		(25.73)		(0.75)	
Regression adjusted, pure subgroups	-0.252	12	-8.67	24	-58.96**	30	-2.203**	12
	(0.80)		(12.30)		(25.89)		(0.76)	
Regression adjusted, mixed groups	-0.647	61	-27.634**	69	-32.553	74	-0.93	60
	(0.79)		(12.76)		(27.92)		(0.75)	
High School Diploma - No High School Diploma								
Unadjusted, pure subgroups	-0.037	17	0.573	22	15.456	22	0.819	17
	(0.69)		(9.55)		(26.56)		(0.70)	
Regression adjusted, pure subgroups	-0.033	17	3.119	22	55.1*	22	1.038	17
	(0.69)		(9.58)		(29.12)		(0.70)	
Regression adjusted, mixed groups	-0.118	58	2.202	50	15.342	53	0.680	55
	(0.68)		(9.56)		(26.82)		(0.70)	
Short-Term AFDC Participants - Long-Term AFDC Participants								
Unadjusted, pure subgroups	0.229	10	-31.602***	31	-110.637***	28	-6.244***	10
	(0.93)		(10.68)		(21.84)		(1.24)	
Regression adjusted, pure subgroups	-0.651	10	-29.659***	31	-103.994***	28	-6.305***	10
	(0.93)		(10.77)		(24.37)		(1.00)	
Regression adjusted, mixed groups	-2.204	20	-42.433***	45	-95.76***	49	-5.408***	20
	(0.82) **		(10.85)		(24.12)		(0.96)	

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Table 24
Summary of Program Characteristics for Programs Reporting Child Outcomes

Program Characteristic	Connecticut	FTP	LA GAIN	NEWWS	MFIP	WRP	Iowa
Financial incentive	Yes	Yes	No	No	Yes	Yes	Yes
Time limit	Yes	Yes	No	No	No	Yes	No
Program impacts on the percentage who							
Were sanctioned (net)	3.0	22.0	21.0	9.1[a]	0	0	[b]
Participated in job search	17.5	20.6	31.1	24.1[a]	12.6	13.8	1.6
Participated in basic education	-0.1	9.8	-0.4	11.9[a]	-1.1	1.8	3.4
Participated in vocational education	0.2	13.1	-2.1	3.3[a]	-2.3	7.2	0
Participated in unpaid work experience	-1.1	6.7	4.6	3.1[a]	0.5	0	1.5
Net benefits received by individual assigned to program group in year 2000 dollars	6,103	1,759	732	-4,439[a]	8,993	1,619	434
Net government operating cost in year 2000 dollars	2,275	8,613	1,448	2,293[a]	200	1,323	180

[a]Average of NEWWS sites.
[b]Data not available.

Table 25
Summary Statistics for Child Outcome Measures

	All Observations		Young Age		School Age		Adolescents	
	Unwghted	Weighted	Unwghted	Weighted	Unwghted	Weighted	Unwghted	Weighted
Behavioral/Emotional Problems								
Mean value for controls	10.78	8.68	6.81	5.83	13.00	15.32	10.56	5.21
Mean effect size	0.01	0.00	0.05	0.05	-0.08	-0.06	0.07	0.02
Standard deviation	0.25	0.13	0.13	0.13	0.18	0.15	0.32	0.12
Median effect size	0.01	0.00	0.01	0.01	-0.02	-0.03	0.01	0.01
Minimum	-0.74	-0.74	-0.13	-0.13	-0.47	-0.47	-0.74	-0.74
Maximum	0.96	0.96	0.26	0.26	0.19	0.19	0.96	0.96
Q-test for homogeneity		418.39		78.70		183.21		114.82
Critical chi-squared at p=.05		55.76		14.07		25.00		26.30
Number of observations	41	41	8	8	16	16	17	17
Ever Suspended or Expelled								
Mean value for controls	17.61	15.38	8.09	7.56	13.01	13.08	26.69	22.60
Mean effect size	-0.04	-0.03	-0.04	-0.03	-0.04	-0.03	-0.03	-0.02
Standard deviation	0.18	0.09	0.06	0.05	0.12	0.09	0.26	0.10
Median effect size	-0.03	-0.02	-0.03	-0.02	-0.05	-0.04	-0.01	-0.01
Minimum	-0.91	-0.91	-0.16	-0.16	-0.21	-0.21	-0.91	-0.91
Maximum	0.39	0.39	0.04	0.04	0.20	0.20	0.39	0.39
Q-test for homogeneity		113.75		12.87		81.53		80.19
Critical chi-squared at p=.05		56.94		14.07		26.30		26.30
Number of observations	42	42	8	8	17	17	17	17
Repeated a Grade								
Mean value for controls	10.90	11.71	9.15	9.80	11.30	12.32	11.22	12.13
Mean effect size	0.04	0.05	0.12	0.06	0.03	0.03	0.03	0.06
Standard deviation	0.22	0.17	0.35	0.30	0.14	0.09	0.22	0.12
Median effect size	0.03	0.03	-0.02	-0.03	0.03	0.03	0.05	0.07
Minimum	-0.46	-0.46	-0.18	-0.18	-0.34	-0.34	-0.46	-0.46
Maximum	0.93	0.93	0.93	0.93	0.28	0.28	0.49	0.49
Q-test for homogeneity		693.23		432.16		82.93		115.64
Critical chi-squared at p=.05		56.94		14.07		27.59		25.00
Number of observations	42	42	8	8	18	18	16	16

(table continues)

Table 25, continued

	All Observations		Young Age		School Age		Adolescents	
	Unwghted	Weighted	Unwghted	Weighted	Unwghted	Weighted	Unwghted	Weighted
School Achievement								
Mean value for controls	13.49	10.12			9.75	8.76	18.00	12.73
Mean effect size	0.06	0.03			0.05	-0.01	0.06	0.11
Standard deviation	0.22	0.12			0.16	0.10	0.29	0.13
Median effect size	0.07	0.00			0.04	-0.06	0.07	0.13
Minimum	-0.30	-0.30			-0.23	-0.23	-0.30	-0.30
Maximum	0.77	0.77			0.30	0.30	0.77	0.77
Q-test for homogeneity		123.93				51.30		47.93
Critical chi-squared at p=.05		32.67				19.68		16.92
Number of observations	22	22			12	12	10	10
Positive Behavior								
Mean value for controls					74.20	44.90		
Mean effect size					0.07	0.01		
Standard deviation					0.10	0.02		
Median effect size					0.03	0.00		
Minimum					0.00	0.00		
Maximum					21.00	21.00		
Q-test for homogeneity						2.27		
Critical chi-squared value at p=.05						7.82		
Number of observations					4	4		
Health								
Mean value for controls					23.81	32.36		
Mean effect size					-0.03	0.01		
Standard deviation					0.11	0.08		
Median effect size					-0.04	0.04		
Minimum					-0.13	-0.13		
Maximum					0.08	0.08		
Q-test for homogeneity						24.80		
Critical chi-squared value at p=.05						7.82		
Number of observations					4	4		

Table 26
Regression Models of the Impacts of Welfare-to-Work Programs on Childhood Behavioral/Emotional Problems
(Standard errors in parentheses)

	Model 1	Model 2	Model 3
Constant	0.003	-0.029	-0.088
	(0.02)	(0.05)	(0.04)
School-Age dummy	-0.049**	-0.061***	-0.066***
	(0.02)	(0.02)	(0.02)
Adolescent dummy	-0.016	-0.006	-0.006
	(0.02)	(0.02)	(0.02)
Intervention included financial incentive=1	-0.135***		
	(0.02)		
Intervention included time limit=1		-0.071***	
		(0.02)	
Program impacts on the percentage who:			
Were sanctioned		0.008***	
		(0.00)	
Participated in job search			0.002
			(0.00)
Participated in basic education			0.004**
			(0.00)
Participated in vocational education			-0.008
			(0.01)
Participated in unpaid work experience			0.024**
			(0.01)
Net cost to the government in 1,000s of year 2000 dollars	0.022***		
	(0.00)		
Adjusted R-squared	0.202	0.232	0.174
F-test	2.956**	3.951***	2.404**
Number of observations	31	39	40

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.
Note: Regressions were also calculated by imputing the mean for missing values, with virtually the same results.

Table 27
Summary Statistics for Voluntary Program Impact Measures

	All Observations		High-Cost Programs		Low-Cost Programs	
	Unwghted	Weighted	Unwghted	Weighted	Unwghted	Weighted
EARNINGS						
7th quarter since random assignment						
Mean value for controls	1109.4	1016.0	1086.6	1168.1	1132.2	873.7
Mean impact	198.1	165.0	229.0	166.8	167.1	163.2
Standard deviation	209.6	132.3	132.3	132.3	184.9	122.8
Median impact	209.6	120.3	225.0	131.1	213.9	109.5
Minimum	-170.1	-170.1	37.8	37.8	-170.1	-170.1
Maximum	545.2	545.2	545.2	545.2	371.8	371.8
Q-test for homogeneity		10.1		5.7		5.1
Critical chi-squared value at p=.05		16.9		9.5		9.5
Number of observations	10	10	5	5	5	5
11th quarter since random assignment						
Mean value for controls	1149.7	1134.2	1191.9	1149.6	1097.0	1110.6
Mean impact	480.7	545.1	674.1	712.1	239.0	290.5
Standard deviation	482.2	578.7	601.3	601.3	209.3	317.6
Median impact	295.8	280.5	200.6	200.9	198.5	172.4
Minimum	55.1	55.1	463.8	463.8	55.1	55.1
Maximum	987.4	987.4	987.4	987.4	482.2	482.2
Q-test for homogeneity		25.5		8.2		4.6
Critical chi-squared value at p=.05		15.5		9.5		7.8
Number of observations	9	9	5	5	4	4

(table continues)

Table 27, continued

	All Observations		High-Cost Programs		Low-Cost Programs	
	Unwghted	Weighted	Unwghted	Weighted	Unwghted	Weighted
AMOUNT OF AFDC PAYMENTS						
7th quarter since random assignment						
Mean value for controls	1214.2	1022.2	1403.1	1136.2	1025.4	916.4
Mean impact	496.6	417.4	811.5	668.2	181.7	184.7
Standard deviation	377.0	344.0	652.4	541.8	90.1	65.0
Median impact	507.9	445.4	546.8	512.6	182.8	162.3
Minimum	-0.6	-0.6	126.7	126.7	-0.6	-0.6
Maximum	1433.9	1433.9	1433.9	1433.9	409.9	409.9
Q-test for homogeneity		267.9		173.2		18.6
Critical chi-squared value at p=.05		16.9		9.5		9.5
Number of observations	10	10	5	5	5	5
11th quarter since random assignment						
Mean value for controls	898.1	768.8	922.8	736.4	867.2	821.6
Mean impact	227.6	186.7	345.2	236.7	80.6	105.5
Standard deviation	124.5	124.5	141.4	124.5	43.0	107.2
Median impact	343.2	291.2	426.8	350.3	138.8	112.5
Minimum	-30.1	-30.1	14.2	14.2	-30.1	-30.1
Maximum	1071.9	1071.9	1071.9	1071.9	266.3	266.3
Q-test for homogeneity		96.7		74.5		4.5
Critical chi-squared value at p=.05		15.5		9.5		7.8
Number of observations	9	9	5	5	4	4

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